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26. *Ship Fever.—The Fiske Prize Dissertation.* By HENRY GRAFTON CLARK, M.D.—*Boston*, 1850. 8vo, pp. 48.

The Diagnosis of Fevers.—If we had been asked some twenty years ago to find an appropriate simile for the disease called Fever, we could have made no better comparison than with our serviceable friend Proteus. Fever, we should have said, is something of everything, nothing of its own; you never find it in the same costume. It transforms itself like a Harlequin, and when you think you have at last got it into a corner, it disappears into the depths below, and rises at the other end of the stage in the likeness of Pantaloon. In Paris, in London, and in Edinburgh, it presents itself with a different physiognomy, or else, "like Cerberus, it is three gentlemen at once." Attempt to draw its portrait, and you make as great a medley as Blumenbach would have done, had he compounded an ideal skull out of his immense collection. It is as difficult as Etruscan, and is to be deciphered by guess-work, like the arrow-headed writing. It is like the sphynx, for it devours its interrogators, and gives them no clue to its riddle. And it is even worse than the sphynx, for that did yield to an Œdipus; whilst the solution of *this* enigma is yet to be found.

In some such despairing strain we should, twenty years ago, have spoken of a disease, which was said to be located in every organ of the body, and yet could be found no where; which presented all symptoms, yet apparently none twice over; which was cured in all ways, but seldom in the same way by any two persons; and which was designated by all sorts of names, the recommendation of each of which was, that it was as applicable as any other. This uncertainty and discrepancy of observation does not, however, now prevail to anything like the extent it formerly did. The difficulties in the study of fevers have yielded, or appear to be yielding, before the close and incessant observation with which they have been regarded. The inquiries even of twenty years since, appear coarse and narrow by the side of our greater knowledge; and we look forward to the speedy coming of the time, when even our observations will seem superficial and ill-directed to the clearer light of advanced intelligence.

We propose now to give as full an account as our limits will permit, of the inquiries which have lately been carried on, respecting the alleged specific differences of several diseases, which were formerly included under the common designation of fever. Our aim is not to give a history of fevers, to narrate their numerous synonyms, or to ascertain the various forms which each observer saw pass before him. So great a scope would be as opposed to the single object we have in view, as it would be impossible to comprise within the space at our command.

It has been asserted within the last few years, that the very different descriptions given of the symptoms and post-mortem lesions of the disease, called in this country, "Continued Fever," are attributable, not to any variations in the character of this fever, but to the presence of two or three diseases, allied by certain common characters, but separated also by peculiar and distinctive marks, and which, being mixed up together in almost every epidemic in various proportions, gave to each epidemic a peculiar character according to the nature of the dominant disease. Such an opinion is not altogether a recent one; but it is only lately that the arguments in its favour have reached a breadth and cogency, entitling it to a dispassionate hearing, if not to an unconditional support.

The easiest way of discussing this opinion of a diversity of diseases having been confounded under the term "Continued Fever," will be to examine as fully as we can the several parts of which this presumed group of diseases is supposed to be made up. We will commence with the certain facts, and proceed afterwards to the more doubtful and disputable opinions.

I. *Let us then first examine whether there is any disease, heretofore included under the term "Continued Fever," which is of so marked and peculiar a character, as to allow us at once to separate it from the affections with which it has hitherto been confounded?*

In the early part of 1843, a febrile disease appeared in Edinburgh and Leith, so different in its course, in its symptoms, and in the amount of its mortality, from any continued fever which had been recently observed there, that it was at once and unhesitatingly declared to be a peculiar and new disease. It was soon known that the same disease had appeared in Glasgow a month or two prior to its outbreak in Edinburgh;* it was more or less prevalent also in Dundee and in other large towns in Scotland, whether it appeared in London or in other English towns is doubtful; at any rate it was not described. It was observed with great accuracy, and recorded in the periodicals of the time, by Craigie,† Alison,‡ Arrott,§ Henderson,‖ Halliday Douglas,¶ Jackson,** and Mackenzie;†† and it was made the subject of two special and excellent treatises by Cormack‡‡ and Wardell.§§

It was soon discovered, however, that although this fever had not been seen in Edinburgh for many years, it was not altogether a new disease. Dr. Christison expressed an opinion that it was similar to the fever witnessed by him in 1817-18, and recorded by Welsh‖‖ in his well-known work, and by himself in the 'Library of Practical Medicine.' That this opinion is correct, and that the Edinburgh epidemic of 1817-18, was in great measure made up of this disease, no one can doubt, who will attentively collate the descriptions of Welsh and Christison, with those furnished

* On the Epidemic Fever prevailing in Glasgow. By David Smith, M.D. (Edinb. Med. and Surg. Journ., 1844, p. 67.)

† Notice of a Febrile Disease which has prevailed at Edinburgh during the Summer of 1843. By David Craigie, M.D. (Edinb. Med. and Surg. Journ., vol. lx, p. 410.)

‡ Scottish and North of England Medical Gazette, No. 1, p. 1.

§ Scottish Medical Gazette, p. 129.

‖ On the present Epidemic and on the Characters which distinguish it from Typhus. (Edinb. Med. and Surg. Journ., 1844, p. 201.)

¶ Statistical Report of the Edinb. Epid. Fever of 1843-4. (Northern Journal of Medicine, Nov., 1844, and Feb., 1845.)

** Epidemic Fever at Leith. (Edin. Med. and Surg. Journ., 1844, p. 417.)

†† The Epidemic Remittent Fever in Glasgow in 1843. By William Mackenzie, M.D. (London Medical Gazette, Nov., 1843.)

‡‡ On the Epidemic Fever at present (1843) prevailing in Edinburgh and other Towns. By John Rose Cormack, M.D.; and Additional Remarks on the Edinburgh Epidemic. By J. R. Cormack. (Medical Gazette, April, 1849.)

§§ On the Scotch Epidemic Fever of 1843-4; in which it is maintained, that the Disease essentially differed from the ordinary Forms of Continued Fever witnessed in this country. By John Richard Wardell, M.D. 1848.

‖‖ A Practical Treatise on the Efficacy of Bloodletting in the Epidemic Fever of Edinburgh. By Benjamin Welsh, M.D. Edinburgh, 1819, pp. 356.

by the observers of the attack in 1843.* But not only in Edinburgh was evidence of its former existence brought forward. It was observed that it had evidently formed part of the Irish fever of 1817-18-19, which had been so minutely recorded by Barker and Cheyne.† It appeared also that this fever had been prevalent in Ireland for many years. Epidemics in 1739 and 1741 were described in unequivocal terms by Rutty; and other epidemics during the eighteenth century, and those in Dublin in 1806 and 1826, presented, among other forms of fever, the peculiar and unmistakeable features of the disease in question.

The fever thus distinguished and elaborately described by the Scotch observers in 1843, became again epidemic in Glasgow, Edinburgh, and other towns in Scotland, in 1847. It did not by any means prevail so extensively, and there was a simultaneous occurrence of other species of fever; among which, however, the eyes of Steele, Paterson, Orr, and others, trained by the epidemic of 1843, had no difficulty in singling out this peculiar form. In 1847 it was also epidemic in London; and having come under the observation of Dr. Jenner, has been very carefully described. Before this time, in 1846, and in previous years, sporadic cases had been witnessed in the metropolis; and since 1847 a case has every now and then presented itself at the London Fever Hospital, and at other institutions.

Instructed by the experience of these observers, it is also easy to perceive that this disease is not confined to these islands. It appears, although imperfectly described and confounded with other forms, in the pages of the celebrated treatise of Hildenbrand;‡ and in the epidemic in 1847 in Upper Silesia, which we intend presently to describe, it evidently formed in some places the great bulk of the cases. Yet it must be said, that although its characters are so striking that the most superficial observer could not overlook them, the German systematic writers§ have made no allusion to it as a separate disease; and even those who observed it have failed to draw that obvious inference to which the Scotch physicians unanimously came, viz., that it is a disease altogether distinct from ordinary continued fever.

We are not aware that any perfectly satisfactory evidence is to be found in French writers of the existence of this fever in France;‖ it has in fact, considering the elaborate descriptions of the Scotch physicians, been some-

* Dr. Christison describes this fever under the name *synocha;* and we believe him correct in surmising, that Cullen actually had this disease before him when he employed this term, although there may be another disease like Cullen's *synocha.*

† An Account of the Fever lately epidemical in Ireland. By F. Barker, M.D., and J. Cheyne, M.D. Two vols. 1821.

‡ Ueber den ansteckend: Typhus. Wien, 1810 and 1815.

§ Canstatt and Wunderlich, for example.

‖ Although, in systematic French writers, this fever is not described with such precision as to enable us unequivocally to recognise it, yet there are some grounds for thinking that it has been observed there frequently. Davasse, in an able inaugural dissertation, has described a short fever, beginning suddenly, and terminating in six, seven, or eight days by sweat and sudden recovery. But nothing is said of relapses or of yellowness. Cullen may have included this short fever under his term "synocha." If different from relapsing fever, it is certainly a rare disease in this country; at least it is so in London, and we presume elsewhere. Since this was written, the French correspondent of the 'Medical Times' has stated, that this fever has been witnessed at the Hospital "pour Enfans Malades." (Feb. 8, p. 158.) Wardell states it to be most common between the ages of 20 and 40; but he has made no correction for population. He says, also, that it is more common in females than males (597 to 494); but here, also, the actual number of each sex in the locality of observation is not given. Halliday Douglas, in 215 cases, had 122 males to 93 females; Welsh, in 743, had a preponderance of females (viz. 424 to 319); but these were probably not all cases of relapsing fever. Halliday Douglas what singularly overlooked in that country, as in Germany.* In America it is not, we believe, known, or at any rate it has not been described. There is reason from Dr. Bell to believe, that it is known in Persia.

The disease to which we allude has received a variety of names, such as the "short fever;" the "five-day fever;" the "seven-day fever;" the "bilious remittent fever;" the "remitting icteric fever;" the "mild yellow fever," &c; but the name by which it is most commonly known, and which, although not free from objection,† is yet useful as pointing to one main feature, is the title we shall generally use in speaking of it, viz. the *Relapsing Fever*.

The characters of this disease, which render its diagnosis so easy a matter in the great majority of cases, do not demand from us, on the present occasion, any lengthened description.‡ We shall select merely the most prominent symptoms, the simple enumeration of which will suggest to our experienced readers the fundamental differences as to the phenomena, which distinguish this from other forms of fever.

Relapsing Fever affects all ages and both sexes, and perhaps in an equal ratio.—In its onset there does not appear to be anything distinctive, unless the suddenness of the accession, and the severity of the early muscular and articular pains, lead to a suspicion of the real affection. But after two or three days, the symptoms, although not absolutely distinctive, become more marked; the feverishness is considerable; the muscular pains and headache severe; and on the second or third day there are, for the most part, more or less severe pain and tenderness about the epigastrium, and vomiting; there is, however, no other abdominal tenderness, and diarrhœa is generally absent. The heat of skin is alternated both with rigors and sweating, so that the resemblance to an irregular intermittent has been noted by several writers. On the third or fourth day, the symptoms are at their height; and a typical case, that is a case presenting the main diagnostic symptoms, and no others, can be generally known by the slightness of the head-symptoms, the chief being headache, and in a small proportion of cases (about 8 per cent., Halliday Douglas,) delirium, by the absence of chest-symptoms, and by the presence of epigastric and splenic tenderness, and vomiting, engrafted on a severe pyrexial state; that is to say, a state characterised by great restlessness and sleeplessness, a hot skin, the temperature of which may rise to 107,° (Wardell, p. 46,) § a white tongue, thirst, and a pulse which is seldom below 100, in more than half the cases more than 120, and in a considerable number is still higher than this, yet whose rapidity and sharpness are not indicative of commensurate danger.‖ In a certain

says, that from 10 to 30 is the most susceptible age; but here, also, there is no correction for population. In 215 cases, there were 27 cases over 50.

* It has been presumed by Dr. Spittal, that Hippocrates observed this fever in the island of Thasus, on the coast of Thrace.

† For example, as pointed out by Henderson, it is erroneous to call that phase of the disease a relapse, which is a constituent part of the ordinary course of the disease. It is, however, not an invariable constituent.

‡ For a very excellent article on this fever, see 'British and Foreign Medical Review,' Vol. XVIII, p. 179.

§ The highest temperature noted by Halliday Douglas was 102 degrees. It is not stated whether this was derived from observation on all his 220 cases.

‖ Henderson found its average frequency, in 38 cases, to be 125 on the fifth day of the disease. In "typhus" of the same date, the average was only 100 in 15 cases.

number of cases, as more particularly noticed below, on the third or fourth day, a peculiar tint of skin becomes perceptible; to use Dr. Cormack's expression, there is a slight "bronzing," which is most marked in the face; this appears to be the commencement of an approaching attack of jaundice, which becomes more fully declared on the fifth or sixth day of disease; the vomiting is now often severe, the matters vomited being bilious, (bright grass-green, Jenner,) or sometimes even coffee-ground like, (Wardell, p. 30,) or being absolutely like the black vomit* of yellow fever, (Cormack.) This jaundice is not attributable to any obstruction in the ductus communis choledochus, as bile passes freely, and even copiously, with the stools, and as after death the gall-duct is pervious. In these yellow cases there is generally tenderness over the liver, which may be also enlarged. The spleen is also often enlarged. If the patient be now bled, the blood is often buffed, (Welsh and Jenner,) and the serum is sometimes yellow, sometimes unusually green.

A day or two after this, when every symptom appears hourly becoming graver, when the restlessness and general distress have reached their highest point, there ensues, in the majority of cases, though not in all, a most remarkable series of symptoms, followed by as remarkable an intermission of all symptoms, and an apparent restoration to health. This period has received the name of "Crisis," although it would have been very desirable if some other term than this, to which so many meanings have been given, had been chosen. For the most part, at this period, the patient falls into a profuse sweat, which lasts sometimes for thirty to thirty-six hours, but is usually shorter than this. The chemical qualities of this sweat have not yet been determined; but it has, like the partial sweats which have previously occurred, a very sour and peculiar smell. When the sweat has passed off, an extraordinary change is found to have taken place; the hot skin has become cool; the quick and strong pulse is feeble and slow;† the feelings of distress and discomfort have disappeared; and in severe cases are succeeded by a state of excessive languor and feebleness, as if the person had been reduced by some immense hæmorrhage. After rallying from this, perfect convalescence seems to have commenced, and the yellow tinge begins to disappear, and in four or five days may have altogether vanished. This so called "crisis" is not always accompanied by sweating; a discharge of some other kind may occur in its place, such as diarrhœa, epistaxis, diuresis, or even sometimes menorrhagia.‡

There seems no doubt that this apparent end of the disease may be the actual one; but in a certain number of cases another phase occurs. After six or seven days of improvement, and (taking the mean) on the thirteenth, fourteenth, or fifteenth day of the disease, the fever suddenly returns. This second attack, the so called "*relapse*," exactly resembles, except it may be in point of severity, the primary attack; the shivering, the severe muscular and articular pains, the restlessness, discomfort, and sleeplessness, the burning skin, the rapid pulse, present themselves over again. If jaundice has not been present in the first accession, it may

* In 220 cases, however, Halliday Douglas never saw anything like "black vomit," unless there was dysenteric or peritonitic complication. Dr. David Smith, also, "never saw black vomit."

† Robert Paterson has observed it to fall from 160 to 45, and in two cases to 40.

‡ Paterson also observed, that the size of the spleen became reduced in an extraordinary manner after the crisis.

appear on the second or third day of the second accession. After four or five days, the symptoms begin to improve, the feverishness abates, and about the twentieth day of the disease, the patient is really convalescent. In an uncertain number of cases, this second accession is terminated by a "crisis" similar to that of the first.*

The disease may thus terminate, either gradually or suddenly, and in the greater number of cases it does really so end. In some cases, however, the second accession is not only terminated by a crisis, but this crisis is succeeded at an interval of four or five days by another accession, which may again be followed by crisis, intermission, and a fourth accession. No less than five of these accessions, or *relapses*, have been known to occur.

Various sequences follow this fever, of which the most remarkable are a form of ophthalmitis, (well described by Mackenzie,) rheumatic-like pains, parotitis, (which may also occur during the fever,) anasarca, and furunculi.

The disease, whose main features we have thus imperfectly indicated, although comparatively unknown ten years ago, has been so attentively studied by British practitioners, that we are, perhaps, better able to determine the mode of succession and the variation in the symptoms, by means of numerical analysis, than in the case of any other fever. Our space, however, will not permit us to do more than glance at the data which bear on the more prominent symptoms. To enter thus far into the subject, may, however, be expedient, as the disease is not generally known, particularly on the continent.

1. *Cutaneous eruptions.* All the recent English writers, with one exception, agree that the peculiar eruption which occurs in typhus was absent.† Welsh, indeed, speaks of a "measly-looking efflorescence, as likewise a duskiness and livid mottled appearance," (p. 21;) but as there is no doubt that he includes cases of typhus in his description, no use can be made of the remark.

Robert Paterson, however, as noticed in the note below, observed occasionally a "measly eruption;" but the number of cases in which it was noted is not given. Halliday Douglas, in one case, found an eruption, quite distinct in all its characters, from the eruption of typhus; Cormack, in one or two cases, (pp. 70, 73,) also observed an eruption, which, however, he also found no difficult matter to distinguish from the exanthematic typhus. To this eruption, which appeared in the Scotch epidemic to be extremely rare, we shall return in speaking of the Silesian epidemic.

Although there was this almost universal absence of any eruption at all resembling that of typhus, a certain number of cases presented petechiæ,

* This crisis of the relapse occurs, according to Halliday Douglas, on the third day (average); but in his cases it was later in females (fourth day) than in males.

† Craigie gives, as one reason for distinguishing it from typhus or synochus, that there were no spots or eruption, like typhus. Halliday Douglas says, that eruption was diligently sought for in all (220 cases), but in only one case (alluded to in the text) were any spots present. Cormack, in his book, says, the "rosy elliptical eruption, resembling measles, is absent in almost every case;" but the word almost is expressly struck out in the paper in the 'Medical Gazette.' In 1200 cases, Wardell found no eruption like typhus. Alison says, "eruption never occurs." David Smith says, the "petechiæ were perfectly distinct from the exanthematic eruption of typhus." Robert Paterson, who forms the exception above alluded to, remarks, that all his typhus cases but four had a measly eruption; but of 141 cases of relapsing fever, two only had a petechial eruption. He afterwards remarks, however, that "this eruption (measles) was not confined to typhus, but was occasionally seen in the relapsing fevers, but only in the first attack." (Op. cit., p. 404.) A very important observation, as we shall presently see.

the so called "purpuric spots." These occurred in the bad cases, and it appears that they were most common in yellow cases. Sudamina were present in some cases; Wardell says, often; the number was, however, usually not great. Douglas noted them in only 12 cases out of 220.

Another symptom was presented by the skin in a great number of cases. This was an eruption of small spots, round, purple, unaltered by pressure, and extremely like flea-bites, in fact so like, that even now it is considered doubtful by many, whether they are to be regarded or not as a true eruption.* But the fact, that in this fever so many patients presented these spots, while the subjects of other diseases, though brought from the same locality, were not flea-bitten, seemed to indicate, that if these spots were merely flea-bites, there must be some kind of proneness to be affected by the bites, from which those who were free from the disease were exempt. This seems to be Dr. Alison's opinion, who thinks that the spots at any rate begin in flea-bites. But others have held them to be perfectly distinct. Thus David Smith states that the patients are often covered by them in a single night, while healthy persons sleeping in the same bed are quite exempt; and Jackson, to solve, if possible, this knotty point, caused two patients to be flea-bitten, and affirms that he found the bites to go through their usual course, and to be distinguished without difficulty from the eruption. Jenner noticed the same appearance in London, but seems doubtful as to the nature of the spots. It is certainly surprising that the characters of so common an appearance as a flea-bite should not yet be so accurately known, as to enable us to say whether the spots in this fever were from this cause or not.†

2. *Yellowness.* The number of cases in which this occurs, is not yet known; but it seems to be a very variable symptom. In 1843 it was more common in Dundee than in Edinburgh, and in Edinburgh than in Glasgow;‡ and it is believed to have been more common in 1843 than in 1847.§ It was probably rather unusual in 1817, as Welsh terms it a "trifling symptom," and noted it only in 1 in $30\frac{23}{24}$ cases (p. 73). It occurred in only 29 out of Halliday Douglas's 220 cases. Of these 29 cases, in 19 the jaundice occurred in the first attack; in 10 in the second; in none did it appear earlier than the fourth day; and in 5 it appeared with the first crisis. Jackson saw it in 31 of 300 cases. David Smith, in 1843, found it more frequently, viz. in 384 out of 1000 cases.‖ Wardell says it was present in one eighth of the cases in Edinburgh and Glasgow; in one fourth in Dundee. Robert Paterson, on the contrary, in 1847-8, in Edinburgh, saw it only four times in 141 cases. Jenner has found it more common in London, and considers that it occurred in nearly one fourth of his cases.¶ In the Silesian epidemic, it appears to have been

* The spots wanted, in most cases, the central point which can be seen in many flea-bites; but still this is no absolute distinction, as the point cannot always be found in flea-bites. This eruption was present, not only in the recent epidemic, but probably in that also of 1817, and is described accurately by Christison, (Library of Medicine, vol. 1, p. 141.)

† We have not alluded to the "taches bleues," which have been seen in typhoid fever, but are not proper to this disease. They have been lately often noticed by Davasse in the fever which he terms "synocha," and which we have before referred to, as a six- or seven-day fever, terminating in a critical sweat, and which certainly much resembles relapsing fever, although relapses are not noticed. But we are not aware whether the eruption has been seen in unequivocal relapsing fever.

‡ Steele, op. cit.

§ James Paterson, op. cit.; Robertson, op. cit.

‖ At least, he speaks in the previous page of 1000 successive cases

¶ Medical Times, Dec., 1850, p. 646.

comparatively uncommon. The tint of the yellowness varies extremely; sometimes it is very deep; at others a mere dinginess.

3. *The crisis.* The phenomenon termed "*crisis*" is very common.* Welsh noted it in 648 of 743 cases, (p. 75.) Halliday Douglas in 83 of 121. Wardell found it "almost always" present. It is, however, not invariable; and some patients, about the time when the "crisis" should occur, begin to improve, without any notable discharge marking a sudden transition from one state to another. The date of the crisis in 83 cases noted by Halliday Douglas, was the sixth day; the earliest date was the fourth day (2 cases), and the latest the tenth (4 cases). Wardell makes the average to be the seventh day, and the limits of variation thé fourth and ninth. Jackson places the crisis at the eighth day. Robert Paterson's average from 84 cases was the fifth day, or between it and the sixth,—Jenner's, the same.

Halliday Douglas has attempted to determine the effect of sex on the date of the crisis. It appeared to occur in females earlier than in males, in one case the average being the sixth day, in the other approaching the seventh. The effect of age on the crisis has not been absolutely determined. It seems earlier in youth. Remedies appear to have no influence on the date.

4. *The relapse.* From the crisis to the relapse there is a period of perfect intermission† in most cases; in some, however, the feverish symptoms continue, but are infinitely less severe. After this intermission or remission, the second accession, the so-called relapse, suddenly comes on, although in some cases it is absent. Welsh only noted it in 133 of 743 cases. Craigie in 110 of 182. Halliday Douglas found it in 218 of 220 cases, and in these two exceptional cases there was some doubt whether it did not occur. Cormack says the relapse is almost invariable if antiperiodic remedies are not used (p. 86). Wardell also says, relapse was "almost invariable." David Smith found it in 712 of 1000 cases. Omitting Welsh's cases, the diagnosis of which is uncertain, relapse is found to occur in almost 70 per cent. in the above noted cases, and it is probable that its frequency is even greater than this.

The relapse occurs, on an average, on the fourteenth or fifteenth day; but it has greater limits of variation than the crisis. Halliday Douglas's average (from 140 cases) is the fifteenth day, and his extremes are the ninth and the forty-eighth days. Jackson fixes on the ninth to the sixteenth day as the usual limits. Robert Paterson's and Robertson's average is the fourteenth day.

The influence of sex on the relapse, appears from Halliday Douglas's observations, to be similar to that on the crisis. Females relapsed both earlier and more regularly than males. Age appears to influence it slightly; it is earliest in youth, and latest in old age, but the differences are not very great. Remedies, according to Cormack, can, in some cases at any rate, prevent its appearance;‡ but, on the other hand, Robertson says that quinine, bebeerine, digitalis, and arsenic were equally inefficacious; and it is confirmed by Robert Paterson, who says that quinine, arsenic, and bebeerine had no influence in stopping the relapse, "which

* Robertson observed, "in five or six cases (out of 589 cases, we presume,) a remarkable form of delirium to precede immediately the sweating crisis. This has been noted, also, in Dublin and London.

† Or there may be pains and soreness in the muscles, like remnants of the previous severe pains.

‡ See expression previously quoted.

came on like a fit of ague almost to an hour." (Op. cit., p. 406.) H. Douglas also gave quinine in 24 cases, and in 22 relapse came on. Smith also tried very fairly the effects of emetics, diaphoretics, and other evacuants, in 150 patients seen on or before the third day of the disease, and in 50 patients seen after this date. In none was the disease arrested; and Smith concludes, "that this disease cannot be cut short by any means."

The second crisis.—The frequency of a crisis of the relapse has not yet been numerically determined. The time at which it occurs is usually on the third or fourth day after the relapse, and about the eighteenth or twentieth of the disease. H. Douglas found it later in females (fourth day), than in males (third day).

6. *The second relapse.*—The number of cases in which this occurs is undetermined. The time of its occurrence in Halliday Douglas's cases was from the eighteenth to the thirty-sixth day.

7. The frequency of a crisis of the second relapse, of a third, fourth, and fifth relapse, and the corresponding crises, is not known, but a third relapse does not appear very uncommon; there seems reason to believe, as observed by Cormack, that after convalescing from the first relapse, many patients leave the Hospital, and undergo one or more relapses, which are usually slighter, at their own homes.

8. *Mortality.*—In uncomplicated cases, scarcely any die; yet in some rare instances, as noted by Jenner and Douglas, sudden collapse may come on, either in the primary or secondary attack, and the patient may die suddenly. In some *yellow* cases also, and in some severe cases, which are not icteric, the patient dies from ureal poisoning.* In other cases in which the result is fatal, this seems to result from complications, either thoracic or abdominal, such as pleurisy, pneumonia, and dysentery. The fatal cases averaged in 1843 from 2 to 6 per cent. In 1847, when the fever was more severe, the average in 2333 of Steele's cases at Glasgow, was 6·38 per cent. At the same period in Edinburgh, 639 cases of Robert Paterson gave only 3·14 per cent. Halliday Douglas, in 1843, had nineteen deaths in 220 cases, or 8·63 per cent.

9. A very marked feature in relapsing fever is the frequency of abortion in pregnant women. This is, however, not invariable, as believed by some. Observations are yet wanting as to the time most liable to abortion.

Our space will not permit us to extend our analysis to other particular symptoms, such as headache, delirium, vomiting, &c.; and at present we are hardly able to give a perfect numerical statement of each symptom, although much information has been collected here also. Our present purpose, likewise, does not demand such an extended notice.

The post-mortem appearances in this fever need be described no farther than to say, that although in many cases there is congestion of, and in some instances extravasation of blood into and beneath, the mucous membrane of the stomach, and in a less degree of the intestines, yet Peyer's patches remain without deposition and ulceration. The spleen is generally enlarged, and according to Jenner, this occurs more frequently and to a greater extent than in any other form of fever. Robertson observed also some kind of deposit in the spleen, which he could not identify with the typhoid exudation, but which, like it, underwent a process of softening.

* Urea has been detected in the blood by Maclagan and Taylor. Wardell, also, has observed these deaths from ureal poisoning.

This detail of symptoms must be sufficient to convince any one, of the difference between this and other forms of fever. To take only the most striking symptom, the relapse, it appears that this is hardly known except in this disease. In "many hundred" cases (1600 to 2000) of typhus, Henderson has never known a relapse; and Jenner, in his extensive field of observation, has also never yet seen such an occurrence in typhus. In the fever, or variety of fever, termed typhoid, or Dothinenteritis, relapse, as noted by Stewart and Jenner, will occur, but it is exceedingly rare. The majority of the so-called relapses, in typhus and typhoid fevers, are simply sudden superventions of some complication, or a sudden exacerbation of some previously existing complication, as pneumonia, pleurisy, &c. In 1145 cases of typhus, treated by Perry, in Glasgow, there were 19 of these so-called relapses, which were all traced to some local inflammatory action.* But putting the relapse aside, the course of the remaining symptoms is completely dissimilar to that in other fevers; and the absence of the cutaneous eruptions common to other forms, is also a strong proof of their non-identity. When to these facts we add the invariable absence of the anatomical sign of Dothinenteritis, viz. the affection of Peyer's patches, the argument against the identity of relapsing fever, and the disease described by Louis, becomes almost absolute; and though the anatomical signs of exanthematic typhus are not so definite, still there are perceptible differences in this case also.

It is therefore not surprising, that the Scotch physicians, who have described the epidemics of 1843 and 1847, should have so unanimously decided on the specific nature of this fever; and the evidence in favour of this view will, we think, appear to every one sufficiently exact.

But, in addition, another most convincing argument in favour of the same opinion has been recently brought forward by Dr. Jenner, which proves that cases of other forms of fever do not give rise to relapsing fever, and that exposure to relapsing fever gives rise only to a similar disease, and not to another form of fever.

Before passing from the subject of relapsing fever, we may remark, that it can probably return several times in the same subject, and even at intervals of some few months only. In this, also, it shows a remarkable variation from the other English fevers.

Relapsing fever appears to predispose to typhus fever, and to be also predisposed to, by typhus. Steele remarks, indeed, that patients who had formerly suffered from typhus, "enjoyed an immunity from this disease;" but this is contrary to the direct and positive evidence of many other observers.

Contagion, in some instances at least, is admitted by all, except Dr. Craigie, who thinks that "although it is, perhaps, contagious, this is rather a presumption than a well-founded inference." (p. 417.) The observations of Douglas and Jenner, however, seem conclusive on this point.

Like other fevers, this disease is influenced in an extraordinary degree

* Hamernjk refers these relapses of typhoid fever to absorption of the deposit from the intestinal mucous membrane by the veins. To the obvious objection, that there is always more or less absorption through the mesenteric glands without relapse occurring, he advances the *hypothesis*, that the absorption of what is poisonous when taken up by the veins, is harmless when taken up by the lacteals.

by the sanitary condition of the population attacked by it. This has been traced out in Glasgow, with care, by David Smith. Wardell says, this fever "was importantly connected with destitution."

Having thus separated Relapsing Fever from the disease, which it has been customary, in this country, of late years, to call "continued fever," and having shown that the opinion of those who have considered it "a fever, *sui generis*," is justified by its strongly-marked and peculiar symptoms, by its post-mortem characters, and by its not arising (as more fully explained elsewhere) from the causes of the other continued fevers, we are prepared to enter on a further inquiry, the object of which we may present in the following question:—

II. *Is the disease, which, with relapsing fever occasionally added to it, formed the affection termed in this country "continued fever," a single disease, or have two or more affections been included here also under a single term?*

For some years an animated controversy on this question has been carried on, especially in France. Our limits will not permit us to enter fully into this interesting debate, which would be indeed unnecessary, as it has already been most ably considered in the pages of one of our predecessors.* Yet it seems advisable, for the sake of clearness, to recapitulate the most prominent points in the argument, that the bearing of our subsequent remarks may be completely understood.

The first unequivocal assertion that there are two different specific continued fevers, is to be found in Pringle's work.† In reply to De Haen, who accused him of using too stimulating a treatment, Pringle also asserted that the fevers of England and Vienna were altogether different; and one of his pupils who saw the Viennese patients, assured De Haen that the petechial eruption in Pringle's fever patients was quite different from that in De Haen's cases, which *was like flea-bites.*‡ In many of the writings at this period, or shortly afterwards, are statements that show the careful observers of the time to have been strongly impressed with the notion of distinct fevers; and Huxham, indeed, distinguished one kind, which he calls "nervous fever," and which was identical, no doubt, with the typhoid fever of the French, from the putrid malignant fever.

But the deficiency of post-mortem examinations prevented the question from being thoroughly examined; and as it is from the difference in the post-mortem lesions that the doctrine of a diversity of fevers has generally derived its chief support, we shall pass over the earlier writers, and come speedily down to the exacter day of Louis, Chomel, and Jenner.

It was well known to many of the older physicians, that in some fevers certain grave alterations in the alimentary canal were found after death. In 1763 these were specially described by Roederer and Wagler, in the epidemic at Göttingen, which was made up of several concurrent diseases. The works of Sarcone, and the paper in the 'Mémoires de l'Acad. de Médecine,' by the elder Louis, followed, in which these lesions were especially noticed. In 1804, however, the true foundation of our knowledge of this subject may be said to have been laid by the great work of

* Vide 'British and Foreign Medical Review,' Vol. XX, pp. 22 and 293; and Vol. XVII, p. 357.
† On Diseases of the Camp and Army.
‡ Could De Haen's fever have been the relapsing?

Prost, who in more than 150 dissections of Parisian fever patients, always found "inflammation," with or without ulceration of the intestinal mucous membrane. The works of Petit and Serres, of Pommer, and of Bretonneau, all bearing on this particular point, followed some years after; and in 1829 appeared the celebrated treatise of Louis. In this extraordinary work, the genius of a single man seemed at once to have reached a point which is usually gained by the combined exertions of generations. He may be said to have profited little by the previous observations on the same subject, although there is a singular accordance between his views and those of Prost and (in some measure) of Pommer, whom he succeeded. But the point in which he surpassed these two able men, was by the employment of a method of observation, which enabled him to give a complete and connected view of symptoms, as well as of post-mortem lesions. The same conclusion which Louis drew from his observations, may be found in Prost; yet the treatise of Prost is hardly known, and that of Louis has a world-wide reputation. Each asserted the connection of a certain intestinal lesion with a definite series of symptoms; but Louis alone described the lesion in terms sufficiently precise, and indicated, with scientific exactitude, the symptoms with which it is concurrent.

The question we are now to consider was not formally proposed by Louis in his first edition, although it naturally arose from the premises he laid down. Louis, indeed, was not at first aware that there was any other species of fever than that which he had described; but in other parts of Europe his description was not found to accord with the experience of the present, or with the recollection of past observation. It was not possible to bring within his definition of "typhoid fever," the facts recorded in the elaborate pages of Sauvages, Cullen, Pinel, and a host of others, who have left to posterity classic and imperishable descriptions of fever. Even in Paris, where Louis could find no fever which was not attended with disease of Peyer's patches,* Chomel, who had witnessed the fevers of many years, and particularly that form which ravaged Paris in 1814, was not disposed, at first, to admit the conclusion of Louis; and it was only when repeated observation of the Parisian fever gave daily confirmation to this conclusion, that he appears to have come to the determination that his own experience was erroneous, or his memory defective; and that in the immense majority of cases of fever, (though not in all,) there was intestinal lesion of a specific kind.† Andral held opinions nearly similar.‡

In other parts of Europe, and particularly in England, the opinions of Louis were not so favorably received. Numerous bodies of fever patients were opened, without finding any disease of Peyer's patches; and the symptoms detailed by Louis were not recognised by many experienced practitioners.

Hence arose at once two opinions, based, not on a difference of symptomatology, as was Pringle's view, but on a post-mortem distinction. The first and most popular was, that Louis had made too hasty a generalization; that he had very much overrated the frequency and importance

* He recorded two exceptional cases in his first edition; but he afterwards considered these to be errors of observation, and his subsequent experience warrants the assertion in the text.

† Vide Leçons de Clinique Médicale. Bruxelles, 1834, p. 440.

‡ It should be stated, however, that Andral has employed the term "typhus," in a generic sense, to include yellow fever, bubo plague, and European fever.

of his "anatomical sign;" and that common as it was in Paris, it was not nearly so frequently met with elsewhere. The constant anatomical sign was thus at once degraded to the rank of an incidental appearance, of an epiphenomenon.* This was the view held, without an exception, in England, where it was the custom to consider fever as a disease with many faces, and of diverse shapes, all springing from a common origin. This appears to have been also the favorite opinion in Germany, where the observations of Louis attracted at once much greater attention, and were more thoroughly understood, than in England.

A second opinion, however, was held by many of the French observers, who could not resist the daily facts presented to them by the Parisian fever, and yet were unable to believe that the assertions made in England, of the frequent absence of Peyerian disease, could be attributed to insufficient observation. That it ought not to be so attributed, was evident also from the fact, that in some other parts of France, as at Toulon, (in 1829 and 1830,) the intestinal lesion was not found by the most careful observers, whose attention was directed to the point by questions addressed to them by the Academy of Medicine.

A second opinion was, therefore, entertained, that there were, in fact, two maladies, which were termed indifferently typhus and typhoid fever; that one prevailed only at Paris, and the other in England, in Germany, and elsewhere, being more or less mixed up with the Parisian fever, as measles may be with scarlet fever. Louis at once adopted this view; and in the second edition of his work, published in 1841, has argued it at some length.

In 1835 the "Académie de Médecine" proposed formally the question as to what were the "analogies and the differences between the typhus and typhoid fever;" and since this time, a host of papers and works have been published in France, in answer to it. Of these the most celebrated are the treatises by Gualtier de Claubry,† and by Montault. This first work affirms the identity of the fever described by Louis, and that termed the typhus of camps and of prisons; and it confessedly proves that the epidemic fevers, occurring in times of scarcity or of war, have been (in many cases, at any rate,) of the same kind as the common Parisian fever. But the question was not absolutely answered; since it still remained clear, even with the affirmation of Gualtier de Claubry's general conclusion, that there *might be* another fever: for his observations, while they extended our knowledge of the range of typhoid fever, did not necessarily set aside the hypotheses of the existence of another and a distinct affection;‡ in fact, M. Gualtier de Claubry expressly excludes English typhus from consideration. The treatise of M. Montault adopted a view diametrically opposed to Gualtier de Claubry, and contended for the non-identity of the two diseases.

* One able writer even spoke of this disease of Peyer's patches as being regulated altogether by locality. Thus, the intestinal disease was said to be most common in Paris, less so in London, and rare in Edinburgh; the fever of all three places being considered identical.

† De l'Identité du Typhus et de la Fièvre Typhoide. Par C. E. S. Gualtier de Claubry. Paris, 1844.

‡ There is every reason to suppose, also, that Gualtier de Claubry, although he proved that epidemics of typhoid fever had been called "camp and jail fever," put a false interpretation on his own facts, when he referred all camp fevers to the same head. Both typhus and typhoid fevers have been termed indifferently camp, jail, and hospital fever; and they were distinguished perfectly by Pringle, in his work 'On Diseases of the Camp and Army.'

The interest excited in France by this question, was not felt in an equal degree in England. Singularly enough, although it was manifest that neither the symptomatology nor the morbid anatomy, laid down by Louis, were correct, when applied to the most common fever of Great Britain, the obvious inference that there might be two fevers does not seem to have suggested itself. This, no doubt, was the result of the strong bias in England towards the doctrine of a single fever, which had been professed by the most eminent observers of the day. Some years after the publication of Louis' work, and when the symptomatology of the Parisian fever had been determined with great accuracy, the descriptions in English works of continued fever were so unsatisfactory, that the French physicians were unable to institute a comparison between the French and English fevers, and were compelled to defer their opinion until more accurate information could be collected in England.

The first persons who appear to have dissented from the prevalent doctrine, were English and American practitioners, who had studied in Paris, and who were intimately acquainted with the characters of the fever there prevalent. When these observers came into contact with certain forms of English and of American fevers, they found themselves in the presence of a disease quite unfamiliar to them, and they were not long in recognising the chief points of difference. The earliest systematic attempt to indicate these differences, is the well-known treatise by Gerhard and Pennock, of Philadelphia. It had already been determined by the observations of Jackson and Gerhard, (two pupils of Louis,) that the fever described by Louis under the name of typhoid fever, existed in America, and presented the same assemblage and development of symptoms, and the same post-mortem lesions, as the Parisian fever. Gerhard compared with this disease, the epidemic fever which ravaged Philadelphia in the spring and autumn of 1836, and found that in every point the two affections were perfectly dissimilar. In 1839 Enoch Hale published an account of the fevers of Massachusetts, and distinguished among them two perfectly different forms of fever, one of which agreed perfectly with the Parisian fever, and the other might be held to represent the fever described by most English writers, if the symptoms and post-mortem lesions of the Parisian fever were abstracted from the compound description they had given.

In 1836, also, M. Lombard, of Geneva, after visiting London, Edinburgh, and Dublin, came, after some vacillation of opinion, to the conclusion that two fevers had been confounded together. Drs. Staberoh, of Berlin, and Kennedy of Dublin, professed the same opinion through the same medium, the 'Dublin Journal.' In the same year, also, Dr. Stewart commenced his observations in the Glasgow Fever Hospital, and after two years' inquiry, passed over to Paris, and examined the fever of that city. The result was a complete recognition of the existence of two fevers, and of their differences.†

Soon after this, the characters of the dominant English fever were noted by Shattuck, (another American pupil of Louis,) who published them in the 'Boston Medical Examiner,' and also communicated them to the Society of Observation, at Paris. Valleix arranged the notes, and drew

* American Journal of Medical Sciences, vol. xix, p. 289.

† Edinb. Med. and Surg. Journ., 1840, p. 289. Is there not some reason to think, that, in this excellent paper, cases of relapsing fever are included under the term typhus?

an elaborate comparison between the disease they represented and the typhoid fever of Louis.* The points of difference which he indicated were in most respects the same as those determined by Gerhard, although, from the small number of cases (6) of typhus which he possessed, he fell into some errors.

The appearance of these papers, and of the treatises of Anderson, Forget, Reid, and Delaroque, gave rise to an elaborate review of the whole question in the 'British and Foreign Medical Review.'† This was reported to have been written by an eminent London physician; and in the minuteness, and yet at the same time force and perspicuity, of the argument, we fancy we are at no loss to recognise the author. The conclusion arrived at in this able article was, that the French and English fevers were varieties, that is, different developments of a common stock, but not specifically distinct diseases.‡ The question was, however, argued as between *French* and *English* fevers, and not between the *typhoid* fever of Louis, and *another presumed form.*

In America the opinions of Gerhard, Jackson, and Hale, appear to have been generally adopted. They were formally embodied by Dr. Bartlett of Philadelphia, in 1842, in a work on 'Typhus and Typhoid Fevers,' which expanded in 1847 into a treatise on all the fevers of the United States. In Dr. Clark's little pamphlet on 'Ship Fever,' which is the latest American treatise we have seen, these opinions are advocated, and are illustrated by the experience of the English typhus, which the emigrant ships have afforded to the American physicians of the seaboard towns.

Our space will not permit us to follow this controversy in the pages of the German writers; the value of Louis' researches had been early recognised by them, and his typhoid fever received the name of abdominal typhus. By some this was considered altogether a distinct disease from the "typhus exanthematicus," described by Hildenbrand; by others it was thought to be a variety;§ by a third party it was imagined to be the only form of fever. At the present moment these three opinions still prevail.

Thus, in the great work of Canstatt, abdominal and petechial (or exanthematic) typhus are separately described, yet it is still considered uncertain how far they are distinct.‖ Wunderlich, in his comprehensive treatise, describes at great length the typhoid fever of Louis, while he devotes only two pages to jail and camp typhus, which he considers a modification of the former disease, and states that it prevails in England and Ireland. He seems almost unacquainted with this disease, ("the non-localised typhus,") and does not discuss the question of non-identity.¶ Hamernjk, on the other hand, has never seen in Prague any other fever than the disease described by Louis; and decides that it is the only kind

* Archives Générales de Médecine, Oct. et Nov., 1839.

† Vol. XII, p. 293.

‡ If our suspicion be correct, the views then entertained by the writer of the article have been very much modified, and he now believes in the specific distinction of the two fevers.

§ Pommer thought the intestinal changes were present in the sporadic, but not in the epidemic form. Others (Schönlein, Autenrieth,) used the word typhus as a general term, and spoke of abdominal typhus, in opposition to a typhus in which the chest or the brain were more immediately engaged.

‖ Canstatt says: "In wie weit der Petechial typhus eine ganz und streng vom Abdominaltyphus gesonderte Typhus-form sei, ist eine andere Frage. Es ist unläugbar, dass es zwischen beiden krankheiten Mittelformen gibt." (Die Specielle Patholog. und Therapie. Erlangen, 1847. Zter. Bd., p. 591.)

¶ Handbuch der Pathologie und Therapie. Dreiter Band, p. 1015.

of fever.* But, as we shall have occasion to observe presently, this opinion, which was formed in 1846, has been quite set aside by the experience of subsequent years, in Vienna, in Prague, and in Silesia.†

Up to the present time, this important question has remained without any settlement either way. In England, the relapsing fever was distinguished by the Scotch and some English physicians, though not by all; and all other forms of fever were, in spite of Stewart's admirable contrast, considered as identical. In America, the typhoid fever of Paris was well known, and distinguished from the fever without intestinal lesion, which was termed typhus; but relapsing fever was unknown.‡ In France, the non-identity of typhus and typhoid was maintained by the majority; but there were some powerful dissentient voices, and under the term typhus, relapsing fever was included. In Germany, where the pathological researches of Rokitansky and Engel had strongly directed public attention to typhoid fever, any other form was little known, and was by most considered as a variety of the more common type.

Under these circumstances, the inquiry was undertaken by an observer, who seems to have possessed all the qualifications which could be useful, in a locality which presented every advantage for their exercise. In London it has been long known that cases of fever, with the deposit in and under Peyer's patches, and in the mesenteric glands, were common; and so were cases of fever, without any trace of this deposit. If, then, the two fevers were distinct, this was the place to observe their differences; if they were the same, this was the place to find the transition forms. In 1846 Dr. Jenner entered systematically on this inquiry in the London Fever Hospital.§ Convinced of the necessity of collecting numerous facts, he patiently accumulated case after case of fever, until he had nearly two thousand accurate reports before him. He separated from these all cases of relapsing fever, and then instituted a rigorous comparison of the remaining cases.

It is necessary, however, to state his method more fully, as it appears to us the only one which can possibly solve the question, and it is a model of close observation and logical induction. From the great number of histories of fever-patients he possessed, he selected the fatal cases which had been examined, and the diagnosis of which, therefore, had been confirmed. He found that he had 66 such cases and post-mortem examinations. Of these 66, 23 had the intestinal and mesenteric lesion,

* "There is," says Hamernjk, "only one typhus, which, like most diseases, arises out of a peculiar blood affection, and out of the products arising therefrom." (Op. cit., p. 2.) He admits, however, that he has not always found the Peyerian lesion, and says that, after twenty days' fever, he has found "only the hyperæmia and swelling of the mucous membrane and of its follicles, which precedes the deposit, without actual deposit." The inference is, that the typhoid deposit may occur later than the 20th day,—a doctrine which is not borne out by a single fact in the works of Louis, Chomel, Valleix, Bartlett, or Jenner.

† Hamernjk speaks still more decidedly afterwards:—"Thus," he says, "the presence of the typhous deposit in the small intestine is absolutely essential to the existence of a typhus. When Andral declares, that in 100 typhus cases, scarcely two do not present the intestinal alteration, we must remark, that we do not consider the two cases referred to as typhus."

‡ Bartlett knows no other form of fever than typhoid, typhus, periodical, and yellow:—"I do not know anything of any other distinct fever among us. As to an inflammatory fever, distinct from typhus or typhoid fever, I can only say, with Nathan, Smith, and Chomel, that I have no knowledge of any such disease." (Preface to Second Edition.)

§ It is pleasing to observe the liberality with which the physicians of this hospital accorded to Dr. Jenner the privilege of the *entrée;* and we trust that the same liberality will enable him to continue the investigation of a subject so important.

which Louis says is the anatomical sign of typhoid fever, and 43 were without this appearance. Now, if Louis' doctrine be correct, that no case is typhoid fever unless it presents this appearance, it was to be seen, whether the 43 cases in which the intestinal lesion did not occur, differed so much in symptoms and other post-mortem appearances, from those in which it did occur, as to render it impossible to suppose that they were the same disease; or whether, contrary to Louis' opinion, the symptoms were so similar as to lead to the belief that the presence or absence of intestinal lesion was a matter of little consequence. Accordingly Dr. Jenner took these two groups and compared them throughout, and found that while the symptoms and post-mortem appearances of the 23 cases were exactly the same as those described by Louis in his great work, the symptoms and post-mortem appearances of the other 43 cases were entirely different, so different, indeed, as to render their separation from the other cases a matter of absolute necessity, if any certainty was to be introduced into the description of these diseases, and into their treatment. The disease which affected the 23 patients, he called after Louis, typhoid fever, and to the other affection he gave the name of typhus.* Between these two diseases, no transition forms could be observed. The results arrived at by Jenner agree remarkably with those of Gerhard, Stewart, and others.

The comparison of these two groups is stated at length, in the work which heads our Review, and which is a reprint from the 'Monthly Journal.' It would be almost impossible for us to epitomize this work, so condensed is it already; but we shall endeavour to place its most important parts before our readers, and in order to do so, we shall enumerate the points of difference, in the order adopted by Dr. Jenner in his recapitulation. (p. 92.)

If we collate the chief works which have been written on the two diseases by those who have had a full persuasion of their distinctness, we find that typhus and typhoid fevers are said to differ:

1. In the *age* of the patients they affect. Typhus affects all ages, young and old; typhoid, chiefly persons under 40; it will affect older persons, but with difficulty.†

2. In their *modes of attack*. Typhus being sudden, typhoid insidious, as a general rule.

3. In their *duration*. Typhus fever is of much shorter duration than typhoid, as has been noted by Gerhard, Stewart, and Jenner. The average duration of Jenner's fatal cases of typhus was fourteen days; of Reid's (143 cases) thirteen days; of the typhoid cases, twenty-two days. In the cases of recovery, the difference is just as well marked. The average of 255 typhoid cases noticed by Jackson in Massachusetts was twenty-two

* So many applications have been made of these names, that it would have been advisable to have employed new ones altogether; but we have not ventured to suggest any.

† There is rather a singular accordance in the average age of typhoid patients in different places. Thus, Louis' average, in Paris, was 22½ years; Jackson's, in Massachusetts, 22 years; Gerhard's, in Philadelphia, 22 and a third; Jenner's, in London, (of fatal cases,) 22·08. The average age of typhus patients is at least ten years more than this. Jenner's average of 43 fatal cases is 41·8 years; but this is probably over the average of all cases. The difference of age is well marked even in the pages of writers who consider fever as a single disease. Thus, Southwood Smith relates 54 cases of fever without intestinal lesion, the average age being 33 years; and 40 cases of fever with entero-mesenteric disease, whose average age was only 22 years and a third. (See also Bartlett, Second edition, p. 249.)

days. Hale's average, in the same locality, was thirty-nine days. Jenner's average is from twenty-one to thirty days. The average of non-fatal cases of typhus appears to be much less than this.* Jenner states that after twenty-one days, local lesions sufficient to cause death were always discovered in typhus, that is to say, that after this date, death did not occur *from the fever alone,* as may be the case before the twenty-first day. He states the average duration to be from fourteen to twenty-one days; but not infrequently, in very mild cases, it terminates before the fourteenth day, in the same way as mild cases of scarlatina will cease before the average time arrives at which the fever is usually held to terminate.

4. *In the kind of eruption.* We have already (vol. VII, p. 368) given Dr. Jenner's account of these symptoms. We need now only remark, that it perfectly agrees with the account of Gerhard, Bartlett, Valleix, Peebles, and many other observers. And we may be allowed to say, from our own experience, that nothing can be more distinct than the repeated scanty crops of rose-spots in typhoid fever, with their bright colour, their disappearance under pressure, and their duration of three or four days, compared with the permanent, dark red, or mulberry coloured, ineffaceable copious rash of typhus. Mistakes sometimes arise, however, from the typhus eruption being seen on the second or third day of its appearance, at which time it disappears under pressure, as was noticed forty years ago by Wedemeyer, in his account of exanthematic typhus.†

5. *In the colour of the skin, the expression of the face, and in manner.* Typhus patients often present in its highest degree those characters which the older writers often termed "oppression" and "prostration;" the face is darkly and generally flushed, the complexion muddy, (particularly after the sixth day,) the manner stupid and confused, and the eyes unintelligent.‡ On the contrary, in typhoid fever, the complexion does not get muddy, except in a very slight degree; consequently the flush of the cheeks,

* "The short comparative duration of the cases of typhus fever here considered is another remarkable point of difference, totally inexplicable by the hypothesis, that typhoid fever is typhus fever with intestinal ulceration. Had the cases eventually recovered, it might have been said, that the intestinal lesion prolonged the disease in the cases of typhoid fever; but that all the fatal cases of fever, with a local lesion of so severe a nature as that recorded to have been present in the cases of typhoid fever, should have had a much longer course than all those other fatal cases of fever, in which no organic change of structure could be detected after death, seems to me inexplicable, on the supposition that the former is simply the latter disease, with this serious lesion superadded. Let me repeat,—by this hypothesis we are asked to imagine that death is retarded in fever by extensive ulceration of the small intestines, and enlargement, softening, and even suppuration of the mesenteric glands. Surely it behoves the supporters of such a statement to bring forward cogent proofs of the identity of the specific cause of the two affections, ere they ask us to admit its truth." (Jenner, Op. cit., p. 97.)

† De Febre Petechiali. Gott., 1812. — The rose spots appear in about 85 per cent. of all cases of typhoid fever. The rash of typhus appeared in 136 of 152 cases seen by Jenner, i. e. in 89·5 per cent. Age exerts a great influence over it: in all the sixteen cases in which it was not present, the age was less than 22, and in 13 of the 16 it was under 15. So that, if this observation be correct, the following rule may be laid down:

In 100 typhus patients under 15, the rash will be absent in 25.
,, between 15 and 22 ,, ,, 14.
,, above 22 ,, always present.
(See 'Medical Times.')

Sudamina have been supposed to be more common in typhoid than in typhus fever. Jenner has pointed out, that age exerts a great effect on the appearance of these vesicles. Over the age of 40, they are uncommon; therefore their apparently greater frequency in typhoid is attributable to the younger age of the subjects of this disease. Under 40, they are equally common in typhus.

‡ Armstrong described this appearance accurately in his account of "typhus," and contrasts it with the state of things in "common continued fever," (which is probably typhoid fever.)

when present, is bright and pinkish, and not dark red, and it is often circumscribed to the cheeks, and then is strongly contrasted with the surrounding pale skin. The manner, also, is often natural, or even a little sharp, provided there be no delirium. These differences are marked, even in the slightest cases, although in extremely slight cases of typhus the muddiness and flushing may be insignificant compared with the severe cases. In relapsing fever, the complexion is clear, or has a slight yellow or "bronzed look." (Cormack.) It is very conceivable that the peculiar complexion of typhus has not always obtained the attention it merits, on account of the confusion of cases of typhoid and relapsing fever, in which this complexion is not seen.

6. *In the severity and course of the head symptoms. Headache* is an almost constant symptom in each. Although there is considerable variation in individual cases, yet on throwing large numbers together, it becomes apparent that both in typhus and typhoid fevers, the *headache* has a determinate duration. In typhus it ceases usually on the tenth day, and always before the fourteenth day; in typhoid fever, about four or six days later, and may last till nearly the end of the third week. *Delirium* commences earlier in typhus than in typhoid, by several days. In Jenner's fatal cases, it was more *active* in typhoid; the patients were more vivacious, and disposed to leave their beds. *Somnolence*, although frequently absent in both, is more common and earlier in typhus than in typhoid. The peculiar symptom which has been appropriately called "*coma-vigil*," and in which, as Jenner defines it, "the patient lies with his eyes open, evidently awake, but indifferent or insensible to all going on around him," occurred in one fifth of his fatal cases of typhus, but in none of the typhoid patients.*

7. *In the degree of loss of muscular power.* Typhus patients almost always take earlier to their beds, and are more completely prostrated at an earlier date than typhoid cases. This is well illustrated by Jenner's cases, as here typhus and typhoid cases were lying side by side in the same wards.

8. *In the frequency of epistaxis*, which is very rare indeed in typhus, rather common in typhoid, (one third of Jenner's available fatal cases; nearly half of Louis'.)

9. *In the condition of the eyes.* In typhus fever the conjunctivæ are generally injected, and the pupils contracted; in typhoid fever, the conjunctivæ are pale, and the pupils larger than natural.

10. *In the state of the tongue*, which is drier, browner, and larger in typhus; is more frequently small, fissured, red, or partially covered with a pale-brown fur in typhoid.

11. *In the chest symptoms.* Sonorous rhonchi are very frequent in typhoid; rare, comparatively, in typhus. Dullness of the depending portions of the lungs, (a little above the bases,) depending on hypostatic congestion, is common in typhus, rare in typhoid.

12. *In the state of the pulse*, which is much more variable in typhoid than in typhus fever.

13. *In the abdominal symptoms.* The abdomen is *painful* on pressure, in about three fourths of typhoid patients; is almost always quite painless

* Chomel's definition of coma-vigil is, however, somewhat different from this. He applies the term to a state in which the patient sleeps for hours, and then declares he has never closed his eyes.

in typhus, or if painful, this is slight and transient. *Gurgling* exists in perhaps a fourth of the typhoid cases; in one fortieth of the typhus, The abdomen is *distended* and *resonant*, more or less in almost all cases of typhoid; it is, with scarcely an exception, natural in shape, or even a little concave, in typhus. *Diarrhœa* exists as a rule in typhoid, as an exception in typhus. *Intestinal hæmorrhage* occurs in one third of fatal cases of typhoid; in no case of typhus without dysentery. The *discharges from the bowels* are different in the two diseases; in typhoid they are loose, watery, fawn or dark brown colour, alkaline from fixed alkalies, and contain a large proportion of soluble salts, and a small quantity of albumen. In typhus they are generally solid, often acid, or if alkaline, are so probably from ammonia, and in most cases do not appear altered from health, unless medicines have been taken.* Although diarrhœa, meteorism, and abdominal tenderness are the rule in typhoid fever, it must not be supposed that they are always present. In some of the worst cases, the first abdominal symptom may be announced by peritonitis, consequent on perforation. But when vast numbers are collected, these exceptional cases are lost in the large proportion of those in which these symptoms exist in greater or less intensity.

14. *In the occurrence of epiphenomena and of sequences.* Sloughing from pressure is equally common in both diseases; but erysipelas, phlebitis, and local inflammations and ulcerations are much more common in typhoid fever. So also tubercular deposition in the lungs is decidedly more frequent as a sequence of this disease than of typhus.

15. *In the continuance of the eruption after death.*† The spots of typhus last uneffaceably after death; the rose spots of typhoid fever cannot be found.

16. *In the duration of cadaveric rigidity,* which ceases more quickly in typhus than typhoid cases, according to Jenner's interesting observations.

17. *In the more rapid dissolution* (so to speak) *of the tissues* in typhus than in typhoid. As an instance of this, it appears from Jenner's researches that the epithelium detaches itself very rapidly indeed from the basement membrane, a fact which is well seen when a microscopic section of the kidneys of typhoid and typhus patients are examined,‡ or when the surface of the œsophagus is observed. To the same class of facts must be referred the abnormal facility with which, in typhus patients, the pia mater and arachnoid separate from the surface of the brain.

18. *In the frequency of the occurrence of hæmorrhage into the arachnoid* in typhus, which occurred in one eighth of Jenner's fatal cases, while it was not found in one of his typhoid cases, nor in any of Louis' or Chomel's cases. The amount of intra-cervical serosity is also decidedly greater in typhus.

* These statements are taken from a clinical lecture by Dr. Parkes. (Medical Times, June 1, 1850.) The typhoid stool usually separates into two parts,—a sediment and a supernatant fluid. The average specific gravity of the latter is 1015 or 1016; the average solids 38·5 per 1000, of which about 10 per 1000 are soluble salts. The fluid gives a dark red, purple, or mahogany tint with nitric acid. Lehmann (Physiologischen Chemie, Zr. Bd., p. 143,) has lately described the stools in nearly similar terms,—the alkaline fluid contains "more or less albumen, but only little bile, with a large quantity of soluble salts, and especially chloride of sodium."

† In the enumeration of the differences in the morbid anatomy, we have followed Jenner very closely, as no one has approached him at all in the accuracy with which this part of the subject has been observed.

‡ Of course, in such a case, every precaution is supposed to be taken to have the kidneys under the same conditions, and to choose such as are, in all other respects, comparable.

19. ***In the frequency of ulcerated mucous membranes in typhoid fever, and the rarity of ulceration in typhus.*** In typhoid fever, ulcerations in the pharynx exist in about one third of the cases; but in typhus such lesion is never found, or is excessively rare. In typhoid, ulceration of the larynx and of the œsophagus occurs once in every 15th case; in typhus, ulceration of the larynx happens once in every 26th case; ulceration of the œsophagus very seldom indeed. In typhoid the gall-bladder and the urinary bladder occasionally participate in this ulcerative tendency; in typhus they are never attacked, or very rarely indeed. So also the mucous membrane of the large intestine suffers frequently (in 7 of 20 cases, Jenner,) in typhoid; but scarcely ever in typhus, unless there be concurrent dysentery, which is a composite disease, and distinguished without difficulty.*

20. ***In the occurrence of a peculiar exudation into and under the patches of Peyer, and into the mesenteric glands in typhoid fever.*** This is constant in all cases which present the symptoms of typhoid fever; it is never absent, although its amount varies greatly in intensity. After the publication of Louis' work, Andral, Chomel, and others, observed cases which they thought were typhoid fever, without the anatomical sign; but these cases did not bear examination, and subsequent experience has proved that this lesion is constant. It never occurs in typhus fever.

21. ***In the greater softness and flabbiness of the muscular tissue of the heart,*** in typhus than in typhoid.

22. ***In the frequency of lobular and lobar pneumonia in typhoid fever, and the rarity of these local inflammations in typhus.*** In typhus, consolidation, or perhaps we should rather say, carnification of the depending portions of the lungs from congestion, is more common, but true inflammation is rare.†

23. ***In the more frequent occurrence of pleurisy in typhoid fever,*** (40 per cent. of fatal cases, Jenner,) than in typhus (5·5 per cent. of fatal cases.)

24. ***In the degree of softness of the spleen,*** which is greater in typhoid than typhus.‡

25. *In mortality;* that of typhoid being decidedly greater than typhus.

In addition to those differences, there is some evidence of different constitutions of the blood; but as the data are not very exact, we pass them over.

It is also possible that other differences may hereafter be indicated; thus comparative observations have not been made on the urine with sufficient care; the relative temperature of the two fevers, and other points of the like kind, have yet to be considered.

Such is the statement of differences which we have been able to glean from the writings on the subject. Although some of the distinctions may appear slight and trivial, yet others are not so. Thus among symptoms,

* Jenner observes, that the special tendency of typhoid fever appears to be towards disseminated inflammations and disseminated ulcerations, while that of typhus appears to be towards disseminated congestion and hæmorrhage.

† It is, however, probable that the tendency to pneumonia may vary in different epidemics and in different countries. Pneumonia is merely an epiphenomenon; but Jenner's cases of typhus and typhoid together prove, that, *under the same conditions*, pneumonia is most common in typhoid.

‡ It has been thought, also, that there was a difference in the size of the spleen; but a very interesting analysis by Jenner, on this subject, points to the following conclusions: as a rule, the spleens are larger in typhoid cases, but this appears to be because the subjects are, on the average, younger than typhus cases. Take away the influence of age, and the typhus patients show as great an average weight of spleen as the typhoid. Age, also, in typhoid disposes to softness. Before the age of 50, typhus spleens are harder than typhoid; after that age, they are softer.

the absolute diversity of the eruptions, and the state of the skin, the differences in the duration of the disease, in the mode of onset, and in the pronounced abdominal symptoms of one, with the absence of these in the other, are very marked. So also among post-mortem lesions, the entero-mesenteric disease, the tendency to ulcerations of mucous membranes, and to local inflammations, of typhoid fever, are strongly contrasted with the absence of pronounced local changes in typhus, and with the epithelial separation, apparently from prior injury to structure, which is so marked in that disease. So great are these differences, that we do not hesitate to affirm, if any one will take two confessedly distinct, yet somewhat kindred diseases, such as measles and scarlet fever, and will compare them in the way we have compared these continued fevers, he will not find a stronger case made out for their separation, than for that of typhus and typhoid fevers.

Jenner concludes his comparison in the following way:

"At the commencement of this analysis, I proposed to examine whether typhoid fever and typhus fever differed from each other in the same way as smallpox and scarlet fever differed from each other; and for the purpose of comparison, I laid down certain grounds, as those on which we founded our belief in the non-identity of the two last-named diseases. Those grounds were:—

"1st. In the vast majority of cases the general symptoms differ; i. e. of smallpox and scarlet fever.

"This holds equally true with respect to the general symptoms of typhoid and typhus fevers.

"2d. The eruptions, the diagnostic characters, *if present*, are never identical, i. e. in smallpox and scarlet fever.

"The particulars detailed in the foregoing papers prove that this is as true of the eruptions of typhus and typhoid fever, as of those of smallpox and scarlet fever.

"3d. The anatomical character of smallpox is never seen in scarlet fever.

"Just in the same way, the anatomical character of typhoid fever, i. e. lesion of Peyer's patches and the mesenteric glands, is never seen in typhus fever.

"4th. Both, i. e. smallpox and scarlet fever, being contagious diseases, the one by no combination of individual peculiarities, atmospheric variations, epidemic constitutions, can give rise to the other."

That this holds good of typhus, typhoid, and relapsing fever, the author has shown by the paper in the 'Medico-Chirurgical Transactions.'

"5th. The epidemic constitution, favorable to the origin, spread, or peculiarity in form or severity of either, i. e. smallpox and scarlet fever, has no influence over the other, excepting that which it exerts over disease in general." (Op. cit., p. 100.)

So far we conceive the advocates for the non-identity have made out their case, for if these differences are not sufficient, all diseases are identical; but we have to look at the question from another point of view.

III. *Are these differences always well founded, or is it not possible that, existing in well-marked cases, they may yet not be constant, but may disappear in some transition cases?*

It is but fair to the supporters of the doctrine of non-identity, to admit that the weight of positive evidence is all on their side; and it is also but right to state, that few points in medicine have been submitted to so rigid and searching an examination as this. The method of Louis, accurate as it was in his hands, has been used by Jenner with a skill and care which surpass even those of its originator; and the treatise of our countryman may be honorably placed beside the immortal work of the great Frenchman. On the other hand, those who have asserted the identity of the two diseases, have done so chiefly on the authority of old and imperfect observation. To this there is a single exception, to which we shall presently refer.

In addition to this, the assertors of the non-identity state that they have never been able to find any transition forms. Jenner, in London, during the last four years, has seen 2000 cases, all of which could be referred to one or the other form of fever without difficulty. Gerhard and Bartlett, and the other writers on the same side, assert the same thing.

Some valuable corroborative evidence can be found also in some of the treatises we have under consideration, for other points connected with fever. Thus Dr. Steele of Glasgow, in the paper we have already quoted, thus writes:

"The two principal forms of fever, which prevailed throughout the country, were the bilious or relapsing, and the common typhus, peculiar to the country, the latter disease proving by far the most deadly. Another and a still more formidable type of fever prevailed, though in a less degree than the others, and contrived to show itself occasionally during the year in the form of an intercurrent epidemic; this was the typhoid or dothinenteric fever, the returns of which are specified in Table xix."

He afterwards gives the mortality of each; viz. of relapsing, 6·38 per cent.; of typhus, 21·2 per cent., and of typhoid, 33·8 per cent.

Similar evidence is to be found in that vast mass of instructive matter, which the zeal of the editor of the 'Dublin Medical Journal' has collected. In the history of the terrible Irish fevers of 1846-47 and -48, contained in that excellent periodical, we find the most undoubted evidence, that exanthematic typhus and relapsing fever were the two great scourges, and that typhoid fever, dysentery, and scurvy were here and there intermixed. Frequently, when the writers have been little aware of it, they have given the strongest proofs of the existence of several intercurrent forms of fever.* We would willingly extract much from these accounts, were we able at present to use them for any other purpose than is demanded by the subject we are considering.

The exceptional evidence to which we have just now referred, as affording the only positive facts which run at all counter to those just stated, is to be found in an essay by M. Landouzy, on an epidemic which prevailed at Rheims, in 1839-40. Although this essay has been often quoted, we shall make no apology for shortly considering it. On the 1st of October, 1839, one of the prisoners in the jail at Rheims fell ill with a fatal disease, in which stupor, severe cephalalgia, and great prostration, were the prominent symptoms. Three others were soon afterwards attacked; and the disease becoming general, spread through the prison, attacked 138 persons, and lasted seven months. A full account of the symptoms is given, from which it is evident that the disease was typhus; the eruption is perfectly described. It is distinguished absolutely from the rose-spots,† and

* Archives Générales, vol. xiii, pp. 1 and 306.

† Landouzy says: "J'avais besoin d'établir nettement les caractères qui distinguent les deux genres d'eruption, afin de montrer *qu'il est facile, avec une attention suffisante, de les distinguer au lit* du malade, et ce n'est pas sans étonnement, que j'ai vu *l'exanthème rosé lenticulaire* confoundu dans le mémoire de M. Gualtier de Claubry avec l'exanthème pétéchial." (p. 22.)

corresponds accurately with the "mulberry rash" of Jenner. Yet "rose-spots" were also present, and mixed up with the true typhus rash. Landouzy says, also, that during the convalescence he has seen the eruption reappear, and that then it was always formed "par des taches roseés." This is in great discordance with all who have carefully studied typhus, and who deny relapse. Although the symptoms of typhus had been so pronounced, M. Landouzy states that the post-mortem lesions were those of typhoid fever. Peyer's patches were tumefied and ulcerated, and the mesenteric glands swollen and red.

Let us, however, examine these cases a little more closely. Seventeen cases died, of whom six only were examined, *and only two cases are recorded.* The first case was indubitably typhus; the mulberry rash was well marked. Landouzy says he looked in "vain for the rose-spots of typhoid fever which existed among other patients." (p. 313.) Nevertheless, at the necropsy he found alteration of Peyer's patches. But after a careful examination of the report, we have great doubt on this head. The small intestines were first examined by M. Chabaud, and two other physicians, and *pronounced to be sound;* then M. Landouzy entering and re-examining it, detected what he considered evidence of incipient change in Peyer's patches, with general slight tumefaction of the solitary glands. But we doubt whether M. Landouzy has made out his case; the alterations he signalises are not sufficiently defined to bring it within Louis' definition of typhoid fever.*

The second case was one in which there were two eruptions on the skin; 1, rose spots; 2, an eruption like flea-bites, (comme des piqûres de puce.) The symptoms are not so unequivocally those of typhus; they appear to us more like typhoid fever, only there was no diarrhœa. After death there was decided evidence of typhoid deposit in Peyer's patches. In this case, however, with every disposition to receive M. Landouzy's facts, we cannot admit that these were unequivocally symptoms of typhus. The typhus eruption is not like flea-bites at all; and we cannot help believing that this may have been simply a case of typhoid fever, (which we incidentally learn was prevalent also in the prison,) with this flea-bite eruption, which at present is a very puzzling one, superadded.

Now, since these are the only two recorded cases, and neither of them is satisfactory, and since only four other post-mortem examinations were made, the details of which are not given, and the value of which is, therefore, necessarily shaken by the exception we must take to the two recorded cases, we do not think we can do otherwise than consider that the importance of this memoir of M. Landouzy has been very much overrated, and that the facts recorded in it cannot be received without the greatest doubt, as evidence on this point.† We feel no hesitation, then, in putting this memoir aside, as being infinitely less precise, and based on far narrower details, than the writings on the other side of the subject.

* The appearances Landouzy describes are perfectly well known to the German observers as dependent on a "catarrhal condition" of the intestinal mucous membrane, which may occur in typhus, as in pneumonia or any acute disease. No accurate observer could confound this with typhoid deposit in and under Peyer's patches.

† Landouzy himself perceives so many differences between typhus and typhoid fever, that, in spite of his belief that he had found the anatomical sign of typhoid in typhus, he writes: "I find between the typhus of Rheims and typhoid fever, too striking differences to allow me to regard these two maladies as identical." (p. 329.) He says, however, that they are analogous, but not identical.

It should be also remembered that the positive evidence in favour of the non-identity has been gradually accumulating for several years. ***Every one who has paid attention to the subject*** has adopted the same conclusion. And that the differences between the two diseases are not dependent on any variety of epidemic constitution present in one year, and not in another, is proved by the length of time over which the observations run; Stewart's, Shattuck's, and Jenner's paper make up an aggregate period of observation of nearly fifteen years, during which time every one who has attended to the point, has recognised these distinctions. In America, also, Gerhard's observations were made sixteen years ago; and every year since that time has more firmly convinced him and other American observers of their accuracy.

When to this strong argument, that the positive evidence is (with one probably unimportant exception) all on one side,—and when, in addition to the fact that Pringle and Huxham,* who were so familiar with fevers, distinguished two forms, which it is almost certain were the typhus and typhoid of our own days, and that Armstrong also, more lately, has observed and described two fevers,† and that since Louis fixed the symptoms and the post-mortem lesions of typhoid fever, the evidence in favour of the correctness of his views, and consequently of the specific difference between it and other fevers, has been constantly augmenting,—when to all these arguments we are able to add, that if we admit these several forms of fever, the discrepancies between the observations of different countries disappear, order is introduced into this intricate and perplexed subject, and light suddenly on its dark and obscure outlines,—we are certainly strongly impelled to admit at once the truth of Jenner's conclusions, and to separate typhus, typhoid, and relapsing fever, as completely as we do smallpox and scarlet fever.

To the reasons we have above adduced for adopting the doctrine of a diversity of fevers, we are able to add one more, viz., the fact that fever of one form always gives rise, as far as observation at present goes, to a similar disease, and to no other. It was noticed by Henderson, in Edinburgh, that typhus did not arise from intercourse with persons affected with relapsing fever. His conclusion is, "that in not a single instance has the typhus fever presented itself in circumstances that warrant the opinion that it must have been produced by the contagion of the epidemic fever." (Op. cit., p. 218.) The same question has been elaborately argued by Jenner,‡ and noticed by us at page 369 of our last volume. Typhoid fever produces a like disease, but not typhus or relapsing fever; and each of these two latter diseases produces its own kind, but no other.

Still with all these facts before us, we think there is something to be said on the other side. Although we have put aside M. Landouzy's evidence, it does seem extraordinary that so many able men, both in England and in Germany, should have held that these various diseases were transmutable into each other. In this country, typhus and typhoid fever have been constantly before our eyes, and we have failed to find out

* The petechial and the miliary fevers of Pringle; the nervous and the putrid malignant of Huxham.

† His typhus and his "common-continued fever," the first being our typhus, and the latter typhoid.

‡ Medico-Chirurgical Transactions, vol. xxxiii, p. 23.

their differences, striking as these appear to be in the pages of Jenner or Stewart. We know, however, that to this argument from omission, too much weight should not be attached, and we do not desire to forget how strangely habit and education blind us to the most obvious facts. Yet some weight should undoubtedly be given to this argument; and a few years more are required, before we are able to decide absolutely that there is no measure of truth in it. We may, therefore, state our answer to the proposed question in the following terms:—The facts adduced in evidence of the specific differences of typhus and typhoid fever are sufficient, if they be hereafter proved to be universal; there is no evidence for the identity of the three diseases, which can be at all compared, in point of precision and extent, to that which goes to prove their non-identity; and it is, therefore, excessively probable, strange as it may seem, that the progress of inquiry will soon enable us to decide positively that in the *fever* of Great Britain, diseases the most diverse have been bound together by the enthralling yoke of a simple name.

IV. *We pass on now to a further consideration of this question, by inquiring what support is given to the doctrine of the diversity of these diseases, in the facts lately recorded in Germany on this subject?*

The years 1846 and 1847 were marked, not only in this country, but on the continent, by a series of famines, and succeeding wide-spread fevers, which were appropriately crowned by the terrible political convulsions of 1848. Among the states of Europe, one unhappy country attracted universal pity by the intensity of its sufferings, and the depth of its despair. No history is extant, which reveals profounder misery than that which crushed the buoyant heart, and quelled the bold spirit, of the Irish Celt. Succeeding ages will look back with awe and wonder at this spectacle of a nation's pangs, and will anxiously endeavour to trace out what may have been the causes which drew from the Divine Providence an infliction so severe.

England and Ireland, however, were not the only instance of the union of wealth and poverty, strength and weakness, happiness and misery, rejoicing and despair. In another part of Europe, a parallel presents itself, so remarkable in many respects, that the observer pauses in surprise at the discovery, that there could be a similar political union of different races, a similar manifestation of industry and sloth, of success and failure, of prosperity and adversity, and of life and death. The discovery is indeed remarkable, and should lead those who think that the evils of Ireland are to be traced solely to the misgovernment of England, to a conclusion founded on more accurate and more extended research.

The province of Upper Silesia is a dependency of Prussia. It is inhabited, however, not by Saxons, but by a race of Poles, who have been severed from their nation for 700 years, and yet have preserved their language, their religion, and their unwillingness to labour, although they have lost the inventive genius and the chivalrous spirit of their parent stock. Separated thus from Prussia by differences of blood, of religion, and of language, the utmost efforts of that enlightened country have failed to teach them Saxon industry, or to give them Saxon comfort. The schoolmasters who have been sent among them have learned Polish, but have not taught German; the Protestant teachers have only excited in them a more fanatic zeal for their Catholic priests; the profound literature of Germany awakens in them no response; and amidst the clash and tumult of modern progress, they remain silent and unmoved in their antique isolation. Like the Irish, the potato is their staple article of food, to which they add buttermilk and sauerkraut. Their dwellings are the prototypes of the Irish cabins, and in the smallest and dirtiest huts, persons of all ages and sexes are crowded together.

Nor does the parallel to Ireland end here. The relations between landlord and tenant appear to be on as false a footing as those which exist in Ireland, only that here a still more oppressive state of servitude may be found. The aristocracy, also, as in Ireland, adopt a system of absenteeism, and spend in Berlin or Vienna the small portion of wealth which the labour of their miserable dependants creates. The mental character of the peasant of Upper Silesia, also, appears closely to resemble that of the Irishman. Virchow found them intelligent and apt to learn, but so depressed by their miserable condition, that in the late epidemic many welcomed the disease, which relieved them from a life so joyless. The impulsiveness of their nature is exhibited by a fact which strongly recalls Ireland and Father Mathew to mind. The Silesians, like the Irish, were excessively intemperate. On a market day, the roads used to be covered with drunken men and women. The habit of drinking began even in infancy; drams were given to infants at the breast, and in after age the habit seemed unconquerable. A few years ago the priests determined to eradicate the vice, and all the means in their power were put into operation. In a single year, a priest, Father Stephen, administered an oath of temperance to the whole people, and the apparently inveterate habit gave way, for a time at any rate, before the priestly appeals to their intelligence and their faith.*

It is not a little singular, and the fact will no doubt attract the attention of every one who looks attentively into the causes of disease, that this Prussian province, while it presents many of the social conditions, presents also some of the diseases, of Ireland. It is true that our knowledge of these diseases is not extensive; but still the observations already made enable us to extend the parallel we have presented a few steps farther. Thus Upper Silesia, like Ireland, is a country where fever of some sort has always prevailed, and from time to time has become epidemic. In 1847, after the failure of the potato, and the consequent famine, the endemic fever became epidemic,† and so terrible were its devastations, that it attracted the attention of all Prussia. This epidemic appears, as far as we can make out, to have been chiefly relapsing fever, with probably intercurrent cases of exanthematic typhus; just as the Irish epidemic of the same date was chiefly constituted by these diseases. At least, this appears to us to be the most likely interpretation of the numerous and often discordant facts in the works before us.

The longest and most important memoir on the Silesian epidemic is by

* This was in 1844; in 1845 the first traces of the potato disease appeared, and shortly afterwards the notion arose among the people, that it was a judgment upon them for despising the gifts of God!

† Virchow says: "All the local physicians agreed, that the epidemic could not be distinguished from the endemic typhus of Silesia." (p. 253.) Dümmler asserts the same thing. The Plessner physicians are also quite decided on this point.

Virchow of Berlin, who was sent by the Prussian government to Silesia. He left Berlin on the 20th of February, 1848, and returned to it on the 10th of March. In these eighteen days he collected much valuable information respecting the country and its inhabitants, and also observed the fever then prevalent. It must be remembered that Virchow's description of the fever may have been drawn from an examination of numerous cases; but if so, this examination must have been cursory and incomplete. No single case could have been observed for more than two or three days, as the whole of Upper Silesia was traversed in less than three weeks.

In one point Virchow's description agrees with the observations of the numerous other observers. In six dissections which he witnessed, there was no disease of Peyer's patches or of the mesenteric glands, that is to say, the anatomical signs of typhoid fever were not present. Stich, who has published an excellent paper on the morbid appearances of this fever, has confirmed this observation by many other dissections; and Dümmler, Lorenz, and others, have dissected so many more, that the non-existence of the typhoid deposit in the small intestines and elsewhere is a proved fact.*

That the Silesian disease was not *typhoid* fever, appears also from the description of the symptoms.†

The disease attacked all ages and both sexes; its mortality differed in intensity, in some places being slight, in others severe. It was characterised by great prostration, and often by severe pains in the limbs, by an absence of diarrhœa, by the frequent presence of an eruption, and by the frequent occurrence of relapses.

The eruption is described by Virchow as presenting two forms. He calls the first "roseola typhosa," or "rubeolous eruption." This appeared on the third or fourth day, and was characterised by little flat, or but slightly raised, pale-red spots, deeper coloured in the centre, disappearing completely under pressure, not connected with the hair-sacs, and of an irregular shape, with an uneven border. It appeared first on both sides in the region of false ribs, and in the epigastrium, and then spread itself over breast, abdomen, limbs, neck, and sometimes, though rarely, the face. Its duration was very short; even on the day after its appearance, it began to get paler, or in a day or two after this. This eruption Virchow saw in many cases, but was informed that cases were seen, which did not present it. The second form of eruption was the petechial, viz. little, perfectly round, flat, red spots, about the size of a pin's head, unaltered by pressure. Before Virchow went to Silesia, this "petechial" eruption was not marked; but after he pointed it out to the physicians there, many observed it.

The work of the Plessner physicians, and the treatise we have placed next to it, give a full account of the devastations of the epidemic, and

* In his critical remarks upon the treatises which have been published on this subject, Virchow says: "All observations agree, that neither the digestive mucous membrane nor the mesenteric glands were importantly and characteristically changed. Only when diarrhœa had been present, the customary effects of intestinal catarrh were found." (p. 182.) The effects, viz., which Landouzy found in his case of Typhus before alluded to, were—slight projection of Peyer's, and general enlargement of the solitary glands.

† Let us beg the attention of our readers here to this circumstance. In an extensive epidemic, a certain morbid appearance is invariably absent. Now, if this morbid appearance be merely, as some English writers have considered it, an incidental occurrence which is met with every now and then, why should it never have been met with in the Silesian epidemic?

rival, in their gloomy details, the Irish reports. The disease is proved to have been contagious, and is considered to have been the ordinary endemic fever, augmented by famine and distress. Nearly a tenth part of the inhabitants died from hunger and disease.

The paper of Bärensprung is known to us only by the abstract in Graevell's 'Notizen,' and by the extracts from it in Virchow's critical remarks. He describes a constant increase of symptoms till the twelfth or fourteenth day, when a sudden change occurred.* The patient sank into a deep sleep, which lasted sometimes, with short broken intervals, for thirty-six hours, and during which, the before quickened respiration became slow, the frequent pulse quiet, and the body was covered with warm perspiration. On awaking the patient was quite free from pain in the head, but complained of loss of strength and pains in the limbs. The fourteen days' duration from the commencement of the fever to the crisis ("vom Eintritt des Fiebers bis zum Erscheinen der Krisen,") was so constant, that Bärensprung found it in one half of his patients. In the other cases the commencement of the disease was not precisely known. In some cases there was no crisis, but gradual subsidence of symptoms. Relapses occurred in some instances, in three months from the first attack. An eruption, "roseola typhosa," was very constant, and appeared on the fourth, fifth, or sixth day of the disease.

In the memoir of Dümmler, which, in many respects, is the best we have seen, the author insists much more upon the occurrence of relapses than Virchow does: "*not infrequently*," says he, the patients were thrown back by one or more relapses "which were true repetitions" of the disease, and serve to separate it still further from abdominal typhus. (p. 336.) He goes on to speak of the "certainty" with which the crises† (die krisen) occurred on the seventh, eleventh, or fourteenth day; "nay, more," continues Dümmler, "the period of the relapses also appeared to observe a seven-day rhythm, so that a typhus with many repeated relapses at equal intervals resembled strongly an intermittent fever." (p. 337.) In many cases, also, Dümmler says that the symptoms indicated "gastritis, lienitis, and hepatitis;" the sequences of the fever were a peculiar ophthalmitis, parotid swellings, rheumatism, and not unfrequently anasarca, as after scarlet fever, in fact the very sequences of relapsing fever.

It may seem unnecessary to cite further evidence that the fever described by Dümmler was relapsing fever; but it so happens that we are enabled to bring forward stronger evidence still on this point. Dümmler himself suffered from the fever, and relates his own case at length. We cannot insert the full particulars, but a short abstract will be interesting and instructive.

* Virchow says, he never saw the sudden disappearance of the symptoms described by Barensprüng and by Dümmler.

† Dümmler writes thus of the phenomena of the crisis: "The transition to convalescence, which usually was observed on the eighth or the eleventh, but seldom on the fourteenth day, occurred with important so-called critical appearances or not. In the last case, the symptoms diminished gradually for some days, and a light sweat occurred. This happened in very mild cases, in those combined with severe lung-catarrh or diarrhœa. *Much more frequently* the convalescence was marked by extreme changes in the symptoms, which were entirely altered, and by copious discharges from the secreting organs, with the cessation of which a decided and untroubled convalescence commenced, so that, in the greater number of cases, the end of the disease was suddenly reached." (p. 346.) He says afterwards: "In many cases the disease is not finished with one attack; it repeats itself in one or more relapses, (sie wiederholt sich in einem oder mehreren Recidiven.)" (p. 348.)

Dümmler was taken ill on the 26th of March, with extremely severe frontal headache, heaviness in the head, shivering, and hot flushes, heaviness and trembling of the limbs, and loss of appetite, although the tongue was clean. In the evening he took a purgative, which brought away six bilious, greenish stools. The night was sleepless, with copious sweat. On the following day an eruption appeared, (termed roseola,) which spread over the whole body. In the course of the day, pains in the muscles of the right shoulder and right upper arm, which had been felt the day before, increased so much, that they "resembled the severest rheumatism." The pulse was 120, and there was a disagreeable sensation at the epigastrium. On the following day, (the third,) the eruption began to get paler; the tongue was white and drier; the other symptoms the same—there was constipation. On the following night, being very restless, he took six grains of Dover's powder, which made him sick ten or twelve times, the vomited matters being bilious, and purged him also; the vomiting and purging brought on a state of collapse and syncope. On the 30th (the fifth day of disease) the "irritation of the stomach" was even worse, and there was pain on pressure in the epigastrium; the weakness was very great; the pulse small and feeble; the voice toneless; the pains in the shoulder and head somewhat better. In the night there was delirium. On the morning of the 31st, (the sixth day,) jaundice came on, and during the following night the sweat was very profuse, the pulse 120. On the 1st of April (the seventh day) the symptoms continued the same till the evening; the jaundice becoming intense. During the night the sweat diminished, the tongue became moister; the weakness continued, but there was a feeling of improvement. On the 2d, moderate sweat, and a warm skin. At night, some hours' sleep; the pulse fell to 60; the appetite returned; the urine was copious and pale; and there was a general feeling of convalescence. The yellowness began to go off. On the tenth day there was epistaxis; on examination the liver and spleen, which had been large, were found still swollen; the strength was returning. Recovery seemed to advance to the 8th of April, (the fourteenth day,) when suddenly shivering came on, succeeded by noise in the ears, headache: in the night, which was sleepless, there was great heat, the pulse rose to 90. On the following day, the epigastrium became again painful, there was severe pain in the stomach, "and all the gastric symptoms of the first attack came back again." The course of the symptoms was precisely similar for some days. On the 14th of April, (the twentieth day,) the feet swelled. On the 16th, (the twenty-second day,) the symptoms abated, and the appetite returned. On the following day there was copious epistaxis. Convalescence advanced as before, although the muscular pains continued. On the 19th, (the twenty-fifth day,) desquamation of the cuticle commenced in the palms of the hands. On the 23d (the twenty-ninth day) the appetite again failed; there was a feeling of illness, followed by delirium at night, and on the following by shivering, headache, tinnitus aurium, and frequent pulse. In the afternoon ensued great (enorme) heat, and in the evening great sweating, bilious vomiting, "and return of all the inflammatory abdominal symptoms." On the 30th (the thirty-sixth day) the symptoms again suddenly diminished, and permanent convalescence was established, which was impeded in some measure by bronchitis, which came on during the third exacerbation.* "The periods of these three attacks of typhus," writes Dr. Dümmler, "were as follows: the first, from Sunday the 26th of March, to Monday the 3d of April; the second, from Sunday the 8th of April, to Sunday the 16th; the third, from Sunday the 24th, to the evening of Friday the 29th." (p. 361.)

Lorenz states that the typhus exanthem was constantly present; but as we are acquainted with his paper only through the abstract in Canstatt's 'Jahresbericht,' in which the eruption is not described, we do not know precisely what he means by the term.

In Suchanek's paper the fever of Teschner, in Austrian Silesia, is described, and two forms of fever and of eruption are mentioned. In the first form, the attack commenced suddenly, with pain in the head and spine, followed by fainting, noise in ears, heaviness, and extraordinary weakness. After six or eight hours appeared catarrhal symptoms; there was constipation, sleeplessness; and, in many cases, pneumonia came on. On the fifth and sixth days, yellow suffusion of the face, uncircumscribed redness of the cheeks, the tongue red and pointed; the spleen slightly swollen. At the same time appeared the "Roseola typhosa," *in the form of light flea-bites!* (in Gestalt leichter Flohstiche,) and lasted seven or eight days, then faded, and, in some cases, desquamation followed. On the eleventh day, convalescence. No patient died of this disease.

The second form began with heaviness and dyspepsia, for about a week; the patient then suddenly felt a great burning and constricting pain in the region of the stomach and heart, extending back to the spine. The face was pale; the eyes muddy or dim, (trube,) the head covered with sweat, the knees weak; the patient then vomited copiously. In the following night, the patient wakened suddenly with great constricting pain in the chest, the face was œdematous, the lips blue, the eyes staring and glazed. Towards morning there was relief. Between the third and fifth days, the eruption appeared in the form of somewhat *elevated little round pale red and yellow spots, which presently became darker coloured, became confluent, dark blueish red, and somewhat ecchymotic, and lasted ten to twelve days unaltered.* During convalescence, the feet swelled, and even general dropsy came on.

Suchanek subsequently mentions, that dietetic errors often produced relapses, ("gab oft Anlass zu Recidiven," Op. cit., p. 113,) and that the after-diseases were parotid swellings, abscesses, and inflammations of the joints.

With reference especially to post-mortem appearances, full details are given by Stich, Heller, Ideler, and Meyer. Our present purpose forbids us from going into these details; but we may mention, that during the first seven days the eruption disappeared after death, although the day before it may have been vivid; the spleen was greatly enlarged, and of variable consistence. After seven days, there were sometimes petechiæ on the body. Sometimes there was swelling and, perhaps, suppuration of the parotid. There was no deposit in Peyer's patches. Pregnant women

* As signs of these attacks, Dümmler found his nails marked by three white lines, indicating the failure of nutrition during the pyrexial state. Ophthalmitis was left behind, and troubled Dümmler even at the time he wrote his memoir.

were found to abort frequently, chiefly in the early days of the disease; extravasation of blood was found between the decidua and chorion.

Let us, however, leave Silesia for a time, and pass to another part of Germany. In 1847-8, a very terrible fever prevailed at Prague, and has been described by Schütz and Finger.

Schütz's description is as follows:

"The typhus," says he, "which invaded us in the previous year, and lasted from October, 1847, to May, 1848, was in respect alike of its course and its characteristic symptoms, *entirely different* from the epidemics previously observed, and it necessitates, therefore, some corrections in the previous mode of considering typhus......It is a form of typhus, which can run its course without intestinal changes." (Op. cit., p. 34.)

After it appeared in October, 1847, no case of ilio-typhus was seen for a long time. The eruption appeared in three forms: *erythema*, which was rare, roseola typhosa, and petechiæ, which were uncommon, but were mixed up with the roseola. Schütz examined many cases of ilio-typhus, (typhoid,) but could never find this "roseolous" eruption. On the contrary, in this epidemic *it appeared in every case, and was rather brown red than rosy red. When the eruption passed into petechiæ, it became darker in colour, and finally black blue.* Constipation was constant; alterations of mind and special sense constant; the spleen, tumour, and eruption constant; pulmonary complications, such as catarrhal and lobular pneumonia, very frequent. The disease spared no age, sex, or station; the youngest was 9, the oldest 80; the mortality was 12½ per cent. No consecutive dropsy mentioned. Parotid swellings were uncommon. Relapses were sometimes seen.

Another disease was, however, present, which Schütz says was at first mistaken for typhus. The patients complained of heaviness, frequent shivering, pain in the head, disordered sleep, anorexia, pain in the extremities; the skin hot, the tongue moist, not swollen; a tolerably good, not bis-feriens pulse; bowels regular; urine normal. After six or eight days, instead of an exanthem, as was at first expected, the symptoms disappeared in a body, and the patient was cured. "Upon the nature of this complaint," says Schütz, "I can say nothing determinate."

Finger's description is drawn from patients admitted into Oppolyer's wards, and is very accurate. The disease spared no age; the youngest was 5 weeks, the oldest 78 years. It was very contagious, as observed also by Schütz. An eruption was frequent; it appeared between the third and fifth days of the disease, first in the epigastrium and front of the chest, and often spread over the whole body, and was so copious that, at first, it might be taken for measles. At first it was in spots, which disappeared momentarily on pressure. It afterwards passed, in some persons, into petechiæ, and did not now disappear on pressure. There was no diarrhœa; the spleen tumour constant; lung-affection, especially pneumonia (lobular and lobar) frequent, endocarditis was sometimes observed. Anasarca often occurred, but was seldom accompanied by ascites; the urine in anasarcous cases often contained albumen. The mortality of all cases was 13·3 per cent.

Köhler has also described the same disease; and states, also, that cases of ilio-typhus were mixed up with it, which were sometimes not distinguishable before death. As we are only acquainted with this paper through Eisenmann's abstract, (in which this opinion is sharply criticised,) we shall not further allude to it.

We have been obliged to pass so rapidly over these elaborate papers, many of which are of great length, that we feel we have hardly done justice to their authors; but we have adduced evidence sufficient for our present argument.

In this Silesian epidemic, the common German fever, the ilio-typhus or typhoid fever of the French and English, did not form an element. Those who observed the Silesian fever most deeply, Bärensprung and Dümmler for example, were convinced of its specific difference from the fever described by Louis.

If, then, it was not typhoid fever, the Silesian fever must have been typhus, or relapsing fever, or both, or some fever unknown to us. We may put aside this last hypothesis as unnecessary, since we can explain this epidemic by a reference to the other two diseases.

It is absolutely certain that relapsing fever formed part of the Silesian epidemic. The description of Dümmler, and the account of his own case, and also the observations of Bärensprung, are irreconcileable with any other supposition; and, although Virchow has not indicated those striking characters which the Scotch observers have indicated with such precision, this is easily explained, when we remember the necessary hurry and imperfect observation which a short official journey necessitate.

But one symptom is noted by Dümmler, by Bärensprung, Virchow, and every one, which was not common in the Scotch epidemic, viz., a copious eruption. Collating the accounts of this eruption, we find it to have presented the following characters:—It appeared very early, generally on the second day, (Dümmler,) or on the third, (Trustius,) or from the third to the sixth, (Bärensprung;) it commenced on the flanks, and then spread to the body and limbs; it was spotted with darker centres than borders, rosy or pale red, (not dark,) effaceable by pressure, and not obvious after death. Its duration was extremely short; in twenty-four hours it began to grow pale, and in two or three days had disappeared. It was followed by desquamation. It was not constant, as Dümmler saw four cases which had each two relapses without it; and other observers often did not find it. In all these characters it was different from the typhus eruption.*

Such an eruption has not been frequently noted in England in relapsing fever, but we think it has been recorded. Thus Halliday Douglas found, in one case, an eruption of bright, rose-coloured maculæ on the extremities; the colour disappeared under pressure; on the following day it began to fade. It appeared on the sixth, and lasted till the ninth day. Douglas remarks, that "this eruption is different from that observed in typhus." (Op. cit., p. 10.) Still this eruption was not quite the same as that of the Silesian fever. Cormack also describes, in one case, (p. 73,) a "rosy lenticular eruption;"† and Robert Paterson mentions, that he observed

* The characters correspond pretty well with the mulberry rash, during the first three or four days of its appearance; but then this becomes deeper in colour, ineffaceable on pressure, and does not disappear after death. It is hardly possible to suppose, that if typhus and relapsing fever were mixed up in the Silesian epidemic, the eruption of one should have been observed so loosely, and should have been attributed, also, to the other disease.

† It was not, however, macular or blotchy; there were only ten spots on the chest. In another case (p. 70), there were also spots "of a pale lake colour."

also a measley eruption, sometimes in the first days. Possibly, the very evanescent nature of the eruption, and its appearance in the early days before patients come into hospital, may have prevented its detection in England and Scotland, or it may not have been so frequent here as in Silesia.

Admitting that a light-coloured, macular, or spotty eruption may occur in relapsing fever, all the other symptoms mentioned by Dümmler agree entirely with those laid down by the Scotch physicians. Jaundice did not seem common, but did appear as in his own case. The sequences were exactly the same.

The first eruption described by Virchow was probably this macular evanescent exanthem; the second was perhaps the flea-bite eruption, which was often noticed in England, and which, as it occurred in Silesia also, we think must now be held to be a true eruption.

Now as decidedly relapsing fever was present in Silesia, and as there is no question but that this is a distinct fever from exanthematic typhus, we have to see whether the varying accounts of writers can be reconciled by supposing that relapsing fever was the only one. There appears certainly to be some evidence, in the great mortality in some places, and in the absence of relapse and crises (which could not have been always present, else Virchow would probably have mentioned their constancy), that intercurrent cases of true typhus were mixed up with the relapsing fever. But this is not very certain.

Suchanek's second form is evidently typhus, although his first is probably relapsing. The Prague fever, on the contrary, appears to have been almost entirely exanthematic typhus. So that we may suppose in Upper Silesia, relapsing fever to have been most common; to have been interspersed with typhus in Austrian Silesia; and to have been entirely supplanted by typhus in Prague.

We cannot but regard the facts recorded of the Silesian, or rather the German epidemic, as strongly confirming the doctrine of a diversity of specific fevers. It is true that if exanthematic typhus was mixed up with relapsing fever in Silesia, the acute German observers did not distinguish the two. But then it must be remembered that they are not so familiar with typhus as we are; it is very rare in many parts of Germany; and it was not till 1847, that many German observers believed that such a disease existed. In Scotland, in 1843, the relapsing fever presented itself to men who were thoroughly acquainted with the endemic typhus; and as it was almost dominant, they were enabled to study it with great precision.

It may be that our conclusions are erroneous, and that the Germans are right in not distinguishing relapsing fever from typhus; but if so, we know not that any medical evidence whatever can be relied upon.

We must now draw this long analysis to a close. It has been to us a pleasing labour to have accomplished it, for we are persuaded that there is no subject so important as an accuracy of Diagnosis. For diagnosis is the very foundation of therapeutics; and he who clearly indicates how a disease can be recognised, is fellow-labourer to him who points out how that disease may be cured. As long as the specific fevers were confounded together, the treatment of fever was necessarily vague and changeable. Like Sydenham, men watched each new phase of the disease, and had to discover what might be its appropriate treatment. But now with our improved knowledge of those diseases we may anticipate the time, when the readiness in diagnosis will have led to its inevitable consequence, ease, and safety in treatment.

We have now alluded to three well-marked and distinguishable diseases, which were formerly included under the common appellation of fever. It does not follow that there may not be a fourth disease, which is also occasionally mixed up with the three former, or prevails more or less extensively by itself. We refer to a short fever, lasting six to eight days, and which has been deemed by some febricula.* The fever described by Cullen as synocha may possibly be in part referred to this head, although we think that Cullen's type of *synocha* must have been Relapsing Fever.

ART. II.

Guy's Hospital Reports. New Series. Vol. VII, Part I.—*London,* 1850. 8vo, pp. 271. With Ten Plates.

THE first and most important paper in this part, is Mr. Wharton Jones's 'Astley Cooper Prize Essay,' on the State of the Blood and Blood-vessels in Inflammation, which has already received full notice in our last volume. We now present our readers with our customary analysis of the remaining papers.

II. *On the Application of Chemical Analysis, and Microscopic Examination of Morbid Products, to the formation of a correct diagnosis;* by Bransby B. Cooper, F.R.S.

We do not find anything very novel in this paper, though it may be read with interest and advantage, and is likely to do good service as recording the strong testimony of a surgeon of high repute to the practical value of histological and chemical researches. Of the various cases related in support of his opinions, we can only refer to one or two; and of those the first which we shall select is chosen, not as illustrative of the application of either of the methods in question, but simply because of its own intrinsic interest:

"Thomas Kirby, æt. 38, was admitted into Guy's Hospital, under Mr. Cooper's care, December 12th, 1849, with enlargement of the left testicle, and a tumour in the left supraclavicular region. About thirteen years previously, he had been attacked by inflammation of the testis, without any apparent cause. The attack readily yielded to treatment, but a slight degree of enlargement remained permanently. The testicle became every now and then painful, and after each such attack, there seemed to be an increase in its size; but, on the whole, the patient continued in nearly the same state until within six months of admission, when a surgeon, considering the disease to be hydrocele, tapped it; only a few drops of fluid, tinged with blood, escaped. At the time of his admission, the testicle presented a somewhat oval tumour, about the size of an elongated orange, hard, without fluctuation, and free from pain, unless handled. The abdomen was swollen, and presented a regular enlargement, as if from ascites, and fluctuation was quite distinct. This swelling, as the patient stated, had existed about two years before as a tumour on the left side, which disappeared under mercurial treatment; the swelling soon, however, returned, and became generally diffused over the abdomen;

* Dr. Jenner thinks that the disease described by Davasse, under the name synocha, is the short fever, termed in England febricula.

at this period paracentesis abdominis was performed, and afterward twice repeated. He described the fluid to have been always of a white milky appearance. The tumour in the neck was as large as the fist; it was situated on the left side, above the external half of the clavicle, between the mesian line of the neck and the anterior edge of the trapezius muscle; it was tense, and slightly fluctuating, freely moveable, and painful to the touch, producing a constant uneasy sensation in the shoulder, extending into the subscapular region. This tumour in the neck was first noticed about ten weeks before he came into the hospital, at which time it was comparatively of small size. He grew gradually worse, became affected with a troublesome cough, and died on the 7th of March.

"The body was examined twenty hours after death by Mr. John Birkett. The head was not opened. The right lung was normal, with the exception of slight recent serous effusion into its tissue, and one or two small patches of consolidation were discovered. The left pleura contained a large quantity of bloody serum, and there were patches of ecchymosis beneath the pleura costalis; the lung itself also was collapsed, but was easily inflated, and appeared to be in a normal state. A large mass of medullary sarcoma was developed around and compressed the left bronchus, and its canal was completely obstructed, and rendered impermeable to air by thick tenacious mucus. This medullary mass was in continuity with another, occupying the clavicular region, constituting, indeed, the tumour in the neck which has been already described as being apparent during life. The heart was remarkably small. The abdominal cavity was filled with an enormous cyst, over which the descending colon passed into the pelvis, where it became adherent; it was evident that the descending mesocolon formed a portion of the walls of the cyst, probably assisted in its formation by a layer of the mesentery, as the cysts were entirely behind the peritoneum, and the small intestines were pushed into the right lumbar and hypochondriac regions. The cyst was multilocular, and contained an immense quantity of perfectly white fluid resembling thick milk, and had adherent to its inner surface masses of medullary sarcoma; some smaller cysts contained matter like clotted cream. The aorta was surrounded by medullary sarcoma, as well as the left kidney, which was also atrophied. The right kidney was hypertrophied, but in other respects healthy. The left testicle was converted into a large medullary mass, and the spermatic chord was diseased up to the external ring, above which it appeared quite healthy. The lacteal vessels were in many places much dilated; and it was the opinion, from the appearance, that the diffused white fluid was a collection of chyle, which had been poured out in consequence of the bursting or ulceration of the lacteals, owing to the obstruction of the thoracic duct from the pressure of the tumour in the abdomen, and that on the left side of the neck; the latter had evidently been the great source of obstruction to the entrance of the chyle into the thoracic duct, shown by the presence of a clot of blood, just at the point where the duct terminated in the vena innominata. The tumour also pressed, at this point, on the left bronchus. We can but suppose, therefore, that the chyle meeting with this obstruction, throughout its whole course, became extravasated into the mass of fungoid mesenteric glands, and by slowly collecting, stretched the layers of the mesentery and meso-colon, and thus formed the cysts which have been described. Much of the medullary sarcoma presented a gelatinous appearance, and answered to the description of the 'gelatinous cancer;' a large portion of it descended into the pelvis, and was in a sloughing state.

"The fluid, under the microscope, presented all the appearances which have been so well described by Mr. Gulliver; the molecular base was distinctly seen, and covered the field of the microscope, having the larger chyle globules floating in it; and, upon the addition of ether, the molecular spheroidal bodies quickly disappeared; the globules themselves remained unchanged; some few blood corpuscles were also seen intermixed with those of the chyle. In comparing the microscopic appearance with that delineated in Mr. Gulliver's book, I found them so similar, in every respect, that there could be no doubt of the fluid being chyle, but mingled with some malignant cancer cells." (pp. 112-115.)

In this case the microscope did no service during life, but it afforded valuable aid in explaining, after death, what would otherwise have been sufficiently obscure. In the next case which we shall quote, the instrument enabled Mr. Cooper to form a correct diagnosis, and to decide on the right treatment, neither of which he would probably have done without its assistance:

"A young lady, æt. 28, called at my house on the 20th of February, 1850, under the following circumstances. She stated, that fifteen years before, she had fallen upon her nates, causing her considerable pain, especially on the right side of the part injured, and that from that period she had perceived an enlargement in the right gluteal region, which had gone on gradually increasing in size. Upon examination, I found a tumour about the size of a child's head, tense and fluctuating, perfectly painless, even upon manipulation, and without the slightest discoloration of the skin. With a small trochar and canula I drew off about ℥xij of perfectly limpid fluid, as clear as from a mountain stream; the specific gravity was 1008; by analysis it was found to contain chloride of sodium and a small quantity of phosphate of lime, properties so exactly coinciding with those of the fluid of the spinal marrow, that I was at first led to suspect some communication between the tumour and the vertebral column; but upon more minute inquiry into all the symptoms, the perfect absence of any approach to affection of the spinal nerves, convinced me that the opinion was fallacious. Upon placing the fluid under the microscope, I soon detected numerous echinococci, which cleared up the case at once, proving the tumour to depend upon the development of hydatids. I determined, therefore, not to interfere further with the tumour, having intended, before I made this discovery, to inject it; but, from my experience in cases of hydatid tumours, I have found, that the constitution of the patient remains unimpaired, so long as the animalculæ are alive; and that irritative fever is excited directly they are killed, as they immediately begin to act as any other extraneous matter, and create considerable local as well as constitutional irritation. I saw the lady again in March, when I found the tumour had scarcely increased since I had drawn off the fluid, and that it had remained quite free from pain; but she told me that her face and neck had become covered by an eruption on the day I drew off the fluid, which remained nearly a week, and then disappeared." (p. 116.)

In cases of abscesses occurring after lithotrity, or from stricture, or a blow on the perinæum, which abscesses may form in the groin, or even the upper part of the thigh, as well as in the perinæum, Mr. Cooper very properly points out the importance of examining the purulent matter for urea, so as to determine whether the abscess has been caused by extravasation of urine, or merely results from irritation. It is easily done by evaporating a small portion of the pus to the consistence of syrup, rendering it quite cold by placing the vessel on an ice-bath, and then adding a few drops of pure nitric acid. If urine be present, crystals of nitrate of urea will be quickly formed.

III. *Cases selected from the Ward-Books of Petersham House: with remarks;* by J. C. W. Lever, M.D.

All these cases are interesting, and deserve perusal. We select two for special notice, as being the most unusual. The first is one of retention of urine from ruptured aneurism. H. B—, æt. 33, a married woman, was admitted March 17th, 1850. About twenty-five years before, she had been run over by a cart, and for some time suffered much in consequence. During the last five years she had had acute rheumatism, chiefly affecting the left knee and ankle. In October, 1848, she had symptoms of peritonitis, and again in January, 1849. In February, 1850, she again had acute rheumatism, especially implicating the knee-joint, from which she was recovering well, when on March 15th she was knocked down by a cart, the wheel of which went obliquely over the pelvis, and caused retention of urine. On the 17th, Mr. Wilkinson and Dr. Power attempted in vain to introduce the catheter, and she was brought to the hospital.

"When admitted, she was in a state of extreme collapse, the countenance of a deadly pallid hue, expressive of extreme anxiety and of intense agony; the lips blanched; and the whole surface of her body pale and cold; the tongue dry and furred; the pulse extremely small and very rapid. The abdomen enormously distended by the widely expanded bladder, and extremely tender on pressure. On the right thigh, three old and deep cicatrices were observable. On making a vaginal examination, the pelvis was discovered to be completely blocked up by what appeared to be an immense mass of malignant disease, there being barely room for the introduction of our finger. The tumour seemed to proceed from the right side, pushing the bladder towards the left. After some exertion and trouble, Mr. Cock detected what appeared to him to be the orifice of the urethra, and through it introduced a small probe into the bladder, and taking this for his guide, passed No. 1 male catheter; the bladder was speedily evacuated, and the patient considerably relieved. The catheter was left in the bladder; but the following morning there was tenderness and pain over the abdomen, with tympanitis. From this period she gradually sank, and died exhausted, March 20th, at 1 p.m.

"Necropsy twenty-five hours after death. The vagina and pelvis were completely filled with a mass of coagulated blood, which on further examination was proved to have resulted from the bursting of an aneurism, situated just at the division of the common iliac artery of the right side. There was no malignant disease, nor was there any stricture in the urethra; but the clot had, no doubt, firmly pressed against it, and thus obstructed its canal. A false passage was found beneath the urethra, from the introduction of the catheter. There was also a large sloughy opening in the posterior wall of the vagina. No peritonitis nor any visceral disease.

"It is probable in this case, that the old injury had given rise to such mischief in the coats of the artery, as to lead to the subsequent formation of an aneurism, which the recent violence had sufficed to burst. The attacks of rheumatism supposed to affect the bone, might derive some of their pain from the pressure of the aneurism upon the compressed nerve; while the former difficulty and recent impossibility to micturate were, no doubt, owing to the pressure of the tumour, and more recently to the gradual effusion of blood, the accumulating pressure of which was sufficient to constrict the urethral canal."

The next case to which we shall refer is one of *retained catamenia*, which was not of a very ordinary character. The patient, æt. 24, had been ill five months. About twelve months before admission, after more than usually prolonged exertion, she was, for the first and only time, unable to evacuate her bladder, and required the use of the catheter. She commenced to menstruate between 17 and 18, and was regular in all respects until within the last few months, when, as she affirmed, the discharge had become more profuse, and attended with a sense of weight, oppression, and pain in the back, and bearing down in front. About five months ago, and two prior to the supervention of the last catamenial period, she detected a small hard tumour, low down in the iliac fossa of the right side; it was then nearly as large as a hen's egg, and it continued gradually to increase, without pain, until very lately, when she had occasionally suffered from a sharp momentary pain, piercing the centre of the mass.

On examination, a large, irregular, and somewhat flattened tumour, of about the size and shape of a full-grown fœtal cheek, was easily to be detected through the abdominal parietes of the right side, extending from the lowest point of the iliac fossa (where no limit could be felt to its course) upwards to within an inch of the umbilicus, occupying a space to the left of this of about an inch and a half in its transverse diameter, and to the right as much as three or four inches. At the top of the mass, on the extreme left, a small hard swelling, about as large as a duck's egg, could be felt, which seemed to be separated from the general mass, as it could be moved independently of it, though any movement of the latter communicated its motion to the former. Exactly corresponding in position to this smaller tumour, another hard, very irregular portion, could be detected to the extreme right, which, however, seemed to be a part of the general mass, differing merely in its general hardness and irregularity of surface. The whole tumour, though easily moveable by the hand from side to side, or from below to above, was not in any way affected by the position of the body of the patient. On minute manipulation, it afforded that peculiar obscure sense of fluctuation which is often felt in a malignant mass. The stethoscope gave no evidence of a fœtal heart, or placental souffle.

Per vaginam a large fluctuating mass could be felt through the anterior wall, coming close down to the vulva, and could be made to protrude externally by pressure on the tumour above. The sensation conveyed to the finger was precisely like that of a tough bag of membranes distended by a labour pain. The anterior wall of the vagina was much infringed upon, not being more than half an inch in length immediately behind the symphysis; on passing the finger gradually round towards the right sacro-iliac-synchondrosis, the length was found about one inch and a half; to the left it was impossible to reach the end of the canal with the finger. The os uteri could not be felt. It was seen with difficulty through the speculum, high up posteriorly, and to the extreme left. A gum-elastic catheter was passed through the os, but could only be introduced about a couple of inches.

Next day, probably as the result of all these manipulations, an immense quantity of chocolate, brown-coloured fluid, was discharged from the uterus; and the parts gradually recovered nearly their normal condition.

It is clear that this was a case of extreme anti-flexion of the uterus. A similar one was in the hospital some years previously, and then the tumour was tapped by the late Mr. Aston Key. Dr. Lever saw the woman a few months ago, and found two openings to the uterus—the one, the natural one, the os; the other, small and round, the artificial one.

IV. *On Anæmic Murmurs and their Diagnosis;* by H. M. Hughes, M.D.

This is a useful, practical paper, though containing little that is novel. Dr. Hughes seems to incline to the opinion that the seat of these murmurs is rather the pulmonary artery than the aorta. He believes that they depend mainly, if not entirely, on two causes: first, the watery state of the blood itself, by which the celerity of its motion may be augmented, and its molecules more easily passed over each other, and by which, therefore, eddies and waves are more easily produced, and attrition necessarily increased; and, secondly, the increased *velocity* with which the blood is propelled through the large arteries arising from the heart.

To assist in the diagnosis, which, after all, is quite the most important part of the question, the following tabular view may be of service :—

Anæmic Murmurs.	*Organic Murmurs from Valvular Disease.*
1. The anæmic condition necessary.	1. The anæmic conditions occasional, but not necessary.
2. Congenital disease, or rheumatic fever, in young subjects, almost necessarily excluded.	2. Congenital disease, or rheumatic fever, in young subjects, almost necessarily included.
3. Absent while the pulse is slow.	3. May be, and usually are, present while the pulse is slow.
4. The pulse assimilates to the impulse of the heart.	4. The pulse does not assimilate to the impulse of the heart.
5. Almost exclusively confined to those little advanced in age.	5. Most common in, though not confined to, those advanced in age.
6. Venous murmur universal at one time or another, under favorable circumstances.	6. Venous murmur absent, unless combined with anæmia.
7. Confined to the vicinity of the aortic valves.	7. Not confined to the vicinity of the aortic valves.
	Sound from pericardial or pleuritic roughness.
8. Audible at all times during the systole of the ventricle, but only if the pulse be frequent and vibrating.	8. Audible only, or more especially, during the inspiration, and whether the pulse be slow or frequent.
	Arterial murmur from disease of the lung or pleura.
9. Equally audible during expiration, as well as during inspiration.	9. Audible only, or more loud during inspiration.

V. *Cases and Observations in Medical Jurisprudence;* by A. S. Taylor.

Like all Dr. Taylor's productions, this paper is full of interest and instruction; but being a collection of cases, each selected for the special lesson it is calculated to teach, not as illustrative of any one law or set of laws in the action of poisons, it is scarcely susceptible of analysis. The first eight are instances of arsenical poisoning, and a perusal of them will show how many new points careful observation will bring out, even in a matter already so well investigated. The antidotal power of the hydrated sesquioxide of iron is not substantiated by them; it failed in all the instances in which it was used. But, on the other hand, the value of tartar emetic, in the treatment, is strikingly illustrated by two cases which occurred in the practice of Mr. Beardsley, of Heanor. The first was that of a child only six years old. The quantity swallowed was a teaspoonful, about 150 grains, an enormous dose at such an age. It was swallowed early in the morning when the stomach was empty; and the rapid action of the poison was shown by the very early occurrence of the symptoms. The antimony acted freely and rapidly upon both the stomach and bowels, and the child recovered without one bad symptom. In the second case, 120 grains of arsenic were taken also on an empty stomach, and in half an hour the girl was seen by Mr. Beardsley, with all the symptoms of arsenical poisoning, well developed. The exhibition of nine grains of tartar emetic in two doses placed her out of danger, by the apparently complete removal of the poison from the stomach.

We consider these two most important cases; the more so, that the treatment is one against the employment of which there is a strong prejudice.

A case is narrated in which the application of the *acid nitrate of mercury* to the throat destroyed life in three quarters of an hour, by asphyxia. We perfectly agree with Dr. Taylor in his strong condemnation of the use of this escharotic in such a situation.

A case of poisoning by *red oxide of lead* is interesting, as being the only one of the kind on record. The dose was very large, a table-spoonful, about $2\frac{1}{4}$ oz.; but the woman recovered without any bad symptoms, after vomiting and purging.

We would also direct special attention to the report of a case of poisoning by the *bichromate of potash.* The patient recovered after an illness of four months, the urgent symptoms being those of severe gastro-intestinal inflammation.

The last case recorded is one of *vagitus uterinus.* It was a face presentation, the head being apparently arrested at the brim. The medical attendant, Dr. R. Crothers, of Moy, County Tyrone, says: "I endeavoured to move it into a more favorable position, when, happening to introduce a finger into the child's mouth, I was very much surprised to hear a *distinct cry,* which was repeated two or three times, and so loud as to alarm the mother and attendants."

VI. *Select Cases of Hernia: Fourth Series;* by E. Cock.

VII. *Insuperable Constipation, Symptoms of Strangulated Hernia, Peritonitis, Stricture of Rectum;* by John Birkett.

The most remarkable feature in this case was the absence of anything to lead attention to the state of the rectum, though after death there was found complete obstruction in its upper third over an extent of two inches. The patient's complaints were entirely referred to an old rupture, which, upon operation, was discovered not to be strangulated.

VIII. *Ophthalmic Cases, with Remarks;* by J. F. France.

This paper contains a report of two very interesting cases of *dislocation of the crystalline lens.* In both there was entire loss of vision at first, and in both it was subsequently restored. The treatment was directed at first to the prevention or removal of inflammatory action, and then to the prevention of absorption, so as to relieve the retina from oppression. Mr. France insists strongly, and we think most judiciously, on the necessity of allowing the lens to remain in its abnormal position, under the conjunctiva, for at least a fortnight, until the wound in the sclerotica becomes perfectly healed. It is then readily removed by incision.

IX. *Case of Malignant Disease of the Tongue, in which the Lingual Gustatory Nerve was divided;* by John Hilton, F.R.S.

The last member of the above sentence expresses the one point of interest in this case. The operation had no influence upon the course of the disease, but it removed the pain for a time, and enabled the surgeon to take away portions of the diseased mass by ligature, without suffering to the patient. It is worthy of note that exactly one month from the date of the division of the nerve, the patient began to feel again at the tip of the tongue, and sensation gradually returned more and more completely.

X. *Case of Chronic Laryngitis, Pregnancy, and Tracheotomy: with remarks;* by H. R. Rump.

This also is an interesting case. The patient was nearly worn out with the disease, when the operation was performed. She began to improve immediately afterwards, made an excellent recovery, and gave birth to a child at the full period, five months after the operation.

Art. III.

Fifth Annual Report of the Commissioners in Lunacy.—30th June, 1850. (Parliamentary Paper.)

The eighth and ninth of Victoria, c. 100, § 2, provides that there shall be paid to each of the six Commissioners in Lunacy, over and above their respective travelling and other expenses, the yearly sum of one thousand five hundred pounds, by four equal quarterly payments. As the times go, this is a handsome sum of money; and as Christmas-day is made one of the "quarter" days, that festival must be particularly festive to "the Commissioners in Lunacy."

It is very reasonable, indeed, that these gentlemen should make a succinct statement, from time to time, of their travellings and inspection, as well as of their travelling expenses. "Work and labour done" is a significant phrase; and we presume the report before us is the "little account" of the Commissioners, in this respect, presented to John Bull through the Lord Chancellor. Whether the Lord Chancellor has audited and found the account correct, we do not pretend to say; we fear not, however, for his lordship has ample employment for his time in his own court, if the lamentations as to Chancery arrears be well founded. Such being the case, the report must be audited extra-officially, and subjected to the scrutiny of public opinion. The labours of the Commissioners must be great, if they *properly* fulfil the duties assigned to them by the act. They have to visit each licensed house within their immediate jurisdiction "four times at the least in every year;" and, if in the country, "twice at least in every year," and every hospital, gaol, and workhouse, in which there are lunatics, "once at least in every year." They are to inspect every part of each licensed house, see every patient, examine the order for admission and the medical certificates, inquire into the cases of personal restraint, and make a minute of the condition of the house, the patients, and the certificates, and touching various other matters. There are 24 asylums, (county establishments,) containing 7140 inmates; 13 hospitals for lunatics, with 1208 inmates; 45 private asylums in the metropolis, containing 2945 persons; and 92 in the provinces, with 3786 inmates; making a total of 15079. Of these, the sexes, &c., are as follows:

Private.		Pauper.		Criminal.	
Males.	Females.	Males.	Females.	Males.	Females.
1906	1868	5168	6137	211	53

In addition to these establishments, the Commissioners visited 268 workhouses, (some twice and thrice in the year,) containing about 3000 idiotic, imbecile, and insane persons. If we are correct in our inferences, from the statements in this report, and from the provisions of the act, the Commissioners have to pay at the rate of one visit annually to 30,000 lunatics, or, taking 300 working days in the year, to see 100 insane persons per diem, the year round. By the same calculation, we come to the conclusion that they have to pay at the rate of one visit annually to 577 establishments, and make the inquiries and observations already stated. In addition to these labours, they have to sit as a board from time to time, receive reports, make special inquiries into individual cases, &c. The Commissioners state that board meetings (each of several hours' duration) have been held almost weekly. Two of their number are also to be the active members of a "private committee," and inspect those insane individuals who are inmates of private houses "at all reasonable times." All this work and labour is professed to be done—really performed—well and faithfully. Whether the performance be equal to the profession, is to us very doubtful—nay, almost incredible—for we think it almost impossible.

With regard to the condition of the insane, and of their residences, the report is favorable. Amongst the 15,000 insane there were only 8 suicides. The various asylums throughout the country are in an improved state; and the conduct of the superintendents, officers, and attendants, in reference to the treatment, the patients, and the management of their establishments, is for the most part humane and judicious. The Commissioners report, however, that in several instances they have found it necessary to animadvert upon the excessive use of mechanical restraint; on the neglect of cleanliness; on inadequate ventilation; on want of sufficient attendants; on improper or deficient diet; on the dirty condition, or scanty supply, of bedding and clothing; on irregularities in the medical books; and on other defects. In about 70 instances, these defects were deemed of sufficient importance to require the special interference of the board. We believe some of these "instances" were very trivial affairs indeed, and the special interference of the board was of dubious dignity. In one case with which we are acquainted, this was certainly the case; and probably the mention of it may call the Commissioners' attention to their own conduct, and render them more careful. Two of their number visited an asylum, and went out of their way to report that the cases were detailed "in a satisfactory manner." No alteration was made in the method of entry into the case-book; yet, within a few months, the same Commissioners, at their next visitation, censured the method. It so happened that the entry of a solitary instance of mechanical restraint had been accidentally forgotten; for it is not possible for the most careful to be always *totus teres et rotundus*. Whereupon, the board specially interfere without the slightest communication with the medical attendant; and, without any inquiry or seeking any explanation, forthwith require the superintendent to communicate to him their censure, and their threats of prosecution for penalties. Now the medical attendant in this case was as anxious to do his duty as the Commissioners, and they would have lost nothing of their dignity if they had communicated with him directly; or, before they censured and threatened a prosecution, had, at least, afforded him an opportunity for explanation. If the Commissioners act thus hastily, they will, sooner or later, we are satisfied, find themselves in a difficulty. The statute under which they act is acknowledged by every one at all acquainted with its provisions to be difficult of comprehension, loaded with details and indefinite penal clauses, absurdly inquisitorial, and in various other ways objectionable. It is, therefore, the more necessary that the Commissioners should be very careful how they inspect, censure, and threaten.

An instance has lately occurred, in which the conduct of the Commissioners on another matter may admit of question. Our readers may have observed an advertisement in the medical journals, of a very peculiar character, headed "Lunacy.—Certificates of Insanity." By the 8 and 9 Victoria, c. 100, a certain form of certificate is provided; and by § 45, it is provided that it shall be separately signed by two legally qualified medical practitioners, not in partnership, "each of whom shall, separately from the other, have personally examined the person to whom it relates, ... and shall have signed and dated the same on the day on which such person shall have been so examined: . . . . and any physician, surgeon, or apothecary, who shall knowingly sign any such medical certificate, as aforesaid, which shall untruly state any of the particulars required by this act, shall be guilty of a misdemeanour." With this explanation, we subjoin the advertisement referred to:

"Lunacy.—Certificates of Insanity.—Whereas, the Commissioners in Lunacy have recently instituted proceedings against us, the undersigned medical practitioners, for having offended against the 45th and 49th sections of the Act 8 and 9 Victoria, cap. 100, in reference to certain certificates of insanity, which had been signed by us irregularly. And, whereas, the said Commissioners being now satisfied that such certificates were so signed by us in ignorance of the law, and not from any corrupt motives, but merely for the purpose of placing without loss of time a dangerous lunatic under proper protection and care, have consented to stay such proceedings, in consideration of our publicly acknowledging our offence. Now, therefore, we do hereby acknowledge that we have offended against the provisions of the said statute, and declare our deep regret and contrition at having so acted: and we do hereby, as required by the Commissioners in Lunacy, make this acknowledgment public, as a warning to all medical practitioners who may be called upon to give certificates of insanity, that they carefully and strictly observe the requirements of the statute in that behalf."

And then follow the names of the two penitential exemplars, which we suppress for their sake. And what will strike the reader at once, is the fact, that the Commissioners, after assuming their function of public prosecutors, stop the operation of the law, on certain conditions. Such a proceeding, on their part, is altogether unwarranted, as we believe, by the act. The 106th section gives them, and the visitors appointed, at quarter-sessions, the sole authority of prosecuting offenders against its numerous penal provisions; and any two justices may convict in those cases in which the punishment is a fine; also, "such justices, may, if they shall think fit, reduce the amount of the penalty by this act imposed for such offence, to any sum not less than one fourth of the amount thereof;" but we can find no discretionary authority vested in the Commissioners to make examples of offenders, and "warnings to all medical practitioners." The two delinquents referred to committed a very grave offence. They deliberately certified, that they had, separately from each other, on that day on which the certificate was dated, personally examined the person named in the certificate and order, whereas, they had not seen him nor examined him. No doubt, they knew perfectly well that the person was a dangerous lunatic; but they could only be justified by certifying the truth, and throwing on the law the onus of any damage or danger incurred by the delay which its formalities cause. These, we apprehend, are not of small amount, when the letter of the law is strictly complied with by attendants, superintendents, and medical practitioners; and it is certain that they would be greater if the letter of the law were not very frequently broken, in compassion for the patients themselves as well as their friends. Still, the extenuating circumstances were for a court of justice to decide upon, and not the Commissioners in Lunacy; and if the Commissioners, in the exercise of their duty, felt it necessary to give a warning, they should have advertised the *conviction*, and not the penitential confession of the offenders; or if they were of opinion that a prosecution was not necessary, they might have advertised the fact that individuals had, under the circumstances stated, incurred the *liability* of a prosecution.

There is another circumstance to be noted in connection with this affair. The advertisement appeared in the *daily* journals, "as a warning to all medical practitioners." What object the Commissioners had in view, in giving *popular* currency to the "deep regret and contrition" of the offenders, and their penitential acknowledgment of their offence, is best known to themselves; that it was unnecessary to the attainment of their *expressed* object is apparent, for a much less humiliating advertisement in the *medical* journals only, would have met all the necessities of the case. Or, if they thought it necessary, for reasons best known to themselves, to give the public an impressive example of their activity and energy, they might have prosecuted the delinquents, as in duty bound, recommended them to a nominal punishment on the ground of extenuating circumstances, and then advertised the trial and its results. This would have given their proceedings in this matter a more dignified character; and not made them appear as the diligent promulgators, by implication, of an insidious imputation on the whole profession. Everybody is acquainted with the popular prejudices against "doctors," as to their signing certificates of insanity venally and inconsiderately; in the advertisement of the Commissioners, the public will find a confirmation of these false and calumnious prejudices; consequently, we are justified in stating our opinion that, in their conduct of this simple matter, the Commissioners have struck a most unmerited heavy blow, and inflicted a great discouragement, on the whole profession.

We think this proceeding on the part of the Commissioners will be a more decided warning to the profession than they intended. Medical practitioners will be on their guard against the "red tapery" of the bill, and of its executive; and will take care to give full scope to its operation by a strict and literal attention to its provisions. The evil will then cure itself. The bill is of an exceptional character in English legislation. Its provisions are founded on the implied principle, that the attendants, superintendents, and medical practitioners, who have the care of the insane, are selfish, cruel, negligent, and untrustworthy. Hence the spirit of espionage which pervades it; hence the permission to visitors and Commissioners to violate the domicile, to enter houses containing the insane, at any hour of the night. Hence the continual practice of a sort of espionage by the Commissioners—their sudden and unexpected arrivals—their brusque entrance—their prompt "we are the Commissioners, show us the house"—their vivacious attempts to surprise, and therefore to conceal their movements. It is a painful thing to see English gentlemen condescending to such undignified proceedings; and the more painful because it is notorious that, cunning as they may be in their movements, they have generally met with more than their match. Practices, disgraceful to any well-conducted establishment, have only been accidentally detected *after* the official visit has been paid by the Commissioners, and a favorable report made. We believe them to be morally and physically incompetent to carry out a system of espionage, and we trust that they will avoid any attempts at it. They can never approach a district or house so stealthily, but that the superintendent *can* be forewarned, if he choose to be at the trouble and expense of having *his* spies; nay, where their persons are known, information of their arrival at "the station" or an adjoining town will be expressed by a volunteer spy, simply because the Englishman's instinct revolts against a system which implies cunning and stealthiness, and the office of "informer" in the agents.

The treatment of the insane is yet an unsolved problem. Of late years, there has been a very great improvement—partly due to stricter legislation, but owing, in a much greater degree, to improved *medical* knowledge and morals. The philosophy of insanity, to use a hackneyed phrase, is better understood; and although society still inflicts very heavy punishments on acts really resulting from corporeal disease, it oscillates oftener to the side of mercy. It is the medical profession who are the true guardians of the insane. A man (or a woman) wearies his relations, alienates his friends, wastes his substance. His mental disorder is the upas tree of affection and friendship; it blights the gentler feelings of domestic life and social intercourse,—it does worse than this—it engenders the antagonistic feelings, until at last the father, husband, wife, earnestly desire to bury their mentally and morally dead out of their sight. The necessary result follows. They think less and less about their suffering relation, when at last placed in an asylum, or only connect with his memory the most painful reminiscences. Consequently, they want to see him no more, and to risk no more the recurrence of past horrors. Under these circumstances, the poor sufferer becomes wholly dependent upon those to whom he is intrusted, for all those little comforts and amenities, which, by their sum, make up the earthly happiness of man. The report of the Commissioners notices this point:

"Whilst making our visitations in the course of the past year, we had reason to believe that, in some instances, private patients in licensed houses had not the benefit of that suitable accommodation, and those comforts, to which they were entitled from their circumstances and situation in life. In some cases it appeared, on inquiry, that the relatives were unable to afford a remuneration adequate to the expenditure necessary for proper accommodation and treatment; and, in others, that they neglected to appropriate a sufficient part of the patient's income in promoting his cure, or adding to his general comforts. A few cases also came under our observation, in which it was evident that the sums paid were amply sufficient to provide everything necessary for the comfort and restoration of the patients, but the benefit of which the patients, in fact, did not enjoy." (p. 15.)

A marked instance of this kind having been brought before the board, the Commissioners issued a circular, requiring the proprietors of licensed houses in the Metropolitan district to make out a tabular list, according to a specified form, of their private patients, specifying their station and profession in life, and the annual rate of payment charged for them, including "extras." They found, that "in various instances superior accommodation had been afforded to patients, more with reference to their former habits and station in life, than to the mere amount of money received for their maintenance."

On the important subject of the *property* of lunatics, the Commissioners say little, and that little is little to the purpose. They observe, that it has continued to occupy much of their attention; and that they have had frequent occasion to regret the very defective state of the existing law, more especially as respects the property of patients, tradesmen, and others, of small means, or whose mental malady is likely to be only temporary. All that they can say is, "we earnestly hope that this subject will receive your lordship's early attention." A very hopeless hope!

Previously to resuming our remarks on the working of the Lunacy Act, we will notice some other recommendations of the Commissioners. And first, as to procuring a superior class of attendants:

"In order to carry out extensive progressive improvements in Lunatic Institutions, whether of a public or private nature, it is of importance, in our opinion, that the attendants engaged at such establishments should be of a class superior to those to whose care the lunatic was formerly intrusted. It has been the endeavour of this Board to establish, as far as possible, a central register, in which the names of all attendants at lunatic asylums should be entered, with such remarks, in cases of dismissal for improper conduct, as might tend to prevent the parties inculpated from obtaining engagements at any other asylum. A circular has been issued from this office, in order to obtain, from time to time, the information necessary to establish and keep up this register; but we regret to say, that, owing to the negligence of some persons, and the unwillingness of others, it is far from complete." (p. 10.)

We believe that the "unwillingness" here referred to, is felt by asylum superintendents and governors of high respectability, and arises out of a reluctance to be a party to anything like a system of bureaucratic espionage and despotism. If the "red tapery" method of government were self-acting, and corrected its own abuses, some reliance might be placed upon its justice and efficiency; but it is notoriously the contrary. The managers of lunatic asylums have an immense responsibility, both moral and pecuniary; and they must have some better guarantee than the uncertain results of government patronage, that the bureau (or "*this office*") shall be well and efficiently officered, and the "central" register more effective than their own judgment and experience. "This office" is apt to get officious, presuming, meddling; to think *official* experience is wisdom and sound sense, and that no other experience can be wise and sound. The bare proposition of the ends and scheme of management is startling. An attendant is dismissed by his master "for improper conduct;"—does the "central register" propose to register the dismissal and the improper conduct on the *ex parte* statement of the master, without any inquiry into its nature or its extent, or the circumstances under which it occurred? Truly, when the object of such registration is *final*,—when it fixes the life-long brand of unfitness for his duties upon the dismissed servant,—when "such remarks" are to be made regarding his conduct, as will have the effect of destroying his worldly prospects,—or, in the periphrastic phraseology of the Commissioners, they are to be such "as might tend to prevent the parties inculpated from obtaining engagements at *any* other asylums,"—no dismissals should be registered until after the most careful inquiry, with all the guards and formalities of a court of law. But what a pleasant prospect for masters and servants this would be!

The 86th section of the Lunacy Act gives the Commissioners authority to sanction the temporary removal of patients from asylums, either to the sea-side or on visits to their friends. During the last year 88 patients have been thus indulged, with beneficial results. This is a small number, when compared with those to whom this most beneficial clause in the act might be very advantageously applied; and we would suggest to the Commissioners the propriety of extending its application. In the great proportion of chronic cases, a yearly "outing" would be equally agreeable and useful. The dull monotony of an asylum is far more wearying than the active duties of life; and it might be worth the consideration of persons interested in private asylums, whether a house by the sea-side might not be occupied with advantage. We believe that such an auxiliary has been contemplated by the spirited proprietor of a private asylum, and would have been caried out, but, unfortunately, the project was strangled ere matured by the "red tape" of the Act.

The Report speaks favorably of the training school for imbeciles at Highgate; but of course the Commissioners suggest "improvements." A pleasant thing it is to be always making "improvements" at other people's cost; but this is the perquisite and luxury of red tapery:

"In continuation of our last report, relative to the provision made for the care and training of idiots as a separate class of insane, we have the satisfaction of stating, that the comforts and advantages of the benevolent Institution, Park House, Highgate, and the benefits conferred thereby upon the unfortunate class for whom it is designed, have been justly appreciated by the friends of the patients (who are more properly termed pupils), and by the public. This Board have received from two of their number, who recently visited the asylum, a detailed report, showing that considerable progress has been made in carrying out the objects and maturing the plans of the institution. It appeared, at the same time, to the Visiting Commissioners, that the system admitted of further development and improvement, and they made several practical suggestions with that view. A copy of their report has been transmitted by order of this Board to the Committee of the Asylum, who, the Commissioners feel assured, will give every attention to their suggestions." (p. 11.)

It would have been better if the Commissioners had enlightened the Lord Chancellor (who represents the public), as to the nature of these "practical" suggestions.

An improvement has taken place in the management of the insane poor, especially in the adoption of early curative treatment. This (of course) is also the work of the Commissioners!

"The cases of insanity of recent date or acute form, which have been allowed to remain for a considerable period in workhouses, instead of being promptly transferred, as the law requires, to a lunatic asylum, have been of late years sensibly diminishing in number, and are now comparatively rare; a diminution which we believe is mainly to be attributed to the supervision exercised by the Commissioners over these establishments." (p. 12.)

The Commissioners forget that there are now greater facilities for the cure of pauper lunatics, that there is a widely diffused conviction (reaching even to Boards of Guardians), that early treatment is the most economical; and that there is really such a thing as a large and enlightened body of practitioners exercising "supervision" over workhouses, and giving sound advice to parochial authorities. We apprehend that the latter are entitled to, at least, a large portion of the credit which the Commissioners so credulously "believe" is to be attributed to *their* supervision.

The cholera prevailed in sixteen asylums, namely, in ten licensed houses in the metropolitan district, and in six asylums in the provinces. The Commissioners sent queries to each *infected* house, with a view to ascertain the origin, progress, and treatment of the disease, and its relation to their "sanitary" condition; the results are very meagre, as stated by the Commissioners, who curtly refer (the Lord Chancellor) to the tables for information which they ought to have analysed. We subjoin their "observations:"

"The patients attacked were, with few exceptions, chronic cases of insanity. Among the few recent cases, the symptoms of cholera seem to have supervened in a state of exhaustion following maniacal excitement. The attacks were generally sudden, the patients, in a majority of cases, falling at once in a state of collapse, without having suffered from premonitory diarrhœa. Nearly three fourths of those attacked by cholera had no premonitory diarrhœa. This circumstance, so much at variance with the usual course of the symptoms, becomes the more remarkable, when we take into consideration that several of the patients attacked with cholera had been subject to diarrhœa previous to the appearance of the epidemic in this country. The course of the disease was, in most instances, rapid, several patients being carried off in six or seven hours. The results were remarkably fatal, as appears by the fact, that out of 454 patients attacked, 311 died." (p. 49.)

Of the ten metropolitan asylums infected by the disease, six contain 2415 patients; these being, Peckham, Hoxton, Bethnal, Camberwell, Grove Hall, and St. Luke's. The other four are minor establishments, containing 182 inmates. In the provinces, the West York County Asylum, containing 633 inmates, lost 98 out of 133 attacked; the Wrekenton Asylum, near Gateshead, containing 37, lost 18 of 23 attacked; while the four others, containing, in the aggregate, 387 inmates, had only 43 attacked, and 28 deaths; of the latter 11 occurred in the Kingsland Asylum, near Shrewsbury, out of only 13 attacks,—a fearful mortality, if the facts be correctly stated.

The Annual Report of the West Riding County Asylum, at Wakefield, Yorkshire, presents us with some interesting details regarding the progress of the epidemic in that establishment. The account is the more interesting, as a most careful inquiry was made into the origin of the disease. The disease appeared in the neighbouring House of Correction in February, and in July and August many severe cases occurred in Wakefield and its suburbs; but the inmates of the Asylum continued in their ordinary state of health until the 17th of September, when E. F—, an epileptic female, was brought from the Morley Union Workhouse. The relieving officer who brought this person, brought also the intelligence that the cholera was spreading among the inmates of that workhouse, and that two persons had died from it on that morning. E. F—, the new arrival from this place, appeared well until night, when the nurse reported her to have been affected with vomiting, severe cramp, and rice-water evacuations. The following day she was removed to the hospital. On the 22d of September (five days after the admission of E. F—,) a patient in the same ward was attacked, and died on the 23d. On the 2d and 6th of October, two other females in the same ward were similarly affected, and died within ten hours; another was attacked in the same ward on the 8th of October, and died on the 10th. The attendant in a male ward (No. 4,) is by trade a joiner, and had, in that capacity, attended to the interment of the four patients just mentioned. On the 15th a delicate feeble patient in his own ward (with whom, from his infirm state, the attendant was brought more closely into contact than the rest,) was attacked, and died on the following night. After this case the epidemic became general. "No part of the household seemed to be exempt from the poisonous influence; though it would seem, from the virulence with which it spread in the most airy, pleasant, and healthy wards, that local circumstances were unconnected with its existence." It is worthy of remark, that of all the deaths, in one only, and that the last, was there the smallest expectation of a cure of the mental disorder.

The origin and whole course of this epidemic afford a strong indication that it was due to a contagious *materies;* if the facts we have stated do not show this, then no facts can ever demonstrate whether cholera be contagious or not. There is a circumstance of a negative character which, however, deserves record, as being only explicable by the same inference. When the cholera was epidemic in Wakefield and the neighbourhood in 1832, "the Visiting Justices held a meeting, and, as a precautionary measure, made a resolution, that no patient should be received from an infected home or district. *The asylum then escaped without a solitary case;* and the patients watched from the windows the numerous funerals of the victims removed for interment, from the neighbouring suburbs of the town, called 'East Moor.'" We think the Visiting Justices must regret that they did not take the precaution in 1849 which they so successfully carried out in 1832; for they must, upon reflection, come to the inevitable conclusion, that such a measure would have excluded the epileptic female (the first case) brought from the Morley Workhouse when the cholera was amongst its inmates, and with her might have excluded the *fons et origo mali.*

The case of Nottidge *v.* Ripley, and "certain dicta and opinions reported to have been pronounced by the Lord Chief Baron of the Exchequer" thereon, are referred to. The "letter" contravening these dicta (like all the labours of the Commissioners) was, of course, of great public advantage, "it served to counteract the mischief which we feared might ensue from the adoption of the principle laid down by the Chief Baron." This is rather ambiguous phraseology. What mischief *might* have ensued, and *did* ensue, is stated. A "careful analysis of the patients in the Lancaster Asylum" (we presume of the symptoms of their disorder) showed, that if Baron Pollock's doctrine were adopted in respect to them, 300 persons "totally unfit to take care of themselves must have been turned loose upon society." This is the mischief which *might* have ensued. What did really occur was, that "some of the pauper patients in a public asylum were examined, by direction of a Board of Guardians, with a view to ascertain how many of them ought to be discharged as 'not dangerous to themselves or others.'" It appears, further, that four patients were admitted into the Royal Edinburgh Asylum, who had been dismissed from confinement in consequence of Baron Pollock's remarks, and had committed sundry insane vagaries while at large. We think, that without derogating at all from their own merits, the Commissioners might allow some beneficial influence to the opinions of medical practitioners and others in charge of the insane, and some credit to the common sense of their friends. Generally, the friends of the insane have already had too much trouble and anxiety with the poor sufferers, to take advantage of a hasty expression of opinion from the bench, and recommence their troubles, as if they were something desirable.

The Report refers to *lunatics at large*, to express regret that no amendments in the law (though more than once suggested in former reports) have been submitted to parliament. The Commissioners observe, "we deem it most important that the public should be protected from the perils arising from the neglect of friends or parish officers to place lunatics under proper care. Instances have, during the last year, occurred which confirm our formerly expressed opinion, that danger or serious annoyance may be apprehended, especially to public characters or individuals of high station, owing to the defects of the law; defects which we have been much disappointed to find no measures have been taken to remedy." This seems to us mere querulousness. What plan or method do the Commissioners *propose?* Everybody "deems it most important;"—everybody is equally well convinced that danger or serious annoyance may be apprehended from the acts of uncontrolled lunatics; but common-place sentiments like these are altogether out of place in an official report, except as introductory to a plan (or principle, at least,) of action. The class of maniacs who are thus dangerous, are not the furiously, but the morally insane;—they are like a Pate, a M'Naughten, or a Thorn. To detect their insanity requires an experienced judgment; to prove it to the satisfaction of an unlearned bench (unlearned in the pathology of insanity), and an ignorant and prejudiced jury, is a hopeless undertaking. It is obvious, then, that unless more confidence be placed in medical skill and integrity, or the public mind be so much more enlightened that medical testimony and opinion be more willingly received, such maniacs cannot be deprived of their liberty, or their friends or legal guardians made responsible for their acts. We are of opinion, that the prejudices expressed by the public press, and a portion at least of the bench, against the reception of *professional* dicta as to what is or what is not insanity, will effectually bar any efficient change in the law. They will also prevent that wholesome action of the professional mind upon public opinion, which ultimately leads the legislature into a right course of action; for it is not likely that the profession will subject themselves to vilification and abuse, for a matter which concerns *them* less than any other person. These prejudices the Commissioners will do well to combat; and it is because they have (as we think) sought a meretricious popularity by pandering to them, that we have noticed the matter of the penitential advertisement and one or two minor points. The condition of the insane has been a prominent point of consideration by various members of the profession for many years; and the improvements in their treatment and in the general management of asylums and hospitals for the insane, dates from a period long anterior to the official existence of "the Commissioners in Lunacy." Those gentlemen will, therefore, do well in co-operating with the previous labourers in this field, rather than in entering upon their labours and virtually ignoring their existence.

After a careful consideration of this subject, we are satisfied that the whole subject of the management and treatment of the insane is but imperfectly developed; and if there be any significance in the laboured report before us, the Commissioners in Lunacy are of the same opinion. We are equally convinced that the method of government by irresponsible Commissioners, (or *quasi* irresponsible,) will produce evils of no trifling magnitude applied to the treatment of the insane. The crafty and unprincipled have already discovered that, like other men, the Commissioners have their foibles and follies, and that a system which relies mainly upon the espionage of elderly gentlemen (some not very acute of vision) need have no terrors for them. All the recent exposures of mal-treatment and mismanagement in asylums, have been almost accidental; certainly not to be attributed to the rigors of official inspection and supervision. As we have already shown, a rigorous or even full inspection or supervision is not possible. With one day in the week devoted to board meetings, and one day to Sabbath rest, there remain 260 days for personally examining at the rate of 30,000 patients, and inspecting and examining at the rate of nearly 600 licensed houses, hospitals, &c. When we remember that some of these are of great magnitude (for there are private asylums with 400 to 500 patients), that they are scattered over the country, and all require *two* Commissioners for an official visit,—when, in addition, it is borne in mind that *two* of the six Commissioners have a great part of their time occupied as a "private committee," is it possible to conceive that there is that due inspection and supervision which the public believes to be carried out? Yet the Commissioners breathe not a hint to the Lord Chancellor of their imperfect labours in this respect,—the great and principal business of their official existence,—nor give the slightest intimation that abuses have existed in asylums, which they had failed to discover.

It is obvious that much after all must be left to the integrity and educational and moral fitness of the proprietors and superintendents of asylums themselves; that without these qualities in *them*, no system of inspection can protect the lunatic. What guarantee, then, is there that the proprietors or superintendents shall be fit and competent to perform their responsible and important duties? None whatever. Any illiterate man may become such a proprietor; the particulars to be specified on applying for a license refer to the *house* (according to the Act), and not to the individual; consequently we find, that when the license is withdrawn from an *individual*, the Commissioners have extended the disqualification to the *house;* of course inflicting a pecuniary loss on mortgagees, and other innocent parties. What method should be taken to ensure the fitting qualities in the superintendents of asylums, is not easily stated; we may, however, refer to some of these. In the first place, no *inexperienced* person should have a license granted; secondly, no one not educated at least in the physiology and pathology of the brain and nervous system. As such an education necessarily implies a *medical* education, it follows that one of the qualifications of superintendents should be a diploma or license to practise medicine.

s regards *hospitals* for the insane, this is provided for by the 43d section of the Act, which enacts, "that every such hospital shall have a physician, surgeon, or apothecary, resident therein, as the *superintendent* and medical attendant thereof." We cordially agree with the Commissioners in their opinion as to the importance of every resident medical officer having "paramount influence" vested in him; but we can see no reason why this doctrine should not be extended to licensed houses, and the Act be amended in this respect, taking care that it have no retrospective operation. A provision that no person who had not a *medical* qualification, should be granted a license for the reception of two or more lunatics into his house, would go far to provide an educated class of proprietors and superintendents of private asylums.

It is obvious that as the system of inspection cannot be abandoned, it ought to be extended. To this end the inspectors should be multiplied, and a *weekly* visit instead of a *yearly* or *half-yearly* inspection should be made. Such a duty might be assigned to the officer of health for a district; and a knowledge of domestic hygiene, as well as of insanity, ascertained by examination, should be a necessary qualification for the office. With this multiplication of officials, there should be also a guarantee to provide against arbitrary and unjust conduct on their part. It is one of the most grievous defects of the present system, that the Commissioners may, if they please, inflict serious pecuniary injury and loss upon individuals, without any practical check upon their motives and conduct. It is no libel to say that they are human in their prejudices and their foibles, and *may* therefore exercise their authority rather with reference to their prejudices and foibles, than to the strict requirements of their office. Against such possible conduct there is at present no available remedy; but we hold that there should be.—Lunatics at large, and all criminals pleading insanity in court, might be brought under the notice of inspectors in lunacy, if multiplied in the way we recommended. The certificate of *one* of the body should be a warrant for the immediate committal of a lunatic to safe custody; and the certificate of *two*, a warrant for his detention from week to week. In trials in which the criminals plead lunacy, a paid medical jury, of not less than three practitioners, should be empanneled to hear the evidence for the plea in open court, with an inspector presiding as assessor and reporter of the verdict. In this case, if the plea of insanity were not established, the trial in the ordinary way would then proceed; but if the person were found insane, then the verdict of "not guilty on the ground of insanity," would be recorded by order of the judge.

Such a staff of inspectors might also be made available in Commissions *de lunatico inquirendo*, and in protecting the persons and property of lunatics without immediate friends, or labouring under a temporary aberration of intellect. The centralization system, now in operation with reference to these and other branches of the subject, often renders the law practically inoperative, because so cumbrous, slow, and costly; and lunatics are self-pauperised, or plundered by knaves, without any check whatever. Instances of this kind must frequently come under the notice of medical practitioners, and as the first and best friends of the insane, it is their duty to lose no opportunity of attacking the system which fosters and sanctions such abuses.

With regard to the existing Commissioners we may add, that we believe they *earn* their £1500 a year; and although we are not of opinion that they are, as a body, the best qualified men to be found within the four seas, they are diligent, painstaking, and earnestly desirous to do their duty. These are no small merits, which we willingly accord to them; and if in other respects we have been less lenient critics than the Lord Chancellor, they must please to remember that criticism is our vocation as well as theirs, and is an important duty. If there were no press, "Commissioners" of all kinds would run riot, armed as they are with powers virtually irresponsible, indefinite, and despotic. As guardians of the public interests they are useful, but without the press, *Quis custodiet ipsos custodes?*

ART. IV.

1. *Observations on certain of the Diseases of Young Children.* By CHARLES D. MEIGS, M.D., Professor of Midwifery and the Diseases of Women and Children in the Jefferson Medical College at Philadelphia, Vice-President of the College of Physicians of Philadelphia, &c. &c.—*Philadelphia,* 1850. 8vo, pp. 211.
2. *On the Management of Infancy; with Remarks on the Influence of Diet and Regimen, &c.* By CHARLES HOGG, M.R.C.S. L.A.C., &c.—*London,* 1849. 8vo, pp. 132.
3. *Hydrocephalus Reconsidered, and its Relations to Inflammation and Irritation of the Brain defined; with Cases from Hospital and Private Practice, in Exemplification of its Pathology, Prevention, and Successful Treatment.* By THOMAS WEEDEN COOKE, M.R.C.S. Eng., &c., Medical Officer for the Diseases of Children at the Royal Free Hospital, &c. &c.—*London,* 1850. Small 8vo, pp. 112.

IT was established by the able and respected editor of one of our predecessors, (*vide* Brit. and For. Med. Review, Vol. XXIV, p. 586,) that an important canon of the laws regulating the critical *surveillance* of his Review should be "that of regarding works under review as literary as well as technical productions, and thus holding the authors responsible for the form as well as the matter of their productions—for the language as well as the science." This proper and necessary canon we have retained, and feel it incumbent on us once more to subject the Vice President of the College of Physicians of Philadelphia to the bar of its judgment.

Our readers must distinguish between the author of the work now under notice, and the writer of the 'Practical Treatise on the Diseases of Children,' reviewed in a former number (VI); our present Dr. Meigs being the writer on 'Obstetrics' (*vide* Nos. VII, VIII), and father of the author of the 'Practical Treatise.' In respect to the origin of the 'Observations,' Dr. Meigs informs us, that the Course of Lectures at Jefferson College having commenced in the Session 1849-50, earlier than usual, and—

"As a considerable number of students had already assembled, I engaged to address to them several preliminary lectures on the subject of children's diseases. The following pages contain the substance of what I then said to the class who honoured me with their attendance.

"To the medical public I beg to say, that these observations on certain of the disorders of young children, make no pretension as a systematic work. Indeed I have not indulged any intention to make a systematic work on the subject, seeing that the place is already occupied with numerous valuable books, presenting a complete body of doctrines on children's diseases." (Preface.)

Dr. Meigs divides his work into twelve chapters, bearing the following titles:—*Diagnosis, Caput Succedaneum, Inflamed Eyes, Coryza, Bowel Complaints, Jaundice, Dress, Cyanosis Neonati, Respiratory Disorders, Hooping-Cough, Laryngismus, Scarlatina.* These titles, however, are by no means always indicative of the actual subjects of the chapters, for under *Diagnosis* we find introduced the diseases of intra-uterine life, the question of "non-viability," atelectasic accidents in the birth, apoplexy, syncope, ectopy, danger in pelvic presentations, and many other subjects beside. In Chapter III "sore mouth" is discussed; and under "Cyanosis" and "Laryngismus" we have an exposition of the transcendental physiology of Oken and Flourens. In defence of the discursiveness of the chapter on Diagnosis, Dr. Meigs thus expresses himself:

"We are speaking of the diseases of children, and we may ask the question,—Who are children? All children; children in the womb are children; and their mothers speak of them as if they knew them to be children:—'the *child* moves'—'the *child* is restless'—'the *child* is asleep'—'there must be *two*'—'I do believe I shall have *twins*.' Now in using these words she admits that the unborn of the womb are children, and, indeed, they are often sick children; they are children wounded; they are children dying, and needing the aid of a physician, and depending on his skill and judgment for their rescue. They require his care; and it is his duty to be the conservator of their health, conducting them through the narrow portals of existence, and placing them securely upon the great stage of the world." (p. 23.)

On the vague and unscientific character of the titles of some of the chapters, we need not dwell. If there is one who will have his own way, though this way be a bad one, it is certainly Dr. Meigs. On a former occasion, (Vol. III, p. 139,) we deemed it necessary to pass our animadversions upon him for the same faults as we find predominating in the work now before us; and parting as we did from him "with a high opinion of his talents and originality," we expressed a hope that if we met him again as an author, we "should have to notice his solid substantial merits only, without such an obtruding meretricious dress" as he had chosen for the 'Series of Letters' then under notice. We are sorry to say that our remonstrances have not been very successful. It may be that the colouring of the present work is not quite so garish as that of the other, nor the "picture painting" so frequent; but, as it seems to us, only so because the subjects upon which our author now treats will not so readily admit of such an exaggeration of those errors of which we had before to complain.

Dr. Meigs still "knocks at the door of the functions, and they admit him to see for himself;" knows "the voice of the ear-ache," and "the cry of a suffering intestine;" has "supervised the birth of a child," and is acquainted with "what the writers of the renaissance would probably denominate a dry and a dust blood;" speaks of "the digestion of the gastro-morphous coagulum of milk" of "maladive" and "patible conditions," of a "disc" that "was exscinded," of "the iron by hydrogen;" and affirms that "one may look through the eyes down into the soul, into the most intimate life-cell, and read its expression there." Under *Cyanosis neonatorum,* the question of free-will is touched upon, and Saint Paul quoted, and Saint Matthew appealed to upon the propriety of taking oaths. Passages like the following, are also now and then met with:

"I have seen many other children born, who, in stature and in osseous proportion, generally had been well-developed, but who came greatly atrophied into the world, evidently in consequence of inadequate magnitude of its placental mass." (p. 31.)

"Asphyxia of the capillaries of the skin or of the extremities, is not inconsistent with life. But asphyxia of the encephalic capillaries, when carried to a certain extent, is mortal. Mortal asphyxia is always so, because the capillaries of the brain are the seats of the malady." (p. 96.)

But any slight obscurity that our readers may perceive in such statements as these, is faint in comparison with the darkness of much of the physiology discussed in Dr. Meigs's pages. Of this branch of medical science our author seems particularly fond, and enters at large and *con amore* upon some of the deeper doctrines of Oken and Flourens. Now, without for a moment canvassing the vantage ground upon which these eminent physiologists stand, or seeming to be careless of the respect due even to the more cloudy of their physiologic enunciations, we may yet express our opinion that such details as Dr. Meigs has ventured upon, are entirely out of place in a work upon the diseases of children; and that the members of the class of obstetrics at Jefferson College might have had their time better occupied, than in listening to what follows:

"In contemplating such a being, in whatever grade of the zoological series it may be stationed, we are compelled to admit that of its parts, some are of more and others of less importance. It has parts that might truly be called noble, and others that are common or vile. Whether it be an annelide or insect, a radiate, vertebrate, reptile, fish, bird, or mammal, the *Ens*, the living creature, the *Verb*—that which can do, be, or suffer, of it, is composed of the nervous mass of the creature which is noble, and all the rest is vile, common, and of less account." (p. 102.)

"To look upon the figures at page 4 of Milne Edwards's volume on the Invertebrata, wherein he has represented the nervous system of an earwig, a grasshopper, &c., one sees the real abstract animal." . . . . . "The nervous mass,—the creature, —the Ens, is left entire—naked—alone, in an abstract state." (p. 102.)

"It is for the conservation of this nervous Ens—this nervous mass, as Oken denominates it—that its servants and ministers, the anatomical organs and histological tissues, are added to it as its endowments and properties. IT is the seat and source of their vitality. They are regulated and maintained in a co-ordinated life by ITS biotic force." (p. 103.)

Dr. Meigs then asks, "but what is this nervous mass?" And after a consultation with Oken and M. Cerise, thus continues:

"Nothing lives save in the presence of oxygen. It is even true that the spiritual soul being present, all life is a result of a process of oxygenation. Hydrogen, azote, chlorine, nor carbonic acid, cannot evolve nor sustain life. Oxygen is the vitalising, not the vital principle. It is the cosmic agent for producing vitality out of nervous mass." (p. 105.)

"I do not regard the nervous mass as the soul, neither the force developed in it by the reagent oxygen, as the soul." (p. 170.)

Dr. Meigs confesses that he does not know what the soul's essence is (p. 170); but of this at least he is satisfied, that both consciousness and free will "are qualities or faculties of the soul exercised through the nervous mass under the force of the great cosmic reagent, oxygen." (p. 107.)

"The heart is not an asymetrical, but it is a symetrical organ; it has a zygo-zoar nature. In health the two symetrical halves of it are innervated in the same time, and with equal force or intensity. But the synergy and the synchronousness may become asynergy and asynchronism under circumstances of disease or irritation, or faulty crasis or constitution, either of the organ itself, or of the nervous mass, or of the blood." (p. 108.)

One of Dr. Meigs's patients "probably flooded her medulla oblongata with carboniferous blood, and ceased to breathe, in consequence of the annihilation of biotic force evolved from the medulla." (p. 117.) Whilst, in attacks of puerperal eclampsia, "an impetuous sanguine circulation gives rise to unmeasured, I had almost said explosive, evolutions of biotic force." (p. 118.)

Under "Laryngismus," Dr. Meigs returns to his physiology; and in full energy, confessing that—

"Without feeling in general bound to adopt whatever explanations or rationales of life may be presented to me by men we call authorities, I freely admit that there is in me a tendency to surrender my judgment to the dicta of Lorenz Oken, notwithstanding the salutary declaration of St. Augustin, '*Quod scimus debemus rationi; quod credimus auctoritati;*' expressions that ought to serve as a motto and general declaration of independence for all persons devoted to scientific pursuits. But of such authorities as Oken, it might almost be correct to say, in believing after him, '*Quod credimus scimus.*'" (p. 172.)

In thus surrendering the reins of his judgment to the guiding hands of Oken, Dr. Meigs appears to be led to form a very poor opinion of the modern school of humoral pathology; and no greater contrast of opposite doctrines can be found, than in comparing those inculcated by our author, and such as are taught, *e.g.*, by Dr. Todd, and by ourselves leaned to, in an article upon the 'Physiology and Diseases of the Nervous System,' in our Fifth Volume, and in one upon 'Poisons,' in our Second:

"The nervous mass makes the animal; of course the nervous mass makes the organs of the animal......In this sense, the optic nerve makes the eye......another nerve makes the lungs......another nerve develops the liver, and it is the bile nerve ..... and so forth, to the entire edification and composition of the living sentient animal." (p. 174.)

"Is such an animal sick? is any one of its organs sick? The nerve that composed and that dominates that organ is sick. It is impossible to suppose that the organ can be sick through any other way; for the life of the organ is in the nervous mass of it, and it cannot vary but with the varying crasis or forces of the same nervous mass." (p. 175.)

"If I am invited to state what are the modifications of nervous mass that cause the morbid manifestations of force observed in disease, I must reply that I know them not." (p. 175.)

"Yet, what fine modifications of the nervous system are those, that enable a Grisi, or a Jenny Lind, to hold in rapturous suspense the crowded audiences that breathlessly listen to the tuneful modulations of the voices of those enchantresses!" (p. 177.)

"......It will not suffice to say, as is often said, that scarlatina is a case of poison in the blood—an expression which explains nothing, as, in fact, it means nothing." (p. 194.)

"Is scarlatina a case of poisoned blood? I cannot comprehend how the cause of scarlatina should poison the blood." (p. 200.)

But our readers may tire of this; we will crave their attention but once more, to show how Dr. Meigs's pages pass "from grave to gay, from lively to severe."

"Men say they are sick here, when, in fact, they are sick elsewhere. They know not where they are sick, nor how; it is the physician's affair to find it out." (p. 19.)

"'How do you do, to-day?' said I to a lady.

"'I am very sick, indeed, doctor.'

"'How are you sick—where are you sick?'

"'I have had a terrible chill, which made my teeth chatter together, followed by fever, violent headache, pains in the back and limbs, and unappeasable thirst. I am dreadfully ill.'

"'Have you any pain in the abdomen?'

"'No.'

"'Have you any pain in the thorax?'

"'No.'

"'Pain in the great joints?'

"'No.'

"'Have you not a lump in your breast?'

"'No, I have not.'

"'Yes, you have.'

"'Indeed, I have not.'

"'No,' says a witness, 'I have examined it with the greatest care.'

"'When was it examined?'

"'Just now.'

"'Will you let me examine it?'

"'Yes.'

"'Well, then, does not that hurt you?' I touched the breast. The answer was an outcry; she had a lump in her breast; she had a *weed* in her breast, and did not know it." (p. 19.)

Comment on the above is useless. Dr. Meigs states, that it is considered, that "often the doctor is a *pis aller*, when a child is sick." (p. 18.) If the doctor was always as sharp and as sure as *he* seems to be, a son of Galen would not be so bad "a last shift." We will now leave what we presume Dr. Meigs must regard as the *ornamental*, and refer to what we may consider as the *useful* in his pages.

In his chapter on Diagnosis, we are glad to find that so much stress is placed on the value of what we before (Vol. III, p. 408,) denominated a physiognomic semeiology in helping our diagnosis of the diseases of children. But we must confess that our author has arrived at a point more advanced than we have, in making the *ear* subservient to his aid; if the following—which occupies a place between two funny stories—is not too highly coloured. We admit, and daily act upon the truth of some of the statements; but that of others we have not yet appreciated.

"If a child have a disease of its lungs which makes it cry, it is probable that the cry will be modified by the modification of the respiratory movement involved in the pulmonary disease; and, therefore, the pulmonary cry will be a peculiar cry. Such a cry will be very different from that occasioned by a flatulent colic, or by a griping of the bowels; because the cry of distress from a troubled intestine does not necessarily involve any modification of the respiratory act; therefore, the cry will be different. The same is true with regard to the phonic expression of cephalalgia or headache. The child with earache may cry for hours; one with pulmonary pain will scarcely utter sharp and protracted cries of distress. With a pain in the head there will be an occasional scream, short, sharp, and quick. The cry of a suffering intestine is loud and prolonged, with frequent intervals of rest; the mouth and throat are opened wide, so as to utter the loudest sounds. A child with a pain in a great articulation will utter a cry occasionally only, which will coincide with some spontaneous or forced motion of the joint.

"A young child suffering with strong pain in the urinary bladder, or the lower part of the rectum, will express the distress with a peculiar cry; in like manner it will tell if the pain is in its lung." (p. 20.)

Every organ, according to Dr. Meigs, has a language of its own; "so that the body may be compared to a great polyglott, since so many organs as it hath, so many vernaculars hath it." (p. 22.)

Alluding to the expulsion of the meconium, the author remarks:

"Where a child passes a whole day without having its first evacuation, it becomes the duty of the physician to inquire into the state of the rectum, in order to know whether it be subject to some imperforation or congenital narrowness of the bowel, or not. In the case of imperforation, if the absence of the bowel is not too complete, and if the upper segment of the intestine be found a short distance from the anus, it is possible, and it has sometimes been practised, to make a communication with the interior of the gut by a bistoury or trocar; but these operations mostly fail in the long run; yet some have proved successful." (p. 48.)

In allusion to a case—

"It struck me, while attending this patient, that some of the cases of supposed imperforation are only cases of congenital narrowness; and that a more careful examination might reveal to the surgeon the existence of a canal, which, without great care, might escape his observation; and I would recommend that, in all cases of suspected imperfection, this point should be settled before making any incision or perforation with the trocar. In any such cases, it would be much preferable that the child should be cured by the dilatation of a natural passage, which, after the cure, leaves a natural surface, rather than it should be healed by incision with a bistoury or trocar, which could leave nothing but a cicatrized surface, and which, being unnatural, is always liable to disease." (p. 48.)

The caution thus urged by our author we believe to be by no means unnecessary; for, like him, we are of opinion that some cases of so-called imperforate rectum have been nothing more than exceeding narrowness and retraction of its orifice and upper portion of the bowel. We have seen three cases of "no passage," which were congenital strictures of the rectum; two of which we dilated by a small glass chemical rod, the other by a flexible bougie. The stricture in the former two was quite at the extremity of the bowel; in the latter it extended rather high up.

In the work of Alois Bednar, noticed by us in our Thirteenth Number, the portion below the constricted and narrowed part of the intestine, which has not performed its function since birth, is described as containing a tenacious mucus, which can generally be softened and dislodged by enemata of tepid water; the length of the dislodged cylinder indicating, to a certain extent, the distance of the stricture from the anal aperture.

Under the vague title of "Caput Succedaneum," Dr. Meigs says but little, and adds nothing to our knowledge, concerning cephalæmatoma.

"I have never seen a child born with this fluctuating tumour, though I have met with a great number of them in which the fluctuating swelling has been discovered by the end of the first day." (p. 49.)

"Among the numerous examples that have fallen under my notice in thirty years, two only have ended in suppuration.

"I believe that a caput succedaneum, not excessively large, will generally disappear in the third week." (p. 50.)

Dr. Meigs advises that the tumour be not punctured and evacuated, as it exposes the scalp to the "risk of erysipelatous or suppurative inflammation." A poultice of bread and milk, afterwards thickened with the petals of chamomile, and "trusting the cure to the absorbents," are deemed all that is necessary.

Much that our author states regarding "sore mouth," as he terms it, appears to us very confused and unsatisfactory, particularly as regards the pathologic characters of the various forms of stomatitis. Muguet, for instance, is said to be distinguishable from aphthæ "by its conical form!" Dr. Meigs differs from many practical men, as to the more frequent cause of the stomatitis of young children:

"It is common among practitioners to attribute the sore mouth to a faulty condition of the primæ viæ, consisting of an acid or mucous-saburral state of those organs. There is little reason to doubt, that an unhealthy state of the digestive organs may introduce an irritable condition of the mouth and fauces, and thus give rise to a greater disposition in the parts to inflame under the cupping action of the mouth in sucking; but very certainly we meet with numerous examples of aphthous children, in whom the health of the stomach and bowels continues to be perfectly good, and who therefore do not require any doses of physic." (p. 55.)

"Nor is there any great reason to be surprised, that parts so tender and delicate in their structure should become inflamed very soon after the first application of them to the purpose of sucking. Indeed, the suction power of a young child is so strong, as most frequently to produce some degree of inflammation in the mother's nipple, and in many instances to inflame it so severely, as to cause painful ulceration. The same force that is employed to blister, to inflame, or ulcerate the nipple is equal to the development of a certain degree of inflammation of the mouth of the child; for, in employing the force, it is decomposed, one half of it being exerted on the nipple, and the other half on the mouth of the child. Hence, when we hear of a child's sore mouth, we are almost sure to hear of a mother's sore nipple, and *vice versá*, for they mutually hurt each other; hence, also, the general opinion, that the child's sore mouth is contagious, whereas it is merely mutual, as betwixt the nipple and the mouth." (p. 54.)

We entirely differ from the advice given, that the slighter cases of "aphthæ" are to be treated "by washing the mouth carefully with a linen rag dipped in cold water." (p. 55.) Such "washing" becomes, in the hands of a nurse, rubbing and scrubbing, which in our opinion only further tend to increase the already existing inflammation of the buccal mucous membrane.

In his remarks upon *coryza*, our author is both original and practical; and though to some this affection may appear too simple or trifling to occupy much thought, to those having much to do with children, the following observations are well worthy of attention:

"When one of the nostrils of a neonatus becomes stopped by dried phlegm, by crusts and scabs on the orifice, or by any foreign body detained within it, a certain degree of respiratory distress is the consequence, because the instinct of the child leads it to respire only by the nares, and not by the mouth. Such respiratory distress is caused partly by the lessened aeration of the blood, and partly by the fatigue or exhaustion consequent upon extraordinary exertion of the respiratory muscles." (p. 57.)

"Under these circumstances, should the other nostril become obstructed or wholly occluded, it will happen in many young children that they shall persist in their efforts to respire only by the obstructed nasal passages. In such a case, after making two, three, or four attempts to respire in vain, the infant starts forward, throws its hands wildly abroad, and, opening its mouth and throat, admits the air in a large stream into its lungs, and then immediately resumes its efforts to breathe through the obstructed nostrils again."

"One full aspiration by the mouth relieving its present and most pressing want, leaves it again to the dominion of its instinct, which is to respire, as before, through the occluded nostrils only. But it is manifest, if this representation be correct, that such imperfect and long-intermitted respiration will not suffice to aerate the blood, and that an impure current, partly oxygenated, partly carboniferous, in the brain, will at length lay the foundation of irregular innervations, putting the child's life into danger, and which, if continued to a certain extent, must bring it to its close." (p. 58.)

Dr. Meigs proceeds to illustrate these views by several very apposite and instructive cases. With regard to the treatment to be adopted, we may sum up our author's remarks, and state that the removal of as much as possible of the nasal plug, the after-application internally of the "ointment of cucumbers," and especially the wearing of a flannel cap, are its main constituents.

Concerning *jaundice*, although some of its relationships are left unnoticed, much that is said of it is practical and good. Two chief varieties are referred to; one in which, from the colour of the urine and the evacuations, it is evident that the common duct is pervious: "Shedding the bile into the duodenum as in health, and leaving no doubt on the mind that the constant flow of bile into the duodenum must at length suffice to relieve the liver of the condition which had led to the regurgitation into the blood," (p. 80;) the other, in which there exists evidence for believing that the flow of bile into the duodenum is obstructed. In the greater number of cases of the former description, non-interference is advised by the author, who considers that mercurials are more likely further to disturb the digestive organs, than to relieve them of a trouble which promises to be transitory. But in the other form, the aid of the therapeutist is clearly necessary. As a diaphoretic for the young child, the wine or syrup of ipecacuanha, combined with a very small quantity of nitrous æther, is most to be recommended.

The consideration "of the child's dress" is next entered upon, displaying the strange allocation of subjects adopted by the author. The climate of Pennsylvania appears to be marked by great and sudden variations of temperature, as changes of thirty or forty degrees within a period of twenty-four hours are stated to be not rare; yet, according to Dr. Meigs, "The potent spell of custom and fashion induces a majority of our women to leave exposed nearly a moiety of the thorax, and almost the whole of the arms of the young child, at all seasons." (p. 89.) Not any difference is made by many mothers, in preparing the clothing of children to be born in February or in August; the result of which is, that "great numbers of little children have had occasion to be put under medical treatment in my practice, solely because of the improper, imperfect, and disproportioned dresses in which they were clothed." (p. 89.)

Medical men in Britain will well appreciate the spirit of the above observations from their experience at home.

Dr. Meigs now discusses "Cyanosis Neonatorum," a subject he largely dilates upon, it being apparently a favorite with him. With much that is ingenious and practical, there is mixed up such an amount of, to us, mystical physiology, that it is not always easy to arrive at his meaning. However, we will try, by dint of some extracts transposed from their sequence in our author's pages, and a few remarks of our own, to give the reader an insight into the leading points of the argument.

Dr. Meigs commences by enforcing upon our attention, that he has no design to treat of all the affections that may in anywise serve to contravene the aeration of the blood—for all such diseases are causes of cyanosis; his intention being "to treat only of those cases that are coincident with permanency after birth of the characteristics of the fœtal heart." (p. 92). In this statement, however, it appears to us, that Dr. Meigs very nearly, if not actually, commits a *petitio principii*, notwithstanding his previous admission of the relation of other morbid conditions to the production of cyanosis; most of the conditions he alludes to at p. 92, being scarcely applicable to the term cyanosis neonatorum, as generally employed. *One* of the main points of his argument appears to us to be the proving that cyanosis neonatorum is always associated with a particular morbid condition; and yet he, at the onset, warns us that he is only going to allude to that form of the affection connected with this condition. It is true that he admits, as we shall presently see, that such condition may now and then be associated with other states having important relations to the disorder in question; but the only observation which has struck us as indicating that cyanosis may be produced, and yet Dr. Meigs's apparently *necessary* condition be wanting, is the following—the italics being our own:

"I have seen children at five, and at five and a half, at six, and at seven months, vainly attempting to carry on respiratory life, and found them all to perish with the signs of cyanosis, whether from too large a foramen ovale, *or from imperfect development of the respiratory machinery of the lungs by atelectasis.*" (p. 97).

We proceed with the exposition of our author's theory:

"The child is born with an open foramen ovale." (p. 95.)

"Numerous explorations of the bodies of neonati have shown, that the fœtal characteristics of the auricular septum are not entirely laid aside until after the third day, and often not until after the tenth and the twentieth day, and that in some persons it remains unclosed until the latest date of advanced age. It is, however, covered by its valve." (p. 107).

"This may show that there is no inevitable inconvenience connected with the persistence of the opening after birth, which is a physiological, not an accidental nor a morbid condition; it is common to all the placental animals, and in all of them continues during a certain portion of their respiratory life." (p. 108.)

"In myriads of children, its openness is attended with no inconvenience, nor would any inconvenience result even in the absence of the valve, provided such patency should not be followed by mixture of the venous and arterial blood, which could not happen under a co-ordinated innervation of the symmetrical halves of the heart." (p. 108).

But—

"As in the uterine life aërated blood passes through the foramen, so in the respiratory life carbonated blood, if any, passes through the opening to fill the left auricle. Whenever the left auricle is filled with venous blood, it is injected by the [illegible] ventricle into the brain and whole system."

[illegible] injections produce cyanosis. Cyanosis is a state of non-aëration more or less complete and universal. Cyanosis of the capillary system of the brain is true asphyxia."

"The degree of intensity of the blue colour in cyanosis is not a certain criterion of the effect produced by the malady."

"One individual may tolerate a greater degree of cyanosis than another, with less inconvenience and distress than that other individual." (p. 95).

Hence it will be seen that in cyanosis the foramen of Botal is open, and admixture of the venous and arterial blood ensues. But what is the cause of the blue colour? Dr. Meigs says—*asphyxia*, (pp. 96, 110). What is asphyxia?

"In my opinion, asphyxia essentially considered is black blood in the capillaries of the brain." .... "Cyanosis is the sign of the presence of non-oxygeniferous blood, which is dark or purple or black blood, as Bichat calls it. This purple, or dark hue of cyanosis, is caused by the presence of black blood only in the capillaries. But when this dark hue of the cutaneous capillaries is seen, it is evidence of a similar hue of all the capillary blood, whether in the abdominal, the thoracic, or the cephalic cavities and organs. This purple state of the blood is not fatal except it exist in the brain, whose power it suspends." (p. 110.)

According to Dr. Meigs, therefore, where there is cyanosis of the extremities and surfacial parts, there is "black blood" in the brain; where there is "black blood" in the brain, the functions of the latter are suspended; why, then, does not the child die? Because the "black blood" being—

"Chased out of the brain by oxygeniferous streams of arterial blood, all the organs and tissues that lie under the control and dominion of the nervous system immediately recover their power." (p. 110.)

But whilst Dr. Meigs thus explains the cyanotic colouration, he entirely forgets or shirks the essential point in the question, which is, viz.:—Is the colouration due to an admixture of the venous with the arterial blood, and the circulation of this through the body; or is it due to mal-oxygenation at the lungs, no such admixture alone being sufficient to produce it? This is a litigated question, and Dr. Meigs does not answer it; or if he may be said to do so indirectly, it is by the following assertion only that he does it:

"I can by no means adopt the views as to the essential nature of the malady set forth in Professor Wood's late work on the 'Practice of Physic.' That author, like others, appears to me to have mistaken the symptoms; to wit, the *blue colour* for the disease, which, as I have so often said, is essentially a failure of innervation from absence of oxygen in the brain." (p. 119.)

Not feeling at all sure as to the true meaning of the latter clause of the above sentence, we referred back to our Fourth Volume, in which some views of our author on "Heart-Clot" are quoted, hoping there to find the key to the difficulty in question. It is there stated (p. 294), that Dr. Meigs considers cyanosis to be essentially "not blueness of the surface, but a state of the nervous mass produced by the absence of oxygen in the brain-capillaries." Our comment upon this showed that we were then rather at a loss to understand it; and now, with the advantage of Dr. Meigs's further elucidations of the matter in his present work, we confess to the same dullness of comprehension.

Several of the continental pathologists have paid considerable attention to the subject before us. According to Billard, an elaborate investigator of the matter, whose observations have been in the main confirmed by Berndt of Vienna, the fœtal apertures are not obliterated immediately after birth; and the period at which their obliteration occurs is extremely variable, but from eight to ten days are generally sufficient for the purpose. The umbilical arteries are first closed, next the umbilical veins, then the arterial duct, and finally Botal's opening. Patency of the latter, or the passage of the venous blood into the arterial system, does not necessarily involve the production of the cyanotic colour; since we have on record numerous examples of "vices of conformation" of the circulatory apparatus, which did not give rise to it, though we have every reason for believing that admixture did ensue. This is Billard's opinion; and he is in agreement with many eminent pathologists. Bouillaud says: "If, notwithstanding the communication of the two auricles, cyanosis does not ensue, it is probably because the amount of blood which traverses the lungs is of such quantity, and sufficiently oygenised, as to impart an oxygenating influence to the venous blood with which it becomes mixed." (Maladies du Cœur, vol. ii, p. 616.) Here we assume that Bouillaud admits the admixture, though its influence becomes afterwards negatived. But such admixture, as a *necessary* result of foraminal patency, has been doubted, as we shall find presently. In other cases where cyanosis was present, Bouillaud at first attributed it always to an admixture of the two bloods, but afterwards admitted that he had laid too much stress upon such fact; and believed with many that maloxygenation at the lungs was not unfrequently its true cause. Corvisart appears to have been much of the same opinion. Louis, partly influenced by some observations of Morgagni, regards a stagnation of the blood in the right side of the heart, and as a consequence in the whole venous system, as the origin of the colouration. We before observed, that some have doubted the fact of patency of Botal's opening necessarily giving rise to the admixture at all, whether there be cyanotic colouration or not. We assume M. Gintrac to be of this opinion, when he says, "*tout communication* between the right and left cavities of the heart is not inevitably followed by the passage of black blood, &c." M. Fabre admits, that the first effect of such communication would appear to be a mixing of the two kinds of blood; but that, on a more careful consideration, it will be found such admixture does not ensue so readily as might be supposed. Afterwards he is led to speak still more decidedly, as he remarks:—it is then extremely probable that the admixture ensues far more rarely than might, at first, be presumed. (Mal. des Enfans, vol. i, p. 380.) M. Louis admits that, in persistency of the foramen, however large it may be, during the *contraction of the auricles*, no admixture can ensue so long as there is an equality of size and action of the parietes of the opposed cavities, and no narrowing of the auriculo-ventricular opening. But inasmuch as the moment of the contraction of the cavities is not the only period at which mixture may ensue, (for it will be produced by the effect of "impulsion" at the instant when the blood enters the cavities which are the seat of perforation, whether there be or be not narrowing of the opening, and before contraction of the cavities ensues,) it must be assumed, that in all cases of communication of the right and left cavities of the heart, admixture to a greater or less extent actually follows. Having so far entered upon this subject, we may as well recall to the recollection of our readers the opinion of M. Ferrus, which rejects the constant *congenital* origin of all cases in which, after death, a patent foramen has been found. For further information upon this point, however, we must refer to the argument as conducted by M. Fabre in his first vol., p. 378 et seq., and revert to Dr. Meigs:

"Having practised midwifery for many years, I had on many occasions witnessed the fatal termination of cyanosis neonatorum both in the premature and the mature child.

"I reflected upon the structure of the fœtal heart, and the route of the fœtal circulation, and I said, if I bring the septum auricularum into a horizontal attitude, will not the blood in the left auricle press the valve of Botal down upon the foramen ovale, and thus save the child by compelling all the blood of the right auricle to pass by the iter ad ventriculum, and so to the lungs to be aerated?" (p. 97.)

"In the case now under consideration, I placed the child, which seemed nearly dead, upon a pillow, on its right side, the head and trunk being inclined upwards about twenty or thirty degrees. Upon placing it down in this manner, it became quiet—began to breathe more naturally; to acquire a better hue of the face, hands, and feet; until, in a very short time, it was quite well again, and did well; having no further returns of the attack of cyanosis neonati.

"I shall not conceal the satisfaction I derived from the successful result of my reflections." (p. 97.)

Dr. Meigs refers to numerous cases so treated in his own practice and in that of others, and states that his "invention has become extensively known, and is, to a reasonable extent, understood and practised in this country." (p. 98.) It is not, however, always successful, as might well be expected, for, "after vainly applying the treatment, I came to the conclusion that other causes, not patency of the foramen ovale, must exist, to contravene the curative tendency of the method." (p. 98.) But—

"There is no other treatment for cyanosis neonati, than that I have suggested; at least there is no other reasonable treatment." (p. 120.)

"There are many of my medical brethren who deny that my explanation of cyanosis neonati is correct, or even philosophical, contending that cyanosis is a status of the lung, or of the vessels of the heart, bringing about a modality of the lung alone; while I aver that the condition of the lung, or of the trunk and members, is nothing in the category, which relates only to the state of the brain." (p. 112.)

"While, therefore, one gentleman sees only in a contracted pulmonary artery, or in a transposition of vessels, a cause of cyanosis, I am not to expect that he will come over to my way of thinking because I think so, even had I the authority and power of the man of Pergamus who ruled us for fifteen hundred years. I am, however, less concerned to witness the acceptation of my rationale, than the adoption of my precept. If they will turn the cyanosed neonatus upon its right side, and shut down the auricular valve, I ought to be satisfied; and, indeed, one distinguished author recommends the practice, while he dispraises the principle upon which it is founded." (p. 113.)

Under "respiratory disorders," our author deals chiefly with the consideration of *croup*. As in our former article on the Diseases of Children, we have scarcely touched upon this subject, and then only in relation to the operation of tracheotomy, (Vol. VI, p. 154,) we shall take advantage of the present opportunity, and make a few additional remarks. Our readers are fully aware of the great confusion which formerly existed in connection with several diseases of the upper air passages in the child, and that under the term croup very different affections were embraced. By degrees pathologists sufficiently distinguished some of these affections from each other; and now all practitioners who observe carefully and write plainly, can both diagnose and describe them with sufficient ease. But notwithstanding what has been effected, it appears to us that all is not yet so clear as it should be, arising partly from a deficient analytic pathology, and partly from an indefinite application of terms. Even in works of high standing, both in general medicine and in children's disorders, we have met with this want. However, no one, be he observer or writer, fails at the present in recognising and drawing the distinctions between cynanche trachealis or laryngea, or true croup, and laryngismus, stridulous or false croup, or spasm of the glottis: the one a severe acute inflammation of the laryngeo-tracheal mucous membrane, accompanied by membranaceous exudation, and of which the prognosis is generally unfavorable; the other a nervous affection specially influencing the glottis, and which, although it must be confessed the prognosis of it is uncertain, yet appears to be fatal in the minority of cases only. Such distinction, indeed, is so universally drawn, that it appears almost trite to refer to it. A more refined diagnosis than this, however, is made, we believe we may say, by the majority of practitioners, if not by all. It is considered that of true croup there are two distinct forms: one characterised by the inflammation and exudation attacking primarily the air-passages below the glottis, which inflammation is generally of a more or less sthenic character—simple croup; another in which the uvula, tonsils, throat, &c., are first affected with inflammation and the morbid deposit, and these, sooner or later, spread into or affect the parts below the glottis, the affection being usually of an asthenic character—diphtheritic croup. The former is generally held to demand, in by far the majority of cases, decisive antiphlogistic measures for its treatment; the latter very frequently to require considerable modifications of such a plan. But assuming the recognition of these affections to be correct, it may be yet asked—if the analysis already made is sufficient to exhaust the ordinary diseases of the air-passages in children? We think not,—we are of opinion that more minute investigation will determine the existence of another not unfrequent malady. Before making any further allusion to this point, however, we shall refer to certain views held by different pathologists respecting the two varieties of true croup just noticed. It will be observed that we employ the phrases cynanche trachealis and cynanche laryngea as the appropriate or ordinarily employed scientific terms for true croup—the simple form of the malady being specially in view. The use of the double cognomen thus arises. There is a party who maintain that it is inflammation, &c., of the mucous membrane of the trachea, and not of the larynx, that essentially constitutes true croup; and that inflammation of the latter is very rare in children, although in some cases the diseased action may extend upwards from the trachea to it, and so give rise to cynanche laryngea. "The essence of the complaint," says Dr. Watson, "is violent inflammation affecting the mucous membrane of that portion of the air-passages which lies between the laryngeal cartilages and the primary bronchi, in one word, of the trachea or windpipe." (Lect. vi, p. 833.)

An opposite party, on the other hand, as confidently maintain, that true croup is primarily inflammation of the larynx, with often more or less secondary tracheitis following; whilst a third pay little regard to the locality, affirming that there is but one true croup, admitting of a division into laryngeal, tracheal, and bronchial varieties of the disorder, and having alone, as its essential character, albuminous, membranaceous, or croupose exudation. (*Vide*, e. g. Kesteven, 'Med. Gaz.,' 1850; Dr. Green, 'Brit. and For. Med.-Ch. Rev.,' vol. IV, p. 167.) Leaving the *locality*, we will refer to the *deposit*. On the one hand, it is asserted that, unless this exists, croup is not present; on the other, "though so generally met with as to have suggested to medical writers the terms angina polyposa, angina membranacea, as appropriate designations, it is neither invariable in its occurrence, nor of an uniform extent in all cases." (West, Lect. xix.) From certain peculiarities of climate, of hygiene, and from epidemic causes, sufficient reasons have been assumed to exist by many continental writers for affirming, that true croup is *always* the diphtheritic form of the malady; one set affirming that the laryngeo-tracheal affection is necessarily preceded by the anginal disorder; another party believing that, although not always preceded by it, yet that, sooner or later, it will be accompanied with it, which equally proves their identity. "The *point de depart*," says Fabre, (Op. cit., p. 9,) "is almost always from an antecedent guttural or pharyngeal diphtheritis." One of the latest writers upon the subject, Emfris, in a recent number of the 'Journal für Kinderkrankheiten,' thus expresses himself:*

"Membranous croup, once established, is, from its very nature and locality, such a dangerous disorder, that the physician is forced to direct his whole attention towards the threatening danger of suffocation. In a general way, it may be said, in respect to this malady, that, on account of this danger to life, more stress must be laid on surgical than on medical therapeia; and when a hitherto healthy child is attacked (no epidemic raging), and when the constitution has afforded no opportunity for a very rapid increase and spread of the diphtheritic exudation, it may be regarded from a purely local point of view. It must be admitted, however, that a diphtheritic diathesis is indicated, and that, consequently, a child, suffering under such a disposition, will sooner or later exhibit diphtheritic exudation on all susceptible surfaces; but, from the first manifestation of this disease, or rather of this morbid product, at the entrance of the air passages, a quickly fatal event must follow and carry off the patient before the diphtheritic diathesis shall have manifested itself further. If a person maintains that he can comprehend and decide upon the diphtheritic diathesis from so fatal a local malady as croup, he is not very unlike one who would describe the family of the Graminaceæ in all its fulness from a stalk of grass broken off by the wind. To fully appreciate this diathesis, it must be remembered, also, that the tendency of diphtheritis is to repeat itself at different parts of the organism, increasing, up to a certain degree, in a ratio with the number of the mucous and cutaneous surfaces already covered with deposit. This can be distinctly observed only in large hospitals, and under the influence of epidemic increase of the malady in populous towns; and it fell first to the lot of Brettoneau, of Tours, under such favorable conditions, to unravel the subject of diphtheritis, and display it from a general point of view." (Op. cit., p. 17.)

Notwithstanding all that has been done upon this subject, then, our readers will perceive that much yet requires settling. Our own opinions, founded upon both observation and reading, are as follows:—First, that —as we see the affections in this climate—there are sufficient reasons for regarding simple croup and diphtheritic croup to be different diseases. 2dly. That the former may consist in inflammation, &c., of the larynx, chiefly or almost alone, or of the trachea, with more or less involvement

* Studien über die Diphtheritis während einer im Necker-Hospitale in Paris im Jahre 1848 stattgehabten Epidemie, Band xv, s. 8.

of the larynx, bronchi, &c. 3dly. That exudation of false membrane may be found existing in the larynx alone, or in the trachea and not in the larynx; and therefore we differ most entirely from the view which maintains that simple croup is always a tracheal and never a laryngeal disorder. Scarcely an hour ago, we examined the air-passages of a child, (male, æt. 1 year 6 months,) who died from a disorder we designated and treated as simple croup; we found that comparatively very slight inflammatory action in the tracheal mucous membrane appeared to have been present, while the course of the larynx was completely blocked up with membranous deposit. With respect to the views held by Dr. Green and his party, we would thus express ourselves:—If the term *croup* is to imply alone a particular morbid anatomical phenomenon, then, of course, without the presence of the deposit, we know not of the existence of croup. But if it is rather to be applied to an aggregation of symptoms, accompanied, in the majority of cases, perhaps, by a membranous deposit, which symptoms must exist ere we can predicate the probable presence, or *in futuro* existence, of this deposit, then we differ from such views;—the more decidedly, because we have ourselves experienced, that a sufficiency and identity of such symptoms may be present, from which the immediate or future existence of the exudation are in most cases rightly predicated; and yet in certain instances, after death, no deposit shall be found. Yet, notwithstanding, in such instances we have felt that, considering the aggregate of symptoms which had existed, we were fully justified in having applied the term croup to the disease, though the absence of the pseudo-membranous exudation was plain. Upon this point, the Lectures of Dr. West may be consulted with advantage.

We before remarked, that besides the severe and distinct forms of simple croup and diphtheritic croup, and of spasm of the glottis, there was another affection of the air-passages—these *janua vitæ*,—not uncommon in children. It is an affection intermediate between simple laryngeal or tracheal croup and laryngismus stridulus, partaking of a certain amount of the inflammatory action of the one and of the nervous symptoms of the other; the individual and relative intensity of its double characters varying very much in different examples. It is the *spasmodic laryngitis* of several writers. In some instances, the spasmodic element is exceedingly slight in intensity, but the inflammatory so severe, as to give rise to apprehensions for the safety of the little patient. But the inflammatory action in the upper portion of the larynx is here far more under the control of remedies than in croup, though in numerous cases it is subdued to a certain extent only, and a chronic form of the disorder is left behind, continuing for months even, and which is finally got rid of after the subjection of the patient to a course of mercury. We speak thus from practical experience; we have treated a child, who had been primarily attacked four months before we saw it. Many of these cases have been, we think, confounded with true croup; whilst others, in which the spasmodic element has been marked and the inflammatory very slight, have been often placed under the category of *spasm of the glottis*. It might be replied, that there is but little use in affirming the existence of another malady which can be referred so easily to one or other already admitted existing affections; and, in fact, such a view is taken by a majority of writers. But although, in what we may call the terminal forms of the malady, it appears to run into true croup and laryngismus stridulus, in the majority of cases it appears to us distinct enough. It stands out by itself as slight (in comparison with croup) inflammation of the upper part of the larynx, liable to become chronic. Upon the inflammatory element we lay most stress; in general we think but little of the spasmodic. We would remark, that several of the continental writers draw the distinctions between pseudo-membranous laryngitis and laryngismus stridulus; whilst Dr. West thinks, there is no advantage "likely to arise from constituting a new species of croup out of a modification in its symptoms produced by the idiosyncrasy of the patient." (Lect. xx.) But we shall now appeal to our author for information, premising that he appears to place more stress on the spasmodic than on the inflammatory element, and that he is rather confused in the use of his synonymes:

"The word croup, in its most general acceptation, is expressive of a supposed spasm of the larynx, wherefore it is called spasmodic laryngitis; and, from the peculiar sound of the voice, stridulous laryngitis, Millar's and Wichman's asthma.

"It is quite true that, in spasmodic croup or spasmodic laryngitis, it often happens that, although the patient may have been very suddenly attacked, and almost as suddenly relieved, he is notwithstanding left, for some time after the relief, affected with signs of an altered condition of the windpipe; that is to say, he will be a little hoarse, and, upon any attempts at rapid or sudden aspiration, he will find that the croup sound is still there, as is the case, also, in the act of coughing; and this though in any ordinary rate of breathing not the least sign of difficulty can be perceived." (p. 126.)

"This seems to me to show, that what is called spasmodic croup is not merely spasm, but that there is a substratum of congestive or inflammatory disorder, which ought not to be lost sight of by the medical attendant. To look upon it as merely spasmodic, and thus wholly contradistinguished from the grave forms called pseudo-membranous laryngitis, is to make a serious mistake, as diminishing the apprehension of danger, which, in fact, is not gone as soon as the spasm of the larynx has been removed by the therapeutic influence of a nauseant, an emetic, or both." (p. 127.)

"I have now enumerated the different forms of laryngeal disease that have formerly been interchangeably called croup, viz. spasmodic laryngitis, laryngismus stridulosus, and pseudo-membranous laryngitis, of which the latter alone is deserving to be called true inflammation of the organ, though, in the first-named case, there is reason to suspect the presence of engorgement at least, and also, perhaps, a slight inflammation of the mucous membrane, as I remarked in a former page." (p. 131.)

With regard to treatment:

"In the spasmodic croup, whose assault and ordinary progress I have already described, I have rarely found it necessary to resort to venesection in the treatment." (p. 131.)

"It will always be for me a grievous reflection, that, in a case of pseudo-membranous croup, the malady will not admit of an appeal to the lancet. . . . . When I perceive the impossibility of a hopeful resort to the lancet, my reliance on the medical art for the cure of the patient is at once struck down." (p. 152.)

"It is a curious circumstance, and one calculated, perhaps, to show how large a part is taken in this malady by the pneumogastric nerves, that emetics often fail to excite vomiting, though administered in large doses. This appears to be generally admitted." (p. 132.)

"I was taught, so far back as 1812, to make use of alum as an emetic for those cases in which the stomach refuses the influence of moderate doses of ipecac., antimonial wine, or tartar emetic; and I have for thirty-six years accustomed myself to rely upon it as the most certain, prompt, and, at the same time, safe emetic for croups and catarrhs." (p. 133.)

In respect to the operation of tracheotomy in true croup, Dr. Meigs remarks, that a recent success in its employment "has been so gratifying to all the persons interested in the case, as to convince me that the profession ought more frequently to adopt it under the circumstances proper for it."

We before (Vol. VI, p. 154) expressed an opinion on this score; and will now only say, that the difficulty of being able to distinguish those cases which are laryngeal and not tracheal, and the necessity felt by most practitioners in this country of putting off the operative procedure until a very late period in the course of the malady, are the chief reasons why it has been so unsuccessful and so little practised amongst ourselves. According to Dr. Meigs, "where all other hope fails," he should consider himself "obliged to state the possible advantages to be derived from opening the trachea." (p. 146.) Our remarks in a former article are very apposite to these of Dr. Meigs.

We shall now, without comment, lay before our readers Dr. Meigs's views regarding laryngismus and hooping-cough:

"In the disorder called Kopp's asthma, or laryngismus stridulosus, there appears to me to be reason for supposing the whole of the phenomena of the first attack to result from a sudden hyperæmia of the respiratory lobe or medulla oblongata." (p. 160.)

"The absence of all signs of laryngismus in many of the attacks has persuaded me that the larynx is not in all the cases interested in the evolution of the phenomena; but the same observations filled me with the conviction, that the diaphragm is *always* most deeply involved in the malady, and that it is indeed the principal organ affected by the nervous principle of the disease." (p. 166.)

"I am persuaded that there is more concern of the diaphragm than of the larynx in those paroxysms of so-called laryngismus. And I beg to call the attention of the practitioner to the phenomena in all cases of pertussis in which we have the most remarkable and stridulous laryngismus coinciding with convulsive actions of the diaphragm and other respiratory muscles, very rarely indeed, yet on some occasions extending to the voluntary muscles also, just as happens in our cases of laryngismus stridulus." (p. 169.)

"What then is whooping cough?" (p. 158.)

"Without pretending to be able to give a definite answer to this question, I may venture to declare my opinion again, that there is not in the case any disease of the lungs, or fauces, or larynx, sufficient to account for the phenomena. I have, therefore, no other means of accounting for the phenomena, than a reference of them to a state of the nervous system—I mean a state of that part of the nervous system that presides over the respiratory office." (p. 158.)

"To refer the cause of pertussis to the medulla oblongata, is equivalent to calling it a nervous disorder, which is just what I wished to do, and in so doing to designate that part of the nervous mass which I suppose to be the seat of the malady." (p. 159.)

"I do not pretend to say that the medulla is inflamed, or indurated, or softened, or anæmical, for I do not know the nature of the lesion with which it is affected." (p. 159.)

In relation to the treatment of laryngismus:

"Having frequently in the spasms of the limbs in cholera, in the spasms of the masseter and temporal muscles, in the locked jaw of hysteria, observed the instant resolution of those spasms or cramps upon the application of cold to the affected muscles, I have pursued the same plan of treatment for obviating the paroxysm of laryngismus, or rather phrenismus."

"As soon as the patient gives evidence of the approach of an attack, I direct a lump of ice, wrapped in a handkerchief or napkin, to be applied to the epigaster and moved along the arch of the hypochondria. It will be almost universally found that the touch of the ice resolves the spasm or cramp of the diaphragm, and that the child begins immediately to cry, and that without laryngismus and without passing into convulsions." (p. 179.)

Beside this means, intended only to resolve the "phrenismus," which affection of the diaphragm "is of the nature of the cataleptic rigidity and fixity of muscles," (p. 178,) the use of Valerian, of a liniment of oil of amber, and of quinine, is recommended; and moreover, as "the nervous disorder is intimately connected with, and dependent indeed upon, a state of the blood and its delimitory membrane, the endangium," Dr. Meigs has been induced, where a tonic treatment was desirable, to employ "the iron by hydrogen." (p. 188.)

The last chapter of the "Observations" is devoted to the subject of scarlatina; upon some points connected with which disorder the author holds as peculiar, and, to us, heterodox opinions, as he does upon some of the topics previously handled. As we have already very fully entered upon the consideration of this malady, (Vol. VII, p. 498,) we shall simply lay before our readers a few extracts indicative of the views of the Vice-President of the College of Physicians of Philadelphia.

"It is very generally allowed by physicians, and by the public, that scarlet-fever is a contagious disorder; and I presume that those writers who take this view of it mean to be understood, that the cause which produces it is a material generated within the body of a patient suffering from it, and from no other source.

"The notion of the contagiousness of scarlatina, then, is one that depends upon faith or argumentation, rather than upon evidence or proof." (p. 192.)

Accordingly, Dr. Meigs assumes he may—

"Not only conclude, with the late Professor Dewees, that the evidence of its contagion is imperfect, but wholly deny that it is so." (p. 192.)

"Many of my friends have said, while we have together attended upon cases, 'the blood is poisoned,' 'the poison is in the blood,' 'the disease is a disease of the blood.' For my own part, I do not admit that there is any poison in the case; for I deny the contagiousness." (p. 200.)

"If we should say, and truly, the blood is poisoned, I see not how scarlet-fever should result from it." (p. 200.)

"The truth is, that the true blood-vessel is that tissue which the blood touches; and that is the one called by the anatomists *membrana vasorum communis*, which Dr. Burdach has denominated (and I humbly after him) the endangium, or inner lining of the vessels." (p. 195.)

"I have now to say, that scarlatina may be considered as an inflammation of the true blood-vessel, or endangium." (p. 197.)

"What is it which gives the scarlet hue?" (p. 198.)

"To me it seems that not the papillæ only are inflamed, but the channels that carry the blood to the whole outer aspect of the organs; and those channels are capillaries whose essential physical solid is endangium, and naught else." (p. 199.)

From the following observations it might be presumed that the author, having arrived at so satisfactory a conclusion as to the nature of scarlatina, had made great progress towards its satisfactory treatment; but, from what is afterwards stated, we must have doubts upon this score.

"In taking the view of scarlet-fever that I have now endeavoured to set forth, I find myself, in practice, freed from the necessity of fatiguing my mind in the vain search after a rationale of the symptoms, and a discovery of the indications of treatment. I have no poison in the blood to eliminate. I do not perceive a state of the liver that requires calomel as an alterative. It is not a pneumonia, nor a gastritis, or duodenitis, or dysentery, or nephritis, that I contend against; but I find in my scarlet-fever cases a vascular disease, an inflammation of the endangium."

Yet, notwithstanding,—

"I am free to confess that the views I entertain on the subject of our disorder, do not liberate me from a sense of doubt, and even of incompetency, when I come to assume the conduct of a case of scarlet-fever." (p. 202.)

We must now bring our notice of Dr. Meigs's "Observations, &c." to a close; a notice more extended than we should have given, were it not for the high position which our author holds in his own country, and for the undoubted proofs that he affords of being capable of original observation, notwithstanding that the details of his remarks are mixed up, in these pages, with a great deal that we can understand, but cannot approve, and with very much that we cannot approve of or understand either.

It would have been to us a source of much pleasure, to have passed from one work with which we have found it necessary to find fault, to another upon which we had nought to pass but commendation. But unfortunately we are deprived of this consolation. We deem it right to bestow upon Mr. Hogg's *brochure*, as he terms it, entire disapproval. For whom has Mr. Hogg written? Not for his professional brethren surely, for they have already perused Adair, Sinclair, Conquest, Combe, Dick, Pereira, Mayo, Glover, and our old friend, Dr. Underwood, and many others, from whose writings Mr. Hogg has so profusely filled his book. The favours of our author must be meant, then, for the public; the more evidently as the cases so *ably* reported show—How "John Howlings" "was placed under Mr. P— of Bethnal Green, who attended him six months," but did him no good; how "he was afterwards under the care of Mr. N—, of Mile End," who wanted to cut his leg off; but then "he consulted us," and "was restored to health within four months." (p. 95.) How Captain Wallace took "the best advice in Glasgow and Edinburgh," without permanent benefit;" but how "we ordered a vapour bath," &c. &c., and "he had no return of the disease." (p. 97.) How S. L— was under the care of Mr. Ferguson at King's College Hospital, and "had been taking medicine, either by the advice of regular physicians or surgeons," without benefit, when "she consulted us," and "for five or six months there was not the slightest indication of any return" of her disease. (p. 99.) How "the medical officers of Guy's and St. Thomas's advised amputation" of Robert Lawson's arm, "but the patient would not submit;" and how "we saw the patient," and by ordering the vapour bath, senega, &c. &c.; "he informed us he had had no return of the disease; respecting which "certainly very great pains were taken, and no expense was spared, with a view to save this poor man's arm." (p. 103.) How C. S— "consulted" Mr. Coulson, "who prescribed steel," with "but little benefit;" and how then, under Mr. Hogg's care, "the child gradually recovered in three months." (p. 105.) How "we attended the case at the request of the Rev. Dr. Fletcher;" how—but no, the further conquests of our Æsculapean hierarch must be learnt from the work itself.

But though Mr. Hogg thus so clearly seeks public approval, he appears to have been sorely puzzled as to the method which he should adopt in kindly putting the public in possession of his prescriptions and formulæ. Sometimes they are written in Latin, sometimes in English, sometimes in both languages, or in half-and-half—as the dissecting-room porter would say. At one time the proportions of the ingredients are placed first, at another they follow; indeed, on this point, Mr. Hogg exhibits the powers of a most discursive genius, as will be seen from what follows:

> "Two to three grains of hydr. c. cretâ;
> Three or four grains of pulv. rhei;
> Three grains of pulv. cinnam. co." (p. 56.)
>
> "Of concentrated decoction of sarsaparilla, one ounce;
> Liq. potassæ, two drachms;"
> &c. &c., in English. (p. 58.)
>
> "Take of—
> "Olive oil or cocoa-nut oil, four ounces;
> White wax, two and half ounces;
> Spermaceti, half an ounce." (p. 60.)
>
> "Emp. sapon., half an ounce;
> Emp. plumbi, half an ounce;
> Extract of belladonna, two drachms." (p. 104.)

It is not unlikely, however, that Mr. Hogg had in view the difficulty he has apparently found; and we have perceived it, in his "system of prescribing," when he penned the following paragraph in his Preface:

> "The author is aware that many inaccuracies will be found in this his first *brochure;* his aim has been more at utility than classical embellishment." (p. vii.)

Nevertheless, this same Preface finishes with a quotation from Pope's 'Essay on Man;' and in the body of the work Cato the Censor, Kant, Celsus (more than once in Latin), are quoted; Hippocrates, Galen, Theodorick, Bishop of Cervia, &c. &c., are referred to; and a flowing account given of the bathing and ablutions of Venus and Ulysses, as condensed by Dr. Currie from Homer;—or, to be precise as Mr. Hogg is, from "Odyssey, lib. viii, pp. 362 to 367." (p. 45.) In the Introduction especially, the author is lavish of signs of ! and ?; and the editorial *us* and *we* are most recklessly appropriated by our Baconian teacher, who, however, we trust, will not number us among those—

> "Men always to be found, who are opposed to the progress of knowledge; but their efforts are now too late; the night of ignorance is well nigh past; and the phantom of prejudice, which has hitherto curtailed the growth of intellect and impeded the circulation of knowledge, flits and dwindles away before the light of science. While philosophy is extending her influence over new regions, and enlightening men who were before rude and barbarous, surely there ought to be no hesitation in stating boldly that which has truth for its basis, and utility for its end." (p. 12.)

The above will illustrate the subdued style; the following, the science and professional experience, of our author:

> "Struma is generally defined to be swellings of the conglobate glands, indolent, suppurating slowly and imperfectly, healing with difficulty, and occurring mostly in young persons having a thick upper lip and a florid complexion." (p. 72.)

"We have been repeatedly told by patients, that while taking this medicine [taraxacum], they could feel 'as if something was searching every corner of the liver.'" (p. 110.)

As in the case of Dr. Meigs, we have had a reason for thus lengthily (considering Mr. Hogg's merits) dwelling upon our author, though the reason is not the same. Mr. Hogg advocates "the advancement of knowledge in a popular form," and is able to "point out the happy results of the publication of several small manuals by two or three physicians of eminence," (p. 12;) which being the case, and it being evident that "the permeability of the skin is a fact of the highest importance, not only in the practice of medicine, but in the investigation of certain questions connected with the welfare and happiness of society" (p. 114.), Mr. Hogg feels that "on reconsidering the matter, we think it more desirable to reserve the subject for a separate pamphlet." (p. 115.) Our motive, then, for having thus detained our readers' attention in respect to Mr. Hogg, has been to place them on their guard as to what they may expect to find in his "pamphlet," should his intention be fulfilled. We trust, for the sake of the profession, that it will not be.

Engaged as we have now been for some time, in paying rather more than ordinary attention to the diseases of children, and during that period having felt an especial attraction towards the study of the morbid conditions of the brain and its membranes, we commenced the perusal of Mr. Cooke's work with a very natural interest, and strong hopes that it might add to the amount of information we already possessed, and assist us in our daily clinical duties. Whether our anticipations have been fulfilled or not, we shall leave our readers to judge from the following extracts, premising that from them they can derive the "key-note" to Mr. Weedon Cooke's labours, which were undertaken for reasons, and based upon opportunities, sufficiently indicated in these passages:

"Early in my professional career, I found that 'water on the brain' had, by prescription, acquired an hereditary right to kill every child it attacked. It was not for lack of treatment; but do what we may, the subject of it would die. And so they did. For were they not leeched? were they not bled? were they not calomelanized? And yet for all that they died; and others were attacked, and they were subjected to the like heroic treatment, unhesitatingly, without discrimination: and they died too.

"This melancholy experience directed my attention, at an early period, to diseases of the brain in children; but a stronger impetus still urged my mind to the consideration of this subject: a near and dear little female relation having been cut off in the most joyous period of her young life, after undergoing the routine treatment thus practised; a system which, I fear, must now be characterised, if the truth dare be spoken, as the routine of slaughter." (p. 5.)

"Looking upon this great loss of life at so early an age from a higher point of view,—regarding the social unhappiness thereby produced,—seeing, as we do daily, the Rachels 'weeping for their children, because they are not,'—how imperative is the necessity for all those whose opportunities," &c. &c. (p. 4.)

"Opportunities for observation have been, alas, too frequent; and in my present appointment at the hospital, indeed, overwhelming." (p. 5.)

"Mainly, is it due to your enlightened conviction, that the immense numbers of children who flock to the Royal Free Hospital, should be under the treatment of an officer especially devoted to their diseases, that I have been enabled, from so large a sphere of observation," &c. &c. (Preface, p. iii.)

"Solely from the study of the great book of Nature, for the reading of which I have enjoyed the most extensive opportunities in the practice of the Royal Free Hospital," &c. (p. 24.)

"Children who are highly fed will get inflammation of the brain upon the occurrence of a common cold, a tumble, or from the irritation of teething; whilst in all it may be induced by the partial or entire suppression of the eruptions of scarlatina and measles; and I have known it occur in the course of an attack of hooping-cough." (p. 33.)

"It has been said by West, and Maunsell and Evanson, and Abercrombie, that infantile cerebritis or meningitis is a rare disease. This statement is due entirely to the jumble of names which, even in these estimable authors' works, is still kept up." (p. 36.)

"The general course of the symptoms, the post-mortem results, and the agents necessary for the treatment of infantile cerebritis, and the acute hydrocephalus of the books, being invariably alike," &c. (p. 36.)

"While this is going on, calomel should be pushed into the system, and continued unsparingly until the symptoms are subdued. Most beautiful is it to witness the results of this vigorous treatment." (p. 37.)

Mr. Cooke holds that the data he has obtained are "not insufficient indications of the truth,"—

"That hydrocephalus, acute, subacute, chronic, and strumous, hydrocephoid [!] disease, &c., are all *verba non res*, which, pointing to the latest results of a perhaps long-existent malady, lead the young practitioner to overlook those early symptoms, by the due treatment of which 'water on the brain,' that terrible slayer of children, and justly alarming bugbear of parents, may and ought to be controlled, and the disposition thereto—notwithstanding the assertion of Dr. West, that this disease is rarely cured, and almost invariably returns—thoroughly eradicated." (p. 63.)

We must conclude, feeling ourselves something like the Student in Faust:—

"*Schüler.*—Kann euch nicht eben ganz verstehen.
Meph.——Das wird nächstens schon besser gehen
Wenn ihr lernt alles reduciren
Und gehörig classificiren.
Schüler.—Mir wird von alle dem so dumm
Als ging' mir ein Mühlrad im Kopf herum."

ART. V.

The Commercial Hand-Book of Chemical Analysis; or Practical Instructions for the Determination of the Intrinsic or Commercial Value of Substances used in Manufactures, in Trade, and in the Arts. **By A. NORMANDY.—*London,* 1850. Fcap. 8vo, pp. 640.**

NEARLY fifty years have elapsed since Accum published a work entitled 'Death in the Pot;'—a production that startled the public, not less by its name than by the terrible truths which it brought to light. Since that time many similar treatises have appeared, some in this country, and others on the continent, informing us more fully on the subject of adulterations. In France, for example, the writings of Garniers and Harels, Bussy and Bourton, Charlards, Dumas, and Guibourt; and in England, those of Mitchell, Ure, M'Culloch, Brande, and Pereira, have done much to expose the nature of the sophistications practised, and to point out the means whereby such frauds might be discovered. It is remarkable, however, that little or no good has resulted from the labours of those gentlemen; and that nothing has yet been done, either by the legislature or by the public, to remedy the evils complained of. The consequence of this is, that almost every article of food, drink, and raiment, is made the subject of gross adulteration. Had Sir John Falstaff lived in our time, he might have said, with greater truth than ever, "There is nothing but roguery to be found in villainous man;" for it is a fact beyond dispute, that the brewer, the publican, the confectioner, the baker, the grocer, the sugar refiner, the sausage maker, the milkman, and even the druggist, can each, in his own particular way, perform his dangerous tricks of sophistication without calling for that active interference which the importance of the matter really deserves. True it is, that the government has, on various occasions, recognised the necessity for parliamentary interference; and that many statutes have been framed, ostensibly for the purpose of putting an end to such dishonest practices; but a slight examination of the question will show, that the laws so made are totally inoperative; for they have been constructed more with the view of enlarging the revenue derived from the excise, and of giving strength to an imperfect fiscal system, than in the hope of benefiting the public, or protecting the health of the community; and, consequently, no man fears to abuse them. In point of fact, it would appear from memoranda recorded from time to time on the Treasury Minutes, and also from statements lately made at public meetings, held in London and elsewhere, on the subject of the proposed duty on chicory, that the spirit of competition had become so strong, as to induce men of high standing in the mercantile world openly to abuse the law, by adopting a systematic plan of falsification and fraud. While such things are permitted, the community can have no protection from the more dishonest practices of less scrupulous traders.

But to go back to the question before us; it might, perhaps, be thought, that with such a list of authorities at our disposal, there was no necessity for the production of another work on the same subject, or even for the weekly reports of an "Analytical Sanitary Commission;" but a brief consideration of the extent to which the art of adulteration is carried, and a knowledge of the fact, that this art is practised, not merely by the small retailer, but likewise by the manufacturer and wholesale dealer, are enough to prove that public attention has not yet been sufficiently awakened to the serious nature of this matter; and we trust that the remarks which we are about to make on the work before us, will not in any way tend to weaken the impression which an acquaintance with the facts therein mentioned must have on the reader's mind; for we perceive that we have a double duty to perform,—a duty which compels us to attach great importance to the subject-matter of Mr. Normandy's book,—and a duty that leads us to criticise, and it may be to find fault with, the way in which that matter is discussed.

A glance at the title of the work under examination, would lead us to believe that the contents were of a practical nature, and were well suited to the wants of both the merchant and the consumer. We regret, however, that the perusal of the book does not tend to confirm this opinion; but rather to show that the author has been occupied in bringing together the labours of other men, without having been successful in giving them a practical form. We should think, in fact, that Mr. Normandy has not had much experience in the subject which he has undertaken to discuss; and that he has not been favoured with an opportunity of testing the value of many of the processes which he describes. To take an instance or two. At pages 66, 98, and 591, we are directed to decolorise beer, brandy, and wine, by means of animal charcoal, and then to test the defecated liquids for sulphate of iron, opium, blue vitriol, and sugar of lead; but the author is evidently ignorant of the fact, that animal charcoal has the power of absorbing, and, in some instances, of decomposing, large quantities of these adulterating agents, and so of removing them from the liquids to be tested.

Again, at page 66, we are told, that oxalate of ammonia, nitrate of baryta, nitrate of silver, and acetate of lead, should each produce only a slight precipitate when poured into ale or beer; and, moreover, that chloride of platinum should render these liquids only slightly turbid; but who, let us ask, has ever met with ale or beer so perfectly pure, that they would not give copious precipitates with all the reagents just named? In fact, it must be evident to every one who knows anything of chemistry, that the salts of lime and soda contained in the very water with which these beverages are made, together with the colouring matter removed from the malt, and the carbonic acid produced by fermentation, must each produce a like set of reactions, which are very likely to deceive the inexperienced operator.

In testing liquids for iron, Mr. Normandy frequently directs the reader to add prussiate of potash to the suspected solution while it is in an acid state; but a very slight acquaintance with the art of analysis will inform us, that such liquids almost invariably produce a blue precipitate with prussiate of potash, though they may not contain a particle of the suspected impurity.

At page 581, the author says, "if lead is present, (in common water,) "sulphuretted hydrogen will produce a black precipitate, or only a brownish tinge, if only a trace be present." Now, although we do not by any means wish to moderate the importance of the fact, that lead is too frequently found as a constituent of ordinary water, and that it has no business to be there, yet we think that we should be doing a great injustice to both the water and the metal, if we were to conclude, with Mr. Normandy, that every discoloration produced by sulphuretted hydrogen was owing to the presence of lead, knowing that iron, copper, tin, and other metals, have the power of producing a very similar reaction.

The reduction process, which is described at page 85 as a means of discovering arsenic in sweetmeats, is another example of a process wholly unsuited to the purpose for which it is intended; for as the arsenical pigment is constantly associated with organic matter, the latter will, by its decomposition in the red hot tube, give off so much empyreumatic vapour, that the false sublimate, thus produced, will either mask the poison which is really there, and so prevent the discovery of the noxious metal, or else it will occasion such a fallacious deposit in the upper part of the tube, as to mislead the operator, making him conclude that it is an arsenical crust.

Lastly, whenever Mr. Normandy undertakes to explain the appearances, which the articles used in the adulteration of foods, &c., present under the microscope, he either misleads the reader altogether, or else he leaves him to find his way by himself. We will give an instance of this: at page 55 he informs us, that the adulteration of arrow-root may be detected by the help of the microscope; and he goes on to say, that, "in effect, the particles of potato-starch are of an irregular form, of various sizes; whilst arrow-root consists of particles of an even size, and of an ovoid form, their surface being smooth and even. With respect to common starch, its presence may be detected, because it consists of particles of a larger size, and of a dull appearance, when examined through the microscope; whilst the particles of arrow-root are bright, pearly, and finer." And then, after describing some uncertain chemical tests for these starches, he proceeds to state, that ground rice can be distinguished only by the microscope; "the particles of rice having a sharp, spicular, horny appearance, which can hardly be mistaken." For our own part, however, we think that with no better aid to their discovery than that furnished by Mr. Normandy, the reader is very likely to be mistaken.

In this way we might proceed, until we had either questioned or disproved almost every statement made on the authority of Mr. Normandy; and we might likewise occupy ourselves in exposing many of the errors which he has committed, in assigning the values of proportional numbers in his accounts of quantitative analyses, (as instances, we refer to pages 51, 65, 43, 148, and 416.) It is very questionable, however, whether the prosecution of such a task would be attended with any advantageous results to our readers. We propose, therefore, to lay aside the duty of the critic, and at once proceed to offer a few illustrations of the frauds practised on some of the commoner articles of general consumption, quoting from Mr. Normandy, whenever his remarks are to the purpose.

Bread.—Few of the inhabitants of this city, or, in fact, of any large town, have ever had the chance of indulging in the luxury of good wholesome bread; for the spirit of competition has urged the baker to the practice of some of the worst kinds of adulteration. Not content, for example, with the employment of potatoes, indian meal, and bad flour, he is led to use alum, chalk, blue vitriol, crushed bones, magnesia, clay, and plaster of Paris, in order to give the bread a white appearance, a spongy structure, and a faculty of holding water. The truth of this may be made evident by a slight reference to the large products obtained from a given weight of raw material: a sack of flour weighs 280 lbs., and, according to a statement made in our good old parliamentary statutes, it ought yield 80 loaves of four pounds each; but it is found in practice that 94 such loaves can be easily obtained from it; and, in many cases, the baker procures as many as 100—that is, he increases the weight of the original flour by about 44 per cent. A great part of this increase is due to the presence of water, which is retained in the bread by such drugs as alum, blue vitriol, &c.

In the 'Encyclopædia Britannica,' it is stated that some workmen are in the habit of using as much alum as common salt in their bread; so that they add about two and a quarter pounds of this astringent compound to every sack of flour. Now, supposing that they make the sack into 100 loaves of four pounds each, then each loaf will contain the enormous proportion of 157 grains of ground alum. It is, perhaps, needless to state, that the daily use of such a quantity of this powerful astringent must be attended with injurious consequences. All bakers may not, perhaps, employ alum to the same extent; but if they use it at all, they must, as it appears from the remarks made by Mr. Normandy, employ it in the proportion of at least one part in 906 of flour, or at the rate of 31 grains to the quartern loaf, in order to produce any appreciable effect.

The use of alum, in this way, is attended with two results: in the first place, the bread is rendered white by it, and, consequently, the baker can make use of an inferior flour. Secondly, the dough is enabled to take in and retain a larger quantity of water, than it would otherwise do. From this we may conclude, that the presence of alum in bread is at all times a sign of sophistication and fraud; and it may be stated in a general way, that there is a direct relation between the quantity of this salt present, and the proportion of bad flour contained in the loaf.

In many instances, the flour is charged with alum before it reaches the baker; this is done in order that he may avoid the penalties which the law might otherwise inflict. As an instance of this, we may mention that, a few months ago, a miller residing near Barslem was fined twenty pounds, for having flour in his possession that was mixed with 10 per cent. of alum. The mixture was no doubt prepared for the use of dishonest bakers; and the infliction of the penalty shows that the law has provided a remedy for the evil, even when it has its origin at the fountain head.

It is said that most of the continental bakers are in the habit of using sulphate of copper, or sulphate of zinc, instead of alum. Fortunately for us, however, this practice is not much resorted to in England; but it appears, from the statements made by MM. Dumas and Kuhlmann, that blue vitriol is almost invariably employed by the bakers of France and Belgium. M. Kohlmann, who was the professor of chemistry at Lisle, informs us that he has been frequently called upon to investigate this matter; and he states that the salt referred to exercises a very powerful influence over the fermentation and rising of dough. He finds that it gives a remarkable sponginess to bread, even when the vitriol is used in the small proportion of one part to 70,000 of dough, that is, to the extent of one grain in 10 lbs. of bread; but the proportion that acts best, is one part in 20,000, or one grain in 3 lbs. of bread. This is the proportion in which the continental bakers usually employ it; but, as M. Kuhlmann very justly observes, there can be no safety whatever to the public when such a practice is permitted, because avarice and ignorance are always apt to increase the quantity of the poisonous compound.

Again, it has been affirmed, that bakers occasionally resort to the use of chalk, carbonate of magnesia, carbonate of soda, carbonate of potash, and carbonate of ammonia, for the purpose of neutralising the free acid which invariably forms in stale and over-fermented bread, and in that which is made from bad flour. "From the experiments of Mr. Edward Davy, it would appear, that the addition of carbonate of magnesia to flour of inferior quality, in the proportion of 25 or 30 grains of carbonate per pound weight of flour, somewhat augments the quantity of bread manufactured with such flour, and it likewise gives the bread a whiter appearance." We have had occasion to examine some of the commoner kinds of biscuits manufactured in the neighbourhood of Wapping, and in two instances we found an abundance of chalk in them. One of the samples which we were requested to analyse, contained as much as 4·5 per cent. of this substance; so that each of the biscuits contained about 85 grains of carbonate of lime, a quantity sufficient to produce, as it really did in this case, very injurious effects on the healthy human body. The circumstances which led to the examination of the biscuits are deserving of record:—A captain of a trading vessel was induced to purchase his stock of bread from a cheap baker in Wapping; fortunately for him he happened to distribute a great part of the stock among his crew some days before they put to sea, and it struck him as being very remarkable, that the men complained of great constipation of the bowels. On making further inquiry, he found that this unpleasant symptom made its appearance directly after the distribution of the new biscuits: this led him to suspend their use, and ultimately to forward them to us for analysis, when the cause of the mischief was readily detected. The employment of plaster of Paris, clay, bone, earth, and the common kinds of fecula, is, according to some authorities, not at all unfrequent with dishonest bakers and millers. The last-named substances can in most cases be discovered by means of the microscope; and the former, by incinerating the bread in a platinum or Cornish crucible, and collecting the ash. Pure dry bread never gives more than two per cent. of a very soluble white ash; of this about half is soluble in water, and the rest in dilute muriatic acid without effervescence.

"The process recommended by Kuhlmann for the detection of alum in bread consists in incinerating about 3000 grains of bread, porphyrising the ashes so obtained, treating them with nitric acid, evaporating the mixture to dryness, and dissolving the residuum in about 300 grains of water, with the help of a gentle heat; without filtering, a solution of caustic potash is then added, the whole is boiled a little, and filtered; the filtrate is tested with a solution of sal-ammoniac, and boiled for a few minutes. If a precipitate is formed, it is alumina, which may be collected on a filter, washed, perfectly dried, carefully ignited in a platinum crucible, and then weighed. 50 grains of alumina represent 474 grains of the crystals of alum." (p. 113.)

To detect copper, the incinerated mass from the 3000 grains of bread is to—

"Be reduced to an impalpable powder in an agate mortar, mixed in a porcelain capsule with about a quarter of an ounce of nitric acid, and heated until all the acid has evaporated, and a clammy mass only is left, which should be digested with about 300 grains of pure distilled water, with the help of heat. The liquor should then be filtered in order to separate the portions which have resisted the action of the acid. A slight excess of ammonia, and a few drops of subcarbonate of ammonia, are then added to the filtrate. The bulky precipitate which this addition may have produced should be separated by filtering. The liquor filtered therefrom should next be boiled for a few minutes, in order to expel the excess of ammonia, and reduce it to about one-fourth of its bulk, after which it should be acidified with a drop of nitric acid, and divided into two portions, to one of which ferro-cyanuret of potassium, and to the other, sulphuretted hydrogen, or hydrosulphuret of ammonia, are to be added. If even so small a quantity as the 1-70,000th of copper is present, the solution tested with ferrocyanide of potassium will assume a pink tinge, and after a few hours a slight crimson precipitate will appear. The other portion, tested with sulphuretted hydrogen, or with hydrosulphuret of ammonia, will, under the same circumstances, turn brownish, and after standing for a few hours, a slight brown or black precipitate will be deposited." (p. 116.)

The presence of an alkaline or earthy carbonate is known by the effervescence of the ash when it is treated with nitric or muriatic acid.

Another method of distinguishing these important adulterations, is to digest about 3000 grains of the bread in two ounces of distilled water for an hour or so, and then to express the liquid by squeezing the pulpy mass in a piece of coarse linen cloth. On allowing the liquid to stand for a short time, all the grosser particles will subside; and if potato-pulp or potato-starch have been used in the preparation of the bread, the fecula of this vegetable may be easily recognised among the particles composing the precipitate. Carefully pour off the supernatant liquor, and evaporate it to about half its bulk; filter it, and divide it into three portions. Test one portion with liquor ammoniæ and sal ammoniac for alumina; a second with prussiate of potash for copper; and the third with nitrate of baryta and nitric acid for a soluble sulphate, as for alum, plaster of Paris, and blue vitriol. If the bread be pure, these reagents will merely occasion a very slight turbidity in the aqueous solution of it; but if impure, the first and last will give a white precipitate, and the second a rose or chocolate tint. The residue of the bread after the action of water ought not to yield more than 1 per cent., or about 30 grains of ash.

Several chemists have lately referred to the fact, that unfermented bread, that is, bread made with carbonate of soda and muriatic acid, often contains traces of arsenic, the poison being derived from the acid made use of. Mr. Davis and Dr. Daubeny have reported a case, in which the employment of this kind of bread gave rise to nausea, severe pain in the stomach and bowels, and other symptoms of malaise, which lasted for a period of three weeks before the cause of the mischief was discovered. It is proper, moreover, to know, that the spirits of salts of commerce often contains as much as the 1-600th of its weight of arsenic, this quantity of the poison having been detected in a sample analysed by Reinsch; and other chemists of authority, as Rees, Gmelin, Wackenroder, and Dupasquier, have likewise discovered this metal in commercial muriatic acid.

Flour is often adulterated with the same substances as bread; and in addition to the mineral impurities just mentioned, it sometimes contains the fecula, or powder of darnel, ergot, and other fungi. Dr. Tait, of Edinburgh, has recorded an instance of six persons having been attacked with symptoms of irritant poisoning, in consequence of their having made use of bad American flour; and there is reason for believing that the diseases known in France, Switzerland, Denmark, and Germany, by the names of *morbus spasmodicus, convulsivus, malignus, epidemicus, cerealis*, &c., which sometimes spread like a frightful epidemic over large tracts of country, are due to the existence of ergot, or spurred corn in the flour. Sigebert states, that as early as the year 1089 a pestilence raged over all the western parts of Lorraine; it killed the people by consuming them with an inward fire; their limbs became rotten and black; and the sufferers either perished miserably, or else by losing their limbs were spared for a more terrible death. It is very probable that these effects were due to ergot; for Bayle says, that the bread eaten at that period was of a violet brown colour.

Good flour is known by its microscopic characters; by the fact that it yields not less than 10 per cent. of gluten, and not more than two per cent. of ash. The water, which has been used in washing it, should not give any precipitate with either acid, nitrate of baryta, prussiate of potash, or ammonia and sal-ammoniac.

Our readers may not perhaps be aware, that Acts of Parliament were passed during the first, second, and third years of the reign of George the Fourth, prohibiting the use of alum, &c., in the flour, meal, or bread, under a heavy penalty. It was decreed, moreover, that loaves made of the flour of any other grain than wheat are to be stamped with a large Roman M, provided they are made without the city or its liberties, and beyond 10 miles of the Royal Exchange; and every person exposing such loaves without such mark, shall forfeit not more than 40*s*. nor less than 10*s*. for every loaf so exposed.

Confectioners and Lozenge Makers are even more reckless in the practice of their craft than millers or bakers; for not only do they adulterate their confectionery with plaster of Paris, chalk, starch, sulphate of baryta, and things of that kind, but they even venture to use some of the most poisonous pigments in the fabrication of their dainty looking sweetmeats. Bronze, copper leaf or Dutch metal, white metal or leaf tin, arsenite of copper, carbonate of copper, verdigris, chromate of lead, orpiment, oxychloride of lead, red lead, and vermilion, are all more or less extensively employed for this purpose. Nearly all the ginger lozenges of English commerce, and every one of the bright yellow comfits exposed for sale in the confectioner's windows, contain a poisonous salt of lead. We have some of the last-named bodies in our possession, that contain as much as 1·5 per cent. of this metal; and we perceive by a letter published about two years since by Professor Louyet of Brussels, who is one of the members of the Central Committee of Public Health of that city, that he has recently received from London a yellow coloured bon-bon, which contains chromate of lead in very large proportion. It consists, he says, of a species of aromatised sugar, coloured yellow throughout its mass: it is flavoured with lemon, and encrusted with a transparent shell of red currant confection. His object in publishing his letter was to call the attention of English authorities to the dangerous character of the substances employed in London in making coloured confectionery; but his purpose failed, as have many others of like import, for the same kind of bon-bon is still to be seen and purchased in all the poorer localities of this city. We believe that confectioners are not generally aware that chromate of lead, or, as they term it, chrome yellow, is of a poisonous nature; but we may inform them that Orfila has succeeded in killing animals with it, and that the introduction of lead, in any form, into the animal body, is commonly attended with dangerous consequences. It must be remembered, moreover, that many persons are accustomed to eat a large quantity of ginger lozenges, during the winter season; that they are, in fact, constantly partaking of them; and though each lozenge may contain but a small portion of lead, yet the continued administration of the metal can hardly fail to produce injurious results.

Bad, however, as the employment of this pigment is, the danger is outdone by the use of a still more violent poison; namely, arsenite of copper or emerald green. We would direct the attention of our readers to the fact, that the splendid ornaments which decorate the tops of twelfth-cakes, and the fanciful devices in sweetmeats, which are now so commonly sold at all the confectioners' shops in poor neighbourhoods, are frequently coloured with this mineral. In the month of June, 1850, Dr. Letheby reported three cases of poisoning by Scheele's green, which came under his notice; and he stated that between 30 and 40 children were poisoned at the same time by sweetmeats sold to them by a Jew in Petticoat Lane. He stated, moreover, that as many as seventy cases of poisoning have been traced to a similar source within the last three years.

Surely, it is high time that some measures should be taken for imposing an efficient check on the dangerous and indiscriminate use of mineral pigments. That this might be effected without interfering with the activity of the confectioner's trade, or even with the fancies of those who are the great consumers of sweetmeats, is evident from the fact, that the beautiful and much esteemed bon-bons of France and Belgium are, in most cases, quite harmless; and the reason of it is this: in 1830, an order was made by the Prefect of Paris, to the effect that no mineral substance should be used for colouring liqueurs, bon-bons, sugar-plums, lozenges, or any other kind of sweetmeat or pastry. It was also forbidden to wrap sweetmeats in paper glazed or coloured with mineral substances; and it was ordered that every confectioner, grocer, and dealer in liqueurs, bon-bons, sweetmeats, &c., shall have his name, address, and trade, printed on the paper in which the above articles are wrapped; and, lastly, that manufacturers and dealers shall be responsible for the accidents traced to the sweetmeats made by them. More recently, the ordinance of 1841 has prohibited the use of bronze, copper, lead, zinc, and other finely powdered metals, except gold and pure silver. With the penalties of such a law before him, what would a French confectioner say, were he to visit a fair at Greenwich, Bow, Bartholomew, or Stepney, and see the gingerbread profusely ornamented with Dutch metal or copper leaf in lieu of gold, and tin instead of silver?

Again: who, at first, would credit the fact, that a poison six times more deadly than the prussic acid of the London Pharmacopœia, is freely used by every cook and confectioner in this country, as a flavouring ingredient for puddings, cakes, custards, tarts, and cordials? But such is the case; and the flavouring agent which they employ is ratafia. Now, if it is absolutely necessary that the essential oil of bitter almonds should be sold and employed in the indiscriminate way in which it now is, it ought at least to be deprived of the hydrocyanic acid which it contains, before it is sent into commerce. By this means a large amount of the danger attendant on its use would be removed. In proof of this, we may refer to the experiments of Wöhler, Frerichs, and Mitscherlich, which show that when the oil is distilled from slaked lime and sulphate of iron, it is entirely deprived of its deadly poisonous properties, although its flavouring quality is not in any way affected. To subject it, therefore, to such treatment before it is placed in the hands of the public, would be to take the best means of perventing the fatal accidents which so frequently result from the unrestricted sale of it. The daily papers of the last year have furnished us with accounts of ten cases of poisoning by this essence.

Sugar.—We are informed by every authority, from Rowland Hill downwards, that this substance is adulterated with sand, treacle, plaster of Paris, chalk, sawdust, starch, and potato-sugar. In addition to these matters, the analytical sanitary commissioners have mentioned fungi, and a sort of pigmy mite, as being present in the brown sugars of commerce. All these impurities, disgusting as they are, are nevertheless harmless in comparison with another impurity, viz., lead, which has lately been discovered in several samples of white sugar and treacle. Our readers may, perhaps, be aware, that, in the year 1847, a patent was taken out by Dr. Scoffern of London, for the defecation of sugar by means of acetate of lead; the excess of the salt being removed by sulphurous acid, a gas which formed with it an insoluble compound. The economical advantages attendant on the working of this process soon led to its adoption both here and in the colonies; but the government, fearful that it would be attended with danger to the public health, very properly caused an inquiry to be instituted into the real merits of the scheme. Messrs. Graham, Thomson, and Hofmann, were requested to undertake the analysis of the sugar, bastards, and treacle, yielded by the process, when it was worked on a large scale; and samples of the products were obtained from Messrs. Goodhart and Co. of Limehouse, and Messrs. Evans and Co. of Cork; samples of brown sugar were likewise furnished by the Custom-house authorities. The results of the chemical inquiries go to show that minute quantities of lead exist even in the *raw sugar of commerce;* for a quantity of this metal, equal to from 0·39 to 0·19 of a grain of sulphate of lead, was obtained from four pounds of West Indian sugar. The lead was, however, unequally diffused; and it is highly probable that it existed among the insoluble matters, which were very considerable.

White sugar, refined in the ordinary manner, yielded only traces of lead; while that obtained by Dr. Scoffern's patent furnished from 0·0 to 1·05 gr. of sulphate of lead per four pounds weight; the proportions in nine samples being 0·0, 0·08, 0·09, 0·10, 0·10, 0·13, 0·37, 0·66, and 1·05; and the mean 0·286 of a grain.

The bastards from the old refinery gave from 0·09 to 0·25 of a grain of sulphate of lead per four pounds weight, the mean being 0·22: while that from the lead process yielded from 0·45 to 1·83 of a grain; the amounts in three samples being 0·45, 0·73, and 1·83; and the mean 1·003 of a grain.

The treacle produced in the old way furnished from 0·62 to 1·21 gr. of sulphate of lead per four pounds weight; the mean being 0·91 of a grain: while that from the new process gave from 0·87 of this salt to 6·32 grs.; the results of five analyses being 0·87, 2·19, 2·73, 5·16, and 6·32 grs.; and the mean 3·454 grs. of sulphate of lead.

From this it is evident, that however perfect the process may be, either in theory or laboratory practice, yet, in the actual working of it on the large scale, it is far from being satisfactory or complete; for the results show that the products yield in each case a much larger proportion of lead than do those of the old plan.

In concluding their report on this subject, the three Chemists spoke with great diffidence on the probable effects of this excess of lead on the health of those who consumed the products; and, in consequence of this, the government considered it advisable to have the opinions of three physicians and medical-jurists, who were, from their position and practice, conversant with the effects of small doses of lead on the human body. The Chancellor of the Exchequer applied to Dr. Pereira, Dr. Alfred Taylor, and Dr. Carpenter; who, in addition to the report furnished to them by Messrs. Graham, Thomson, and Hofmann, received information from Mr. Phillips, of the Inland Revenue, that the consumption of treacle in manufacturing and agricultural districts varied from about one-third of a pound to one pound per head weekly; at Ravenhead it amounted to about half a pound per individual weekly; at Bury to about one-third of a pound; and at Blakely, Middleton, and Alkrington, to rather better than one pound. Taking the two maxima of these reports as the bases of their calculations, namely, that the treacle contained a portion of metallic lead represented by 6·32 of the sulphate in every four pounds, and that the consumption of this article of food was 1·1 lb. per head weekly, they came to the conclusion that 1·261 grains of lead would be administered to each individual weekly; and that this quantity would be very likely to prove highly injurious to health;—that it might, in fact, in the course of a few months, give rise to alarming symptoms of poisoning. In support of this opinion they referred to the fact, that at Claremont, thirteen persons were poisoned in the course of seven months by making use of water which contained only one grain of lead in the gallon; and it might be further stated, on the authority of Mr. Herapath, that water containing no more than the 1-500,000th part of lead, or, in other words, one grain in the seven gallons, cannot be used as a beverage with impunity; for he mentions a circumstance which came under his own notice, in which the proportion here mentioned produced indigestion, loss of appetite, flesh, and colour.

At the time when these reports were furnished, no experiments had been made on the physiological characters of sulphite of lead,—the salt which is formed during the working of the process, and, consequently, the patentee and his friends were in a condition to oppose the general conclusions arrived at by the medical jurists referred to. Relying on the great insolubility of the salt, Dr. Scoffern even went so far as to declare that it was perfectly innocuous; and he actually offered to submit himself and family to the action of sulphite of lead, for as long a time as an equal number of other persons would ingest chalk. It is fortunate for him, however, that his offer was not accepted; for the experiments of Mr. Greaves, and others, have since shown, that the salt in question possesses all the poisonous properties of a soluble salt of lead. Our own investigations are quite confirmatory of this; and we have noticed that the molecular or cohesive condition of the sulphite may very much affect the rapidity of its action. If the salt is given in a moist state, that is, when it is just precipitated, it acts very quickly; but if it is dried at a temperature of 212°, or exposed for some days to the air, before it is given to the animal, it then loses a great deal of its virulence, and passes through the alimentary canal without producing any immediate effect; though, in the course of time, if it be persevered in, it will occasion the usual symptoms of lead poisoning. This circumstance may, perhaps, serve to explain the discrepancies which exist in the statements made by Dr. Gregory and Mr. Greaves. With regard to the remarks made by some other chemists of authority, it appears that very great reliance has been placed on the great insolubility of this salt in water, and hence an erroneous conclusion has been arrived at concerning its insolubility in the fluids of the animal body. In answer to this it may be said, that the experiments of Messrs. Redwood and Solly have demonstrated, that sulphite of lead is easily decomposed by weak acid liquids, and by solutions of the alkaline chlorides; and, consequently, that it cannot be brought into contact with the secretions from the alimentary canal, without undergoing those changes which are sufficient to render the lead of the salt soluble. In offering these remarks, we beg to say, that we are not anxious to provoke anew the angry controversies which have engaged men in the discussion of this subject; but, on the contrary, we hope that all further evidence, whether on the one side or the other, will be offered with that becoming temper and courtesy which the dignity of science demands.

Many of the grosser impurities contained in sugar may be discovered by putting a given weight of it into a glass of cold water. After standing for a short time, the insoluble matters may be perceived; and on collecting them in a filter, and then transferring them to the field of the microscope, the acari, starch, saw-dust, fungi, and other organic impurities, may be readily detected. The remainder of the insoluble deposit may be ignited in an iron spoon, and tested for chalk, clay, sand, and plaster of Paris.

The presence of grape sugar, glucose, or sugar of starch, may be known by boiling a small quantity of the suspected sugar with twice its weight of liquor potassæ. If the sugar is pure, no discoloration takes place, but if the material contain glucose or grape sugar, it will rapidly assume a brown or even a black appearance. Kuhlmann says, that from 2 to 3 per cent. of the impurity may be thus recognised. Trommer's mode of detecting the fraud, consists in the addition of a drop or two of a saturated solution of sulphate of copper to the alkaline liquid, before it is boiled; if grape sugar is present, the heat instantly produces a bright orange-yellow precipitate of sub-oxide of copper.

The existence of lead in sugar is not so readily discovered, although the presence of this metal may be suspected, if a solution of the sugar becomes brown or deposits black flocculi when it is subjected to the action of sulphuretted hydrogen. If the liquid thus treated be mixed with a solution of egg-albumen, then agitated pretty briskly, and afterwards heated to coagulation in a porcelain vessel, it will be found that the coagulum so formed contains the whole of the lead in the form of sulphuret; and by being collected in a filter, incinerated in a muffle, fused with a slight excess of carbonate of soda, and afterwards exhausted with water, the residue will enclose all the lead, together with portions of iron, and, perhaps, a small trace of copper. To remove these impurities, it is to be boiled for a short time in concentrated nitric acid, diluted with water, filtered, evaporated nearly to dryness, treated with a drop or two of pure sulphuric acid, and, finally, drenched with an ounce or so of distilled water;—that which remains in the form of an insoluble white powder consists of sulphate of lead, every 100 grains of which represent 68¼ grains of metallic lead.

Water.—In speaking of the presence of lead in sugar, and of the dangerous effects that may arise therefrom, we are led to the consideration of another subject of great public importance, namely, the existence of lead in the waters used for domestic purposes.

In the work before us Mr. Normandy has not touched on this matter, further than by saying that the metal may be detected by means of sulphuretted hydrogen, which gives it a dark brown, or black appearance. It is highly probable that he has avoided the question, in consequence of the many difficulties that beset it; but this is not the proper way to deal with so important a subject; for, in our opinion, it is right that the public should be made acquainted with all the facts connected with the inquiry. We are bound to say, however, that the statements hitherto made by chemists respecting the solvent action of ordinary waters on lead, are most contradictory; and we are glad to find that the government has at length appointed a commission of chemists to ascertain the facts of the case. The members of the Board of Health, to whom this task lately belonged, have candidly admitted, that the evidence brought before them, in respect of the whole matter, is too inconsistent for any useful purpose. Up to the year 1850, the experiments of Guyton de Morveau, Christison, Yorke, Phillips, Taylor, Daniell, and some others, led us to believe, that the purer the water was, the more readily it acted on metallic lead. It was even thought that the presence of sulphates, carbonates, muriates, and phosphates, in common water, was actually necessary to the protection of it from a plumbeous impregnation. In the fourth edition of Dr. Christison's 'Treatise on Poisons,' it is clearly shown by a number of well-directed experiments, that pure water, exposed freely to the air, acts readily on lead, forming at first a partially soluble hydrated oxide of the metal, which, in the course of a short time, absorbs carbonic acid from the air, and becomes a less soluble granular powder. Snow and rain-water were also found to act on the metal in a very marked manner; and he noticed that the same was the case with certain mineral waters of unusual purity; as, for example, of those of Dumfrieshire, Tunbridge, and Banffshire, which respectively contain the 22,000th, the 38,000th, and the 16,000th part of saline matter. In point of fact, it was determined that water could not be safely conveyed through leaden pipes, unless it had more than a 15,000th part of earthy salts in solution; and it ought not to be stored in a leaden cistern, unless the water hold as much as a 12,000th part of them in solution. This was proved by an examination of the water supplied to the city of Edinburgh, in which case Dr. Christison found, that although the water (containing the amount of salt mentioned) does not contract a sensible impregnation of lead on remaining a few days in contact with it, yet a sufficient action ensues in the course of a few months, to show that it might be dangerous to keep that water long in a lead cistern. Experiment has likewise demonstrated, that water containing as much as the 4900th of its weight of saline matter, consisting chiefly of earthy carbonates, may be kept for any length of time in lead vessels. This is the case with the water furnished to the house Phantasie, in East-Lothian. It is conveyed to the building in a lead pipe, which runs for a distance of a mile, and is received into a cistern, which, after a year's wear, was found to be singularly clean, and free from incrustation. The same is true of water containing a large proportion of these salts; and Dr. Christison refers to the mineral water of Airthrey, near Stirling, as an example of a spring water which does not act on lead at all; for it contains no less than a 77th part of its weight of saline matters, which are chiefly muriates, and partly sulphates. His more direct experiments show, that those salts act most energetically in protecting lead, whose acid forms with oxide of lead the most insoluble compound. Hence it is that phosphates and carbonates are most powerfully protective; chlorides, acetates, and nitrates least; and sulphates intermediate.

It must be mentioned, however, that many natural waters which are exceedingly hard, do, under some circumstances, rapidly corrode lead. In these cases it is found, that the salts present consist chiefly of alkaline or earthy chlorides; and it is probable, that galvanic action, as well as chemical decomposition, may be concerned in the change. But judging from the entire body of results, recorded by the authors just mentioned, we are naturally lead to the conclusion, that the corrosive power of water is in an inverse ratio to the proportion of saline matters contained in it. This is the opinion of almost every living toxicologist of repute. Mr.

Taylor, for instance, says, "it is important for the medical jurist to bear in mind, that the purer the water, or the less saline matter it contains, the more liable it is to acquire poisonous impregnation." And the general results arrived at by Dr. Christison are,—

"That rain and snow water, for culinary use, should not be collected from leaden roofs, nor preserved, nor conveyed in leaden vessels;—that the same rule applies to spring waters of unusual purity, where, for example, the saline impregnation does not exceed a 15,000th of the water;—that spring water, which contains a 10,000th or 12,000th of salts, may be safely conveyed in lead pipes, if the salts in the water be chiefly carbonates and sulphates;—that lead pipes cannot be safely used, even where the water contains a 4000th of saline matter, if this consist chiefly of muriates;—that spring water, even though it contain a large proportion of salts, should not be kept for a long period in contact with lead;—and that cisterns should not be covered with lids of this metal."

Now, it is interesting to compare those facts, and the conclusions deduced from them, with the results obtained by "the Analytical Sanitary Commission," and also with the evidence lately given to the "General Board of Health." The former have endeavoured to show, that waters containing earthy carbonates in solution, rapidly act on lead, and that the same is the case with regard to the alkaline and earthy chlorides. And the latter have, in many cases, given the direct lie to the statements made by former investigators. We cannot venture to report the whole of the evidence given; but must content ourselves by refering to one or two of the remarks which were made by a few of the leading witnesses. Mr. Thomas Spencer says:

"I find that water, which derives its hardness chiefly from super-carbonate of magnesia, is more dangerous than water which altogether or in greater part derives its hardness from super-carbonate of lime. I find that the following salts of hard water are capable of dissolving the hydrated oxide of lead; they are placed in the order of their solvent power, and consist of super-carbonate of magnesia, super-carbonate of lime, chloride of sodium, chloride of magnesium, and chloride of calcium."

The Honorable William Napier says, that he has examined 64 cisterns and pumps, in which waters of various degrees of hardness have been stowed or contained; and he finds that the amount of corrosion bears no relation whatever to the quantity of saline matter present, but rather to the proportion of free carbonic acid. In 13 house-cisterns at Farnham, in which water of one degree of hardness had been contained for a period varying from one and a half to twelve years, there was not in any case a visible corrosion of the lead. One pump, namely, that raised over the soft-water tank in the Market-place of Farnham, showed but slight traces of corrosion, after a period of forty-two years. In 50 other cisterns of leaden pumps placed over wells, containing water of various degrees of hardness, he found that 40 of them were corroded and 10 not. He noticed, moreover, that the water of the non-corroded ones was always void of carbonic acid, while that of the other 40 contained it in large proportion. In point of fact, the degree of corrosion was in a direct ratio with the quantity of gas present.

Several plumbers were likewise examined by the Board, and they gave evidence to a like effect.

Our readers will naturally ask, what, amidst all these contradictions, are the real facts of the case. In answering this question, it is highly necessary that the respondent should not allow his judgment to be influenced either by a disposition to advance the Government scheme, or by an anxiety to serve the cause of the existing Water-companies. Dispossessing our minds of all controversial feeling, we may state that the facts elicited serve to establish the following conclusion:

1st. That pure water, freely exposed to the air, readily acts on lead, forming a hydrated oxide of the metal, and a small portion of hydrated carbonate; these compounds are mostly thrown down in a pulverulent or semi-crystalline state, but a small portion of them becomes suspended in the water, and a still smaller undergoes solution; for the former is soluble in 10,000 parts of water, and the latter, according to Fresenius, in 50,551 parts.

2d. That the presence of saline matters, especially sulphates, carbonates, and phosphates, delay the corrosive action, but they do not entirely check it; as is proved by the observations of the Hon. W. Napier, and by the remarks made by Christison, who says, "that spring-water, even though it contain a large proportion of salts, should not be kept for a long period in contact with lead." Fresenius has demonstrated that sulphate of lead is soluble in 22,816 parts of water; and in our judgment the phosphate is soluble to about the same extent.

3d. That the great agents which effect the corrosion of lead, are atmospheric oxygen and carbonic acid. It appears, moreover, from the observations made by Professor Christison, that galvanic action is often concerned in the changes which take place wherever metallic impurities rest in the lead, or are contained in it; for it has frequently been noticed that a corrosion of the metal is there quickly effected. This opinion is strongly confirmed by a case reported by Mr. West at the British Association at Cork. Water had flowed from a spring, through leaden pipes, into a leaden cistern, for sixty years, without injury to either. But on being conveyed a further distance through iron tubes, it rapidly acquired metallic impregnation; and so destructive was it to the bottoms of the leaden cisterns into which it next flowed, that some of them had to be renewed in five or six years.

It appears, moreover, from the experiments made long since by Colonel Yorke and Dr. Clarke of Aberdeen, that both soluble and insoluble salts of lead may be removed from water, by filtering it through common blotting paper. Colonel Yorke believes that the former is retained by reason of the affinity which cotton fibre has for a plumbeous salt. Charcoal also has the property of absorbing, and, in many cases, of decomposing the soluble salts of lead. We have taken advantage of this property, in constructing a very simple filter, which serves to purify water for domestic purposes. It consists of a wooden funnel, made of the same shape as the funnels commonly used by brewers for filling casks. It is 14 inches high, by 10 in diameter; and the stem or tube which pierces the lower part of is, is 6 inches long and 2·5 across. This stem fits into a hole in the top of an earthenware receiver,—the receiver used is a common brown ware cask, having a tap from which the filtered water can be drawn off as it is required. The stem of the funnel is closed at its lower extremity with a piece of finely perforated zinc, and it is filled with a quantity of well-washed fine sand. The bottom of the funnel is covered to the depth of six inches with coarse-grained animal charcoal, which has also been well washed in order to separate the finer particles from it. This apparatus filters the water with great rapidity, and removes every kind of noxious matter from it. Our own filter has been in constant use for about eighteen months, and it still performs its duty in a satisfactory manner.

With regard to the saline impurities, as they are termed, of ordinary water, a great deal of unnecessary importance has, in our opinion, been attached to them. True it is that they may impart to the water a certain degree of hardness; but, as experiment shows, a great portion of this is removed by exposing the water to the air, or by raising it to a boiling temperature. Most of the waters at present supplied to the inhabitants of this metropolis, may, by a simple process of ebullition, be brought down to such a low degree of hardness, as to be entitled to the name of soft water; and considering that they are generally submitted to such a process before they are used, either for brewing, washing, or tea-making, we cannot perceive the force of the arguments which have been so frequently urged on this score. Nor can we assign any value to the evidence given by Soyer, and others, as to the inapplicability of these waters to cooking purposes. The Board of Health would, in our judgment, do more service to the public, if they were to employ their efforts in obtaining an uninterrupted supply of the water now in use, instead of busying themselves about such improvements in its quality, as are of comparatively trivial importance.

Beer, Ale, and Porter. It is hardly possible for any one to conceive the extent of adulteration, to which these articles of daily consumption are almost invariably subjected. In fact, there is at the present time a large class of persons, who get their living by sophisticating malt liquors for the publican; and, in addition to this, there are many individuals, called brewers' druggists, who deal in nothing else but the drugs employed for that purpose. These drugs consist of cocculus indicus, quassia, green vitriol, grains of paradise, linseed, aloes, Guinea pepper or capsicum, tobacco, coriander seed, logwood, liquorice, burnt sugar or colouring, finings, opium, salts of tartar, and many other like substances. Several Acts of Parliament have at various times been framed, for the purpose of putting a check on these pernicious practices. In the 56th year of the reign of George the Third, a law was passed prohibiting the sale and use of certain drugs by the publican and brewer, under a penalty of 200 pounds for each offence; but we venture to say, that there is not a publican in London who does not supply beer adulterated with one or other of them. In fact, recipes have been openly published, by those conversant with the business, setting forth the manner in which the tricks of sophistication are to be accomplished; and it appears from a return made to the House of Commons, on the motion of Mr. Ormsby Gore, that in one year, there were no less than 27 brewers convicted and fined for using deleterious articles in their beer. From another return, which was moved for by the Chancellor of the Exchequer, we learn that there were as many as 146 licensed victuallers and beer retailers convicted of a similar offence in one year. Some of the drugs mentioned are, comparatively speaking, harmless; but others are not so; to take a case:—Nothing is more common than the use of sulphate of iron or green vitriol, for the purpose of giving these beverages a 'head'; but for our part we are inclined to think with Mr. James Saunders, the self-taught lecturer, "that so much copperas as would affect the head of a pot of beer, would be not unlikely also to affect the human stomach." At one time the employment of this salt was so frequent, that, at the instigation of the Excise, Professor Faraday was induced to propose a test for the recognition of it. His test consists of a solution of red prussiate of potash in water. It is called Faraday's beer-test; and it owes its action to the deep blue colour which it strikes with a proto-salt of iron. The discovery of copperas in porter, however, is a difficult operation, because the intense brown colour of the liquid prevents the blue of the prussiate from being seen; and consequently the iron must either be precipitated with hydrosulphate of ammonia, collected and tested; or else the porter must be evaporated to dryness, and its residue charred, and treated with dilute muriatic acid, so as to obtain a clear ferruginous solution, in which the iron may be discovered by appropriate tests.

Spirits, Cordials, and Wines, are likewise adulterated in an equally barbarous manner. The first two liquids often contain capsicum, which is introduced for the purpose of giving them a hot, fiery character, that may be mistaken for strength. Wines are adulterated with cream of tartar, alum, sour cider, sulphuric acid, and bisulphate of potash or sal enixum, all of which substances give them an agreeable acid property. And for the purpose of simulating the astringency of good wine, the mixture is often falsified with logwood, catechu, terra japonica, jeropega, or other matters of that class. It sometimes happens that the wine is already too acid. Under these circumstances it is corrected with lime, carbonate of soda, and occasionally with litharge or carbonate of lead, which gives a sweetness to the beverage. It is to this dangerous practice, that the colic so often observed in wine districts is to be attributed. The presence of lead in wines was doubtless the cause of the epidemic observed in Paris in the year 1775, by Bourdelin; and that by Zeller in Germany, and by Letois in the south of France. This fraud is detected by evaporating the wine to dryness, charring the residue, and then igniting it with about twice its weight of nitre, or until it has acquired a white colour: the residuum is to be treated with dilute nitric acid, filtered, and tested with sulphuretted hydrogen, bichromate of potash, and sulphate of soda. If lead be present, the first of these reagents will occasion a black precipitate, the second a yellow one, and the third a white.

Alum is known by the acid solution of the ignited residue yielding a white precipitate with potash, soluble in an excess of the alkali, and reprecipitated by muriate of ammonia. Pure wine yields a purple stain on paper by drying; but when adulterated with sulphuric acid, or bisulphate of potash, it leaves a pink one. According to Chevalier, the fictitious colouring matter of wines may be known by means of caustic potash, the alkali being added in sufficient quantity to saturate the acid of the wine. "If the colour of the wine is genuine, no precipitate is formed; the colour will change first from red to bottle-green, and after some time to brownish green or brown." If elder berries are present, it occasions a purple colour; if logwood, a reddish purple; if beet-root juice, or Brazil wood, a red; and if American grape, a yellow. MM. Jacob and Nees von Essenbeck rely upon the colour of the precipitate produced in the wine when alum and an alkaline carbonate are added to it. In the case of pure wine, the precipitate is but slightly coloured; while in that of adulterated wine, it is more or less pink or violet.

Several methods have been proposed for the determination of the quantity of alcohol in beer, wines, and spirits; but the most simple consists in ascertaining the boiling point of the liquid, and then reading off, from Vidal's or Ure's tables, the proportion of spirit contained in it. It appears from the investigations of the first-named chemist, that the boiling temperature of alcoholic liquors is in most cases proportional to the amount of alcohol present, irrespectively of the quantity of neutral, saline, or saccharine matters dissolved in them. His experiments show that proof spirit, or any liquid containing alcohol in the same proportion, boils at 178·6° F.; and that for every succeeding 10 degrees under proof down to 90, it boils at the following temperatures:—179·70°; 180·4°; 182°; 183·4°; 185·6°; 189°; 191·8°; 196·4°, and 202°. Other rules are given by Dr. Ure, and quoted by Mr. Normandy, for the determination of the amount of saccharine and extractive matters present; but we cannot venture to give examples from them.

Vinegar is commonly adulterated with oil of vitriol, alum, or tartaric acid; besides these, it often contains pepper, capsicum, chilies, or mustard, which are added for the purpose of increasing its pungency; and it now and then receives an impregnation from copper, lead, and arsenic,—the first two being derived from the vessels with which it is brought into contact, and the last from the oil of vitriol employed in decomposing the crude pyrolignate of lime, from which acetic acid is sometimes distilled. All the vinegar of English commerce contains free sulphuric acid, from the circumstance that the manufacturers are permitted by law to use a small portion of it, in order to check the growth of fungi and the putrefaction of gluten; but, as Dr. Ure very properly remarks, "this is a miserable shift, or pretended necessity, in the present advanced state of organic chemistry. It offers, besides, an easy source of fraud, since neither the retailer nor the consumer of the article is competent to distinguish how much of the sourness is derived from the mild fermented acid, and how much from the corrosive mineral; and he adds, moreover, "that all the pickles in which our *bourgeoisie* so much delight, are polluted by the same sophistication." It might be asserted, in fact, that most of the cheap vinegars of commerce are very little else than weak solutions of oil of vitriol. The fraud is easily detected by means of nitrate of baryta, which occasions a copious white precipitate.

London milk has, for a long time past, been in very bad repute. Mr. Rugg and Mr. Dickens have done their best to expose the nature of the adulterations practised upon it, and to show that it is in many cases a villainous mixture of water, molasses, chalk, dextrine or artificial gum, and boiled starch; to say nothing of other questionable materials, as the mashed brains of animals, and the emulsion of oil-cake.

Good milk ought to have a density of 1031, or thereabout. After standing from ten to twelve hours in a tall vessel, called a lactometer, it ought to yield from 10 to 16 per cent. of cream. When coagulated by heat and a little acetic acid, the dry curd should weigh about 9 per cent.; and the cold whey ought not to strike a blue colour with tincture of iodine. The solid matters contained in good milk never exceed 11 per cent.; and the ash that results from the incineration of them ought not to weigh more than one third of a grain. Examined under the microscope, good milk is found to consist of oil globules and serum; if any other constituents are present, they indicate fraud on the part of the dealer, or disease in the animal which yielded it. Lastly, milk has been known to acquire a metallic impregnation from the zinc and copper vessels in which it is occasionally received and kept.

Tea.—About six years ago, Mr. Warrington, of Apothecaries' Hall, directed attention to the fact, that the green teas of commerce were often largely adulterated with poisonous agents. He found, indeed, that they were sometimes entirely composed of an inferior black tea, coloured superficially with a green powder, consisting of Prussian blue and a yellow matter: this yellow matter is sometimes of vegetable origin, at others of mineral, and then it consists of chromic acid and lead. The particles of good green tea have generally a sort of bloom upon them, and they present a semi-glazed appearance. To imitate these characters, the tea is shaken with magnesia, clay, lime, plaster of Paris, talc, or steatite. Black teas are also sophisticated to a very considerable extent. They are touched up with black lead and charcoal to improve their appearance; with kutch, catechu, or terra japonica to add to their astringency; and they are frequently adulterated with elder leaves, sloe leaves, and old tea leaves, in order to increase their bulk. Most of these frauds may be detected by drenching the tea on a coarse sieve with a little cold water; by this means the colouring matter and other fine particles are washed off and carried through the sieve. By allowing the water to stand for a short time, the impurities are deposited in the form of coloured precipitates. The ash that is furnished by good tea, when it is incinerated, never exceeds 5·5 per cent.

Coffee, chicory, &c.—Every housekeeper is aware that these articles of food are adulterated with a great variety of substances; the former, for example, is often sophisticated with the roasted roots of chicory, beet, carrot, parsnip, madder, mangel-wurzel, dandelion, &c.; with roasted beans, peas, lentils, corn, biscuit, acorns, and potatoes; and to these might be added the names of other matters of a more disgusting nature, as sand, earth, ochre, red brickdust, mahogany sawings, colouring matter, finings, and red pottery. We give this list on the authority of Messrs. Baring and others, who have memorialised the government on the subject; and likewise on the credit of some statements made at a public meeting of grocers and coffee-dealers, held at the London Tavern, in the month of March last. On that occasion, one of the speakers made the following observations:

> "I am a grocer of twelve years' standing, and have been sixteen years behind the counter. Last week, an intelligent man came into my shop and asked for me. He handed me an article for inspection. I looked at it, and found that it was composed of—first, burnt peas; secondly, dog-biscuit; thirdly, burnt earth; and fourthly, of a substance which I will not describe—it is too horrid to name. There were four tons of it ready for me if I would purchase them. The man told me that the material was commonly used as a substitute for chicory and snuff, and that tons of it were manufactured every week."

Hitherto the legislature has not imposed any check on the gross, and now well known, adulteration of coffee. In excuse for their negligence, they have resorted to the story, of saying that the fraud could not be detected. A statement of this kind, however, argues much for the inefficiency of our excise; for, as we shall presently show, it is totally untrue.

On some occasions, they have endeavoured to ignore the fact of falsification altogether; and that, too, in the face of evidence of the most conclusive nature. All persons who are acquainted with the working of free-trade measures, are aware that there has been, for some years past, a progressive increase in the consumption of every exciseable article of food. Among the rest, coffee has been in more extensive demand. But how, let us ask, has the demand been met by the supply. When, in the year 1846, the duty was removed from coffee, and the price of the article suffered a reduction of from 15 to 22 per cent., there was imported into this country as much as 36,793,061 lbs. of it; in 1849, the amount sank to 34,431,071 lbs.;—showing a decrease of upwards of two millions of pounds, notwithstanding that the new tariff had so far lowered the price of it, as to bring it within the reach of every man's means. We may go further than this, and show that there was a difference of 6,245,313 lbs. between the deliveries of 1847 and 1850. Only one construction can be put on these facts, namely, that the article is adulterated to a most enormous extent. This, the Chancellor of the Exchequer has, at length, thought proper to admit; but, instead of adopting some plan whereby the dishonesty might be prevented, he has actually devised a scheme for the purpose of making it lawful. He proposes, in fact, to put a duty on chicory; and, by this means, to legalise the fraud, and at the same time to give an impetus to the already too common practice of adulterating the chicory itself. It is asserted by Mr. Samuel Younger, who is a large grower and manufacturer of this article, that many farmers have adopted the plan of sophisticating it with the roots of all sorts of plants. These they add in the proportion of from 25 to 50 per cent.; making a parcel, which is sold by the farmer to the wholesale dealer; who forthwith adulterates it anew with pulse and other things, to the extent of sometimes 25 per cent., sometimes 50, and sometimes 75. On its getting into the hands of the grocer, it is again sophisticated, until, at last, the identity of the article is entirely lost. To frame a law, therefore, which will tend to give additional impetus to the practice of such frauds, is, to say the least of it, an act of great indiscretion. The predecessors of Sir Charles Wood were wiser in their time; for, by the statutes of 43d George III, c. 129, and 3d George IVth, c. 53, the Commissioners of Excise were empowered to seize such substances as chicory, ground corn, &c., whenever they found them in the possession of the grocer or wholesale coffee-dealer; and, on conviction, to fine the delinquent in a sum not exceeding 100 pounds sterling for each offence. These were wholesome laws; and if carried into effect, would doubtless do much to put an end to the dishonest practices of the unscrupulous dealer. We learn that the Excise are most anxious that the laws should be enforced; but this the Government will not permit; for, by a minute of the Treasury, made on the 4th day of August, 1840, the commissioners are particularly instructed not to prosecute the offenders, as "my Lords desire that government should interfere as little as possible." With such facts as these before us, and such difficulties to contend with, we have but one alternative; and that is to protect ourselves, by taking care that we do not purchase or use the adulterated article. One means of guarding against this, is to purchase the berry in an entire state; and another is, to submit every sample of coffee to some easy process of analysis. This we will endeavour to describe.

Many of the impurities contained in coffee may be at once recognised by throwing the suspected article into a vessel of cold water. If the powder be pure, it will swim, and will not colour the water until it has stood thereon for a considerable time; but if chicory or any other roasted vegetable matter be present, it will be instantly wetted by the water, and sinking, it will communicate a brown or reddish colour to the liquid. If this experiment is performed in a small funnel, while the finger is stopping the orifice of it, the chicory and other impurities, by rapidly sinking, get into the stem of the apparatus; and then, by a dexterous movement of the finger, they may be run off into another vessel and submitted to further examination.

Under the microscope, coffee presents the appearance of a group of small cells or vesicles, placed at some distance from each other, the intervening matter being a firm, transparent, structureless tissue. The cells contain a number of small oil-globules, upon which the aroma of the coffee depends. Chicory, however, is composed of very large cells, which abut on and overlap each other, there being no connectivum, as in the last case; and the cells never contain oil-globules, but merely a cluster of dark points, arranged, for the most part, in a linear direction, in the centre of the cell. In addition to these, chicory always exhibits a great quantity of spiral vessels and dotted ducts, tissues that are never met with in pure coffee. Most of the other roots present a similar appearance; and the particles of corn, beans, biscuit, and potatoes generally show some sign of the presence of starch granules, the form of which serves at once to distinguish them. Lastly, earthy impurities are recognised by incinerating the material, and examining the ash. Good coffee yields about two per cent. of ash.

We have not space to consider the many other adulterations practised on our daily foods; but we may refer to the fact, that *pepper* is sophisticated with flour, mustard, linseed-meal, capsicum, ground rice, and clay; that *cayenne* often contains brick-dust, ochre, and red-lead; that *mustard* is brightened with turmeric and common flour; that *isinglass* is systematically adulterated with a species of clarified glue; that *cheese* is sometimes coloured with annatto, which has been brightened with red-lead; that *cocoa* and *chocolate* are, in most cases, the filthiest mixtures of clay, starch, animal fats, potato-sugar, and even sand and brick-dust. In short, there is not an article of food that is not subjected to some kind of falsification, by which its qualities are at least deteriorated, if not made poisonous.

Nor will our limits permit us to do more than mention the fact, that hundreds of cases are on record in which danger has resulted from the use of *tainted meat, putrid cheese,* and *bad fish.* The *Wurtemburg sausages* and *saveloys of Cow Cross,* have killed more people than they have fattened; and ere long we may expect to hear of many deaths from the use of *poisoned game;* for chemists have discovered that pheasants and partridges are sometimes sent into the London market with their crops filled with arsenicated grain; and experiment has proved that the flesh of such animals is highly prejudicial to health.

A consideration of all these circumstances leads us to conclude, that there never was a period in the history of any nation, when the art of secret poisoning was carried to so great an extent as it is in this country at the present time. We may, in our knowledge of the records of the past, contemplate with horror the wholesale murders committed by such wretches as Locusta, Spara, the Borgias, Medicis, Brinvilliers, Saint Croix, and Tophania; but the deeds of these persons are as nought compared with the slow and insidious poisonings which are daily practised upon the public in our own time under the guise of honest competition. Great as our surprise is when we contemplate these facts, it becomes greater when we find that the Government is not only acquainted with them, but actually shows no disposition to check the many dangers attendant upon them; for they will neither construct new laws to meet the evil, nor give encouragement to the operation of old ones. Surely and speedily must these things be reformed, or the mischief will become intolerable.

On a future occasion we shall take an opportunity of exposing the many adulterations practised upon Drugs; a subject of no less importance than the present one.

ART. VI.

Die Hämodynamik, nach Versuchen. Von Dr. ALFRED WILHELM VOLKMANN, Professor in Halle.—*Leipzig,* 1850.

The Dynamics of the Circulation, determined Experimentally. By Dr. ALFRED WILHELM VOLKMANN, Professor at the University of Halle.—*Leipsic,* 1850. 8vo, pp. 483. Ten Plates.

THE tendencies of the present generation of physiologists, though professedly admitting the importance of all departments of physical science as bearing upon the vital dynamics, are undoubtedly chiefly directed towards Chemical affinity as the main agent of Nature in producing and maintaining the phenomena of life. Nor is this partial mode of investigating the hidden secrets of natural science peculiar to our age; it is rather the necessary consequence of the limits of human faculties and of individual existence. We are justified in attributing to the present generation a greater freedom from prejudice, and a more philosophical interpretation of the phenomena of nature, than has been before exhibited by the bulk of scientific men. But still, compared to the vastness of the unknown and unexplored regions, how minute and scanty is the information actually obtained. We must not, on this account, be remiss in our labours; we must not, because we are unable to grasp the totality of vital phenomena, shrink from laboriously and patiently pursuing those faint lights which are glimmering on our path; for in all things the individual works with and for the great hive of humanity; and every now and then he experiences the delight of feeling that the wave of knowledge has carried him beyond the reach of former ignorance. From time to time the master-mind is sent into the world, to gather the fragments which have been wrought out by the labourers of less note and power, and to mould and combine them together into a comprehensive whole. The 'coming man' of the present age of medical science, or rather—if the word is allowable—of Biotics, is yet to be looked for. We want a Humboldt to tell us what we really know, and in what relation the materials in our possession stand to each other. When he appears, he will, we may confidently say, find more ground lying fallow to receive his seed, than at any former period of man's history.

One of those fragmentary inquiries which have occupied physiologists at different times, has been characterised by being based upon mathematics; and this has fallen into disrepute, not so much because mathematical calculations are inapplicable to physiology, but because the laws, according to which the various phenomena of life takes place, are as various as these phenomena themselves; and the rigid sequence of mathematical demonstration is not in the same way applicable to them, as to geometric or algebraic problems. It would be difficult to point to two more distinct classes of mental development, than those represented by the pure Mathematician and the pure Biologist; yet it is only on account of the short-sightedness of our intellectual vision, that the union of these branches of science has not as yet been productive of more positive results. The disturbing causes on the one hand, the difficulty of observation on the other, have almost rendered the interference of mathematics in the domain of biotics, a bugbear to the student; and a neglect of a mathematical training in youth is rarely made up for in later life. Still the facts that are proven, showing the applicability of mathematical demonstration to certain physiological phenomena, sufficiently encourage us to hope for larger results, if the questions be more fully examined; and as Chemistry has shown itself able to explain many phenomena formerly attributed exclusively to vital force, so is a refined iatromathematical investigation of vital phenomena of a different class as certain to remove some of them into the domain of Physics;—neither arrogating to itself, we trust, the exclusive priesthood of nature, but consenting to minister—each according to its capacity and range—to truth alone. We do not conceal from ourselves the difficulties in the way of applying mathematics to physiology; but this does not alter the relation between the two departments of science; and we would the rather dwell upon them, as we are inclined to think, that, if our views are correct, a greater attention to pure mathesis, in the curriculum of the medical man's preliminary education, would be very desirable.

We have made the above remarks in explanation of the view we take of the work at the head of this article. At any time we should welcome a work of profound and patient research like the present; but Dr. Volkmann's labours are the more acceptable just now, as directing attention to an almost forgotten region of science, and working out the doctrine of the Circulation upon a mathematical basis. Our author does not, as Borelli did before him, found a complete system of medicine upon his doctrine; but, in the true spirit of inductive science, attempts to stretch his inferences no further than he considers himself fairly warranted in doing by his premises.

It is manifestly not feasible to reproduce, in the brief limits of a review, the mathematical demonstrations of our author. They are so linked together, as to render a disruption unfair to him; whilst their partial development would prove unsatisfactory to those of our readers who would take the trouble to follow us through the numerous formulæ. We must content ourselves, therefore, with stating the results arrived at; while we may here and there glance at the means employed, requesting those interested in the detail to refer for these to the work itself. The object pursued by Dr. Volkmann, is to ascertain the physical laws according to which the Circulation of the blood takes place, and to reduce them, if possible, to the terms of ordinary Hydrodynamics. We may at once state, that he arrives at the conclusion, that the elements in both instances being the same, the laws regulating the movement of the blood, in the living organism, are identical with those determining the movement of fluids in artificial tubes. The experiments on the movements of fluids in artificial tubes, and the relations between the channels and their contents, are investigated in the first four chapters. The First treats of the movement of liquids through rigid and straight tubes of uniform caliber, and especially examines the laws regulating the current force, the lateral pressure, and the impeding or resisting forces; the first being determinable by the velocity of the current;--the second, by the height which the liquid attains in a tube erected perpendicularly to the moving liquid;—and the third, by the fixed relation existing between themselves and the lateral pressure. The observations are made on tubes of several millimetres in diameter; and the following are some of the chief conclusions at which the author arrives:

"The lateral pressure results from the adhesion and friction occurring between the liquid and the parietes of the tubes.

"The lateral pressure, at a given point of the tube, is proportional to the resistance to be overcome by the movement of the water at this very point.

"The lateral pressure, exerted at different points of a tube of uniform dimensions, is inversely as the distance of these points from the discharging orifice.

"The ratio $\frac{L}{S}$ apppears a constant one, (L representing the length of the tube, S the lateral pressure.)

"The lateral pressure is almost inversely as the diameter of the tubes; still Sd increases, as the diameter diminishes, (d being the diameter.") (p. 38.)

With regard to the laws regulating the movement of liquids in capillary tubes, Professor Volkmann entirely coincides with Poisseuille. The most important of these is, that, owing to the increased adhesion in capillaries between the contents and the parietes, the resistance is proportional to the velocity, and not to the square of the velocity, as in other channels.

Chapter the Second is devoted to the consideration of the movement of liquids through rigid tubes of unequal caliber or angular shape. The author sums up the results of the experiments and calculations contained in the first two chapters, in their relation to physiology, and considers that they "have, on the one hand, given a scientific basis to certain doctrines long since acknowledged in physiology, whilst they have, on the other, disproved a dogma which has hitherto been widely accepted."

"I refer," he continues, "the first part of this remark to the recognised retardation of the blood-movement in those sections of the circulating apparatus, which are at a distance from the heart, and which are dilated by subdivision of the vessels; the dogma, which, according to our present experience, I esteem as no longer tenable, is Poisseuille's assertion, that the pressure is equal throughout the arterial system."

A more complicated series of experiments, and, consequently, a much more intricate mathematical inquiry, follows in the Third chapter; in which the laws are investigated that govern the movement of fluids in a system of branched tubes. The following are the main results bearing upon physiology, which the author arrives at:

"1. The pressure of the blood is subject to a gradual diminution from the commencement of the arterial to the termination of the venous system; exceptions to this law occurring only at points presenting congestion, (stauung, accumulation.)

"2. Points of the vascular system lying at equal distances from the commencement of the system, are not unfrequently exposed to varying degrees of pressure; in which case those will suffer the greater pressure, that occupy paths which the blood traverses with difficulty.

"3. The capillary network, inasmuch as it occupies a middle position between the arteries and the veins, is exposed to a degree of pressure amounting to more than half of the maximum occurring close to the heart." (p. 73.)

The Fourth chapter brings us to the consideration of the undulatory movement of water in elastic tubes, produced by an intermittent propelling power. The points that concern us chiefly, are: that the distention of the tube, caused by intermittent injections, is followed by a contraction dependent upon the elasticity of the tube; that this distention, and the consequent contraction, do not occur simultaneously throughout the entire tube, but are propagated in waves from the commencement to the point of discharge: and that this undulatory movement of the liquid is a condition essential to the production of a current. We are unable to enter either into the particulars of the experiments, or of the mathematical calculations and tables connected with them. But it may be well to point out the bearing of the main inferences above stated, upon our theory of the pulse; as Professor E. H. Weber teaches the wave to be distinct from the act of propulsion of the fluid; whereas Volkmann has succeeded in demonstrating the correctness of Bichat's views relative to their identity. Thus it would appear self-evident, that the same amount of blood which quits the heart at the contraction of the ventricles, must be received into the heart at the diastole; and that the wave is propagated from the ventricle to the corresponding auricle in the period of one pulse.

Next to the great fact of the circulation of the blood, none of its elements deserve more attention, than the questions regarding the moving power, and the extent to which the central moving power, the heart, exerts its influence. These points are ably discussed, and with less algebraical display, in the succeeding chapters, under the headings:— "On the pressure of the blood," "On the velocity of the blood-movement," "On the force of the heart," and "On the laws regulating the relations between the pressure of the blood and its velocity."

We have already seen that Dr. Volkmann differs on an important point from the chief experimenter on the pressure of blood, Poisseuille. In the fifth chapter, he examines more closely the results obtained by Hales and Poisseuille; and suggests a very useful alteration in the hæmadynamometer of the latter. He also describes a new instrument, invented by himself, and termed a manometer, by which he calculates the pressure of the blood by the compression exerted upon the air in a closed tube. The apparatus consists of a small horizontal brass tube, terminating at both ends conically; upon the middle of this a long glass tube is erected, so that the cavities of both tubes are in communication. The upper end of the glass cylinder is closed. A small stopcock is so fixed, that the experimenter is able to shut off the communication with the perpendicular tube, without interrupting the course of the circulation. Dr. Volkmann thus explains the mode of making observations with his manometer:

"Supposing the instrument to be 500 millimeters (nearly twenty inches) in length, and assuming the blood to rise in it to 100 millimeters, (about four inches,) the air would be reduced to $\frac{4}{5}$ of its original bulk. It follows, that the pressure producing this condensation must amount to $\frac{5}{4}$ of the pressure that existed before the admission of the blood, *i.e.*, $\frac{5}{4}$ of our atmosphere. If we deduct our atmosphere, we find the blood-pressure equal to $\frac{1}{4}$ atmosphere, or $\frac{760}{4}$ millimeters (7·5 in.) of mercury." (p. 146.)

Dr. Volkmann speaks in terms of high praise of Ludwig's kymographion, or wave measurer, which has already been described and figured by the inventor in Müller's 'Archiv,' 1847, p. 247. It acts upon the same principle, and records its own observations, if we may so explain it, as the self-registering anemometer, such as is used in the Royal Exchange; and by the accurate representations which it yields of the undulations of the blood, it affords an unerring test of the correctness of the analogy between the vital dynamics of the circulation and hydrodynamics.

The special value of the kymographion consists in its exhibiting the relation between the waves of the pulse and the undulations produced by respiration. Volkmann had previously objected to Poisseuille's doctrine, that the rise and fall of the quicksilver in the hæmadynamometer was due to respiration; and the delineations of Ludwig's instrument fully prove that his view was the correct one, as it also confirms other deductions at which he has arrived, either by observation or calculation. Numerous fac-similes of delineations made by Ludwig's instrument are appended to the work; as well as an accurate drawing of the kymographion, from which a mechanic would have no difficulty in constructing a similar apparatus. The experiments have been made in the venous as well as in the arterial system; and the general law of hydrodynamics, that there is a uniform decrease in the pressure from the entrance of the liquid to its discharge from the tube, has been proved to be equally applicable to the entire vascular system as to other channels. Thus, the pressure of the blood is found by Dr. Volkmann to be more considerable in the large than in the small arteries; and, *vice versâ*, in the small than in the large veins. The following table gives the author's observations on a large mastiff: (p. 167.)

Observ.	Carotid Artery.			Branch of Femoral Artery.		
	Elevation.	Depression.	Mean.	Elevation.	Depression.	Mean.
1	194	174	184	180	178	179
2	206	174	190	170	168	169
3	194	174	184	170	168	169
4	200	152	176	174	172	173
5	200	146	173	174	172	173
6	206	162	181	174	172	173
7	206	158	182	178	176	177
8	200	162	181	178	176	177
Mean	200	162·7	181·3	174·8	172·8	173·8
Reduced*	190	154·6	172·3	166·7	164·2	165·1

* The reduction refers to the subtraction of the pressure, caused by the solution of potash used in the instrument.

As a mean of 8 observations, we thus find the pressure in the carotid to be 7·2 millimeters (or 0·27 inch) more than it is in a branch of the femoral artery.

The mean of 10 observations made on a calf was, in the carotid, 122·4; in the metatarsal artery, 94 millimeters; or, when reduced as above, respectively 116·3 and 89·3, showing a difference of 27 millimeters, (or 1·05 in.) Another observation made by Dr. Volkmann is, that when an artery has been tied, the pressure is uniformly less in its peripheral than in its central (cardiac) portion, in which case the circulation in the former must be established by anastomotic currents; this militates as much as the experiment on direct currents against Poisseuille's doctrine. The following table affords a comparative view of the diminution of pressure at four points of the vascular system in the same animal; the first two observations on the goat and the horse being made on the central and peripheral (or distal) portions of the carotid. The four points are numbered in the order of their respective distances from the heart. (p. 173.)

	I.		II.	
Goat	Carotis centr.	135	Carotis peripher.	126
Horse	„	122	„	97
Calf	„	165·5	Art. metatarsi	146
	III.		IV.	
Goat	Vena facialis	41	Vena jugularis	18
Horse	Small vein of neck	44	„	21·5
Calf	Vena metatarsi	27·5	„	9

The observations on the subject of sanguineous pressure made by our author, lead him to infer that Poisseuille was in error, when he assumed the arterial pressure to be the same in all mammals, and when he concluded that the mean pressure of 160 millimeters was applicable to man. A long list of observations with the hæmadynamometer, on the arterial pressure exhibited in the carotid of numerous animals, shows that the results vary too much to permit of a satisfactory conclusion as to the pressure of the blood in man. The list is too interesting to be omitted. It includes a few observations made by other inquirers. (p. 177.)

	Millimeters.	
Horse	321	(Ludwig)
Horse	214	
Sheep	206	
Calf	177	
Large Dog	172	
Cock	171	
Sheep	169	
Dog	166	(Spengler)
Calf	165	
Goose	162	(Blake)
Stork	161	
Pigeon	157	(brachial artery)
Dog	157	
Sheep	156	
Calf	153	
Horse	150	(Spengler)
Cat	150	(Ludwig)
Dog	143	
Goat	135	
Calf	133	
Large Dog	123	
Horse (old and worn out)	122	
Young Goat	118	
Old Horse	110	(Spengler)
Rabbit	108	(Blake)
Young Dog	104	
Sheep (old)	98	
Rabbit	90	
Hen	88	(brachial artery)
Pike	84	(trunk of brachial artery)
Barbel	42	(Ditto)
Pike	35·5	(Ditto)
Frog (weighing 113 gr.)	29	(Left arch of aorta)
Turtle	23	(Ditto)
Frog (weighing 114 gr.)	22	(Ditto)

The great difference exhibited by these numerical data is remarkable; and though they appear to bear out the author's statement, that we are not justified in calculating from any of the series of numbers presented by the latest experiments, the pressure of the blood in man at any given point of the vascular system, there are certain general facts deducible from them, which bear strongly upon human physiology. Whilst it is evident that the pressure of the blood in the same artery of various mammals varies as much as treble its value, we may fairly draw the following conclusions from the foregoing table:

1. Warm-blooded animals possess a higher arterial pressure than cold-blooded.
2. Arterial pressure appears to be lowest at an early and an advanced age, while it is highest in the prime of life.
3. The pressure is not in the ratio of the size of the animals.

The first is an inference which we should probably have been most inclined to adopt *à priori*, as we naturally assume the existence of a relation between the calorific process and the force of the circulation. Nor are the second and third at variance with this proposition; but as size does not exert the control that theoretic reasoning might lead us to assume, we should probably be tempted to give to early youth a very high average, as compared with old age. On this point, one upon which the observations made on warm-blooded animals would permit a legitimate conclusion as to man, our author's experiments do not give a sufficient basis for establishing a law, though it is probably one which would exert a greater influence on the treatment of disease, than other deductions which he has placed on a firmer footing.

We now come to the consideration of the velocity of the circulation. The importance of determining the question is self-evident; and that it is still *sub judice*, is proved by the great discrepancy of opinion existing in reference to the quantity of blood discharged at each systole. If we know the actual velocity with which the blood moves in performing the circuit, there can be no difficulty in determining the exact amount that quits the ventricles at each pulse. We possess numerous data showing the velocity to be much greater than the older physiologists assumed it to be; but the difficulty of making precise experiments has not been overcome sufficiently to enable us to determine the actual ratio. We have to deal with two questions in regard to the velocity of the blood-movement; we have either to fix the time required by the blood to traverse the entire vascular system, so as to complete the circuit; or we have to ascertain the space which a particle of blood passes over in a given period. These points might, at first sight, appear identical, or at least convertible; but it is not so, for we have already seen that the blood does not move with equal velocity in every part of the system; and the varying capacity of the channels of the blood gives rise to a considerable variation in the rapidity of the movement communicated to the fluid. Supposing the calculations made with regard to the rapidity of the current in the aorta to be correct, as applied to this portion of the system, they necessarily are inapplicable to other parts. But, unfortunately, even this element in the deductions rests upon no secure basis, as the dimensions are stated variously by various observers. The estimates of the velocity of the blood in the aorta, as calculated by the dimensions of the vessel, and the measurements of its contents given by the different physiologists who have turned their attention to this matter, present results which are so wide apart as to merit no confidence. It is this question, regarding the time required by a blood molecule to traverse a given portion of a vessel, that Dr. Volkmann attempts to answer in the present chapter. The movement of the blood has been repeatedly examined, and its rapidity approximately determined, by the aid of the microscope. This, however, is only available for transparent parts, such as the frog's web, the mesentery of a lizard, or the tail of a tadpole; and the preparations necessary to place the animal in a position to permit of observation, necessarily interfere considerably with the rhythm of the circulation. On the other hand, the results are scarcely applicable to a class of beings so far removed from the animals experimented upon, as the higher mammals and man. Volkmann's observations have not added to the certainty of our knowledge. The following table shows the results obtained by several physiologists:

		Mean velocity in a second.
E. H. Weber.	Tail of tadpole . . .	0·573 mill.
Valentin.	Web of frogs . . .	0·507 „
Volkmann.	Branchiæ of larva of salamander,	0·245 „
„	Tail of tadpole . . .	0·4 „
„	Caudal fin of a small fish .	0·12 „ (p. 184.)

Our author has ingeniously contrived an instrument, which he terms a hæmadromometer, for the purpose of measuring the velocity of the blood's movement in any part of the body, (p. 185.) The main feature of the apparatus is a long glass tube, bent in the shape of a hair-pin, which is substituted for the segment of a vessel. The tube is filled with water, which has to be propelled by the blood that comes from the heart. The time required by the column of blood to displace the water, is the basis of the calculation by which the velocity of the blood, in a given period, is determined. The objection to the method that will naturally strike the reader at once, is, that the two fluids will mix so speedily as to impair the correctness of the conclusions: this Dr. Volkmann denies; and the great anxiety evidenced throughout the work to avoid every possible source of error, and the scrupulous candour with which he adverts to such sources, must weigh strongly with us in favour of his assertion; but even admitting that this error can be entirely avoided, the delicacy of manœuvring, the entire consentaneousness of eye, ear, and hand, necessary to secure an observation, appear to throw such difficulties in the way of the employment of the hæmadromometer, that we much doubt whether the results attainable by it will prove more safe than those we already possess. We are willing to admit the ingenuity of the contrivance; we must leave experimental physiologists to repeat the experiments, and to determine its actual value. Numerous observations are given, from which the author deduces the following conclusions:

1. The velocity of the sanguineous current is enormously greater than that observed in the capillaries.

2. The velocity is greater in the arteries lying near, than at a distance from, those of the heart.

3. The velocity of the blood in analogous vessels is not identical in different animals; but the variations are not greater than those which occur in animals of the same species.

4. The mean velocity of the blood in the carotid of the mammals

hitherto experimented upon, is 300 millim. (nearly 12 inches) per second. (p. 196.)

It is interesting to see, by another series of experiments, that the diminution of the velocity of the circulation stands in a direct ratio to the loss of blood by hæmorrhage, which is attributed to the loss of tone of the vessels; on the other hand, Dr. Volkmann's tables show, that up to a certain point the frequency of the pulse may increase, whilst, in consequence of hæmorrhage, the velocity of the current diminishes. In the treatment of disease, physicians are constantly acting as if this had been a long-accepted dogma; but we apprehend that few would have been able to account for their empirical proceedings otherwise than by a statement of the fact. The limits have yet to be determined, within which there is a direct ratio between the pulse and the velocity; and we doubt whether we shall ever obtain a scale by which we can determine the exact value of the pulse in man, as so many elements affect its condition; and nothing but the large experience of a scientific and intelligent practitioner is likely to determine its value in the individual case. Still there are many anomalies which will be cleared up in the progress of time; and when the relation of the nervous, and more especially of the ganglionic, system to the vascular apparatus, shall be placed upon a more secure footing than that on which it now rests, we shall recur to Volkmann's investigations with increased interest. By comparison of the results obtained in different animals, in which our author concludes that there is a definite relation between the velocity of the blood and the total weight of the body, he determines the quantity of blood discharged by the human heart at each systole to be 6·2 oz., the highest estimate that we remember to have seen; even Valentin's estimate is only 5·3 oz.

The preceding remarks would lead the reader to expect a still greater discrepancy between Dr. Volkmann's calculations of the moving power of the heart, and those of other physiologists. He only dwells upon the estimate of Poisseuille, who states it to be equal to four pounds three drachms; and although he entirely differs from the French philosopher in theory, he arrives at the same practical conclusion. Poisseuille overlooks one of the two elements constituting the force of the heart, the force which sets the blood in motion; and his estimate applies exclusively to the other element, the force which overcomes the resistance offered by the circulating apparatus. Volkmann attempts to remedy this defect, upon the basis of his previous formulæ. His mathematical deductions lead him to infer, that the force required to set the blood in motion is very small; and he thus concludes that Poisseuille's estimate is probably but one quarter lower than it should be. If the force of the heart is termed H, and of its constituents, the one which sets the blood in motion, f, and the one which overcomes the resistance $=w$, Volkmann's theory is expressed by the formula:

$$H = f + w;$$

or, according to previous calculations:

$$H = 8{\cdot}2 + 2700 \text{ millim.}$$

This, if correct, reduces f, the element which was neglected by Poisseuille, to about $\frac{1}{300}$th of the entire force; and hence his neglect of it was, as our author admits, without any practical importance.

Chapter VIII discusses the laws regulating the relation between the pressure and the velocity of the blood. By means of a small addition to the hæmadromometer, (see page 217,) the two elements are made the subject of observation. Dr. Volkmann concludes, from his experiments and his mathematical calculations, that the same law that applies to the movement of fluids in rigid channels, governs the circulation in animals, viz., that there is a direct ratio between the pressure and the velocity of the current.—The following chapter is devoted to some details of the author's method of experimenting, which we pass over.—Not without some surprise we find the heading of Chapter X to be: "On the Duration of the Circulation." Dr. Volkmann is not free from the German fault of prolixity, and we trust that he will see the necessity of generally abridging his work, should a second edition be called for; it is scarcely fair to separate two subjects which are so closely allied as the velocity of the circulation and its duration. We have already seen that the former of these subjects was discussed in the sixth chapter, or rather one of the two questions into which it may be resolved, the time required by a molecule of blood to traverse a given space in the vascular system. The author now treats of the other question, the time required by the blood to perform the entire circuit. Assuming the total amount of blood in the adult at 30 lbs., the weight of the blood ejected at one systole at 6·2 oz., and the duration of one pulse 0·85 second, he calculates the period of the circuit to be 67·5 seconds. He admits the data upon which this is based to be only approximative; and expresses his willingness to admit the correctness of Hering's and Poisseuille's observations. They have found, by repeated experiments with the well-known test, ferrocyanate of potash, that in horses the entire circulation is invariably completed in less than a minute, and generally in from 25 to 30 seconds. Here, again, we meet with a confirmation of the practical remark, that there is no uniform relation between the frequency of the pulse and the velocity of the circulation; or, in the words of the author:

"The duration of the circulation was observed in a very extensive series of experiments, whilst the pulse was in the normal or in an artificially accelerated state; it was demonstrated that a double, or even treble acceleration of the pulse, produced no acceleration of the circulation." (p. 262.)

A very decided influence is exerted upon the velocity of the circulating current by the addition of various media, such as distilled water, saline solutions, alcohol, and serum; and, as Poisseuille's experiments were found to yield the same results, whether made in artificial channels, in the dead, or in the living organism, the effect may be fairly set down to hydrodynamic influences. Thus, in glass capillary tubes, alcohol (p. 265) was found to diminish, whilst the addition of nitrate of potash increased, the velocity of the circulation. The current of serum was nearly double as slow as that of distilled water. If alcohol was added to the serum, the current was still more retarded; if, on the other hand, one of the salts was added which encourages the flow of water, the current was again accelerated.

The same effects were observed in the dead and the living subject to follow the same influences. The greatest deviation from the normal velocity of the circulation in horses was noticed on the introduction of alcohol; for the average velocity being 25 to 30 seconds, the injection of alcohol caused a retardation of from 40 to 45 seconds. Whether or not this fact has any bearing on the secondary physiological effect of alcohol on the human system, this is not the place to determine. Several anatomical points—the valves of the veins—the *retia mirabilia* of certain animals—the structure of the capillaries, and their strength—are the subjects treated of in the following chapter. A most important discussion on the forces which move the blood follows in the Twelfth chapter; for the conclusions of which our readers will probably be prepared, by what has already been stated regarding our author's views. He enters into a very full analysis of the various doctrines that have been broached on this subject in modern times; and discusses the objections raised to refute the Harveian doctrine, that the heart is the sole moving agent. An interesting experiment is detailed, which alone is sufficient to obviate Bichat's objection, that the absence of pulse in the veins proved the heart's action not to extend beyond the capillary system. It goes to prove that the influence of the capillaries is to convert the jerking movement perceived in the arteries into a uniform current. The objection based upon the vacuity of the arteries in the dead subject, Volkmann removes, by showing, that the greater tension to which they are exposed during life, causes a relatively greater contraction after the cessation of life, which, even after the heart has ceased to act, propels the blood into the veins and distends them. That the sanguineous fluid does not return to the arteries as they lose their vitality and re-distend, is owing to its coagulation in the capillaries. This is supported by the observation, that in those cases in which the arteries are found to contain blood, this blood appeared to have lost its power of coagulation. The suction-power of the heart, as one of the forces acting in the production of the circulation, is not denied by our author; but he sets it down as excessively small. He considers the expansive power of the right auricle as very trifling, and attributes its distention almost exclusively to the impulse of the blood propelled into it by the *vis a tergo*. With regard to the influence of respiration upon the circulation, he admits that, like all muscular action, it does affect the systemic current of the blood; but he finds that its accelerating effect is as great as the retardation it produces; and that, consequently, the entire result is = 0. Still he is not disinclined to admit, that—

"There are other moving forces besides the heart, which may exert an important influence upon the processes of the circulation. Let no one deceive himself as to the object of our inquiries. We have sought to determine the mechanical force which, after impelling a fluid into a system of branched channels, is capable of propelling it through them, in spite of the opposing impediments. This force accomplishes much, but not everything. It propels the blood into the arterial orifices with a pressure, which enables it to pass out at the terminations of the veins with moderate velocity, and to refill the auricles. This alone is the duty imposed upon the heart. The *distribution* of the blood in the various channels formed by the ramification of the vessels, is not in the least influenced by the heart. If more blood is carried to one organ than to another, *i. e.*, more than the normal width, length, form, and arrangement of its vessels justifies, we have to deal with a case in which we must seek for the explanation in new moving forces. Such forces, if I may be permitted the expression, fill up the meagre sketch made by the force of the heart." (p. 341.)

The main agent affecting this local change in the circulation, our author finds to be the variations in the capacity of the vessels themselves; and this variability, or change in the elasticity of the vessels, is set down to organic influences. The heart, if we understand our author rightly, may be compared to the boiler of the steam-engine, which is capable of propelling the steam to the various parts of the apparatus requiring its agency, but it does not at the same time serve as the fly which regulates and controls that action; the equalising and controlling power depends upon the tonicity of the blood-vessels:

"In the same manner as alterations in the caliber of the vessels give rise to modifications in the relations of the sanguineous current in point of space, the act of respiration causes modifications in point of time." (p. 348.)

We give the author's own words, because there is an obscurity which we have a difficulty in reconciling with his previous remarks on the influence of respiration. He distinctly says, that the modifications he now speaks of are not to be confounded with the inquiry as to whether the respiratory movements support the action of the heart. These modifications, in point of time, he investigates by the aid of the instrument already spoken of,—Ludwig's kymographion. The curves it delineates exhibit, in a palpable manner, the influence exerted by the respiration upon the circulation. A double series of curves is represented, of which one figures the impulse of the heart, the other the variations due to inspiration and expiration. We must confine ourselves to the mere mention of the subject; since, without the plates, our explanations would not be sufficiently intelligible, and also because we have already devoted as much space as can be allotted to the analysis of Dr. Volkmann's Monograph. It is for this reason that we cannot enter into a detailed analysis of the three remaining chapters of the work, which treat of the action of the heart and of the pulse. In the first of these are discussed the sounds of the heart, the relative duration of systole and diastole, and the cause of the heart's movements. While Dr. Volkmann does not deny, that the muscular contraction contributes slightly to the production of the first sound, he is inclined to Skoda's opinion, that it is mainly due to the distention of the auriculo-ventricular valves; the second sound he attributes exclusively to the distention of the semilunar valves. His remarks on the relative duration of the systole and diastole, which are based on direct observation and measurement, tend to establish a relation different from what is generally admitted. The times occupied by the systole and diastole were measured by the aid of two half-second pendulums; and by comparing them with the intervals of the pulse in the same individual, he concluded the relation to be as 96 : 100, this being a mean of nine observations. The following is an example of the method pursued:

"N. N—, æt. 34, pulse 84 in a minute:
Duration of the first interval = S = 0·3750″;
„ second „ = D = 0·3798″.

Sum of S + D = 0·7548″,
Duration of a pulsation = 0·7140″,

Difference = + 0·0498″.
Relation of systole to diastole = 99 : 100.

Experiments made with the kymographion, yielded results confirming this method of observation.

With regard to the actual cause of the heart's action, Volkmann passes in review the arguments for and against the Hallerian doctrine of irritability; and discards it entirely. Those who have studied the former contributions of Volkmann to neurology, will be prepared for his theory of the heart's action: he considers no other hypothesis to be tenable, but one based upon the assumption of a nervous centre residing in the heart itself. He views the ganglia of the heart as the parts performing the functions of such an organ, and attributes to them the power of regulating the cardiac movements.

In the Fourteenth chapter of the work, we meet with many valuable observations on the pulse. The actual phenomenon is attributed, as will have been gathered from earlier passages, to the function of elasticity only; and it follows equally from the author's views regarding the action of the heart, that the distention of the arteries diminishes inversely with this distance from the heart. We must refer the reader to the work itself for the experiments and calculations made to determine the relation between the lateral and longitudinal distention of the arteries. They lead to the conclusion that the latter is relatively less than the former, whilst the reverse was observed to be the case in the veins. The influence of age, stature, diet, sex, and pathological conditions, upon the force and frequency of the pulse, is submitted to a careful analysis.

The work concludes with a chapter on the mechanical disturbances affecting the vascular system; and the practical remarks contained in it, with reference to the effect of ligatures, the application of cold or heat to the surface, venesections, and leeches, are not only explanatory of known phenomena, but in some cases eminently suggestive in point of therapeutics.

We must not bring our notice of this Treatise to a close without expressing our high sense of Dr. Volkmann's merits, as an experimental and philosophical physiologist. We have ventured to hint that the value of the work might be increased by a certain condensation, both of style and matter. There are probably few medical men on this side the channel, who will have time or patience to study the 483 pages of the Hæmodynamics; and yet, we think that, in many respects, the author's method of experiment, as well as his careful and scientific mode of reasoning, might serve as a model to our own physiologists.

ART. VII.

On the Origin of Inflammation of the Veins; and on the Causes, Consequences, and Treatment of Purulent Deposits. By HENRY LEE, F.R.C.S., Assistant-Surgeon to King's College Hospital, &c. &c.—*London*, 1850. Post 8vo, pp. 91.

IN reviewing the present state of medical knowledge, there is nothing more remarkable than the gradual change that is being produced by pathological inquiry. The repeated observation of morbid appearances, (post-mortem inspections being the rule, and not, as formerly, the exception,) the advanced state of organic chemistry, the aids of microscope, and the greater precision of experiments, all contribute to supply materials that will form the basis of the principles of medicine. The intelligent physician must perceive that the period is passing away, when theories are received and adopted because they are plausible, and *seem* to be consistent with truth. At the present time *facts*, whether derived from accurate observation, careful experiment, or strict analysis, are more anxiously sought for; and every effort is made to place medicine more completely among the inductive sciences. The golden age of speculation is gone; the iron age of stubborn fact now occupies the attention. We no longer have the solidist and the fluidist, the mechanician and the vitalist, dividing medical opinions; but rather the analytic chemist and microscopist, the morbid anatomist and the experimental pathologist, separately developing principles that harmonise with, instead of opposing, each other; and furnishing their respective contributions to the science which they are aiding to build up on a broad and secure basis.

We need scarcely urge upon our readers the extreme importance of the study of the morbid conditions of the Blood, or the value we attach to any well-conceived and well-executed inquiry into any department of this subject. Such an inquiry, in regard to the poisoning of the blood by pus and other vitiated fluids, has been most carefully prosecuted by Mr. Lee in the Prize Essay before us; and we trust, that at a future time, he will direct his attention to those other and more subtle poisons, that originate epidemic and endemic diseases, and cause such destruction of human life. His present task, admirably executed as it is, has been a very arduous one, because, as he truly observes:

"The difficulty of tracing diseased secretions after they have become mingled with the blood, or of recognising their presence in the vessels, has rendered the investigation of their actions often tedious and inconclusive; while, on the other hand, the changes of structure in solid parts, readily appreciated by the senses, have been more calculated to arrest the attention, and to afford that ready solution of the origin of the symptoms which, whether imaginary or real, has a tendency to relieve the mind from doubt and suspense. Hence it has happened, that the pathology of the solid parts of the body has received a very disproportionate share of attention." (Preface.)

Nothing can be more true; and hence, also, the very terms used, express incorrectly the diseases they are meant to represent. Thus in the subjects before us, phlebitis, inflammation of the veins, a disease of the solids, is meant to signify what is essentially a disordered state of the blood itself. This has not escaped Mr. Lee's attention; and he guards his readers from supposing that he has been so misled:

"Most of the observations which have tended to advance our knowledge of the effects of the introduction of diseased fluids into the blood, have been recorded under the name of Phlebitis, or Inflammation of the Veins; and I have retained this title, although it is obviously inadequate to express those constitutional affections which form the most important and characteristic features of these complaints." (Preface.)

The proper subject, therefore, of Mr. Lee's Essay, is vitiated blood and its consequences. The first disease that he points out, is vitiation by pus. The effects of pus on the blood, much more than that of other fluids, is capable of demonstrative evidence, and will enable us more readily to explain by inference the effects of other morbid secretions. The influence of pus on the blood has been already examined into; but too imperfectly to avoid the objection, that the explanation was too mechanical for truth:

"The introduction of pus into the system," observes Mr. Lee, "has justly been regarded as the most important of this class of diseases. But the theory of the circulation of pus globules with the blood, supported as it has been by much ingenious reasoning, and most conveniently adapted to explain the formation of purulent deposits, has yet never obtained general belief. The stoppage of pus globules in the capillary tubes has appeared to many too mechanical a solution of the origin of these abscesses; and it has become necessary to determine, with more precision than has hitherto been done, the actual conditions under which pus in substance can be received into the circulation." (Preface.)

This is the question to which Mr. Lee has applied himself,—how and under what circumstances pus can circulate. John Hunter ascertained that the admixture of pus with blood caused its coagulation:

"The simple experiment of mixing some pus with healthy recently drawn blood, will at once show that such a combination cannot circulate in the living body. It will be found that the blood coagulates round the globules of pus, and forms a solid mass, which will adhere to the first surface with which it comes in contact; and it will be evident, that it is not till the coagulum thus formed is broken up or dissolved, that its elements can circulate with the blood." (Preface.)

Mr. Lee first points out, by experiment, the effect of pus and other substances on the blood out of the body; then shows, in a similar manner, its effect when injected into the veins of a healthy animal; and, finally, places before us his observations to explain how this natural process is interrupted, and a vitiated fluid is allowed to circulate. In making his experiments, "pus was used in preference to any other fluid; first, because the power of coagulating blood, which it was found to possess, enables its influence to be traced within the body; and, secondly, because, being an animal secretion, the results obtained are likely to be analogous to those produced by the admixture of other secretions with the blood." (p. 4.)

We shall relate two of these experiments; the first, to prove the effect produced on the blood out of the body; the second, within the vessels:

"EXP. I.—On the 25th of September, 1848, having procured four small vessels of equal sizes, I placed in the first some dilute sulphuric acid; in the second, some offensive pus; and, in the third, some water. The fourth vessel was left empty. They were all equally warmed, and some blood from the jugular vein of a healthy horse was received into each of them, so as to fill them to the same level. They were now stirred with separate pieces of wood. At the expiration of two minutes (noted by the watch) the contents of the second vessel (pus) had become coagulated into one uniform mass. The contents of the first vessel (containing the acid) were thickened, and of a dark brown colour; in the third and fourth cups, the blood was of its natural fluidity, but darker coloured in the cup containing water than in the other. At the expiration of ten minutes, the blood in the fourth cup (unmixed) began to coagulate, the blood and water still remaining fluid. At the expiration of a quarter of an hour, the blood had completely coagulated in the fourth cup, containing blood alone; and had very partially coagulated in the third cup, containing blood and water." (pp. 26-7.)

Thus healthy blood, out of the body, occupied fifteen minutes in completing its coagulation; but when mixed with pus, "coagulated into one uniform mass" in only two minutes. Let us now observe, with Mr. Lee, the effect within the vessels. In Experiment VIII, a healthy ass, six years old, was operated upon, the 16th of November, 1848. Putrid pus was injected into the left jugular vein, but this being unintentionally mixed with water, rendered the experiment uncertain; on the 26th of February, 1849, when the animal recovered, it was made the subject of another experiment:

"The right jugular vein having been opened, two fluid ounces of pure healthy pus were injected, and propelled in the course of the circulation, by pressure upon the vein externally. The vein became tense during the operation, and sensibly resisted the attempts that were made to propel its contents towards the heart. *Even forcible pressure was not sufficient to overcome the resistance offered to the return of blood.* Symptoms of constitutional irritation followed, the vein was felt thickened as far as the sternum, and the animal was destroyed, March 7th, about nine days afterwards.

"*Post-mortem appearances.*—The left jugular vein was found completely obliterated. The remains of a firm coagulum obstructed its canal for some distance below the opening which had been made into it, and terminated below, in an elongated conical portion, which adhered to one side only of the vessel. On the right side, an abscess had formed in the course of the vein; and for two inches, the whole of the parts were imbedded in a confused mass of pus and lymph, in which it was impossible to distinguish the structure of the vein. Both above and below this, for several inches, the vein was filled with coagula, which effectually obliterated it. These coagula extended for several inches in the course of the circulation; but beyond, in both directions, the vessel was pervious." (p. 32.)

Thus it was proved, that pus caused the coagulation of blood even more rapidly within the vessels, than out of the body. In the Experiments VI, VII, VIII, "so sudden was the effect, that the mixture of blood and pus coagulated before it could traverse the jugular vein, as indicated by the induration and cord-like feeling of the vessel." Such is the provision of Nature, to circumscribe the poison, and to prevent its access to the vital organs. The vein in which the coagulum forms soon becomes thickened, and is ultimately obliterated, or again rendered pervious; "but, as the experiments cited prove, *this thickening is the effect and not the cause of the stagnation of the vitiated blood in the vessel.*" (p. 6.) The changes that take place in the vein where this stagnation occurs, have been ably pointed out by Mr. Lee, and illustrate some important pathological facts. Mr. Lee first directs attention to the effect of a coagulum on a serous surface:

"When blood coagulates in a serous cavity, a thin pellicle forms upon its surface; and becoming thickened by deposition from the fibrin of the blood, forms a cyst, which completely circumscribes the effusion......Every layer of lymph observed upon dissection, has, perhaps, too generally been considered as the result of inflammation, and hence there has arisen a confusion in the terms employed. That lymph may be derived from the blood directly, and deposited in the form of a membrane, without being secreted by any vessel, has been fully shown by a paper in the 'Med. Chir. Trans.,' (vol. x, pp. 45-82.) Such layers of lymph assume so much the appearance of others derived by secretion from inflamed capillaries, that they have been described as identical. *But their mode of formation is altogether different.* In the one case, the process is a local one, confined to the blood itself, and, subsequently, to the membrane with which it happens to be in contact. In the other case, it is an effort of the constitution, accompanied by constitutional symptoms." (p. 6.)

This former process is Hunter's union by the first intention,—union without inflammation; but which has been constantly confounded with adhesive inflammation. "The highest authorities, both here and on the continent, describe it as identical with adhesive inflammation." The great influence of Bichat's name gave weight to this opinion; and as he stated, that "the cicatrization of wounds in veins after bleeding was the result of inflammation," so, even to the present day, it is a kind of surgical postulate, that inflammation is essential to cicatrization. The doctrine of Hunter,—union by the first intention,—cicatrization without inflammation,—received an able supporter in the late Dr. Macartney of Dublin; who, in his lectures on inflammation, took great pains to point out the process of union by blood, and of repair by the natural growth of the tissues,—a non-inflammatory process, which he contrasted forcibly with union by ulceration, granulation, and secretion of pus,—all the consequences of inflammation produced by the irritation to which wounded surfaces are exposed. He argued, that if the contact of the atmosphere were excluded, and attention paid to equalising the temperature of the injured parts, union by healthy regeneration of tissue would follow, without any granulation or secretion of pus. Hence he looked upon inflammation as a morbid process, and discarded the terms, healthy inflammation, healthy pus, &c. We believe that one of the greatest improvements in modern surgery,—the proper use of water-dressings,—has had its origin in Macartney's development of Hunter's doctrine of union by the first intention. We are happy to find another advocate of the same opinions in Mr. Lee, who applies a similar mode of reasoning to the internal, that Macartney did to external surfaces:

"Now it is submitted," he observes, "that when the blood coagulates either in serous cavities or in veins, the process of union is not unusually one of inflammation, or one in which the powers of the constitution are called into increased activity. It is true, that in both cases, inflammation may take place, and lymph, as the result or such inflammation, may be secreted; but this is only when, to use Mr. Hunter's language, the 'primary intention' has not been fulfilled." (p. 7.)

Mr. Lee then proceeds to show the effect of effusion of blood on a serous surface:

"When a membranous layer of lymph is deposited from effused blood, it adheres with some firmness to the surface with which it is in contact; but as there is at first no vascular connection established between them, it may be separated, leaving the part to which it adhered in its natural condition. Lymph derived from adhesive inflammation, on the other hand, when separated, leaves the surface upon which it was formed rough and uneven. Coagulated fibrin, when recently deposited, may thus be distinguished from effused lymph." (p. 8.)

To this very important distinction we shall again have occasion to refer; for the present we shall follow Mr. Lee in his application of this principle to injured veins:

"The changes which the blood undergoes when effused in serous cavities, may likewise take place when it is detained in injured or exposed veins. The coagulation of the blood in such cases, seems as a bond of union between the sides of the veins (which may be either temporary or permanent), so as to prevent the entrance of any foreign matter into the circulation. When blood thus coagulates in veins, changes may be produced analogous to those mentioned as occurring in serous cavities. If the quantity of blood be large, a thin pellicle is at first formed upon its surface. This membrane becomes thickened, and adheres to the internal surface of the vein. It then becomes vascular; and finally, so firmly united to a part of the circumference of the vessel, as to be inseparable from it without lacerating its lining membrane." (Ib.)

The degree to which coagulation may take place, varies as the extent of the injury and strength of the patient, from the simple coagulum that reunites the edges of a punctured vein, to the firm prolonged plug that precedes its obliteration. Mr. Lee points out three ways in which a coagulum may obstruct the circulation through a vein:

"1. By the outer layer of the coagulum forming a membrane, which contains the more fluid parts of the blood. 2. By the whole of the blood contained in the vessel forming a solid coagulum. 3. By a coagulum adhering to the injured side only of the vessel." (p. 9.)

As in veins after bleeding. We can thus perceive Hunter's principle of union by blood, operating as clearly in the internal cavities, as in the external surface. In both, the coagulum that is formed adheres to the surface with which it is in contact; in both, the deposited fibrin becomes the medium of vascular intercourse between the injured parts; in both, reunion may take place without inflammation. When pus, therefore,—a morbid product, a poison,—enters a vein, the instant effect is coagulation of the contained blood, the circulation in the vessel is at once interrupted, the coagulum unites the sides of the vein for a sufficient distance on both sides, and then the process of contraction of the tissues is set up, by which the vessel is ultimately obliterated. All this may occur without inflammation. Veins are not naturally inclined to inflame; and if there were no foreign matter to get rid of, we should have none in the case supposed:

"When pus, or other diseased fluid, is confined to the cavity of a vein, the constitutional symptoms produced are comparatively mild, as long as it remains limited and circumscribed by adherent coagula: that is to say, so as to be excluded from the rest of the circulating system." (p. 15.)

But the pus so confined must make its way out somewhere; and thus we find the source of abscesses in the surrounding tissues, and the consequent local inflammations. Mr. Lee's 7th Experiment will illustrate this:

"On the 23d of November, 1848, about an ounce of perfectly pure pus (previously warmed) was injected into the right jugular vein of an aged ass; the vein immediately became 'corded,' and the blood appeared to have coagulated in the vessel. The operation did not much excite the breathing; but the pulse, which was naturally 35 in the minute, rose to 60, and subsequently fell to 55.—24th. The animal dejected; appetite indifferent. The vein can be traced as a thickened cord as far as the sternum. Respiration 12 (the natural standard); pulse 50.—25th. The parts around the vein much infiltrated with serum; pulse 55; respiration 12.—26th. *The wound in the neck began to suppurate,* and an abscess subsequently formed in the course of the vein, about midway between the opening and the sternum. The general symptoms continued, with very slight variation, until the 4th of December, when the animal was destroyed.

Post-mortem appearances.—The jugular vein was found to have become inflamed only in the course of circulation, and to be obliterated a short distance below the external opening. The surrounding parts were greatly infiltrated with serum and lymph, and several abscesses had formed in the immediate neighbourhood. The lungs did not present any well-defined patches of congestion, as in the last-mentioned experiment." (pp. 29-30.)

The ultimate result, then, of pus being mixed with the blood of a healthy animal, is its exclusion from the general circulation and discharge by suppuration; but the fatal effects that too frequently follow the vitiation of blood by pus and other poisons, render it necessary to inquire into the causes that interfere with or prevent this salutary process taking place. We are indebted to Mr. Lee for a very clear account of the manner in which the process of repair may be interrupted, and the union dissolved. A very common disturbing cause is the displacement of the clot. "This is practically known to farriers; who, when they want to bleed a second time from the same orifice, break down 'the union by the first intention' by a blow upon the vein. During the time that the parts are united only by fibrin, any violence must tend to produce the same effect." (p. 9.) The coagulum may be disturbed, and the channel of the circulation again opened. Does the poison consequently enter into the general system? Mr. Lee ably demonstrates by his experiments, that Nature provides against and sometimes surmounts this difficulty:

"If the constitution is good, and the coagulating power of the blood unimpaired, the union may be frequently interrupted, and yet be as frequently re-established in the same way. When from any local cause, or from any constitutional peculiarity, the union by the first intention fails at the seat of the injury, it may yet be attempted at some distance up the vein; and then we have coagula formed at different distances along the vessel. If these coagula fill the vein, are firm, and remain undisturbed by violence, the union may be complete, and the vessel sealed at those parts, even although the original wound should suppurate. But it sometimes happens, that the same peculiarity of constitution, or the same local cause, which prevented the union at the original wound, may prevent complete union by the first intention at any other point of the vein; and then its canal is open to any secretion that may be introduced into it. Foreign matter may thus find its way along a vein; but still there is a provision against its being carried the round of the circulation." (p. 9.)

This provision consists in the coagulating force given to the blood by contamination. "This tendency to coagulate around the foreign matter, once impressed upon the blood, cannot be destroyed by the coagulum being mechanically broken up." (p. 10.) Successive attempts are made along the vein to interrupt the progress of the poison, and to discharge it. And while efforts are thus made in the leading trunks to interpose a barrier to prevent access to the centre of the circulation, similar changes are taking place at the circumference to retard the transmission of poisoned blood. "When the principal veins in a part become obstructed, it is natural to suppose that changes should be produced in the smaller veins which supply them." (p. 11.) The return of blood to the heart being prevented, the corresponding capillaries become congested, the surrounding parts œdematous, and bloody patches are met with through the tissues:

"In M. Cruveilhier's experiment of injecting ink into the veins of dogs, he found that in thirty-six hours the legs swelled, and a number of bloody patches (*foyers apoplectiques*) were found in the substance of the muscles and the cellular tissue of the limb. The large veins were distended with adherent coagula of blood, and the smaller veins around the livid patches were also filled with coagulated blood. If the animal were allowed to live, the congested spots suppurated." (p. 12.)

Thus at both extremities of the circulation is the carriage of poisoned blood impeded. If, notwithstanding the coagula of the larger veins, some pus arrive at the heart, and be sent forward through the arteries, it is again arrested at the capillary circulation, there to be expelled by abscess. Every provision is therefore made, to prevent a poison taking the round of the circulation; and although the consequences that follow this obstruction are in themselves extremely dangerous, and often fatal, nevertheless they must be viewed as the result of a salutary effort, and not confounded with the effects of the poison itself. Mr. Lee takes some pains to guard his readers against the error of attributing such effects to inflammation :

"The appearances thus produced in the muscles and cellular tissue of the limb, were evidently not those of inflammatory action propagated along the coats of the veins, for the affection in the capillaries was circumscribed, and terminated in many places abruptly, leaving the veins in the immediate neighbourhood perfectly healthy; still less could the appearance produced depend upon the injected fluid finding its way through the veins (contrary to the course of the circulation) to the capillary system; nor, lastly, could it depend upon the ink finding its way into the general circulation, and producing its effect a second time through the limb; for, not to mention that the capillaries of the lungs and other parts would be equally liable to be affected, one essential condition of the success of the experiment is mentioned to have been, that the fluid injected *should not find* its way along the vein in the usual course of blood. We therefore conclude, that it was the coagulation of blood in the large veins which caused the congestion of the capillaries, those veins remaining unaffected which could discharge their contents by some collateral channel." (p. 12.)

Hence, the morbid appearances which are presented after certain diseases may be readily explained, without assuming inflammation to be their cause. In Dr. R. Lee's case of phlegmasia dolens after child-birth, Mr. Lawrence performed the dissection of the limb after death :

"The external and common iliac veins were filled with a substance like the laminated coagulum of an aneurism. The tube was completely obstructed by this matter, adhering as firmly as the coagulum does in any part of an old aneurismal sac. In its centre was a cavity containing about a teaspoonful of thick fluid of the consistence of pus, of a light brownish-red tint and pultaceous appearance." (Med. Chir.-Trans., vol. xii.)

The femoral vein was also filled, and its coats were red; but, as Mr. Lawrence properly observed, "the red colour of that vein might have been caused by the clot everywhere in contact with it," and cannot, therefore, be deemed proof of inflammation. So also in Mr. Guthrie's case of amputation of the right thigh: the left leg swelled up, and became intolerably painful; but "*upon a careful examination, no pain was felt in the course of the iliac vessels upon that side;*" yet, on examination after death, at the left groin, the iliac vein *was greatly distended with pus.* The termination of the amputated femoral vein sloughed, and pus was evidently conveyed thence to the left iliac vessels. Another illustration of the same fact, Mr. Lee derives from Hunter:

"Mr. Hunter observed, that the whole side of the head, in horses that had been bled, would frequently become swollen and inflamed. The explanation of this fact appears very simple, when viewed in relation to the general principle illustrated by the above cases. The horse has only one jugular vein upon each side; and although in the usual operation of bleeding, its channel is not obstructed, yet, if the wound do not readily heal, its contents will coagulate. The circulation will then be obstructed in all its distant branches, and the blood, if long retained, will coagulate in them also. It will then part with its serum, and give rise to all the symptoms of inflammation in the distant vessels; a pulpy elastic swelling, accompanied with great pain, will then be the principal symptom, while the turgescence on the surface will be less than where the superficial veins have been mechanically compressed." (p. 14.)

Phlebitis is not, then, a necessary consequence of the introduction of pus into the cavity of a vein; it may, as in Mr. Guthrie's case, be distended with pus, and yet give no evidence of inflammation. We therefore give our fullest assent to the conclusions at which Mr. Lee has arrived.

"First, that inflammation of a vein, or phlebitis, is no essential part of the primary affection which precedes constitutional symptoms, even when morbid matter has found its way into the circulation through a vein. Secondly, that when inflammation of a vein does occur, in some instances at least, it is not the cause but the consequence of the introduction of diseased or foreign matter into the blood."

Pus in a vein is the cause, not the effect, of the phlebitis.

"Thirdly, that, although veins are with difficulty inflamed by any mechanical injury, they are susceptible of rapid inflammation, accompanied with constitutional disorder, whenever any irritating fluids are introduced into their cavities." (p. 11.)

"Which the blood fails to circumscribe," is a condition that we think should be added to this third proposition; because it is evident, from Mr. Lee's experiments, and his own admission, that, "when pus or other diseased fluid is confined in the cavity of a vein, the constitutional symptoms are comparatively mild, as long as it remains limited and circumscribed by adherent coagula." If, then, we are prepared to admit that, although pus may, by its presence, excite inflammation in a vein, it may be present without any such effect, it becomes an interesting inquiry how the process of repair is completed. If union by the first intention may take place in the side of a wounded vein without inflammation, may the vein be obliterated by a similar means? To answer this query, we shall quote Mr. Lee's account of the changes that follow in a vein plugged by coagula:

"The tendency of a clot of blood is to contract; and a time comes when the coagulum is either broken up, or shrinks; so that if no further changes are produced, the current of blood through the vein is re-established. Meanwhile, however, the coats of the veins have undergone changes corresponding to the degree of irritation produced by the contained fluids, and the intention or result to which the inflammation tends. If the coagula have long remained, the coats of the veins are always found thickened, sometimes to three or four times their natural thickness, and sometimes so as completely to obliterate the vessels. . . . . As the coagulum contracts in a vein, if the intention is to obliterate the vessel, its sides are gradually approximated. In the smaller veins, and in the divided extremities of large veins, the sides are soon completely drawn together. But the latter, if not wounded, may for a long time retain coagulated blood in their contracted, but not completely closed, cavities." (p. 15.)

The condition of the vein varies according to the time at which it is examined. The contents of the vein are occasionally found to consist, as far as can be seen, simply of coagulated blood; at other times they are found filled with soft yellowish coagula, deprived more or less perfectly of their colouring matter; more rarely, the cavity of the vein will be found filled with dark-coloured membranous layers, leaving still a channel through the vessel; and occasionally it will be found completely obstructed by "dense dark-coloured bluish membranes." (p. 15.) We can perceive, then, that in the veins precisely the same elements of repair are in action, as on the external surface, viz., contraction of the tissues, and union by blood. The sides of the vein contract, and are gradually approximated: the coagulum also contracts, and if undisturbed, becomes the bond of union between them; but as the circulation will always seek to return to its former channels, the residual coagulum may be carried forward to the heart before such union takes place, leaving the parietes of the vein so thickened as to form a very narrow canal.

This thickening of the coats of the vein seems to be attributed by Mr. Lee, in the passage just quoted, to inflammation—the result of irritation. We do not think a coagulum irritates; and we would prefer to adopt a previous quotation, as an explanation of this change. We rather think, that the thin pellicle, first formed on the surface of the clot, "becomes thickened, and adheres to the internal surface of the vein. It then becomes vascular; and, finally, so firmly united to a part of the circumference of the vessel, as to be inseparable from it, without lacerating its lining membrane." The coats of the vein are thus thickened at that part from which the residuary clot has been removed, leaving a very narrow channel for the blood to flow. It appears to us, therefore, that the contraction and obliteration of a vein may follow the formation of a coagulum within its coats, just as readily as a wound in its side is cicatrized without inflammation, affording us another example of union by the first intention. It is very essential to direct attention to the important facts that Mr. Lee has brought before us; because, if the conclusions we have derived from them be true, his essay may correct one of the most popular errors in connection with this subject. The presence of pus in the cavity of a vein, the walls of the vein being thickened, and the adhesion of the opposite sides, are all set down as the results of inflammation, where no inflammation may have existed; and the term "phlebitis," applied to a condition that it will not express. A further confirmation of this view may be derived from the known fact, that veins inflame very slowly; that they may be cut, tied, and bruised, and yet not inflame. But if we assume that the changes we have mentioned may take place without inflammation, how, then, is inflammation produced? Whence arises the violent and rapidly fatal character of phlebitis? Mr. Lee has shown to us that pus becomes a cause of inflammation when the constitution makes an effort to discharge it from the body. The simplest instance is the abscess formed at the point where pus has entered the vein; a more complicated proof is afforded by the successive purulent deposits in the course of a vein: but when the efforts made to circumscribe the poison are defeated, and pus really circulates, it is then that phlebitis assumes its worst and most fatal characters; it is then that its existence is made manifest by the highest degree of constitutional disturbance. If we seek for the causes that produce so dangerous an effect, we must look for it in those that interrupt or prevent the reparative process we have described. We have already alluded to the accidental displacement of the primary coagulum; so also the clot contracting more rapidly than the vessel containing it, may re-open the vein, and permit some pus to escape; but the chief causes that interfere with the provisions of nature, are the peculiar relations of the veins themselves in certain positions. Mr. Lee has, with much ability, classified the conditions under which veins inflame, and produce severe constitutional symptoms.

"The cases in which constitutional symptoms follow inflammation of the veins, will be found to divide themselves principally into three large classes:—1. Those in which one of the larger veins has been opened. 2. Those in which some portion of bone has been involved in the original lesion. 3. Those that occur after childbirth." (p. 17.)

Examples of the first class are found in the larger veins at the bend of joints, or those in intimate relation with the more powerful muscles; in both of which any process of repair is very likely to be disturbed.

"In the structure of the bones, the veins lie in unyielding channels, and are consequently deprived of the assistance derived from the approximation of their sides, as in soft parts, during the process of reparation. As the coagula contract in such a case, there is danger lest the union by the first intention should be disturbed, and the cavities of the injured veins should be left exposed." (p. 16.)

The intimate union between the coats of the uterine veins and the muscular fibres of the uterus, render the contraction or expansion of the one quite dependent upon that of the other; and besides this, these veins possess no valves; so that if the muscular walls of the womb relax, its large short venous trunks, that freely intercommunicate, are perfectly open, and no contraction of the vein can take place; and if the coagula become displaced, there is nothing to prevent the access of pus or any other cause of vitiation to the large venous trunks.

"The coagula which close their extremities secure them against the entrance of any foreign matter; but should these coagula be removed before the vessels are otherwise protected, their open mouths are exposed to any secretions that the uterus may happen to contain." (p. 17.)

Fortunately for the parturient female, these guardians are not so easily removed. The muscular fibres of the uterus are seldom completely relaxed; and the extremely oblique course of the venous canals, the zigzag direction that they take, and the temporary valves that are interposed at every angle where two veins meet, so isolate each sinus, that a coagulum is quickly formed when the uterus contracts; and, unless in cases of extreme hæmorrhage, it is never so relaxed as to remove it. In these instances, the whole are undoubtedly washed away; but the treatment, if successful, increases the contractile force of the uterus so much that the coagula are soon restored. If it were not for this provision, phlegmasia dolens and phlebitis would be the rule, and not the exception, after labour. It sometimes, however, will happen, that these coagula are displaced, or, by their contraction, reopen the vein.

"The period at which the union of a coagulum in a vein is dissolved, is sometimes marked with great precision. In a case recorded by Dr. Davis, a patient was convalescent from an attack of phlegmasia dolens, when death took place instantaneously, while the patient was in the act of changing the sitting for the recumbent posture; the left external iliac vein was thickened, and its internal tunic was studded in several places with deposits of adherent lymph. The portion most remarkable for this incrustation, as well as for other disease, was immediately beneath Poupart's ligament; the vein, although contracted, was manifestly pervious."(p. 17.)

So also with regard to the time at which phlegmasia dolens commonly presents itself, we perfectly coincide with Mr. Lee's remarks:

"The period of the occurrence of what has been described under the name of uterine phlebitis, is marked with much precision, and the affection of the system is often general and sudden. It may be stated, as the result of all observations hitherto made, that it occurs most frequently from the 10th to the 20th day after parturition. (Dr. Lee: 'Medico-Chir. Transactions.') If the inflammation in such cases were propagated along the vessel only, it would be difficult to account for such an apparently capricious selection of the time for its development. The difficulty disappears, however, when the period is observed to be so strictly in accordance with the time at which the same symptoms occur after other local complaints; and to be, moreover, the time at which the coagula formed in the veins may naturally be expected to shrink." (p. 23.)

Hence the term "phlebitis," used by Dr. Lee, very incorrectly expresses this disease; because the symptoms are not the result of inflammation transmitted along the coats of the vessel, but are the effect of a poison conveyed by the blood, which the veins are endeavouring to obstruct in the manner we have described:

"Many of the substances introduced artificially into the circulation by M. Gaspard, produced no action upon the coats of the veins through which they passed; and yet the general symptoms were precisely similar to those originating from genuine phlebitis. In accordance with this, it may be observed, that the uterine veins are often found perfectly healthy when the spermatic or renal, or still more distant veins, are thoroughly disorganised. In either case, the healthy condition of the veins near the original lesion *forbids the idea of inflammation having been propagated along the coats of the vessels*, while all analogy appears in favour of the disease being transmitted through their contents." (p. 21.)

Thus far Mr. Lee has pointed out to us the consequences that followed the introduction of what is called healthy pus into the cavity of a healthy vein, and the causes that interfere with the reparative process that is set up. We have, however, still to consider another view of this question. The causes just stated that prevent union by the first intention, are merely mechanical; but Mr. Lee directs our attention to other and more important causes, that render the blood incapable of accomplishing its purposes. From the experiment first quoted, it will be seen that the admixture of water with blood retarded its power of coagulation. Sulphuric acid did so still more; and hence Mr. Lee proceeds to inquire into the effects of "the introduction of vitiated fluids into the blood, its consequences and treatment;" and examines in what way they interfere with coagulation:

"The conditions under which pus will determine the coagulation of the blood, and those under which it will circulate in the living vessels, require to be accurately ascertained, before we can rightly interpret the discordant evidence which we have upon this point." (p. 43.)

The first discordant evidence is that between Sedillot and Dance. The former always found pus mixed in the blood after death, and therefore infers its circulation; the latter never could. But this paradox is readily explained by Mr. Lee, and shows the great importance of attention to the most minute circumstance, in forming any conclusion from a fact or an experiment. The difference between them arose from the fact, that one observed healthy, the other unhealthy blood. Dance performed his experiments on healthy animals; and when pus is mingled with healthy blood, its coagulating force so compresses the pus globules, that the most experienced eye can no longer recognise them:

"Pus mixed with healthy recently drawn blood, out of the body, will entirely lose its characters in this way; and as the coagulation is by no means retarded in the living vessels, we may, without fear of contradiction, affirm, that globules of pus cannot be detected when introduced into the vessels in small quantities and mixed with healthy blood." (p. 44.)

Sedillot, on the contrary, observed the blood of patients who died of the disease caused by the circulation of pus, in which the blood had, more or less completely, lost its power of coagulation:

"In experiments upon animals, it has always been found, that the power of the constitution, in resisting the effects of the injection of pus into the veins, was much greater at the first than at any subsequent operation..........From the consideration of these facts, and of the experiments previously recorded, it becomes evident that the introduction of pus into the system through an injured or inflamed vein, can rarely be the first step toward purulent infection of the system. Some change must previously have passed in the blood, by which its coagulating power is impaired, or some unusual mechanical means must have been employed, before pus can find its way in the course of the circulation." (p. 45.)

Independently, therefore, of the relation of the veins to the surrounding tissues, the mechanical causes that interfere with the protective efforts of Nature, there are also vital causes existing in the blood itself: its properties may be so altered that it can no longer resist the poison, and pus will circulate. But there yet remains another question which Mr. Lee discusses with his usual clearness. Is the pus found in the vessels distant from the centre of infection, a merely mechanical deposit? Such seemed to have been the idea of Sedillot, which Mr. Lee disputes:

"This hypothesis would not only appear to be at variance with the oft-repeated experiments of MM. Gaspard and Cruveilhier, in which similar effects were produced by the injection of mercury and of putrid fluids; but would also leave unexplained the mode of the introduction of these globules, where there is evidence that the disease has been communicated through the lymphatic system. The changes which all substances undergo in their passage through the absorbent glands, would at once forbid the idea that globules of pus could be thus introduced unchanged into the circulation; and yet we have direct evidence that irritating fluids are conveyed in this way into the system, and lead to the formation of secondary abscesses." (p. 46.)

So also there are cases of secondary abscess, in which no evidence can be obtained of the original lesion having suppurated; and,—

"In some cases, the constitutional symptoms which accompany or are followed by effusions into distant parts of the body, begin before sufficient time has elapsed to allow the supposition that pus can have been fully formed at the original seat of the injury. Such instances occasionally, although rarely, present themselves in extensive burns and scalds occurring in enfeebled habits, and after amputation of the limbs in scrofulous children." (p. 47.)

It follows, then, that pus, which is met with in parts distant from the original injury, is not deposited there; but is the result of a local inflammation produced by the irritation of the poison when retarded in its progress. This cause of irritation may be conveyed directly by blood that has lost its power of coagulating, or indirectly through the absorbents, when they also are wanting in the healthy adhesion which otherwise would circumscribe it. In either case the poison is conveyed, and produces all its fatal consequences.

The symptoms that indicate direct infection of the blood have been well described by Mr. Lee; but as they are already well known under various names, "Constitutional Irritation," "Diffuse Inflammation," "Phlebitis," &c. &c., we shall proceed to consider the *post mortem* appearances. Mr. Lee truly remarks, that "the *post-mortem* appearances observed in those who die in consequence of the introduction of vitiated fluids into the blood, cannot, for the most part, be distinguished from similar changes produced by other causes." We believe this to be especially true with regard to inflammation. The morbid changes which have their cause in infection of the blood, are so similar to those produced by ordinary inflammation, that no distinction has been made between them; hence the terms phlebitis, peritonitis, pleuritis, &c., have been applied indifferently to the *post-mortem* appearances of vitiated blood and true inflammation; and have, consequently, caused no small confusion in our notions of inflammation itself:

"The most characteristic circumstance," observes Mr. Lee, "attending the extension of disease to different organs of the body, through the medium of the blood, is, that several parts of these organs, or even different organs, will be simultaneously attacked. The disease will appear at once in various spots, which will become rapidly disorganised, while the surrounding textures will remain unaltered, either in structure or colour." (p. 51.)

Wherever poisoned blood is retarded in its progress, there the poison shows its effects, and produces corresponding morbid changes; hence the capillaries are the seat of pathological appearances, which vary according to the condition of the blood at the time. If it still retain its coagulating force, however impaired, some one or more of the capillary veins become obstructed and then dilated; the first step is coagulation; the next, local inflammation; the poison irritates, and pus is formed at the seat of congestion:

"The lungs are the organs in which the successive changes may be best observed. When puriform fluid has entered the circulation, the first appearance produced in the structure of the lungs is that of one or more congested or dilated veins of very small diameter. This will be followed by a well-defined spot, of much darker colour than the surrounding texture. Several of these spots will probably appear at the same time, and each one of them will soon become surrounded by a hard spherical patch of purple congestion. Effusion of lymph will now take place, *commencing in the centre* of each affected portion, and gradually extending towards its circumference. If the disease continue, each spot will suppurate, and the different parts will become softened and broken down, in the same order in which they were previously solidified." (p. 52.)

The same changes may be observed in the liver, the spleen, the brain, and the different vital organs after death; but during life they may be noticed in the skin:

"The skin is liable to be affected in three different forms. The first of these occurs very rarely, and consists in small deposits of matter in the structure or upon the surface of the skin, resembling in many respects the pustules of smallpox. The second form is also of rare occurrence, and consists of small congested spots on the surface of the skin. These are generally of a dark purple hue.......The third form presents itself much more frequently than either of the others.. ... It commences very suddenly, and frequently without any particular attention being directed to the part. A large circular patch of congestion, livid or purple in the centre, but becoming of a lighter colour towards the circumference, will form usually upon some part of the lower extremities. The skin of the calf of the leg is perhaps more frequently attacked than that of any other part. In the centre of the congested portion, mortification very rapidly takes place, and is indicated by the part assuming a black or dull leaden colour. In some cases, it would be difficult to say where mortification ceases, and the congestion begins; but in other instances, there is a distinct line of demarcation formed: a zone of bright red congestion will then occasionally surround the mortified part." (p. 54.)

Thus, then, we find that, in the capillary circulation, where the blood moves slowly in very minute columns, diseased blood shows its effects most prominently; and where the blood is most abundant, there these changes are most manifest; hence it happens, contrary to the ordinary laws of inflammation, that "the most vascular parts are those which soonest lose their vitality." The surface of the skin has perished, while the deeper layers recovered. The whole thickness of the skin has been destroyed, without any corresponding affection of the cellular membrane beneath it.

We could have wished that Mr. Lee had directed his attention more especially to the distinction between such effects as these, and the results of inflammation. It would have been desirable to have marked the boundary more precisely between the one and the other; because they are evidently not the same, although the term inflammation has been indifferently applied to both. It would appear, that when poisoned blood is retarded in its progress, as in the capillaries, a local inflammation and suppuration may take place; but if it should not, a dissolution of the tissues supplied by that blood follows without any inflammation: the term "gangrene," or mortification, therefore, very incorrectly expresses the softening thus produced. So, also, further morbid changes in unhealthy blood produce appearances similar to, but not the result of inflammation. This is especially to be observed in the serous membranes. Mr. Hunter noticed, in healthy blood, a disposition to separate into its constituent parts when at rest in the vessels, as in a vein just before depletion.

"This disposition of the blood to separate into its constituent parts, is evinced in a very marked degree in one class of secondary affections. Extensive effusions of serum, lymph, and pus, mixed in different proportions, will take place in the serous cavities of the body, and become infiltrated in the cellular membrane, *accompanied with very slight indications of inflammatory action.* The colouring matter of the blood will also sometimes become effused with its other parts; but when this is the case, the blood will be found to have lost its coagulating power,—in this respect presenting a direct contrast to the effusion from a healthy wounded vessel. The lymph deposited will be found lying in unorganised flakes, wanting its usual adhesive properties, and very slightly attached to parts, presenting little or no increased vascularity. The rapid manner in which these depositions take place, shows that they are separated from the blood without undergoing any very elaborate process. In this condition of the system, any organ upon which the disease falls may rapidly become disorganised, or may readily mortify, and, after death, a tendency to rapid decomposition will be manifested. The veins on the surface of the body may frequently be traced as dark blue lines, as though the skin covering them were stained by the colouring matter of the blood. The lungs and other organs may, under these circumstances, be found in every grade of disorganisation, till they present all the characteristics of gangrene; and even the peculiar fetor which accompanies mortification of the lung, will, in some instances, be present. A tendency to the formation of petechial spots may also be observed in different parts; and even organs which do not appear to have been the peculiar seat of the disease, will be found to have lost their consistency and to break down upon comparatively slight pressure." (p. 61.)

When healthy blood is effused in a serous cavity, we have already seen (p. 7) "that lymph may be derived from the blood directly, and deposited, in the form of a membrane, without being secreted by any vessel," and that the mode of formation of such a layer is altogether different from inflammation. So, also, when unhealthy blood, already separating into its constituent parts, is effused, "the lymph deposited will be found lying in unorganised flakes, wanting its usual adhesive properties, and very slightly attached to parts, presenting little or no increased vascularity." We are disposed to think the formation of these flakes to be also altogether different from inflammation; and therefore regret that Mr. Lee adheres to the term "secondary inflammation," in describing such appearances:

"The serous membranes," he says, "are peculiarly liable to be attacked by secondary inflammation; and, when affected, suppurate with the greatest readiness. They generally exhibit but a slight degree of vascularity, and sometimes scarcely appear more injected than in their natural condition. In the peritoneal cavity, large quantities of *unorganised* lymph are frequently poured out, mixed with turbid serum or pus." (p. 56.)

"A slight degree of vascularity," "unorganized lymph," and secretion of pus are not characteristic of inflammation in serous surfaces. We are inclined to think, that the so-called secondary inflammation is not inflammation at all; and that these morbid appearances only indicate poisoned blood, separated into its elements and effused in these cavities. We are not surprised, however, that Mr. Lee retains the term "inflammation." It requires some boldness to burst the bonds of popular opinion; and this term, called, it is true, unhealthy inflammation, has been so generally applied to such appearances, that it might render his language unintelligible, if he were to adopt a different expression.

Mr. Lee's concluding section is devoted to the Treatment of secondary inflammation. If a vein be wounded, or a coagulum is in the process of obstructing some vitiated fluids, he urges strongly the importance of perfect rest:

"When the powers of the constitution are enfeebled, even the natural motions of a part may interfere with recovery, and rest sometimes become an important object of treatment. How necessary this is after childbirth, when the divided veins are being closed, every one who has attended such cases practically knows." (p. 62.)

In the constitutional treatment, he dwells, with Cruveilhier, on the importance of promptitude. The treatment ought to be concentrated on the first period of the disease, because, if coagulation do not take place, and pus becomes mixed with the blood, medicine is generally of no avail. He objects, with much justice, to the antiphlogistic treatment adopted here and in France. Depletion and mercury are both calculated to interfere with the reparative process that is going on, and are therefore objectionable. On the contrary, those remedies that will support the constitution, allay irritability, and promote coagulation, as wine, bark, opium, and the diffusible stimulants, are the most serviceable. This practical fact is an additional reason for wishing that the terms "secondary inflammation," "phlebitis," &c., were expunged, and some other employed, that would prevent our confounding these toxæmic diseases with inflammations.

Mr. Lee's essay concludes with a collection of forty well-reported cases, to illustrate the doctrines he has laid down. We sincerely congratulate Mr. Lee on his successful exertions, which are well worthy the honour they have obtained; and trust that he may be encouraged to pursue his inquiries still further, and that we may look upon this Prize Essay as only the foundation of a more elaborate work on a class of diseases, the most difficult to treat, and the most fatal, that fall under the notice of the physician.

Art. VIII.

1. *Practical Observations on the Treatment of Stricture of the Urethra, and Fistula in Perineo, illustrated with Cases and Drawings of these Affections: with an Appendix, containing various Letters, Papers, &c., by Prof. Syme, Dr. Mullar, and the Author, connected with the Subject of the Operation of the Perineal Section.* By John Lizars, late Professor of Surgery to the Royal College of Surgeons, and Senior Operating Surgeon to the Royal Infirmary of Edinburgh.—*Edinburgh*, 1851. 8vo, pp. 91. With Eight Plates.
2. *Remarks on the Operation of the Perineal Section, for the Cure of Stricture of the Urethra and Fistula in Perineo.* By F. G. William Mullar, M.D. L.R.C.S.E.—*Edinburgh*, 1850. 8vo, pp. 16.
3. *A Few Words on Perineal Section, as recommended by Professor Syme for the Cure of Stricture of the Urethra; and on Mr. Wade's recent Remarks thereon.* By F. B. Courtenay, M.R.C.S.E.—*London*, 1850. 8vo, pp. 24.
4. *Edinburgh Monthly Journal of Medical Science*, for April and May, 1851.

A recent number of the 'Quarterly Review' contained an elaborate article entitled the "Mysteries of Ceylon." With equal propriety, the present article might be called "the Mysteries of the Perineal Section;" and we will take upon ourselves to affirm that, setting aside the relative importance of the two subjects, and the position of those concerned in the dispute, the one is not a whit behind the other, in its interest, in the variety of its details, or in the perverse ingenuity which has rendered every step of the inquiry painful and laborious, as well as discreditable to most of those who have taken part in the affair. No person who is a stranger to the task of rigidly investigating what are commonly called *facts*, can form any idea of the difficulty of extracting the naked unadorned truth, from amongst the conflicting statements of a variety of individuals, all relating the same circumstances, and all claiming credit for veracity and disinterestedness. No man can have passed through any considerable portion of life without having had occasion, first to wonder at, and then to deplore, the recklessness of assertion, and surprising obliquity of vision, of persons who bear good characters for honesty, which they would feel very much aggrieved to have called in question, and who are in the main actuated by upright and honorable motives—the *eidolon* of which they never lose sight of in their own minds—but who are utterly incapable of giving a straight-forward account of the most trivial circumstance in which their feelings are strongly interested. Much of this proceeds from lax habits of thought, much from loose modes of speaking and writing, and much also from what, for want of a better expression, we suppose we must call a *natural incapacity for recognising truth;* which, however, is almost always combined with a peculiar tendency that leads the individual, without any actual design on his own part, to colour every statement with preconceived opinions, tinted and polished up to suit the purpose of the hour.

This at least, is the most charitable and least insulting supposition; for upon any other, we should find it hard to account for many a misstatement that is certain to be exposed, and quite as certain to cover its author with shame and ridicule.

The task of unravelling such a web, at all times distasteful, becomes doubly so, when it has been complicated with personal invective, vulgar personalities, and ungentlemanly imputations. Such is the labour we have now to perform; such is the miry path we have to tread in search of a little science and a little honesty; such are the pleasant prospects we have to hold out to the patient reader. We shall endeavour to make clear the precise points in dispute, to indicate the origin and progress of the controversy, and to sift the evidence upon either side; in the hope of assisting the profession to form just conclusions as to the merits of the question, and to understand, if it cannot reconcile, the astounding statements of the various disputants. And with this object in view, it may perhaps be well to indicate, first of all, the sources from which we derive this evidence, and the peculiar qualifications of those who contribute it. Besides the little work on 'Stricture of the Urethra,' by Professor Syme of Edinburgh, we have also a recent publication by Mr. Lizars, formerly Professor of Surgery to the Royal College of Surgeons of Edinburgh, and a considerable number of pamphlets, lectures, and communications in the various medical periodicals of England and Scotland. Among the more important of these, are a pamphlet by Dr. Mullar of Edinburgh, who is endorsed by Professor Lizars as a "young surgeon of great promise;" another by Mr. Wade, whose novel and ingenious method of transforming an old edition of a book into a new one, has probably not yet perished from the recollection of our readers (*Vide* this Review for April, 1850); and essays by Mr. Henry Smith, who is well known as the voluminous reporter of the doings of Mr. Fergusson at King's College Hospital and elsewhere,—by Professor Miller of Edinburgh,—by Mr. Gay, who is mentioned by Mr. Lizars as "the talented surgeon to the Royal Free Hospital, London,"—by Mr. Courtenay, of advertising notoriety,—by Dr. Mackenzie, the Junior Surgeon to the Royal Infirmary of Edinburgh,—by Dr. Dunsmure, also Surgeon to that Institution,—and by a number of others, who need not be more particularly mentioned here.

Our readers will now be in a position to follow us through the mazes of this controversy; and we can only express the hope, that their patience will hold out to the termination of it. In the year 1844, Mr. Syme published in the pages of the 'London and Edinburgh Monthly Journal of Medical Science,' the narrative of a case of obstinate stricture of the urethra, in which he divided the stricture by an incision through the perineum, *upon a staff previously introduced into the bladder*. This case was so interesting, that, upon its reappearance five years afterwards in Mr. Syme's work on stricture, we transferred it entire to the columns of this Review (Brit. and For. Med. Review, vol. V, p. 324, et seq.); and shall, therefore, now only very briefly refer to it, and this solely because it forms, as it were, the key-stone upon which all the others depend. The stricture in question occurred in a gentleman between 40 and 50 years of age, and had existed for 20 years. It had repeatedly been *apparently* cured, but as often contracted again in a few days or even hours; a form of the disease, which, though happily not common, is often enough met with to be readily recognised under the name Mr. Syme proposes for it, *resilient stricture.* In this instance, not only the simple bougie, but the retention of a catheter in the bladder for ten days, and division of the stricture by concealed lancets on more than one occasion, had all failed in the permanent cure of the disease, so that it was almost in despair that the patient submitted to have the stricture divided by external incision. With this intention Mr. Syme introduced a grooved staff into the bladder; and feeling for the stricture through an incision in the perineum, "ran the knife fairly through the whole extent of thickened texture." A catheter was then substituted for the staff, and retained for a few days, after which the patient "never required the bougie;" and on the closure of the wound in the perineum, was entirely relieved from all uneasiness. Several years have since elapsed, and Mr. Syme still reports the cure as effectual and permanent; so that, in this instance at least, it may be taken to be definitely proved, that the operation has been quite successful. When reporting this case, Mr. Syme also mentions two others, in which he had performed a similar operation; in one case unsuccessfully, in consequence, he thinks, of the incision not having been carried completely through the indurated textures surrounding the contraction; and in the other with a better result, for the patient was dismissed ten days after the operation, with instructions to return occasionally. So short a period having elapsed between the operation and the dismissal of the patient, it would have been satisfactory if Mr. Syme had reported the subsequent state of the case, when he had watched it for a longer time; but this he has nowhere done, nor does he make any allusion to it in any of his later publications.

From this time until the appearance of Mr. Syme's 'Contributions to the Pathology and Practice of Surgery,' in 1847, we hear no more of the subject; nor was it indeed until last year, when his treatise 'On Stricture of the Urethra and Fistula in Perineo' was given to the world, that the question excited any degree of interest. At length, however, public attention was aroused, and has been in no danger of flagging since. Now we have been particularly desirous to draw the reader's attention to this first case of Mr. Syme's, in consequence not only of the scientific interest attaching to it, but also because it has been made the subject of doubt and invective by those who have constituted themselves the opponents of the operation; and if it can be shown, either that Mr. Syme has mis-stated the facts, or that there is misrepresentation on the other side, it will, by determining the degree of credibility of the witnesses, materially assist us in coming to a sound conclusion on the whole question.

Mr. Syme, in 1849, writing several years after the operation, says, the patient "has never required the bougie, and in every respect enjoys the most perfect health." (Syme 'on Stricture of Urethra,' p. 17.)

Mr. Lizars, referring to this case in 1851, says, "I understand, however, that the operation in this case has ultimately proved unsuccessful." (Lizars 'on Stricture of Urethra,' p. 17.)

Mr. Gay also writes to the 'Lancet,' in February 8th, 1851:

"I am very much mistaken if this gentleman is not at the present moment, or was not, a short time since, almost, if not quite, as bad as before the operation. Well might Mr. Syme alter his tone; and the somewhat dogmatical assertion, that the perineal section 'is the complete remedy of the disease in its most inveterate and obstinate forms,' relapse into the modest appearance of the note before me, 'that the relief thus afforded is *more permanent* (the italics are mine) than that which can be obtained in any other way."

These passages from the writing of Messrs. Lizars and Gay are unmistakeable; and, *were they but true*, would of course tell greatly to the discredit of the individual whom they are intended to injure. Another number of the 'Lancet,' (that of the 15th February,) however, disposes of the matter by the publication of a letter from Mr. Syme, which effectually settles the question of the permanency of the cure; and that in a manner which anything but redounds to the credit of his accusers, whose reputation is thereby established, as culpably disregarding the most ordinary care in making their statements. Here is the letter to speak for itself:

"To the Editor of the 'Lancet.' Sir,—In the 'Lancet' of Saturday last, it is alleged, that a patient, whose case was related in my treatise on 'Stricture of the Urethra,' instead of being rescued from extreme suffering, and restored to perfect health, as I have stated, is, in truth, 'almost, if not quite, as bad as before the operation.' I am at a loss to imagine what circumstance in my history as a teacher or writer should have led you, without inquiry, to publish such a charge. It is utterly and entirely false. The gentleman referred to is now in Edinburgh, where he has resided ever since the operation. He suffers no trouble or inconvenience from stricture, and enjoys the most perfect health."………

It is impossible to withstand such a statement as this; the effect of which is, of course, to make us look upon all future statements of Messrs. Lizars and Gay with great suspicion.

The second case in Mr. Syme's work has been the subject of quite as much dispute, and is involved in quite as much mystery, as the one we have just disposed of.

The history of it, briefly given, is to this effect:

In February, 1849, Mr. Hamilton Bell took to Mr. Syme an Indian officer, who had been obliged, some two or three years before, to return home, on account of ague, and severe stricture of the urethra. Eighteen months of careful treatment having failed in curing the stricture, the patient was reduced to a pitiable state of distress in body and mind; so much so, that he had determined to abandon his profession, the active duties of which he could no longer fulfil. Mr. Syme found the urethra so irritable, that the slightest touch of the bougie produced violent convulsive movements; and it became necessary to administer chloroform, before the state of the canal could be examined. Under these unfavorable circumstances, Mr. Syme divided the stricture, which was situated at the bulb, upon a small grooved director, and inserted a catheter, which was retained for forty-eight hours. In ten days, the urine ceased to come through the wound; and, in another week, the patient went into the country, where he rapidly recovered his former health and strength. The final report of this gentleman's condition was, that a full sized bougie could be passed without the slightest uneasiness, and that he was preparing to rejoin his regiment in India.

Now, to this case, and to many others published by Mr. Syme as cures, it was objected that so short a time (eight months only) had elapsed since the operation, that it was impossible to say whether the cure would prove permanent; and, lately, it was roundly asserted, that this very patient had relapsed, and was in as bad a condition as ever. This statement seems to have originated with Dr. Mullar, to have been endorsed by Mr. Lizars, who has included the pamphlet of the former gentleman in his own work on stricture, and to have received further aid in its circulation from Mr. Gay. It purports to rest upon no less a foundation than letters from the patient himself. Here is the passage which Mr. Mullar quotes from a pamphlet by Mr. Courtenay, containing part of a letter from a patient:

"I am obliged to give a very bad opinion of myself; and I am almost afraid that I have not derived *any benefit* from the operation. On Thursday night last, I was seized with rigors and retention, and obliged to keep my bed on Friday, retaining only a small No. 2 catheter. I now make water very badly, although rather better this morning. I cannot pass more than No. 4, and indeed all instruments are *held*."

To this statement, or, as it turns out, mis-statement, Mr. Syme thus replies in another letter to the Editor of the 'Lancet,' dated March 11, 1851.

"The enclosed letter from Mr. Hamilton Bell relates to one of the cases which have been characterised as 'failure' by Mr. Gay:.........

"'13, Charlotte Square; March 11, 1851.

"'Dear Sir,—I am happy to be enabled to report to you most satisfactorily on the case of Capt. M——, No. 2, in your work. He was enabled, within a few months after the operation, to return to his duty in India, perfectly restored to health. He has now risen in his profession; and by the last accounts received, is 'in as good health as he ever enjoyed;' so you may congratulate yourself on saving my friend from what must have been a life of poverty and misery, if that life had been preserved, of which there seemed little hope in the beginning of 1849.

"'Most truly yours,

"'HAMILTON BELL.'"

"'Prof. Syme.'"

Now, what do our readers suppose is the solution of this extraordinary matter? Why, nothing more or less than that the passage quoted from Mr. Courtenay's pamphlet does not refer to this patient at all, but to No. 5 in Mr. Syme's book; and thus by Mr. Mullar's carelessness, and Messrs. Lizars and Gay's readiness to adopt, without inquiry, any assertion damaging to Mr. Syme, the question, already complicated enough, is still more obscured, and we are put to a good deal of additional trouble by their inattention.

CASE III of Mr. Syme has not, we believe, been the subject of any dispute. Dr. Mullar, however, inquires, as there were two strictures, and only one was divided, what became of the other "at the neck of the glans?" We think it may fairly be inferred from the narrative, that this stricture speedily got well, when the more serious one, lower down in the urethra, had been cured; and this also accords with what experience proves, namely, that stricture at the glans is commonly secondary only, supervening upon organic disease in the membranous part of the urethra.

CASES IV, VI, VII, VIII, IX, are, we conceive, entitled to credit, as evidence, at all events, of the safety and immediate benefit resulting from the operation, though of none of them have we reports after any considerable interval of time.

CASE V in Mr. Syme's book is of importance, because, unless there is here also some extraordinary blunder, it shows that the operation has failed in affording that permanent relief which was expected from it. We think it right to quote from Mr. Syme's work the original narrative:

"CASE V. The following statement, by a surgeon in London, who had charge of the patient to which it refers before he came under my care, will give a better idea of the severity and obstinacy that characterised the symptoms, than any description not founded upon observation.

"'London; April 16, 1849.—I first attended Mr. —— about seven years since. At that time he was labouring under an impermeable stricture, which had resisted, for a considerable period, all attempts to pass instruments beyond it, although made by several hospital surgeons of eminence. One surgeon had used considerable violence, which, besides producing great constitutional disturbance, accompanied by a painful swelling of the perineum, was the cause of an extensive chronic induration of that part. His general health was bad, and the slightest exposure to cold produced severe rigor. Under the treatment I adopted, the stricture was so far removed as to admit full-sized instruments, and the patient returned to the country able to pass them himself. However, after a time, increased difficulty in passing the instruments arose, and the patient gradually diminished their size. At length none but the smallest size could be passed, and the patient came up to me. I need not detail the treatment adopted during the six weeks or two months he remained in London, as no material or permanent benefit resulted from it. The summer before last, he was again very much troubled by continued and excessive soreness at the seat of stricture, spasms, rigors, and occasional attacks of retention of urine. The urine was unhealthy, and deposited a copious mucous sediment, and the general health bad. By my direction, he remained in bed (in the country), with a catheter kept constantly in, and gradually increased until the largest size could be passed with ease. Under this treatment the urine became healthy, and the general health improved. But almost within forty-eight hours after the withdrawal of the largest instrument, No. 2 could hardly be passed. Since then, until about two months ago, the patient contented himself with occasionally passing a catheter at intervals of a few days or weeks, according to circumstances, and retaining the instrument for twenty-four hours, when he withdrew it, and introduced a larger one, which he kept in for twenty-four hours more. About five months ago, catarrh of the bladder, irritative fever, soreness, and spasms, returned and became constant, with frequent rigors and retention of the urine. Two months ago, he again came under my care. By remaining in bed, with catheters retained, and medical treatment, the urine became healthy, and the more urgent symptoms, both local and general, were relieved. But the stricture still did not admit a larger instrument than when he came to town. Being fully assured that *no* treatment with urethral instruments would or could be of any service in this case, and having accidentally read the case reported in your work, entitled 'Pathology and Practice of Surgery,' I mentioned the operation to the patient, telling him of the success you had met with. At the same time, being unwilling to take the whole responsibility of recommending the performance of an operation of which I had no personal or practical knowledge, I proposed a consultation with Sir B. Brodie, who accordingly saw the patient with me, and agreed that some such operation afforded the only prospect of anything like permanent relief to him.

"'P.S.—I should add, that Mr. —— is not a strong man, but, on the contrary, weak, nervous, and irritable, dreadfully alarmed at the prospect of an operation, and sure to suffer rigors from any irritation.'

"This not very promising patient arrived in Edinburgh on the 6th of June, when, in addition to the particulars above stated, I learned that he had been an officer in the service of the East India Company, and retired from it in 1826; since which time he had suffered from the disease. On his way here he had been attacked by a fit of retention at Carlisle, and detained there until he obtained relief by passing an elastic catheter of the smallest size; but which was, nevertheless, so tightly grasped by the stricture, that serious apprehensions were entertained of its being broken in the withdrawal. Painful spasms and copious mucous deposit, consequent upon this attack, prevented me from making any examination for several days, and the patient had recourse to his usual expedient for obtaining relief by lying in bed with a small catheter retained. At length, the urine becoming clear and the spasms subsiding, I ascertained that a small grooved director could be passed through the stricture, which was at the bulb; and being unable to detect any other obstacle to recovery, I did not hesitate to undertake the operation.

"On the 13th, the patient being in a calm deep sleep, induced by the agency of chloroform, I divided the stricture, and introduced a moderate-sized silver catheter into the bladder, without any delay, so that the operation was completed in less than a minute. About half an hour afterwards the patient awoke, and found himself lying comfortably without pain or uneasiness. The catheter was removed at the end of forty-eight hours, when, to his delight and astonishment, the water flowed through the urethra in a full stream, the sound of which was said by him to be more pleasing than the finest music. None of the urine escaped by the wound, and no other inconvenience resulted from the operation. On the 13th of July the patient returned home, where his progress in the recovery of general health will appear from the following extract of a letter, dated the 1st of August:—'I cannot adduce stronger evidence than by stating, that a few days since, Dr. ——, the medical referee of a life assurance office, voluntarily remarked, that he should not have the slightest hesitation in recommending my life for assurance, although in May last he did not consider it worth a year's purchase." (pp. 25-29.)

We gather from subsequent information, that Mr. Courtenay was the surgeon who attended the patient in London, and sent the case to Mr. Syme; and that the late Mr. Morton was the individual who deprecated the operation, and recommended the patient to make his will previously to submitting to it.

Mr. Courtenay, whose pamphlet bears date the 27th of February, 1850, a year after the operation, states respecting it:

"The immediate result of the operation I need not fully state, as it is correctly given in Mr. Syme's recital; but I am bound to say, that the patient assured me that the relief was wonderful, that his sufferings were of the most trifling character, and that he returned home with higher hopes of having obtained more permanent relief than he had ever before experienced. However, before the publication of Professor Syme's work, symptoms appeared which gave every indication that all was not right, and the patient consequently wrote to Professor Syme for his advice. On one occasion the Professor, in reply, recommended the occasional use of bougies; and on another, he advised that the use of all instruments should be abandoned, as he felt that their introduction was only a source of irritation.

"The patient has, for the last two months, been rapidly relapsing into his previous condition; and I cannot illustrate his present state better than by giving an extract from a communication which I received from him this morning (February 19th); he writes:—'I am obliged to give a very bad account of myself; and I am almost afraid I have not derived *any benefit* from the operation. On Thursday night last, I was seized with rigors and retention, and obliged to keep my bed on Friday, retaining only a small No. 2 catheter. I now make water very badly, although rather better this morning. I cannot pass more than No. 4, and, indeed, all instruments are *held*. The state of the urine is, however, much improved,' (he had written shortly before this to say that the urine was in a most unhealthy state.) Such is the account of the patient's miserable condition at this moment. Of course, Professor Syme is not chargeable with withholding an account such as this. Nevertheless, the patient's letters to Professor Syme, previous to his case being published (if I am correctly informed), detailed the reappearance of symptoms of such a character, as would, I should have thought, have led the latter to have published a less glowing and more qualified account of the case than that which appears in his treatise; whilst it should also have suggested to his mind the propriety of not indulging in such exaggerated descriptions of the superiority of his operation, as a permanent means of cure, over those other methods of treatment, which he has so unsparingly denounced. Even supposing, as is most probable, that the Professor attached no importance to the premonitory symptoms of approaching re-contraction which were appearing, surely it would have been more fair, in detailing the case, to mention that the patient was again complaining, and having thus given *all* the facts of the case up to the last moment, he could have left his readers to draw their own conclusions." (pp. 21-22.)

Now, if it be true, as is here stated, that Mr. Syme had received a communication from the patient, "*previous to the publication of his work*," of the nature Mr. Courtenay describes, we can only say, that he has been guilty of unworthy suppression, such as we were unprepared to expect from him, or from any one else claiming the character of an honest man. But even this account is complicated with disingenuousness somewhere; and we are bound to state our belief, that this does not rest with the Edinburgh professor. In the 'Lancet' of the 1st of March, there is a communication from him, which, after suggesting that members of the profession should "see the propriety of ceasing to annoy his patients with inquiries as to their state of health, for the purpose of premature discussion or misrepresentation," goes on to say, "not many days ago I received from the subject of my fifth case (the one we are now considering) a letter, complaining of this improper conduct, containing a communication, in my opinion, not very creditable to an "Honorary Fellow" of the College of Surgeons; and stating, that although, owing to various circumstances, which need not be detailed at present, his recovery had not been so complete as we expected, his health is improved, and that of late he has not required to pass an instrument more frequently than once in five or six weeks."

Having thus expressed our belief in Mr. Syme's favour, we should be glad to learn from him what are the precise "circumstances" to which he alludes; and how the case, as it now stands, can be reconciled with the statement which he elsewhere made: "That division of a stricture by external incision is sufficient for the complete remedy of the disease in its most inveterate and obstinate form." (Syme on 'Stricture,' &c., p. 58.)

Our readers will, doubtless, have observed, that it is from Mr. Courtenay personally, or from his pamphlet, that Mr. Gay, and the other accurate gentlemen, derived the information which they so astutely applied to the wrong case. Mr. Gay, indeed, *after* the publication of Mr. Syme's reply, found out his error; but the mischief was already done, and the triumphant reply had produced its due effect.

Case X requires a passing remark, and it is this:—The subject of it was a young man, æt. 26, who had a congenital adhesion to the glans, together with so much contraction of the anterior part of the urethra, as almost to amount to obliteration. An attempt had been made to restore the canal, by thrusting a narrow sharp-pointed knife along its course for one or two inches, but beyond this no instrument whatever could be passed. After some time spent in introducing "a succession of small wires gradually increased in size," Mr. Syme succeeded in passing an exceedingly small grooved director through the stricture, and divided it by external incision. The result of this is thus reported: "From the day on which the stricture was divided, he never made the slightest objection to the passing of instruments, and can now introduce them without difficulty or inconvenience." (Syme 'on Stricture,' p. 38.)

The reader will be good enough to remember, that this was a *congenital* stricture, or *rather a deficiency in the canal of the urethra;* and that it was complicated with the utmost irritability on the part of the patient who suffered the extreme of distress and despondency, from the circumstance that the urine, flowing only in drops, required him to occupy so much time, "that he felt ashamed to seek relief, except when secluded from observation or remark."

Now, we will ask any one who reads the original history of the case, what must be the *animus* of the individual who thus tabulates it?

No. of Case.	Name and Age.	Time Stricture has existed.	Operation when performed.	When dismissed as cured.	Present state of health.	REMARKS.
10	Gentleman, ætat. 26	Not stated	Not stated	Not stated	Not stated	In this case Mr. Syme tells us that the patient can introduce a bougie himself. Where is the cure of the patient, if he is obliged to introduce a bougie himself?

We regard Dr. Mullar as here guilty, in his zeal against the operation, of a wilful suppression of the facts. Though the exact days and weeks of the operation are not mentioned, yet the narrative is a continuous one; and all the points stated by Dr. Mullar to be wanting, are clearly to be inferred by any unprejudiced person. But beyond this, Dr. Mullar, after informing us that he is ignorant whether the operation has been successful or not, which is entirely a voluntary ignorance, very plainly demonstrates, by the note which he appends to the case, that he does, in truth, comprehend what Mr. Syme intended to convey, notwithstanding his previous disclaimer.

We must now, for a few moments, dwell upon Case XI of Mr. Syme. A. S—, æt. 28, came under his care on the 29th July, 1849, with stricture of long duration, which had been especially troublesome for the previous five years. It was situated at the bulb, and was complicated by a false passage. On the 7th August, a bougie was passed through the stricture; and on the 11th, it was divided on a director. The history concludes in these words:—"The urine escaped partially by the wound for a few days; and the patient, who had been quite well for a fortnight, was dismissed on the 2d September, without any trace of the disease or its remedy." (Syme 'on Stricture,' p. 40.)

Relative to this case, there appeared on the 26th October, 1850, in the 'Medical Times,' the following account:

"Case IV.—18th June, 1850. D. S—, æt. 29, has all the symptoms of stricture of the urethra; and states, that he underwent the operation of perineal section in August, 1850, and that he lost a considerable quantity of blood, having bled for nearly forty-eight hours after the operation. States, that he has lost the power of propelling his urine, the stream falling perpendicularly to the ground. The cicatrix of the perineum is between three and four inches long. The patient being laid on a sofa, Professor Lizars inserted No. 6 catheter with some difficulty." (Lizars 'on Stricture,' Appendix, p. 48.)

Dr. Mullar has also reiterated this statement, in the following words:

"The second case is that of Archibald D. Sutherland, æt. 28, bookbinder, (*vide* Case XI, on Stricture of Urethra;) he was operated upon by Professor Syme on the 11th August, 1849, and was discharged as cured, after remaining in hospital six weeks; and states, that he bled so profusely for forty-eight hours after the operation, that the mattress upon which he lay was completely saturated, and that it was at last checked by ligatures, which Mr. Keith, then the Professor's resident clerk, put on. He was delirious on the afternoon of the day of operation. Since June, 1850, he requires the regular introduction of catheters, and No. 6 is with difficulty introduced; he has lost the power of ejecting his urine in a stream—it falls perpendicularly to the ground." (Lizars 'on Stricture,' Appendix, p. 62.)

Mr. Syme has not replied to these statements; but as the dependence to be placed upon them is based upon their scrupulous accuracy, we must request our readers to notice two points: 1. That while Messrs. Lizars and Mullar declare that the operation was followed by alarming hæmorrhage, Mr. Syme makes no allusion to it; but, on the contrary, expressly says, immediately afterwards,—"Of all the cases in which I have divided the stricture, only one has been followed by any unpleasant result, and that was erysipelas." (Syme 'on Stricture,' p. 40.) 2. That while Mr. Syme says, in his account of the operation, that the perineal incision should be an inch or an inch and a half in length, (Syme, p. 42,) Lizars affirms that the *cicatrix* in this patient is between three and four inches long.

Lastly, we remark also in Dr. Mullar's statement another of those pieces of disingenuousness, which, without being absolutely falsities, do yet convey an erroneous notion. He observes, that the patient "was operated upon by Professor Syme on the 11th of August, 1849, and was discharged as cured, after remaining in hospital six weeks," which, of course, any person would suppose, particularly when taken in connection with the "profuse bleeding," was the time that elapsed from the operation to his leaving the institution; whereas the patient was discharged on the 2d of September, having been, Mr. Syme says, "quite well for a fortnight;" so that, in point of fact, the man *was well of the operation* in eight days, notwithstanding the hæmorrhage "for forty-eight hours."

We still think, however, that this case, as well as several others, requires explanation, which no one but Mr. Syme can afford; but it is only just to him, that our readers should withhold their judgment, until they hear the history of another accusation or two of a similar nature, from the same quarters, to which Mr. Syme *has* replied.

Perhaps the history we are now going to relate is more extraordinary than any of the others. On one side or the other, there *must* be downright falsehood; for the circumstances are utterly irreconcilable.

On the 13th November, 1848, E. M—, æt. 41, a plasterer, was admitted into the Edinburgh Infirmary, with stricture, five inches from the orifice of the urethra, together with great induration of the perineum and scrotum, and two fistulous openings, through which the chief part of his urine escaped. The patient was in the condition of despondency and irritability, that might be expected to result from his distressing situation. Nineteen years before, he had fallen across a beam of wood, and bruised his perineum; since which time he had perpetually suffered from attacks of retention, and, on several occasions, from swelling of the perineum, and eventually from abscess. As he did not seem to derive benefit from the bougie, on the 20th of the same month Professor Syme divided the perineum on a grooved director passed through the contracted part, and introduced a full-sized catheter into the bladder.

The remaining history of the case is given in these words:

"The catheter was withdrawn at the end of forty-eight hours; after which the patient did not make a drop of water through the wound, and was at once completely relieved from his previous sufferings. He quickly regained his sleep, appetite, and strength, and was dismissed cured on the 2d of December." (Syme 'on Stricture,' pp. 71-2.)

We shall, as far as possible, bring Professor Lizars' account of the same case into a reasonable compass; but we find ourselves unable to shorten it much. We learn that Mr. Syme's statement is correct as to the early history of the case; and that the man was originally under the care of Mr. Lizars, who passed instruments for him, until he could insert No. 5 catheter, when the patient considered himself cured, and left off his attendance. The remaining part of the case must be given in Mr. Lizars' own words:

"In 1848, finding the stricture as bad as ever, the patient consulted *a pure perineal sectionist,* who succeeded in inserting No. 3 catheter, but after several trials to insert a larger catheter, he could never succeed; he therefore resolved upon performing the operation of perineal section. The patient was placed on the table, as in lithotomy, a staff inserted, and an incision made from the most depending portion of the scrotum, to the verge of the anus, through the skin, cellular tissue, and urethra. The staff was now withdrawn, and No. 7 catheter inserted. A great quantity of blood was lost during the operation, which lasted upwards of thirteen minutes. Immediately after being placed in bed, hæmorrhage recurred to a fearful extent, and was with difficulty suppressed. In about an hour after, hæmorrhage broke out again, and was stopped. Towards night, bleeding again commenced, and was checked with great difficulty. The patient states, that by these repeated losses of blood he was much weakened.

"Fourteen days after the operation, No. 9 catheter was inserted. In three or four months after, No. 8 only could be inserted. Soon after the operation, he was attacked with palpitation of the heart, which incapacitated him following his occupation, and earning his livelihood.

"He also states, that the stricture is as bad as ever; and on taking off his trowsers to show the cicatrix in the perineum, he was seized with palpitation of the heart, and compelled to lie down on a sofa for a few minutes.

"In March, 1850, he consulted a physician, who gives the following account:—"E. M— is attacked with palpitation of the heart on any exertion, being seized with violent pain in the region of the heart, shooting to the left shoulder, and down the left arm to the fingers, all of which he ascribes to the hæmorrhage consequent on the operation. He has had no rheumatism, no inflammation of chest, no difficulty of breathing or cough, and none of his relations have had any heart disease. He has not been able to work since the operation. A distinct endocardial murmur is heard, obscuring both the cardial sounds. The carotids beat strongly and visibly, and there are no venous pulsations. The pulse is eighty-four and jarring.

"He died in the beginning of August, 1850." (Lizars 'on Stricture,' Appendix, pp. 56-7.)

Mr. Syme addresses a reply, in the 'Lancet' of December 14, 1850, to the three most prominent accusations of Mr. Lizars: 1, "that there was repeated frightful hæmorrhage;" 2, "that symptoms of diseased heart first appeared after the operation;" and 3, "that the stricture was as bad as ever subsequently to it." And in support of his statements, he adduces the following testimony from those who had the best opportunities of learning the real facts of the case:

"Edinburgh Royal Infirmary; Dec. 2, 1850.

"I was nurse in the surgical clinical wards when Edward Monro was operated upon by Mr. Syme, in November, 1849. I distinctly recollect his case; and having almost constant occasion to be in the ward in which he lay, must have known if any bleeding had occurred; but so far as I know, there was none.

"(Signed) JANNET PORTER,
"Principal Nurse of the Surgical Clinical Wards."

"Edinburgh; Dec. 2, 1850.

"Edward Monro applied to me, as medical officer of the Dispensary of the City Parochial Board, on the 12th of June, 1848, labouring under disease of the heart, bronchitis, and disease of the urinary organs. He was sent into the Royal Infirmary in the following November, and placed under the care of Mr. Syme, who performed an operation for the remedy of an obstinate stricture.

"On the 15th of May, 1850, I had opportunity of seeing Monro again, as an applicant for parochial relief, on account of general debility with his old complaints, excepting the stricture, to ascertain the state of which, I passed No. 11, without any difficulty, into the bladder.

"JOHN YOUNG MYRTLE, M.D., F.R.C.P.E. &c."

"Edinburgh Royal Infirmary; Dec. 2, 1840.

"I was dresser in the surgical clinical wards when Mr. Syme operated upon Edward Monro, in November, 1849, and am not aware that there was any bleeding after the operation.

"I was also a clerk in the medical clinical wards last summer, when Monro was a patient in these wards, labouring under disease of the heart. He was then perfectly free from any urinary complaint, and frequently expressed his gratitude for the complete relief he had derived from Mr. Syme's operation.

"CHARLES MURCHISON,
"Resident Clerk, Clinical Surgical Wards."

"31, Abercromby Place; Dec. 2, 1850.

"MY DEAR SIR,—In reply to your inquiries as to my recollection of the patient, Edward Monro, on whom you operated for stricture of the urethra by incision, I regret I cannot give you a correct statement as regards dates.

"Monro, before his admission into the hospital, had been under my treatment for stricture, with an indurated swelling in the perineum. I had more than once dilated his urethra to the full size by the use of bougies; but the stricture had always returned on discontinuing the treatment. As I found he had been admitted into the hospital, under your care, in order to have the stricture divided by incision, I watched the progress of his case with interest, as I believed it to be a good case to test the value of the operation.

"I am not certain whether or not I was present at the operation; but I believe I saw the patient on the same day, after the operation, and I certainly visited him daily for some time afterwards.

"I cannot recollect that there was any hæmorrhage after the operation. Indeed, I am satisfied, that if there was any, it must have been very trifling, for I saw the patient, and looked at the wound every day, and the absence of either local or constitutional disturbance struck me forcibly in his case.

"Some time afterwards (as far as I can recollect, three or four months after the operation), I saw Monro, and questioned him particularly as to the state of the urethra. He assured me, that since the day of the operation, he had never had the slightest annoyance; that the hardness in the perineum was gone; and that he voided his urine as freely as he ever did in his life. His general appearance was much improved, and he had then the look of robust health.

"Monro's case was one which satisfied me, more than any other I had seen, of the benefit to be derived from the treatment of confirmed stricture by external incision.

"I am, my dear Sir, yours faithfully,

"R. J. MACKENZIE.

"Professor Syme, 2, Rutland Street."

We think every candid mind must admit, that the testimony of these witnesses is such as neither Mr. Lizars nor Dr. Mullar can get over. Unless they are all banded together for the purpose of uttering the grossest falsehoods,—a supposition so absurd, that it is hardly necessary we should allude to it,—there is no possibility of any future dispute upon the subject. Notwithstanding this, however, the unscrupulous Lizars positively addresses another letter to the 'Medical Times,' dated the 11th of January, 1851; in which, by a species of sophistry, for which we can find no other term than disgraceful, he attempts to make out that the proof on the point of hæmorrhage is a "palpable failure;" for, argues he, the first witness, the nurse, only says, "As far as I know there was none," and therefore "the proof at best amounts to a *non mi ricordo*," taking care, however, *himself to suppress the fact*, that she distinctly affirms, *that had there been any, she must have known it.*

The testimony of the dresser is also too vague to satisfy Mr. Lizars, who seems incapable of understanding that, when a gentleman says, "I am not aware there was any bleeding after the operation," he merely guards himself from stating *positively, as a fact*, that which, unless he had been constantly day and night with the patient, he could not absolutely affirm.

Dr. Myrtle's testimony also proves, that to state, that the disease of the heart was produced by the hæmorrhage, or the operation, is *false*, for the man laboured under the heart-disease and bronchitis when the doctor was first consulted by him, which was *five months* before Mr. Syme divided his perineum.

It will not escape attention, either, that Dr. Myrtle was again consulted by Monro in May, 1850, six months after the operation, when the stricture was quite well, and No. 11 catheter easily passed through it; and yet, that, if Mr. Lizars statement be true, the stricture must have suddenly contracted again, so as to become "as bad as ever," in little more than *two months*, for the man died in the beginning of the succeeding August.

Even at the risk of wearying our readers, we must next refer to the case of Francis Badger, an Irishman, which has not been published by Mr. Syme at all, but by Dr. Mullar and Mr. Lizars, as an example of failure in the operation; but, as will presently be seen, so cleverly do these sapient controversialists discourse, that they manage to contradict each other in a most amusing manner. The patient, a drunken dissipated fellow, contracted several gonorrhœas in succession, which he aggravated by drinking; and at last, after he had suffered from stricture for a length of time, an abscess formed in his perineum. This was opened by Dr. Mullar, who had been attending him; but two days afterwards, to the doctor's astonishment, the man left the house and became a patient of Mr. Syme in the infirmary.

Here, the doctor believes, some remarks were made to the attendant-students, relative to the improper and baneful practice of opening a perineal abscess in the manner in which it had been done in this instance: "Ultimately," we quote from Dr. Mullar's pamphlet, "he was advised to allow his perineum to be bisected, which was performed in March, 1850; and, although he remained in hospital five or six months after the operation, what is the result? He is now confined to his bed with a stricture much more contracted than ever, an exceedingly irritable bladder, and a perineum having several fistulous openings." This is a very lamentable statement certainly; but what says Professor Lizars of the same case? After giving much the same account of it as his protegé, with the addition, that the operation of incising the perineum was attended with so much bleeding as to soak through the man's shirt, bed, and no less than three mattresses, he adds: "This day, 15th October, 1850, his perineum presents a number of fistulous openings, through which nearly all the urine flows. On being laid on a sofa, Professor Lizars inserted No. 7 catheter with comparative ease." (Lizars 'on Stricture,' p. 48.) Do very contracted strictures admit No. 7 catheter with comparative ease, even when handled by the skilful Lizars?

It would be a mere waste of time to continue this controversy further, as far as Mr. Lizars and Dr. Mullar are concerned. If any of our readers are inclined to investigate their statements still more completely, there is ample room for the gratification of the most ardent curiosity; and we may indicate to them, that, in all probability, the case of Joseph Antonio, with its accompanying plate, will afford them a tolerable field. We are not prepared to state, that there is no truth in what they say; but the observation is forced upon us, that they are so unscrupulous—so little regardful of exactitude—and beyond all, so angry,—that rage has made them blind to consequences that are patent to all the world besides. With the former quarrels of Messrs. Syme and Lizars, we have nothing to do; though we entertain a pretty strong opinion of the temper of both the disputants. The book which Mr. Lizars has published, and from which we have made so many extracts, is beneath contempt. We shall not disgrace our pages by repeating the terms of his advertisement in the London weekly journals; nor shall we here stop to remark on the vulgarity of indicating Mr. Syme by the periphrase, "*a pure perineal sectionist.*" If, too, the late Mr. Liston figures in some future biography, as a vain boaster, much more intent upon his own glorification than upon the welfare of his patients, his admirers have only to thank the refined Lizars. And if, in proof of all that we have asserted, they wish to see an example of want of truth, vulgarity, bad reasoning, and bad taste, all within the same page, let them but read the two halves of pages 19 and 20 of Lizars 'on Stricture of the Urethra.'

Words of congratulation are out of place in dealing with so painful a subject as the perineal section has proved itself to be; yet it is something to be able to assure our readers, that in any future remarks we may feel it our duty to make on this subject, we shall hold ourselves exonerated from taking any notice of what may fall from the individuals whose accumulated mistatements we have now exposed. Still the subject does not lose its most painful feature; and though the characters and acquirements of the actors are changed, we are as far as ever from being able to discuss the question merely as a scientific one, apart from the personalities which have disfigured it. This we can, indeed, unhesitatingly assert: in all our views we are perfectly unbiassed, either by personal predilections, for we are quite unacquainted with any of the parties concerned, or by previously expressed opinions as to the propriety of the proceeding. It may be recollected, that when Mr. Syme's book first appeared, we expressed no opinion, one way or the other, as to the statements that were there made. We contented ourselves with receiving the work with that respect which is justly felt towards a man who has so long and so successfully laboured to advance his profession; and we said merely, that its reasoning was apparently sound, and its facts striking. We have now, for a moment, to see how the perineal section has fared in other hands.

Dr. Dunsmure, one of the Surgeons of the Royal Infirmary, Edinburgh, informs us, in the latter part of last year, that during the previous ten months he had performed this operation on three occasions. The first time was in the person of Antonio, an Italian, to which we referred just now. We cannot enter into the details of the case; but it is quite clear, that from it no judgment whatever can be formed for or against the operation. It is right, however, to remark, that considerable bleeding occurred, which required some trouble to stop it. Dr. Mullar's account of the case is about as disingenuous as anything we ever remember to have met with; and those who are interested in the question, should read Dr. Dunsmure's own statement in the 'Edinburgh Monthly Journal,' for 1850, p. 398, *et seq.* The remaining two cases are too recent to allow us to judge of the permanency of the cure; yet, in reference to the operation itself, it is well to observe, that there was no hæmorrhage or other unpleasant symptom attending it. Dr. Dunsmure remarks, in reference to his second case,—that of William Tucker:

"Like a great many hospital patients, this man has been lost sight of; and, of course, I know not in what condition his urethra is at present. But this, at all events, is certain, that, from the date of the operation, he might have been dismissed from the Infirmary in ten days, with the wound in the perineum healed,—with the power of voiding his urine in a large stream,—and with No. 12 catheter passing into his bladder with the utmost ease." (p. 40, Op. cit.)

He also adds, at the end of his paper, that so convinced is he of the advantage gained by the operation, in diminishing the length of time necessary for the cure, that he intends to adopt it in those cases of bad stricture which may happen to come under his care.

At a late meeting of the Edinburgh Medico-Chirurgical Society, Dr. Dunsmure stated, that his third patient had lately returned to him, and that he found a tendency to recontraction of the stricture. Bougie No. 10, however, passed easily, and was rapidly succeeded by others of increased size.

Dr. Mackenzie, of Edinburgh, also performed this operation in the person of a man, who died eight days afterwards. There was no difficulty in the proceeding, and not much bleeding; but on the fourth day the patient had a rigor, and sank in another four days. The cause of death is not apparent; but we must certainly say, that, even taking the case as put by Mr. Lizars, we do not see how he can claim it as of any value as an argument against the operation. Cases of an exactly similar kind do occur after the most trivial operations, as the removal of a small tumour, or even the evacuation of an abscess. We think Dr. Mackenzie is right in saying, that the result of this case alone would not deter him from repeating Mr. Syme's operation for stricture.

Very recently, another dispute has arisen upon this subject, between persons of no less consequence than Professors Miller and Syme. Stripped of all extraneous matter, the circumstances are these:—At a late meeting of the Medico-Chirurgical Society of Edinburgh, Mr. Miller read to the members a case of perineal section, in which Mr. Syme had operated at his request, and in which it was alleged, that some very unpleasant symptoms had followed the proceeding. This narrative was drawn up by the *patient himself*, and gave a woful account of his sufferings. Much of these, Mr. Syme asserts, were exaggerated in the communication, and calculated to convey a most erroneous impression of the whole case; and, in reference to it, Mr. Syme further complains, that he had no intimation from Mr. Miller of his intention to pursue the novel course of reading the patient's own manuscript to a scientific meeting. The few observations Professor Syme makes upon the general principle here involved, we have much pleasure in presenting to our readers:

"Considering the constitution and objects of the Medico-Chirurgical Society, I regard the precedent, which would be established by allowing this procedure on the part of Mr. Miller to pass unnoticed, as of the most dangerous character. Any man, not belonging to the medical profession, and not even acquainted with its language, might thus, without subjecting himself to criticism or censure, communicate to a body of practitioners, associated for their mutual improvement, his own fancies,—no matter how absurd, and calculated to produce the most erroneous impression, in regard to modes of treatment, however beneficial they may have been to himself, or valuable for the relief of others. Nothing, in my opinion, could tend more to destroy the confidence existing between medical men and their patients, or impede the establishment of sound principles of practice, than the toleration of such a system. I, therefore, consider it right to express my entire disapprobation of this novelty in professional conduct, and will now read to the Society a statement of the case, for which they may hold *me* responsible." ('Monthly Journal of Medical Science,' April, 1851, p. 334.)

To all this Mr. Miller replies, that he communicated the patient's narrative to the Society incidentally, in the course of some observations which he was making on the subject of the perineal section; and that, at the time, he explained such parts of it as required elucidation.

In confirmation of his view, that he has not been guilty of the smallest breach of professional courtesy, he publishes, also, two letters from Sir B. Brodie and Sir Philip Crampton, who give their opinion in his favour.

It is to be observed, however, that neither of these gentlemen lays stress on what is, in our minds, the most important feature in the transaction, which is kept in the back-ground by Professor Miller, in his application to them; namely, the *use of the patient's own narrative*. The answers of both refer specially to the question, whether Mr. Miller was justified in giving publicity to the case at all,—whether, in fact, the patient was to be regarded as *his*, or Mr. Syme's.

Every one must form his own judgment of these facts. For our own part, notwithstanding the respect which we entertain for Sir B. Brodie and

Sir P. Crampton, we are decidedly of opinion, that, considering how vexed a question was here involved, and to how much calumny and misrepresentation the Professor of Clinical Surgery had already been exposed, it would have been far more consistent with kindly feeling, and, indeed, with ordinary courtesy, if the Professor of Surgery had previously informed his colleague of his intention to relate the case, instead of leaving him to guess the fact from the ordinary billet of business of the Society. And it is not to be forgotten, either, that Mr. Miller had previously provided himself with the patient's manuscript, for the express purpose of communicating it to the Society; and, therefore, that it was a deliberate act, and not a hasty determination, formed on the impulse of the moment. And when, finally, we add the consideration, that the *facts* of the case are matters of dispute between Messrs. Miller and Syme, we think there are few professional men who will not agree in the view we have taken of this transaction.

As for the case itself, we have not yet recovered from our surprise at finding, that what Mr. Miller affirms, Mr. Syme as absolutely denies, and *vice versâ*. The patient had long been under Mr. Miller's care; but, at his suggestion, Mr. Syme was called in to divide the stricture in the manner he has recommended. According to the one account, everything went on well after the operation; according to the other, the immediate consequences were most painful and distressing. Our readers must judge between these two statements.

First, Mr. Miller:

"After the operation, suppression of urine took place, during twenty-four hours, along with unpleasant symptoms of *shock*. Fever set in, accompanied by pervigilium, and great general uneasiness. After forty-eight hours, the catheter was removed. On Feb. 3, the constitutional disturbance became extreme; as indicated by violent sickness and vomiting, rigors, loss of voice, cold blue surface, feeble pulse, and recurrence of suppression of urine. After about twelve hours' continuance, these symptoms yielded to stimulants.—Feb. 5: There was great uneasiness about the scrotum and perineum; and on the 7th, abscess had formed in front of the wound. This was opened, and through the aperture urine, as well as pus, was discharged; the former continuing to pass through this wound, as well as through the original one, for many days. The greater part of the urine, however, came per urethram, in a flat, yet free stream, and without that peculiar distress to which the patient had previously been so long accustomed. When the wounds had nearly closed, a bougie was passed; but the effect was, to reopen the wound, with increase of pain in the urethra. And, in consequence, at the urgent solicitation of the patient, the bougie was refrained from till a more advanced period of the case. —Feb. 22: The urine had become very loaded and fetid, and continued to be of a depraved character for nearly a month. Up to this date, 'no appreciable sleep had been enjoyed; the patient never knew that he had slumbered, even in broken rest, for one moment; he was reduced to skin and bone; and now began to feel great discomfort in lying on his right side. At the same time, intense pain occurred in the rectum after stools.' These symptoms increased; and, on the 15th of March, I detected a large abscess pointing in the rectum, about two inches from the anus, and mainly occupying the right side of the pelvis. This I immediately evacuated, with instant relief; and 'the patient, with the aid of morphia suppositories, was afterwards blest with the first genuine night's rest since the 30th of January.' The abscess continued to discharge for about three weeks; and at the end of that time the presence of matter could no longer be detected in the stools. On March 31, the wounds had been closed for fourteen consecutive days, and accordingly it was deemed safe to pass the bougie. Nos. 8 and 9 were insinuated with great gentleness; but next day the perineum was again inflamed; abscess formed; and once more the urine was discharged in front by the opening in the scrotum. From this date, however, the patient gradually recovered. He left Edinburgh for the south on the 18th of May, with the perineum quite closed, and passing his urine in a very satisfactory way. In July he returned, to have a bougie passed; and No. 9 entered without difficulty or evil result. 'The contraction of the urethra, however, was by no means permanently cured;' and accordingly, on the 9th of November, the regular use of bougies was commenced, with the view of securing final and full dilatation. At first, I passed No. 6 with difficulty, but No. 12 now enters without obstruction; and the patient himself having acquired the power of occasional introduction, cure may be considered complete." ('Monthly Journal of Medical Science,' April, 1851, p. 398.)

Next, hear Mr. Syme:

"On the 31st of January, at four o'clock in the afternoon of a winter day, I first saw the patient, having been asked to examine him then under the influence of chloroform, and, if I deemed it expedient, thereafter to proceed with the operation. Having passed a small grooved director through the stricture, I divided it by external incision, and placed a catheter in the bladder. Everything went on most favorably for the next two days, and I then removed the catheter. On the following day, I was called to see a case in Northumberland, and consequently did not revisit the patient until the fourth day after the operation, when I found him with a smiling countenance, clean tongue, soft quiet pulse, moist skin, and plentiful discharge of urine; but was entertained with a woful account of the terrible things that had happened since my last visit, the second day before. Suppression of urine, rigors, delirium, and collapse, were all most graphically described; but, when taken in connection with the patient's satisfactory aspect, so soon after their alleged occurrence, suggested to me no idea of danger either in prospect or retrospect. On the contrary, I concluded that everything was going on favorably; and that the patient, being an irritable, exacting, self-indulgent sort of person, had merely suffered rather more than usual from the nervous disturbance which is apt to attend the flow of urine, after the catheter has been removed, in such cases.

"Accordingly all went on well; and, so far as is consistent with my own personal knowledge, the only obstacle that intervened in the way of his recovery was a small abscess, which formed in the lower part of the scrotum, and was evacuated by incision eight days after the operation. I wished to pass a bougie occasionally; but was allowed to do so only once, when an instrument larger than the catheter employed was introduced without the slightest difficulty. The urine resumed its natural course almost entirely in the course of a month, and by the end of the following month the patient was dismissed from medical restraint. In July he afforded me an opportunity of passing Nos. 8 and 9, which were introduced with the utmost ease, and without any subsequent rigor. He was then, and still appears to be, in perfect health; and, I believe, considers himself completely relieved from a complaint which, but for the operation I performed, would have proved incurable. No. 12, I understand, can now be introduced without any difficulty; and, if the patient is not deeply grateful for the benefit he has received, he certainly ought to be so." ('Monthly Journal,' April, 1851, pp. 334-5.)

And, in reference to Mr. Miller's more particular statement, hear the following from Mr. Syme:

"With reference to the particular statement just quoted, I distinctly declare, and positively affirm, that up to four o'clock of the afternoon of the third day, when I removed the catheter, there was no lack of urine, nor the slightest symptom of local or constitutional disturbance; that, having occasion to go to the country on the fourth day, I did not think it worth while to visit the patient either in the morning before my departure, or in the evening after my return; and that, when I saw him on the fifth day, he was in a condition that seemed to me in every way satisfactory. After this, I saw him with Mr. Miller twice or thrice at considerable intervals; and again in the course of the summer, when he returned to let me ascertain, by passing a full-sized bougie, that all was right. In regard to Mr. Miller's narrative, I may merely remark in the first place, that, as the patient resided at a distance of a few hundred yards, and as Mr. Miller's duty brought him to the hospital daily at the time of my visit, it does seem strange that he should not have acquainted me with the alarming occurrences which he has described, when they were in progress, especially as the responsibility of the result still rested entirely upon myself; and, secondly, that in all my experience of operations for stricture,—now amounting to upwards of forty,—I have never met with such consequences as Mr. Miller has related, so that granting his account to be correct, it would only constitute an exception from the general rule, and could not be of any value as a guide in practice." ('Monthly Journal,' May, 1851, p. 406.)

Dr. Matthews Duncan, in a letter addressed to Professor Miller, confirms his statement, as being, to his "certain knowledge, faithful, unbiassed, and correct in every particular." (See 'Edinburgh Monthly Journal,' April, 1851, p. 397.) And we have also a letter, to the same effect, from the patient to Mr. Miller, in the *May* number of the same journal, at page 461. To decide between these statements, is, of course, impossible. We read them with pain and humiliation, and scarcely dare trust ourselves with any comment. When we consider these things, and recollect the recent speculum and ovarian controversies, we are tempted to ask, if truth has vanished from the profession of medicine? We have every desire to believe, that Professors Syme and Miller are alike possessed with the fullest rectitude of intention; and that their widely discrepant statements of the facts of the case in question have not been wilfully distorted. But after taxing our brains in the vain hope of discovering a satisfactory solution of the enigma, we can only offer to the consideration of our readers the following suggestions, which, it must be confessed, do not go very far to explain it. We believe each party to have been strongly influenced by his previously-formed predilections, Mr. Syme in favour of the operation, Mr. Miller against it; and thus, as always happens in such cases, to have viewed simple facts through a distorted medium. Hence, if symptoms of constitutional disturbance arose, these to Mr. Miller, not practically conversant with the new operation, and led by his prejudice against it to attach too much weight to any unfavorable consequences, might have appeared of greater magnitude and importance than they deserved to be regarded; whilst by Mr. Syme, who, being more experienced in the matter, was not likely to be so readily alarmed by them, and who, moreover, was indisposed to admit any bad consequences from his method, these symptoms were too lightly estimated. We have endeavoured to look upon the question as not one of fact and veracity, but as one of opinion, with regard to the gravity of such symptoms as did manifest themselves. We are constrained to say, however, that, even making the fullest possible allowance for this source of discrepancy, we find ourselves quite unable to reconcile the accounts of the actual occurrences of the first week after the operation; and are only to a slight degree assisted in our attempt to harmonize them, by supposing that Mr. Miller allowed himself to be deceived as to actual facts, (e. g. the suppression of urine immediately consequent upon the operation,) by placing too implicit a reliance on the querulous statements of his patient,—as he seems to us to have obviously done, in regard to the subsequent total want of sleep. And we are further constrained to remark, that it is not a little singular that Mr. Miller should have allowed his patient to go on for several weeks with his urine in a depraved state, without appreciable slumber, and reduced to a state of extreme emaciation, in consequence of an operation of Mr. Syme's own performing, for which Mr. Syme held himself specially responsible, without seeking his assistance in the treatment of the case. We cannot but fear, lest some previous misunderstanding between the two colleagues operated to keep them apart on this occasion. Really it would seem necessary, in any similar case, to enlist the services of some neutral party, as reporter.

It is a duty, however, from which we shall not shrink, to express an opinion upon the whole controversy, and upon the merits of the question generally. If, in the course of our remarks, we may have appeared too favorably disposed towards Mr. Syme, it is because we believe that there is no individual, who, of late years, has done more for practical surgery, and because we give him credit for honesty of purpose throughout. But we are not insensible to his faults; nor shall we hesitate to declare wherein we think he is in error. We took the liberty, on a former occasion, (Vol. II, p. 39,) of offering him some friendly hints, as to his mode of drawing what Mrs. Malaprop calls "odoriferous comparisons" between himself and his professional brethren; and we feel called upon to repeat them at the present time, a little more forcibly, in the hope that, as Mr. Syme must see from the tone in which we have treated this subject, out earnest desire to do him justice, he will consider us as his real friends, and will give us credit for meaning to render him service, even if he do not plead guilty to the faults we lay at his door.

There can, we think, be little question, that there is in Mr. Syme a self-confidence, founded, doubtless, upon real merits, which impels him to commit himself to doctrines, that are, to say the least of them, rash and premature. And herein, not content with stating his own opinion, he decries the honesty or the ability of those who hold adverse views; albeit, a few years before, he may have advocated similar ones himself. What can tend more to lower Mr. Syme in the opinion of thoughtful men, than his assertion that all strictures are permeable, and his unsparing denunciations of those who may have had occasion to puncture the bladder? What can be more foolish and unbecoming, than his boastful challenge to pay the expenses to Edinburgh and back, of any patient whom the London surgeons may send to him with impermeable stricture? Surely, even were it creditable to accept such an offer, Mr. Syme must know full well, that the test is one impossible to apply; and, therefore, that he is perfectly safe in proposing it. Cases of impermeable stricture are, of course, such as are occasioned by a sudden attack of retention, supervening on a stricture of old standing, so that the patients could not wait to go to Mr. Syme, even if he offered an express train to convey them to Edinburgh. And how absurd it is to argue, that *because* since 1844 he has believed all strictures to be permeable to very small instruments, he has never met with one that was not so.

So querulous is Mr. Syme, that he complains of Mr. Miller, for using the term Perineal Section. Things must have names, and we can only say, that the expression complained of has, at any rate, the merit of correctly indicating its meaning; and until we hear of a better, we shall take the liberty of employing it ourselves. In short, the Professor of Clinical Surgery manages to quarrel with all those who venture to differ from him; and thus brings upon himself a great deal of dislike and misrepresentation, which he would otherwise avoid. In his own writings, he permits himself the use of language, particularly directed against London surgeons, which, in England, would never be tolerated, as it would never escape, from Brodie or Lawrence, or others whose position in the profession is fully as exalted as even that of Mr. Syme. For once, Mr. Syme, *take* advice from those who wish you well, and who honour your talents and your industry. Be more tolerant, be more courteous. Remember that abuse is a poor substitute for argument; that Edinburgh alone is not the United Kingdom; and that there are labourers as honest and as able as yourself, who have not the advantage of living in the Modern Athens.

A very few lines must conclude what we have to say on the general merits of the question. Strictures of the urethra had often been divided upon a grooved director, or its equivalents, before Mr. Syme wrote; but to him undoubtedly belongs the merit of systematising the operation, and of exhibiting its peculiar advantages. He has shown that it is a proceeding of no great hazard, and far more effectual than the old operation without a director, as well as much less dangerous to the patient. On this head Mr. Syme observes, in the 'Edinburgh Monthly Journal' of April last:

"As to the danger of the operation, I contend, that if the incision be made in the mesial line of the perineum, there is no vessel of the slightest consequence that can be cut; and that, if the deep fascia of the perineum is not divided, which it never requires to be, there cannot take place extravasation of urine, unless, perhaps, forwards in a slight degree into the scrotum, where it may occasion a little suppuration, utterly unworthy of attention, when compared with the great object in view. I therefore maintain, that the operation is absolutely free from danger; and appeal to the fact, that I have performed it between forty and fifty times without a single fatal result." (p. 337.)

The statement here quoted may be somewhat too strong; for all operations performed on patients suffering from long-standing stricture are notoriously hazardous; and in severe instances, this very operation has ended fatally, as our readers have already seen. The danger arising from hæmorrhage has probably been altogether exaggerated, especially by Mr. Lizars and others, who seem to have been more anxious to calumniate Mr. Syme, than to advance truth.

The important point remains, to determine to what classes of stricture the operation is applicable, and whether the result is permanent or the reverse. It is idle to say that most strictures can be cured, and ought to be cured, by the simple bougie; no one doubts the truism, and least of all Mr. Syme, who throughout his writings has been careful to guard against the indiscriminate adoption of the Perineal Section. But there *are* strictures, which we believe to be absolutely irremediable, except through some such operation as this.

That form of stricture, or almost obliteration of the canal, which takes place after rupture of the urethra from injury, is, we conceive, likely to be very greatly benefited by division; and likewise that kind of stricture which contracts again almost as soon as it is dilated, and which has been called elsewhere the Resilient Stricture.

Lastly, also, where time is of the utmost importance to the patient, as is the case with many labouring men, the operation would, we conceive, be justifiable, when the stricture is very obstinate.

Of the permanency of the cure, we have no sufficient evidence; but that after division, the disease *may* never return, is clear enough from the history of Mr. Syme's first patient, and is also supported by a case reported in the 'Medical Times' of April 27, 1850, by Mr. Henry Smith, in which the old operation was performed by Mr. Fergusson. The reverse of this is likewise proved, however, as well by Dr. Dunsmure's third case, as by others; and, therefore, every prudent person would advise the occasional introduction of a bougie after this operation; as after the cure by simple dilatation.

ART. IX.

Lehrbuch der physiologischen Chemie. Von Prof. Dr. C. G. LEHMANN. Erster Band. Zweite gänzlich neu umgearbeitete Auflage, pp. 464; Zweiter Band, pp. 463.—*Leipzig,* 1850.

A Treatise on Physiological Chemistry. By Prof. Dr. C. G. LEHMANN. First Volume. Second Edition, entirely revised, pp. 464. Second Volume, pp. 463.—*Leipsic,* 1850.

THE first volume of Lehmann's 'Physiological Chemistry' appeared fully nine years ago, and was shortly afterwards noticed, with several other works on the same subject, in the pages of one of our predecessors. (Brit. and For. Medical Review, Vol. XVII, p. 424.) The present edition may, however, be regarded in the light of a thoroughly new book; and we shall not deem it necessary to make any further reference to the review to which we have alluded.

In the "methodological introduction," which occupies the first twenty-six pages of his work, our author endeavours to point out the nature of the results which physiological chemistry, in the present condition, is capable of yielding, and the requirements which it fulfils; and to ascertain the means and the methods most likely to make its various applications serviceable, and, at the same time, the best adapted to promote its further progress.

He commences by pointing out, that most of the errors into which those have fallen, who have been zealous in their endeavours to elucidate physiology and medicine by chemistry, may be classed as dependent on one or other of the three following causes:—In the first place, too little attention has been directed to the laws of a true natural philosophy, whose simplest rules have, in many cases, been wholly disregarded; in the second place, the necessary causal connection existing between chemistry and physiology, histology (or the anatomy of tissues) and pathological anatomy, has too often been entirely neglected; and thirdly, much misconception has arisen from the assumption, that chemistry afforded a satisfactory answer to many questions, which it is either wholly incompetent to solve, or which must, at all events, remain undecided, in the present state of our knowledge.

We shall offer a few remarks on each of these causes of error.

All our readers, will, we doubt not, agree in the general principle laid down by our author, that "it is only by the application of abstract physical laws, by the establishment of certain momenta of empirically-observed phenomena, and by a steady adherence to safely-guiding maxims—in short, by logical sequence—that we can advance in the investigation of vital phenomena." In medicine, however, even from the earliest ages, there has been a mania for explaining everything by hypotheses; and hence it has derived less benefit than many other sciences, from the exact method of physical inquiry. The deficiency in logical sequence, which is so frequently encountered in medicine, is likewise perceptible in the collateral sciences, and in none of them more than in animal chemistry. Facts are not clearly distinguished from hypotheses, or hypotheses from absolute fiction; and this is the more easily accounted for in physiological than in pure general chemistry; for while the latter treats of palpable phenomena, and of facts which are easily reducible to general laws, in the former we are compelled to have recourse to experiments and questionings of nature, whose success must, in a great measure, depend on individual operations of the mind.

"Physiological chemistry has given rise to many delusions from a deficiency in logical sequence, owing to its imperfect development, and to the necessity presented by physiology and pathology for chemical elucidation. Some few isolated deductions were drawn from superficial chemical experiments, and arranged in a purely imaginative connection by the aid of chemical symbols and formulæ, for whose establishment analysis, in many cases, did not even afford any sanction. Thus, for instance, in the attempt to form a conclusion regarding the metamorphosis of the blood from an elementary analysis of its solid residue and of the composition of the individual constituents of the excretions, there is an utter absence of all scientific groundwork; for, independently of the fact that the elementary analysis of so complex a matter as the blood is incapable of yielding any reliable results, and cannot therefore justify the adoption of any special chemical formula, it is assuredly most illogical to attempt to compare the composition of the blood collectively with that of the separate excrementitious matters. In such deductions, expressed by chemical formulæ, the addition of atoms of oxygen, and the subtraction of those of water, carbonic acid, and ammonia, are wholly arbitrary; for chemical analyses do not afford the slightest ground for the majority of these equations. When, on the other hand, we have seen uric acid decomposed by different oxydising agents into urea and other bodies, and when, further, we find the quantity of uric acid increased in the urine in those cases where a diminished quantity of oxygen is proved to be contained in the blood, we are justified in concluding, that also in the animal organism a portion, at least, of the urea found in the urine must have been produced by the oxydation of the uric acid. In the formula which expresses this deduction, we have an hypothesis, but a well-grounded one, which, although requiring further confirmation, is yet wholly different from the frequently condemned, but rarely avoided abuse of chemical symbols. Chemical equations having no other foundation than the presumed infallibility of empirical formulæ, must, however, cause us to deviate from the path of physical inquiry, and involve us in a chaos of the most untenable delusions. Thus, for instance, a chemical equation might lead us to conclude, that glycine (glycocoll, or sugar of gelatin,) was the source of urea and lactic acid in the metamorphosis of the animal tissues; for we might conclude that 2 equivalents of hydrate of glycine were decomposed into the above-named substances, according to the formula, $C_8H_{10}N_2O_8 = C_2H_4N_2O_2$ (urea) + $C_6H_5O_5.HO$ (lactic acid). All experiments hitherto instituted with glycine are, nevertheless, opposed to such a disintegration. If, then, we would deduce urea and lactic acid from glycine, which has not been proved to exist in the blood, we should be neglecting the most comprehensive rule of logic, according to which, one hypothesis cannot be supported by another. It has, however, unfortunately, been too much the practice, in recent times, to employ far more complicated equations, as supports for such purely subjective modes of contemplation, by which a semblance of the most exact method of investigation has been assumed. By these means a number of chemical fictions have supplanted the fancies of that speculative natural philosophy which, in earlier times, encumbered the study of physiology and pathology, and have plunged medicine into the midst of a new labyrinth of untenable theories." (Vol. i, pp. 4-5.)

We proceed to the second cause of error,—the neglect of the necessary causal connection amongst the different allied branches of natural science. If chemical inquiries regarding objects belonging to pathological anatomy are to be of any value, and if they are to afford any true elucidation of pathological processes, every one must admit that such inquiries should be duly considered from an anatomical and diagnostic point of view. Yet, how often do we hear of the chemical examination of diseased bones or other tissues, without any reference to a diagnosis at all in accordance with the present state of pathological anatomy.

"What numerous analyses," observes our author, "have been made of the bones in osteomalacia, notwithstanding that the morbid appearances of these bones vary so much, as to render a definite diagnosis a matter of great difficulty to the pathological anatomist. We even more frequently meet with similar inconsistencies in the investigation of diseased animal fluids. Here, as in the statistical method of observing diseases, none but the simplest form of a disease should be made the subject of such inquiries. Yet the casual results yielded by an examination of the urine and the blood, in the most complicated forms of disease, are frequently made the sole grounds for drawing conclusions regarding the morbid process itself. In many cases, even the true diagnosis of the case has not been given. Thus, for instance, we are told that the blood has been analysed in typhoid pneumonia; yet, when we read the history of the case, we find that the disease was neither ordinary abdominal typhus with pneumonic exudations, nor what is termed pneumo-typhus, but simple pneumonia with cerebral symptoms." (Vol. i, p. 6.)

In all pathologico-chemical investigations, the state of the patient, as ascertained by a physical examination at the period of the removal of a morbid product for analysis, should be fully and carefully indicated, for it is only by this means that we can impart scientific value to such inquiries.

The third circumstance which has led to misconceptions in physiological chemistry, depends upon an over-estimate of the value of chemical auxiliaries, and a complete ignorance of the present condition of organic chemistry. What real benefit have the innumerable analyses of morbid blood and urine, which have been instituted during the last few years, conferred on pathology?

"Although we might have anticipated greater results, we can hardly wonder that the efforts hitherto made should either wholly or partially have deceived our expectations; for although these investigations may have rendered chemistry no unworthy auxiliary to a physical diagnosis, analyses of morbid products could hardly afford an insight into the chemical laboratory of the organism, while the means were wanting to prosecute them with the scientific accuracy attainable in the case of mineral analyses. Animal chemistry is still wholly unable to afford us a precise, and, at the same time, a practically useful method of investigating the blood; and how should it be otherwise, while we continue to be in doubt regarding the chemical nature of its ordinary constituents? The mineral substances of normal blood are not yet determined, or, at all events, continue to be made the subject of dispute; we scarcely know the names of the fatty matters it contains; one of its most important constituents, fibrin, cannot be chemically exhibited in a pure state; we are ignorant of the nature and mode of secretion of the globulin of the blood-corpuscles; we are still far from being able to separate and determine the so-called protein oxides; and we are also ignorant of the excrementitious matters occurring in the blood. How then, amidst these and many other uncertainties and doubts, can an investigation of the blood be scientifically and trustworthily conducted? We analyse healthy and morbid milk, and yet we are ignorant of the substances whose admixture we term casein. The urine, in its morbid condition, presents many varieties; and yet our knowledge of the secretion, frequently as it has been analysed, amounts to little more than an acquaintance with the quantitative relations of some of its principal constituents: creatinine and hippuric acid have not been determined by any analysis, and doubts are still entertained by some chemists (although most unjustly) regarding the presence of the latter in human urine, while absolutely nothing is known regarding the very obvious pigments which occur in this secretion. Many experiments have been made, and theories broached, on nutrition and digestion; and yet, to almost the present day, the existence of lactic acid in the gastric juice has been contested. Although hypotheses are not wanting regarding the mode of action of pepsin, we know nothing of its chemical nature; and we are wholly ignorant of the proximate metamorphosis of albuminous bodies in the stomach during the process of digestion. When such is the state of animal chemistry, can we wonder that there should be obscurity regarding the chemical processes in the animal body, their various isolated and combined actions, their causal connection, and their dependence on external influences and internal conditions?" (Vol. i, pp. 9-10.)

From this brief notice of the deficiencies and errors pertaining to the chemistry of the vital processes, we now pass to the methods and principles by which alone this science can be made to fulful its just requirements. It is obvious that we must thoroughly understand the substrata of which the animal tissues are made up, before we can attempt to decide upon the nature of the process of metamorphosis. If zoo-chemistry (or the study of the organic substrata of the animal body) ever attain to anything like completeness, it must be by the joint aid of chemistry and physiology; that is to say, individual substances must not only be fully examined, in reference to their chemical value, and their place in the domain of pure organic chemistry, but they must also be observed in the more general relations which each may bear to the animal organism and its metamorphoses.

In the first volume, which may be regarded as an independent work on zoo-chemistry, Lehmann adopts the following arrangement. He first treats of the *chemical* relations of each body in reference to its properties, composition, combinations, and mode of decomposition, its preparation, the methods of testing for it, and its quantitative determination; he then notices the *physiological* relations of each substance, endeavouring to determine the positions in which it occurs in the animal body, and its origin, (whether it be produced within or without the body;) and from the above considerations to deduce its physiological value.

After having thus become thoroughly conversant with the organic substrata of the animal body, we are led to the consideration of "the theory of the animal juices;"—those complex and variable parts of the animal body, the knowledge of whose chemical composition forms another basis of physiological chemistry. These constitute the subject-matter of the second volume. Although the results of the chemical analysis of the animal juices may afford many indications of the processes, they by no means enable us to judge of the function itself; and it is only by means of experiments founded on the composition of the fluids, that we are able to arrive at any satisfactory conclusion regarding the nature of the processes in question.

The third great section of physiological chemistry embraces the theory of the metamorphosis of tissues,—of nutrition and secretion. This is the highest department of our inquiry; and it is impossible to enter on its study, without a thorough knowledge of *zoo-chemistry* and *the theory of the animal juices.* In fact, the two volumes now before us are merely the introduction to this one great subject. As we shall postpone the consideration of the metamorphosis of tissues till the concluding volume of this work appears, we will here merely observe, that Lehmann specially refers to three methods by which our knowledge in this department has been signally extended.

The first of these is the *statistical.* By this means it was ascertained, by an accurate investigation of the food, and by its comparison with the constituents of the excreta and of the nutrient fluids, that in the ordinary food of animals, albuminous substances occur in sufficient quantity to compensate for the nitrogenous matters lost in the process of nutrition and in the metamorphosis of tissue; while it was thus at the same time shown, that the animal organism does not necessarily possess the property of generating albuminous matter from other substances containing nitrogen. The question whether the animal organism could generate fat, was answered by similar statistical observations, namely, by comparing the fat contained in the food with that secreted in the cellular tissue and mixed with the excrements. Liebig, Dumas, Boussingault, Marchand, and Valentin, have applied the statistical method to physiological chemistry with much success. This method, however, only gives us *final results*, and does not elucidate the processes or the causal relation of phenomena; it proves that fat *is* formed in the animal body, but throws no light on the *nature of the process* by which it is formed.

To acquire this knowledge, we must have recourse, in the second place, to what Lehmann terms the *comparative analytical* or *chemico-experimental* method; inasmuch as it consists in artificially imitating, as far as possible, the chemical phenomena of the living body, and comparing the chemical metamorphoses of certain substances external to the vital sphere, with those within the influence of the vital processes. As illustrations of this method of inquiry, we may refer to the series of experiments instituted by Liebig and his school on the numerous products of decomposition of fatty matters, and more especially on their products of oxydation, with the view of ascertaining the chemical changes undergone by the fats within the organism; to the experiments of Schlieper and Guckelberger on the products of oxydation of albuminous bodies and gelatin; and to Dessaigne's discovery of the splitting up of hippuric acid into glycine (glycocoll) and benzoic acid.

A third method of investigation is the *physiologico-experimental*, wherein we include that class of inquiries in which observations are made in the living organism, on the result of certain conditions on the progress of a physiologico-chemical process, and on the different stages of that process. Under this head we may place the investigations that have been made in reference to the contents of the stomach during the process of natural digestion, to the chemical change of individual substances in the development of the ovum during incubation, and to the dependence of the products of respiration on different external conditions; the experiments that have been made on the changes of individual substances during their passage through the animal organism, and on the metamorphoses of certain nutrient substances during the process of nutrition; and all pathologico-chemical experiments, as, for instance, observations on the contents of the intestine after the closure of the common bile-duct, and on the blood and other fluids, after extirpating or tying the vessels of the kidney.

The length to which these introductory remarks have extended, compels us to pass over Lehmann's excellent observations "On the place which Physiological Chemistry occupies, or at some future time will occupy, amongst the auxiliary Medical Sciences;" and "On the relation of Pathological to Physiological Chemistry;" and to proceed without further delay to the principal subject-matter of the First Volume, "*The Organic Substrata of the Animal Organism.*"

The following tabular view will show at a glance his mode of arrangement:

I.—NON-NITROGENOUS ACIDS.

1.—The butyric acid group: General formula $C_nH_{n-1}O_3+HO$.
2.—The succinic acid group: General formula $C_nH_{n-2}O_3+HO$.
3.—The benzoic acid group: General formula $C_nH_{n-9}O_3+HO$.
4.—The lactic acid group: General formula $C_nH_{n-1}O_5+HO$.
5.—The solid fatty acid group: General formula $C_mH_{m-1}O_3+HO$.
6.—The oily fatty acid group: General formula $C_mH_{m-3}O_3+HO$.
7.—The resinous acids.

II.—NITROGENOUS BASIC BODIES.

1.—Non-oxygenous alkaloids.
2.—Alkaloids containing oxygen.

III.—CONJUGATED ACIDS.
IV.—HALOID BASES AND HALOID SALTS.
V.—NON-NITROGENOUS NEUTRAL BODIES.
VI.—COLOURING MATTERS.
VII.—EXTRACTIVE MATTERS.
VIII.—NITROGENOUS HISTOGENETIC SUBSTANCES.
IX.—MINERAL SUBSTANCES OF THE ANIMAL BODY.

We shall throw a rapid glance at the most important of each of these several leading sections.

Commencing with the first division of the first section, the *butyric acid group*, we may observe, that its most important acids, in relation to animal chemistry, are formic acid, acetic acid, butyric acid, caproic acid, caprylic acid, and capric acid. Oxalic acid is also included by Lehmann in his consideration of this group, in consequence of its having been (we think, satisfactorily,) demonstrated by Kolbe, that it is the acidifying principle of these bodies; that is to say, that the acids of this group are conjugated oxalic acids; or, in other words, acids in which oxalic acid is so combined with a carbo-hydrogen $C_n H_{n+1}$, as not to affect the saturating capacity of the acid.

Oxalate of lime is the only compound of oxalic acid of any physiological interest. We extract Lehmann's observations on its physiological relations:

"Frequently as oxalic acid, combined either with the alkalies or with lime, occurs in the vegetable kingdom, (Schleiden,* Carl Schmidt,† and others,) it is very seldom

* Grundzüge der Botanik. 2 Aufl. 1846. † Entwurf u. s. u.

found in the animal organism, at least in large quantities. It only occurs in the latter in combination with lime, never being present in sufficient quantity to combine with the alkalies as well as with lime. Moreover, it is much more frequently met with in pathological than in physiological conditions.

"It is in the urine that the presence of oxalate of lime has been most frequently observed; it was for a long time regarded as a morbid product in this fluid; but independently of the circumstance that this body is constantly present, together with carbonate of lime, in the urine of herbivorous animals, it has frequently been found in normal human urine by myself,* Höfle,† and others.

"In examining microscopically the morning urine of healthy men, I have frequently discovered isolated crystals of oxalate of lime; this is not, however, always the case; and further, the oxalate of lime recognisable in such cases by the microscope, is not all that is contained in the urine, for it forms in larger quantities after some time, and during the acid urinary fermentation, so admirably described by Scherer. After allowing morning urine to stand for a considerable time, we often find a great many of these crystals, when the perfectly fresh urine presented no trace of them. The following is an excellent mode of demonstrating the existence of oxalate of lime in normal urine. If it be winter, we must expose fresh urine out of doors till it freezes; in this process, as in the freezing of wine and vinegar, a great part of the water crystallises in a comparatively pure state, and after its removal we obtain a concentrated saline solution, in which microscopic crystals of oxalate of lime may be discovered. That oxalate of lime is at first actually held in solution in filtered urine, and that it does not, as C. Schmidt supposes, proceed from the mucus of the bladder, is a view which is supported by the experiment which I have often repeated, that in urine, which after thoroughly cooling was freed from its mucus and urate of soda by filtration, the most distinct crystals of oxalate of lime might, after a time, be recognised, while no traces of them could either previously be detected in the mucus of the fresh urine, or found after the residue on the filter had been for some time in contact with water. The oxalate of lime, with a few crystals of uric acid, does not separate from filtered urine until after it has stood for some time. We may very easily convince ourselves that oxalate of lime is present in a state of solution, by extracting the solid residue of filtered urine with not too concentrated spirit, and agitating the spirituous extract with ether; after the extraction with ether, there may be observed, in the alcoholic extract, a sediment insoluble in water, which consists of the most beautiful crystals of this salt. While in the acid urinary fermentation the separation of the oxalate of lime increases with the augmentation of the free acid of the urine, in the latter case the salt is separated by the removal of the free acid.

"The quantity of oxalate of lime in ordinary urine is so minute, that, till recently, chemists, from the want of sufficiently accurate means of analysis, were unable to recognise it; good analysts have, however, always found, in the insoluble part of the ash of the extract of urine, a little carbonate of lime, which, at all events, owes part of its origin to the oxalate of lime.

"Crystals of oxalate of lime are most frequently found in the urine after the use of vegetable food, especially of such kinds as contain ready formed oxalates, (Wilson.‡) Donné found that after the use of sparkling wines, the quantity of the salt is increased in the urine; and my own experiments show that there is an increased secretion of oxalate of lime after the use of beer containing much carbonic acid, and of the alkaline bicarbonates and vegetable salts. I cannot confirm Bird's view, that highly nitrogenous food causes a precipitate, or even an augmentation of the oxalate of lime. It is often found in the urine of pregnant women. (Hofle.)§

"From a series of direct experiments on the subject, C. Schmidt ‖ is led to deny

* Wagner's Handwörterbuch der Physiologie, Bd. ii, s. 6.

† Chemie und Mikroskop am Krankenbette. Erlangen, 1848. S. 385.

‡ Provincial Medical and Surgical Journal, 1846, p. 413.

§ Chemie u. Mikroskop, &c., s. 385.

‖ Entwurf, u. s. u., s. 70.

that oxalate of lime introduced into the stomach, passes into the urine; and in this point I can perfectly confirm him, without, however, going so far as to assert that the food exerts no influence on the formation of this body. In the excrements of caterpillars we often find much oxalate of lime which is not formed directly from the ingesta, since I* have very often found the crystals in the biliary ducts of these animals. Preparations can be easily made of these organs; and in consequence of their contractility, a large quantity of their contents may be expressed from the cut tubes, and submitted to microscopic examination.

"With reference to the occurrence of oxalate of lime in certain morbid conditions, Prout, Bird, and others, make very different statements, none of which are yet fully established. Numerous examinations of morbid urine have convinced me, that in this country, at least, the sediments of oxalate of lime are much rarer than they are represented to be by English writers. These investigations have led me to the following results: when the respiratory process is in any way disturbed, we most frequently observe a copious excretion of oxalate of lime; it is most common either in fully developed pulmonary emphysema, or when the pulmonary tissue has lost much of its elasticity after repeated catarrhs; on the other hand, it is not present nearly so often in inflammatory or tuberculous affections of the lungs, (Hofle);† moreover, it is common in convalescence from severe diseases, as, for instance, typhus, mucus-corpuscles being then often associated with a trifling sediment of oxalate of lime.‡ I have only met with actually pure sediments of this salt in three persons, who sometimes (at somewhat considerable intervals) suffered from epileptic attacks. It is by no means constant, according to my experience, in the urine of rachitic children (Simon),§ of gouty adults with osteoporosis, of women with leucorrhœa, of patients with heart-disease, or in urine containing semen. (Donné.) ||

"In the dyspeptic conditions in which Prout and Bird have found sediments of oxalate of lime, I have failed in discovering anything of the sort; on the contrary, I have generally found the sediments in the urine of such patients to be free from these crystals. The reason why the English have so often found this salt in the urine, may be, that in England (as we shall further notice at a future page) the urine is generally in a more concentrated state than in Germany; and as Bird very correctly remarks, oxalate of lime is more rapidly separated from a concentrated than an aqueous urine. Moreover, experience at the bedside teaches every unprejudiced observer, that the appearance of oxalate of lime in the urine, is by no means accompanied by the group of symptoms which certain English physicians describe as pertaining to what they call the oxalic diathesis.

"That the mulberry calculus consists for the most part of oxalate of lime, has been long known; but most other urinary calculi, whether they consist principally of earths or urates, almost always contain a little oxalate of lime.

"This salt has only rarely been found in other places besides the urine. C. Schmidt has remarked that it is often present in the mucus of the gall-bladder, and that it is scarcely ever absent from the mucous membrane of the impregnated uterus. I once discovered oxalate of lime in expectorated matter; but whether it was produced from the pulmonary mucus, or from fragments of food in the mouth, I could not decide.

"*Origin.*—As the use of vegetable food, of which many varieties contain oxalates, increases the quantity of oxalate of lime in the urine, the inference would seem a legitimate one, that the oxalates are transmitted from the food to the urine. The source of this salt must, however, not be sought for only in the preformed

* Jahresbericht d. ges. Med. 1844. S. 25.

† Chemie u. Mikroskop, &c. Nachtrag, s. 176.

‡ The frequent occurrence of oxalate of lime in the urine during convalescence has been independently observed by Professor Walsh. See his paper on the oxalates in the 'Monthly Journal of Medical Science,' Jan., 1849.—REV.

§ Hufeland's Journal, 1841. Dec. S. 73-88.

|| Cours de Microscope, pp. 249, 322.

oxalates, but in the amount of alkalies in combination with vegetable acids present in the food; for, as we have already mentioned, they induce an augmentation of the oxalate of lime. In all the well-marked cases to which I have alluded, the increase of the oxalate of lime seemed to be combined with disturbance of the respiratory process. Thus it may easily be understood why, after the use of drinks rich in carbonic acid, of alkaline bicarbonates, or vegetable salts, oxalic acid is increased in the urine; the superfluous carbonic acid which has entered the blood, or been generated there from the salts of organic acids, must obstruct the absorption of oxygen and the perfect oxydation of certain substances in the blood; hence also the quantity of oxalate of lime has been found to be increased by the partially impeded exchange of oxygen and carbonic acid in the lungs, consequent on emphysema, pulmonary compression during pregnancy, &c. We might, in such cases, assume, according to a formerly prevalent belief, that the kidneys in some degree acted vicariously for the lungs, since under the form of oxalic acid they remove from the organism the carbon which the latter organs would have excreted as carbonic acid.

"Although certain chemists hold a contrary opinion, it is an undoubted fact that the nervous system has an influence on the oxydation of the blood. The occurrence of oxalate of lime in cases of epileptic convulsions, in convalescent persons, &c., might be referred to the disturbance induced in such cases in the nutrition or in the function of the nervous system, and to its diminished influence on the process of respiration, without there being any necessity for the assumption of a special diathesis.

"It seems, moreover, unreasonable to set up such a diathesis, since the establishment of a special disease from a single symptom—that symptom being only the occurrence of oxalate of lime—is entirely opposed to the spirit of rational medicine.

"From Wöhler and Liebig's discovery, that uric acid is decomposed by peroxide of lead into urea, allantoin, and oxalic acid, it has been pretty generally assumed, that the oxalic acid of the urine is due to an oxydation of the uric acid; the oxalic acid, in this case, not being converted into carbonic acid, as usually occurs in the healthy organism. That the formation of oxalic acid may be in part thus explained, is unquestionable; but there are many other substances in the animal organism besides uric acid, which by oxydation yield oxalic acid. No definite numerical ratio between the uric acid, urea, and oxalate of lime in the urine, has been yet established.

"C. Schmidt* has propounded a very ingenious view regarding the origin of oxalate of lime in the urine. He believes that we must seek for the source of its secretion in the mucous membrane of the urinary passages; the oxalate of lime is first produced by the decomposing action of the acid urine on a soluble compound, oxalate of albumen-lime, secreted by the mucous membranes; for oxalate of lime as an insoluble body, could not penetrate with the urine through a series of renal cells; oxalate of lime is also formed from the mucus of the gall-bladder by this mode of decomposition. When oxalate of lime occurs in the urine, we always find an augmentation of the mucus. These reasons do not, however, appear to be so decisive as to induce us to exchange the view we have already given for that of Schmidt; and, indeed, in another place, we find Schmidt† himself maintaining that the urea is in part combined with oxalic acid." (Vol. i, pp. 47—51.)

Formic acid is not a substance of any very great importance in animal chemistry. Its empirical formula is $C_2\,HO_3.HO$; according to Koble's theory, it should be regarded as oxalic acid conjugated with hydrogen $= H.C_2O_3 + HO$; but, according to the views of other chemists, it is assumed to contain a radical *formyl* $= C_2\,H$, which is believed to occur in several other combinations, as, for instance, in chloroform $(C_2\,H)\,Cl_3$.

It has hitherto been much more frequently found as a product of the

* Ann. d. Ch. u. Pharm., Bd. 60, s. 55, ff.

† Entwurf, u. s. u., s. 47.

decomposition of organic substances, than as an educt of the animal body. It has been proved to exist in ants (especially *Formica rufa*); but whether they actually produce the acid, or only obtain it from the juniper berries and the cones of pines, which are known to contain it, and which are much sought after by these insects, is very uncertain. Will has lately shown, that the active poisonous principle in certain caterpillars is formic acid; but it is very possible that here, also, the acid is derived from the plants on which they live, for Gorup-Besanez has recently shown that it exists in the common stinging-nettle, and it will probably be found in many other plants. Scherer (who is one of the most trustworthy chemists of the present day) maintains, that he has found formic, acetic, and several other acids of this group in the juice of flesh, in addition to the lactic, inosinic, and phosphoric acids, which were previously shown by Liebig to exist in it.

Acetic acid, $C_4H_3O_3 . HO$, according to Kolbe's hypothesis, is oxalic acid conjugated with methyl $= C_2H_3 . C_2O_3 . HO$. It probably often occurs in the gastric juice in cases of disordered digestion. In one case, Lehmann detected acetic acid with certainty in the vomited matters, after vegetables and a little meat, but no vinegar had been taken. Simon once or twice found it in the fluid contained in the bullæ of pemphigus.

Butyric acid, $C_8 H_7 O_3 . HO$, (or, according to Kolbe's hypothesis, $C_6H_7 . C_2O_3 . HO$,) has been detected in the urine of pregnant women, and of those who, after delivery, do not suckle their children; in the sweat, (epecially in that of the genitals and lower extremities of corpulent persons;) and it probably sometimes occurs in the contents of the stomach. The milk contains a fat, *butyrin*, which has never yet been isolated in a state of purity, and which on saponification yields butyric acid, together with other acids of this group, namely, caproic, caprylic, and capric acids. Lehmann obtained a fatty matter from the blood of a woman a few days after her delivery, which, on distillation with dilute sulphuric acid, yielded volatile acids of this group. It has also been obtained (by Percy and Ragsky) from the fæces.

Few of the members of the *succinic acid group* are of importance in physiological chemistry. Indeed, Lehmann only notices two of them, namely, *succinic* and *sebacic acids*. Since the publication of the first volume of his work, succinic acid has been detected by Heintz in a cyst containing echinococci in the liver: Dessaigne's recent discovery, that it is a product of the oxydation of butyric acid, may, perhaps, explain its presence.

The *benzoic acid group* next claims our attention. Its representative, *benzoic acid*, is the only member of the group to which any physiological interest attaches. We extract the following remarks on its physiological relations:

"In a physiological point of view benzoic acid deserves a full consideration, although numerous experiments render it probable that it does not exist preformed in any animal fluid. No one has suspected its presence in any animal fluid but the urine; and in this, both in the case of herbivora and carnivora, it occurs very often in the place of hippuric acid. Liebig,* in his classical Essay on 'Fermentation, Putrefaction, and Decay,' attributed the occasional occurrence of benzoic acid, in

* Ann. d. Ch. u. Pharm., Bd. 30, s. 261, ff.

place of hippuric acid, in the urine of horses, solely to a process of fermentation which the latter acid underwent when the urine began to decompose; benzoic acid being formed from it, together with other products. Subsequently,* however, he has changed his opinion, believing that he had ascertained that horses, when very hardly worked, and living on insufficient fodder, discharged urine containing benzoic acid, while, under the opposite conditions, the urine contained hippuric acid. In order to ascertain which, or whether either, of these views is correct, I† analysed the urine of a large number of horses, both well-fed and half-starved, and healthy and diseased; but invariably found hippuric acid and no benzoic acid, unless when the urine had been a good deal exposed to the air at an ordinary temperature. But, on the other hand, when it had stood for some time in the stable, and began to be ammoniacal, it never contained hippuric acid, but only benzoic acid. Hence, too, it is that we so often meet with only benzoic acid in human urine, which, as it contains a far smaller proportion of hippuric acid, must be employed in larger quantities; and if some portions of it have been long exposed to the air, which can hardly be avoided, they produce such a change that only benzoic acid is found in the whole urine. Hence it appears to be the fact, as Liebig assumed, that a ferment is formed in the urine through which the nitrogenous hippuric acid is converted into benzoic acid; for if we mix a specimen of urine containing benzoic acid, whether from man or from the horse, with another specimen containing hippuric acid, on separating the acids from the mixture, we almost constantly obtain benzoic acid alone, the ferment of the urine containing benzoic acid probably acting on the hippuric acid of the fresh urine, even during the evaporation of the mixture. Moreover, the conversion of benzoic acid, conveyed into the organism, into hippuric acid, which was invariably observed by Wöhler and Keller,‡ Ure,§ and subsequent experimenters, is in accordance with the idea that the former, when it occurs in the urine, is only a product of decomposition of the latter." (Vol. i, pp. 87-88.)

The group represented by the formula $C_nH_{n-1}O_5 \cdot HO$, contains *lactic acid*, which, as our author remarks, "deserves a special chapter in every work on physiological chemistry." Before we proceed to the physiological relations of lactic acid, we must notice a peculiar relation which has recently been observed in the crystallisable lactates. The lactic acid obtained from animal fluids, and that produced by the fermentation of sugar, form, with the same base, salts, which present certain differences in the amount of their water of crystallisation, in their degree of solubility, and in their decomposition by heat. This is, however, a subject requiring further investigation; for Liebig thinks he has obtained from the acid of Sauerkraut a zinc-salt corresponding with that yielded by the juice of flesh; and Lehmann, whenever he has analysed the lactic acid of the gastric juice in combination with magnesia or zinc, has always found it corresponding with that obtained from sugar. Engelhardt, to whom, in conjunction with Maddrell, we are indebted for the discovery of two forms of lactic acid, distinguishes that which is obtained from muscular juice as *a* lactic acid, and that produced by the fermentation of sugar as *b* lactic acid.

For reasons which will be presently obvious, we give the microscopical characters of several of the lactates. *Lactate of lime* occurs in the form of white hard bodies, which, under the microscope, are seen crystallising in tufts of delicate needles, every pair of them being so placed in relation to the others, that collectively they resemble overlapping brushes or pencils.

* Ann. d. Ch. u. Pharm., Bd. 41, s. 272.
† Handwörterbuch d. Physiol., Bd. 2, s. 14.
‡ Ann. d. Ch. u. Pharm., Bd. 43, s. 108.
§ Transactions of the Royal Medical and Chirurgical Society of London. New series, vol. vii.

Lactate of zinc has been accurately described and figured by C. Schmidt, who is the only observer who has devoted great attention to the forms of microscopic crystals, with the object of diagnosing such bodies in the animal fluids: he mentions the club-like shape of the crystals during their process of formation, and their curved surfaces, as especially characteristic of this salt. *Lactate of magnesia* crystallises in vertical prisms with horizontal terminal surfaces. *Lactate of copper* crystallises in hard, blue or green, wart-like masses.

The following is Lehmann's method of determining the presence of lactic acid:

"In consequence of the extremely minute quantity of lactic acid to be obtained from the animal fluids, I am in the habit of adopting the following method, which may be readily modified in particular cases, with the view of studying the forms of the different salts under the microscope. The impure lactic acid prepared from the alcoholic extract by sulphuric or oxalic acid is treated with baryta-water, and the excess of the baryta removed by carbonic acid; the solution of lactate of baryta is evaporated to the consistence of a syrup, treated with alcohol, filtered, again evaporated, and then allowed to stand for some time in order that the other baryta-salts (for instance, the butyrate and inosinate,) may crystallise; the syrup is then allowed to trickle away, or if it be not withdrawn, is dissolved in water and decomposed with a solution of gypsum; the fluid from which the sulphate of baryta has been removed by filtration is strongly concentrated, and on examining it under the microscope we can readily perceive the double brushes of lactate of lime which we have already described, in addition to crystals of gypsum. On dissolving these crystals of lactate of lime in alcohol, and adding sulphate of copper to the alcoholic solution, the fluid, after standing for some time (in order that the excess of sulphate of copper and the gypsum that is formed may separate as completely as possible) is evaporated so as to crystallise, and the crystals of lactate of copper are then microscopically examined. If by the above process we do not succeed in obtaining distinct and measurable crystals, we must dissolve the residue in a little water; and (in order to decompose or separate any butyric acid that may be present) we must boil it strongly, filter it, and, after concentrating it, place in it a small zinc bar. Since, as we have already mentioned, lactate of copper is far more soluble in water than lactate of zinc, the zinc very soon becomes covered with white crystals of lactate of zinc, if the fluid be sufficiently concentrated; and these crystals, if they be allowed to remain for some time, may usually be easily measured under the microscope. Distinct crystalline forms may even be distinguished with the naked eye. If, however, in consequence of the want of a Goniometer, an accurate crystallometric investigation cannot be instituted, we must precipitate the solution of the zinc-salt with a boiling solution of protochloride of tin, and allow it to stand for some time; on then making a microscopic examination, we shall find clusters of crystals whose groups are composed of thick rhombic tablets lying close upon one another. When we have in this way prepared and explored the different lactates, (and after some practice, tolerably small quantities are sufficient for this purpose,) we hardly require to make an elementary analysis or to determine the atomic weight, to enable us to decide regarding the presence of lactic acid." (pp. 94-95.)

We regret that we cannot find space for Lehmann's observations on the physiological relations of lactic acid. We need hardly remind our readers of the lengthened discussion that has been carried on regarding the presence or absence of lactic acid in the animal fluids, and especially in the gastric juice. Lehmann was the first who clearly demonstrated the presence of lactic acid and its salts in the last-named fluid. He has never been able to detect lactates in the normal saliva either of man or of the horse; but in the saliva of a patient labouring under diabetes mellitus, he "convinced himself beyond all doubt of the presence of free lactic acid." Whether lactates generally occur in the chyle and lymph, must for the present remain undecided. In the chyle obtained in two cases from the thoracic duct of the horse, (one horse having been fed with oats two hours before he was killed, and the other with starch-balls,) lactic acid was recognised with certainty. There is every probability that it exists in the lymph; for we cannot readily perceive in what other way than through the lymphatics the large quantities of lactic acid formed in the muscles can be carried away.

"The recognition of lactates in healthy blood is just as difficult or impossible as that of urea in the same fluid. It is probable that we shall never obtain a positive demonstration of the existence of alkaline lactates in healthy blood by direct experiment; but the simplest induction proves that they must be present there, even if they only remain in it for a very short period. We know from numerous experiments how rapidly effete matters, and especially salts of easy solubility, are removed from the animal organism by the kidneys; we know with what extreme rapidity iodide of potassium appears in the urine after it has been swallowed; and we know that it is only on that account that urea has not yet been detected in healthy blood, (notwithstanding the assertions of certain persons,) for its sojourn in the blood is so very short, that the quantity occurring in the circulating fluid is scarcely to be recognised with our present chemical appliances. (Marchand).* Hence it is not surprising that the presence of lactic acid has never yet been demonstrated, with all the necessary scientific accuracy, in normal blood, especially when we consider that it is removed from the circulating fluid in more ways than one. The combustion of the alkaline lactates—that is to say, their conversion into alkaline carbonates—exceeds in rapidity and extent their passage into the urine. Until we can prove that the lactic acid, which is accumulated in large quantity in the muscular tissue, and is found in the chyle and in the lymph, undergoes decomposition on the spot, we must assume that it passes into the blood, and the more so because we well know that chemical analysis has not yet attained such a degree of accuracy as to enable us to demonstrate the presence of lactic acid in the blood with due scientific precision. In what other way than through the blood could the lactic acid of the chyle or the muscular fibre pass into the urine? Lactic acid, like urea, may collect abnormally in such quantities in the blood as to be capable of detection by chemical analysis. Scherer† has paid especial attention to the occurrence of lactic acid in morbid blood; he observed that, during an epidemic of puerperal fever, the blood had often an acid reaction, and, as this fluid frequently contained only free albumen and no albuminate of soda, it was clear that it must contain a free acid. Scherer certainly did not demonstrate the actual presence of lactic acid in the blood; but, as he actually separated lactic acid from the exudations which were simultaneously present, and recognised it by the form of its salts, we cannot reject his conclusion that the acid reaction of the blood was also due to lactic acid. I have only thrice observed an acid reaction of the blood under conditions similar to those described by Scherer, namely, in a case of pyæmia in a man, and in the blood of two women (from six to ten weeks after delivery). In no case could I obtain sufficient material to demonstrate the lactic acid with certainty.

"The following experiments,‡ instituted on myself, exemplify the rapidity with which the lactates in the blood are converted into carbonates. Within thirteen minutes after taking half an ounce of lactate of soda (calculated as dry), my urine had an alkaline reaction. Moreover, that the conversion of the alkaline salts of the organic acids into carbonates (as was first proved by Wöhler) does not take place in the *primæ viæ*, but in the blood itself, is proved by direct experiments which I

* Journ. f. prakt. Ch., Bd. 11, s. 149.
† Untersuchungen zur Pathol. Würzburg, 1843. S. 147-194.
‡ Jahresber., 1843, s. 10.

made on dogs, by injecting various quantities of lactate of soda into the jugular vein; after five, and at latest after twelve minutes, the urine exhibited an alkaline reaction.

"In opposition to the view that lactates exist in the blood, it has been urged that the ash of blood has not an alkaline reaction, and further, that it contains no alkaline carbonates. We have shown in another part of this work, that this observation of Enderlin's has not been made or confirmed by any one who has preceded or succeeded him; but that, on careful incineration, carbonated alkali always occurs in the blood; and even if this were not the case, it would be no evidence against the presence of lactic acid, since, on incinerating the blood, there is a combustion of sulphur and phosphorus sufficient to saturate the alkali previously combined with lactic acid. Further, carbonic acid is expelled from the carbonate by ordinary phosphate of soda, which is thus converted into tribasic phosphate of soda." (Vol. i, pp. 100-102.)

Lactic acid and lactates were found by Scherer in exudations after puerperal fever, and in a case of empyema. Its presence in the muscular juice is well known, having been asserted forty years ago by Berzelius, and during the last few years rigidly demonstrated by Liebig. Berzelius and others believe that lactic acid and lactate of ammonia exist in sweat, and that there are alkaline lactates in the bile. The presence of lactic acid in these fluids is, however, uncertain. The following are Lehmann's present opinions regarding the presence of lactic acid in the urine.

"In consequence of the rapidity with which the alkaline lactates undergo a transformation in the blood, it would naturally follow that lactic acid, when it occurs in the urine, would exist there as an extremely variable constituent: and this assumption is confirmed by experience. Earnestly as I formerly maintained the view that lactic acid constantly occurs in animal urine, and that the acid reaction of this fluid is solely dependent on its presence, I have since convinced myself that my earlier modes of analysis, (when I rested satisfied with the mere exhibition of the zinc-salt,) though most carefully conducted, were open to deceptions in reference to this acid; but to maintain that the urine of healthy men and animals never contains lactic acid or lactates, under any physiological relations, is to err just as much in the opposite direction. A more extended investigation has led me to the following results. In all cases where the supply of lactates to the blood is very great,—whether this depends on an excess of acid being formed in the muscles, or on the use of a diet tending to produce it, or on an imperfect process of oxydation in the blood,—lactic acid may be detected in the urine with all the certainty which in the present state of chemistry can be expected in such researches. Hence we can understand why it is, that in the urine of the same individual, lactic acid may on one day be present and on another absent;—why, in many persons, no lactic acid can be detected in the urine, and in others again (and especially in those who, in consequence of repeated catarrhs, suffer from partial relaxation of the pulmonary tissue, and yet often think themselves perfectly well,) it is constantly present in the urine;—why stall-fed animals, living on amylaceous fodder, excrete lactic acid by the kidneys (and in part also by the mammary glands), while under other conditions this acid cannot be discovered in their urine;—and why, finally, in most febrile diseases lactic acid may be recognised in the urine.

"Berzelius,* during his later years, entertained no doubt regarding the correctness of the results which he had so long before obtained in reference to the presence of lactic acid in the urine. Boussingault† has quite recently found lactic acid in the urine of pigs fed with potatoes, as well as in that of cows and horses. (In the urine of the horse he found 1·128§ of lactate of potash, and 0·881§ of lactate of soda.)

* Jahresber., Bd. 27, s. 590.

† Ann. de Chim. et de Phys. 3 Sér. T. 15, pp. 97-114.

"In accordance with this view is the almost universal occurrence of lactic acid in urine containing a considerable quantity of oxalate of lime, so that by a microscopic examination of a specimen of urine, a conclusion may often be drawn regarding the presence or absence of lactic acid. Hence in those diseases in which there is an increase in the amount of oxalate of lime, as in pulmonary emphysema, disturbances of the nervous system, rachitis, &c., lactic acid is always associated with this salt. Scherer* and Marchand† have sometimes observed a considerable augmentation of lactic acid in the urine in rachitic children, and I have also noticed it in the osteomalacia of adults.

"In determining the presence of lactic acid we must always employ fresh urine, if we wish to draw any conclusion regarding the composition of the renal secretion. The admirable investigations of Scherer‡ regarding urinous fermentation, were the first to direct attention to the circumstance that there is a gradual augmentation of the free acid, when the urine is exposed to the atmosphere. The lactic acid must then be formed from some unknown matter,—probably from what we term an extractive matter. I had formerly observed something similar occur in diabetic urine, since when freshly passed I always found it neutral, although subsequently it became acid; § in consequence, however, of diabetic urine containing sugar, these experiments were of less weight than those of Scherer. We may hence fairly conclude that the urine, after its excretion from the kidneys, undergoes a similar acidification in the bladder, and consequently that the lactic acid which is often found in the urine discharged from that viscus is a product of decomposition which is formed externally to the sphere of vital activity. If, however, the occurrence of crystals of free uric acid warrants us in inferring the existence of the lactic fermentation, it is only very seldom that it can occur in the bladder, for the cases are extremely rare in which urine on its emission from that organ contains free uric acid; the statement that has found its way into various books, to the effect that fresh urine often contains free uric acid, being a very erroneous one." (Vol. i, pp. 104-5.)

The lactic acid occurring in the animal body has most probably a double origin. There can be no doubt that the lactic acid found in the contents of the intestines and in the chyle after the digestion of vegetables, is formed from the amylaceous or saccharine matters of the food, which in their passage through the *primæ viæ* become converted into that acid, exactly as takes place in the fermentation of milk; but it is most probable, that the lactic acid, which accumulates in such large quantities in the muscles, proceeds from the decomposition of the muscular substance itself. The only objection to this view is, that lactic acid has not hitherto been produced, either by fermentation or otherwise, from any nitrogenous animal matter, either albuminous or gelatinous; but the recent investigations of Guckelberger, respecting the various modes of decomposition and the products of albuminous bodies, show, that in these substances there must lie concealed a group of atoms, from which sugar of milk or lactic acid might be produced.

Passing without comment over the fatty-acid groups, we arrive at the non-nitrogenous resinous acids, *lithofellic acid* occurring in certain intestinal concretions, and *cholic acid* (the *cholic acid* of Demarçay, or the *cholalic acid* of Strecker), which exists in the bile with glycine and taurine for its adjuncts, and in combination for the most part with soda; namely, as *glycocholate* and *taurocholate of soda*. The formula for this acid, according to Strecker's analyses, is, $C_{48}H_{39}O_9 . HO$; if boiled for some

* Untersuchungen z. Pathol., s. 74 ff.

† Lehrbuch d. phys. Ch., s. 105.

‡ Ann. d. Ch. u. Pharm., Bd. 42, s. 171; and Unters. z. Pathol., s. 1-16.

§ De Urina Diabetica. Diss. Inaug. Lips. 1835.

time with hydrochloric acid it becomes converted into *choloidic acid*, which, as it exists in its salts, is isomeric with cholic acid; on prolonging the boiling, this body loses its previous solubility in alcohol and alkalies, and parts with its acid properties. In this state it forms *dyslysin*, $C_{48}H_{36}O_6$.

The following observations on the method of testing for cholic acid, (which, indeed, is practically the same thing as testing for bile,) are well deserving of attention:

"Cholic acid, even when not perfectly pure, may be recognised by its reaction with sugar and sulphuric acid. This reaction, which was first discovered by Pettenkofer,* occurs with no other substance than cholic acid; it is, however, perfectly immaterial whether the cholic acid be already metamorphosed into choloidic acid, or whether it be combined with its adjuncts, as a conjugated acid. Hence we can apply this admirable test to discover generally either the presence of bile or of one of its derivatives. The following is the best method of proceeding. The alcoholic extract of the fluid to be tested for biliary matter must be dissolved in a little water, with which we must then mix a drop of a solution of sugar, (in the proportion of 1 part of sugar to 4 of water;) and pure English sulphuric acid, free from sulphurous acid, must be added by drops to the mixture; the fluid now becomes turbid from the separation of the cholic acid, but on the gradual addition of sulphuric acid the turbidity disappears, and the fluid again becomes perfectly clear; for the first few moments its colour is yellowish, it very soon, however, becomes of a pale cherry colour, then of a deep carmine, of a purple, and finally, of an intense violet tint. As indeed in all experiments, some practice and attention to certain rules are requisite, without which we may easily fail to apply this test successfully to the detection of bile. For instance, we must avoid the addition of too much sugar, as this is a substance which is easily rendered brown or black by sulphuric acid; and we must be especially careful, as Pettenkofer himself showed, while adding the concentrated sulphuric acid, not to allow the temperature much to exceed 120° F.; but the reaction equally fails when we carry our caution too far, and attempt to avoid any elevation of temperature when the sulphuric acid is added; indeed, my own experience leads me to believe that an elevation of the temperature nearly to 120° F. is requisite for the success of the experiment. Should the fluid at first assume only a cherry-red or a deep carmine tint, it must be allowed to stand for some time, after which the intense violet colour becomes developed. It is, moreover, immaterial which kind of sugar is used for this test: acetic acid may also be employed in place of sugar.

"Van den Broek† maintains that the reaction also takes place with mere biliary matter independently of the sugar, but I have never found this to be the case; without sugar the fluid has at most attained a red or reddish-brown tint, but never the characteristic, deep violet colour. But although Van den Broek is wrong on this point, there are other reasons why his view is correct, that this reaction is inapplicable as a test for sugar; in the first place, because we have the same reaction when other bodies, as for instance acetic acid, are substituted for sugar, and, secondly, because we have many better and more certain means of discovering this substance.

"If it should be necessary to separate the cholic acid from the conjugated biliary acids, or from choloidic acid, as is sometimes required in the examination of the blood, urine, and excrements, the best method is to acidulate the alcoholic extract with a little sulphuric acid, and to extract with ether, in which the conjugated biliary acids and choloidic acid are all but insoluble." (Vol. i, pp. 128-9.)

We now arrive at the Second Section, devoted to the consideration of the NITROGENOUS BASIC BODIES. The non-oxygenous alkaloids, *aniline*, *picoline*, and *petinine*, present nothing worthy of special attention. The

* Ann. d. Ch. u. Pharm., Bd. 53, s. 90-96.

† Holländische Beiträge. Utrecht u. Düsseld., 1846, s. 100-102.

alkaloids containing oxygen are a far more important group. Lehmann considers, under this head, creatine, creatinine, tyrosine, leucine, sarcosine, glycine, urea, xanthine, guanine, allantoine, cystine, and taurine; and to these we may now add, hypoxanthine, discovered by Scherer since the publication of Lehmann's first volume. Several of these substances do not possess any basic properties, and therefore, strictly speaking, should not be classed as alkaloids. These bodies are, however, arranged with the true alkaloids, partly on account of the analogy exhibited in their empirical composition, and partly because, in a physiological point of view, they exhibit tolerably equal values; that is to say, they are derivatives of nitrogenous tissues.

The following arguments are, in our opinion, quite sufficient to overthrow Liebig's view, that *creatine* is an important agent of nutrition:

"When we remember, that creatine occurs in the decoction of flesh, and is a highly nitrogenous body, we might be led to regard it as an important nutritive agent, and as taking an active part in progressive metamorphosis. The analogy, which, in its chemical relation and in its constitution, it presents to caffeine, might, moreover, tend to mislead those who class that substance among nutrient bodies, from its occurrence in certain kinds of food and in certain stimulants. But this analogy is here of very little moment, for we cannot place caffeine among the nutritive agents, without giving a very great latitude to the term. A substance, of which a quantity from two to ten grains will produce the most violent excitement of the vascular and nervous systems,—palpitation of the heart, extraordinary frequency, irregularity, and often intermission of the pulse, oppression of the chest, pains in the head, confusion of the senses, singing in the ears, scintillations before the eyes, sleeplessness, erections, and delirium,—can scarcely be reckoned among articles of nutrition even by the homœopathist, and certainly not by physiologists, when they learn how quickly caffeine becomes decomposed in the organism, and gives rise to an increased secretion of urea.

"The above-named results were yielded by experiments, instituted on myself and several of my pupils with pure caffeine. Five persons, (one of whom was Professor Buchheim, now at Dorpat,) after taking from five to ten grains of this substance, were unfit for any business during the next day; while, in an experiment which I formerly made on myself, ten grains scarcely produced any perceptible action. In all the cases, there was found to be augmentation of the total amount of urea excreted in twenty-four hours.

"If, however, the analogy between creatine and caffeine does not demonstrate the nutrient qualities of the former, it must be asked, whether its occurrence in a substance so nourishing as the decoction of flesh, and its large amount of nitrogen, afford more conclusive evidence in this respect? With reference to the latter, it may be assumed that nature would not suffer substances even more highly nitrogenised than creatine, as the creatinine discovered by Liebig in the urine, and the urea, to escape through the kidneys, if they could be employed to further advantage in the organism, since we find so careful a providence over recognised nutrient matters, as, for instance, albumen, &c., that even in disease they are only rarely found to escape with the excreta. The occurrence of creatine in the decoction of flesh affords even less evidence of its nutrient powers; for, when we consider the small quantity in which it occurs in flesh, and the truly homœopathic nature of the dose which we take with the meat and broth we eat, we must regard its simultaneous appearance in the urine as a proof that its properties are not very highly esteemed in the organism; since, if they were so, this substance would probably not be discharged from the kidneys, but retained in the same manner as albumen and gelatin. We think, however, that Liebig's complete chemical investigations of creatine, which were conducted in a manner worthy of so great a chemist, constrain us, even if unsupported by physiological proof, to regard creatine as a *product of excretion*. From its chemical qualities, we regard creatine as a member of the series indicating the regressive metamorphosis from the point of the highest atomic weights to bodies of the simplest composition. The readiness with which creatine becomes decomposed into creatinine, urea, and sarcosine, which is isomeric with lactamide, all of which are undoubtedly products of excretion, proves beyond a doubt, that creatine approximates more nearly to these substances than to albumen and fibrin, and indicates the great probability of creatine being decomposed, even in the living body, into these and other similar substances. Although such bodies as lactic acid, &c., may be employed for special purposes in the animal organism, they cannot, strictly speaking, be regarded as nutrient substances, that is to say, as materials for the renovation of nitrogenous tissues; and it is only in this light, and not in that of a supporter of heat, that we must consider creatine." (Vol. i, pp. 143-4.)

Our next extracts are selected from the chapter on *Urea*. We regret we have only space for Prof. Lehmann's remarks on the means of testing for urea in albuminous fluids, such as the serum, and for his observations on the origin of urea.

"Urea may generally be very easily recognised by its properties, especially by its behaviour towards nitric and oxalic acids; but when we have to discover very minute quantities of this substance in albuminous fluids, it is often very difficult to determine its presence with scientific precision. It is in alcoholic extracts that we must always seek for urea; but before we proceed to search for it, there are several precautionary measures to be adopted, the neglect of which would render our attempt to discover it futile. In the first place, in reference to the presence of albuminous substances, if we wish to discover small quantities of urea in albuminous fluids, we must not be satisfied with the removal of the albumen by simple boiling; since, by the coagulation of the albumen, the fluid becomes more alkaline, and might, during evaporation, induce a decomposition of the urea; moreover, all albuminous matter is not precipitated by boiling, but a portion remains dissolved by the alkali, and is taken up in the alcoholic extract. On evaporation, this albumen undergoes a change, which probably coöperates with the alkali in inducing the decomposition of the urea. This may explain how it was that Marchand could only recover 0·2 of a gramme of urea from a mixture of 200 grammes of serum and 1 gramme of urea. Hence, before boiling the albuminous fluid, we must add a few drops of acetic acid, so as to give it a slightly acid reaction, whereby not only is the alkalescence of the fluid prevented, but a much more perfect separation of the coagulable matters is effected. If the residue of the fluid from which the coagulated matters have been filtered be extracted with cold alcohol, and the solution rapidly evaporated, so as to cause the chloride of sodium (taken up by the cold alcohol) to separate as much as possible in crystals, on then bringing a drop of the mother-liquid in contact with nitric acid under the microscope, we shall observe the commencement of the formation of the rhombic octohedra, and the hexagonal tablets, in which, if the investigation is to be unquestionable, the acute angles (= 82°) must be always measured. After the determination of the nitrate, we may also obtain the oxalate, and submit it to microscopic examination. A good crystallometric determination yields, however, the same certainty as an elementary analysis, which, in these cases, would never or extremely seldom be possible." (Vol. i, pp. 164-5.)

"The investigations of Marchand have thrown much light upon this subject [the seat of the actual formation of urea]. This accurate observer could only recover 0·2 of a gramme of urea from 200 grammes of serum, to which 1 gramme of urea had been added. He shows that, even if the urea were only separated from the blood at the end of each successive hour, it could not have accumulated in such quantity as to have been discoverable by the present mode of investigation. The following consideration will give us an idea of the small quantity of urea which, according to Marchand's hypothesis, at the most can accumulate in the blood in one hour:—From the experiments of Ed. Weber, which I have in part confirmed, we may assume that there are, in an adult man, at most 6 or 7 kilogrammes [16 to 19 pounds] of circulating blood. Now, if, in twenty-four hours, 30 grammes of urea are discharged, at most only 1·25 grammes could accumulate in one hour in the whole mass of the blood; so that only 0·021$_8$ could be contained in it. This minute quantity can, however, as we have already shown, only be detected in operating on very large masses of blood and by the aid of the microscope. Hence it is easy to understand why, during my experiments with an animal diet, while the urine was loaded with urea, none of this substance could be discovered in the blood.

"If it be now established that the urea is not primarily formed in the kidneys, the question still remains to be answered, whether it is produced in the circulating blood or in the individual living organs, (as, for instance, the muscles,) and from what materials it is principally formed. In the present state of our knowledge, we may answer, that the urea is formed in the blood, and that it is produced from materials that have become effete, the detritus of tissues, as well as from unserviceable and superfluous nitrogenous substances in the blood. No animal tissue presents such vital activity, is so much used, and is so rapidly worn out, as muscular tissue; it is in this tissue that the metamorphosis of matter proceeds most rapidly and abundantly; and yet, in the large quantities of muscular fluid on which Liebig worked, he could detect no trace of urea, although he found substances from which he could produce urea artificially. We must therefore assume, that these substances, as creatine and probably inosinic acid, are decomposed in the blood, by the action of the alkalies and of free oxygen, into urea and other matters to be excreted. Moreover, my experiments, showing that the superfluous nitrogenous food which enters the blood, and the fact that caffeine, glycine (Horsford), uric acid, and alloxantin (Wöhler and Frerichs*), soon after they have been taken, perceptibly increase the amount of urea in the urine, support the view that urea is formed in the blood. It is impossible to suppose that this nitrogenous food is first converted into tissue, and subsequently into urea, &c.; for we cannot think that a process occurs here, analogous to that exhibited by the percussion-apparatus of the Physicists, where a certain number of parts effecting a percussion give rise to the repulsion of an equal number of parts. Hence the conversion of this matter can occur in no other place than in the circulating blood, and therefore it is here that the urea must be formed.

"That the urea is formed from nitrogenous matters could not be doubted, even if it did not contain nitrogen (and that in so large a quantity); for it is especially after the use of highly nitrogenous food, that we find an augmentation of its quantity in the urine. If, however, we should further inquire,—from what substances is it produced, and what tissues principally contribute to its formation?—we could not, in the present state of our knowledge, give any satisfactory answer to this question. All that we know is, that urea is a very general product of the decomposition of nitrogenous matters, both naturally within the animal body, and artificially in the laboratory of the chemist. We have already said enough to show, that urea is so common a product of the decomposition of nitrogenous bodies, that we could hardly any longer enumerate it among true organic substances, if we tried to establish a distinction between organic and inorganic matter. Moreover, when we treat of uric acid, we shall show, that, in all probability, a great part of the urea separated by the kidneys from the blood is the product of the decomposition of that acid." (Vol. i, pp. 172-4.)

We find nothing new in the observations on *xanthine*, or *uric oxide*, as it is, perhaps, more commonly termed in this country. We may, however, take this opportunity of remarking, that our distinguished countryman, Dr. John Davy,† believes that the urinary secretion of scorpions and

* Ann. d. Ch. u. Pharm., Bd. 65, s. 337-8.
† Edinb. New Phil. Mag., vol. xl, p. 338; and vol. xliv, p. 125.

spiders consists, for the most part, of this substance; Will and Gorup-Besanez* have recently found a closely allied substance, *guanine*, in the excrements of spiders. The substance discovered by each is doubtless the same; but whether it be xanthine ($C_5H_2N_2O_2$) or guanine ($C_{10}H_5N_5O_2$), we cannot pretend to decide.

Closely allied to these substances is *hypoxanthine*, which was discovered about a year ago by Scherer, whilst investigating the cause of the acid reaction of the fluid of the spleen. His researches are not yet fully published; but we may observe, that, in this fluid, he found uric acid and hypoxanthine in considerable quantity, certain volatile acids, but no creatine. It is a white, crystalline, pulverulent substance, occurring not only in the spleen, but also in the heart of man and the ox. Its formula is $C_5H_2N_2O$. Hence it is xanthine, *minus* one equivalent of oxygen. Its close affinity to xanthine is, moreover, rendered certain by the fact, that both furnish, when treated with nitric acid, the same yellow body, which acquires a red tint on the addition of potash. Its occurrence simultaneously with uric acid, to which it is so closely allied in composition, is also very interesting and suggestive.

The *guanine*, to which reference has been made, is a peculiar substance, originally discovered by Unger in guano, and at first mistaken by him for xanthine. Little is known regarding its physiological relations.

Taurine is the next substance of this group to which we shall refer. It crystallises in colourless hexagonal prisms, contains 25·6 per cent. of sulphur, and is represented by the formula $C_4H_7NS_2O_6$.

"Taurine has never been found isolated in the healthy organism; it appears to be contained preformed in normal bile, and to occur there as a conjugate of the already described cholic acid; at all events, it only occurs in an isolated state in decomposed or morbid bile. After the removal of the mucus from the bile, its only sulphur-compound in those animals in which sulphur occurs, is taurine conjugated with cholic acid. At the present time, we know, by the researches of Bensch, that sulphur exists in the bile of the ox, the sheep, the fox, the bear, the dog, the wolf, the goat, the domestic hen, and certain fresh-water fish; and Schlieper has found it most abundant in the bile of serpents. From the bile of the pig, Strecker and Gundelach were unable to obtain taurine, and they found no sulphur in it, although Bensch had detected a small quantity. Doubts have been expressed, whether sulphur, and consequently taurocholic acid, exists in human bile; but Gorup-Besanez has so completely set this point at rest, that my evidence, founded on the crystallometric determination of taurine, artificially obtained from human bile, is superfluous. In diseased bile, taken from the dead body, taurine is especially found, when, as is sometimes the case, the bile has an acid reaction; thus, Gorup-Besanez found taurine in the bile of a person who had died from arachnitis.

"Although some of the products of the decomposition of bile occur in the excrements, especially in cases of diarrhœa, taurine has never yet been found there: neither has it been detected in bilious urine." (Vol. i, p. 187.)

The Third Section—the CONJUGATED ACIDS—includes the consideration of picric acid, (chosen partly as a good illustration of this class, and partly because it is a very frequent product of the decomposition of different nitrogenous substances by nitric acid,) hippuric acid, uric acid, inosinic acid, glycocholic acid, hyocholic acid, and taurocholic acid.

Most of our readers are doubtless aware, that Liebig has demonstrated

* Ann. d. Ch. u. Pharm., Bd. 69, s. 117.

that *hippuric acid* is an ingredient of normal human urine. According to his original statement, (and we are not aware that he has ever modified it,) it occurs in the same quantity as uric acid; while, according to Dr. Golding Bird's observations, made shortly after Liebig's discovery, the hippuric acid usually stands to the uric acid in the ratio of 1 : 3. Our own experience accords rather with the statement made in the third edition of the 'Urinary Deposits,' that there is nothing like a fixed ratio between the hippuric and uric acids, but that the former exists in less quantity than was originally supposed.

It seems to occur in excess in acid febrile urine, whether the fever be typhus or be associated with pneumonia or any other morbid process. Before hippuric acid was discovered in healthy urine, Lehmann had detected its presence in diabetic urine; and, as he has never failed in finding it in such urine, and as it has also been found there by Ambrosiani, Hünefeld, and others, it is probably never absent. Lehmann adds, that, in the strongly acid urine which is sometimes passed in fevers, the acid reaction is, in a great degree, dependent on the hippuric acid; and that we can often obtain the most beautiful crystals of this acid from the ethereal extract of such urine, even without the preliminary addition of an acid.

He refers to the two well-known cases of *hippuria*, recorded by Bouchardat and Pettenkofer, and (somewhat inaccurately) to a case briefly referred to by Dr. Golding Bird, in his paper in the 'Medical Gazette,' on hippuric acid. He seems unacquainted with Garrod's case, and makes no reference to a case witnessed by Frerichs,* where the patient was a man suffering from inflammation of the lungs, whose sole diet was a mucilaginous drink. He has failed in detecting any connection between the quantity of excreted hippuric acid and any definite group of symptoms.

The chapters on *Uric acid* and on the *Acids of the Bile* are full of most valuable matter; but we must proceed to the HALOID BASES AND HALOID SALTS.

The subjects discussed in this Section, are oxide of lipyl (C_3H_2O), glycerine ($C_6H_7O_5 . HO$), the fats or salts of oxide of lipyl (stearin, margarin, olein), and the lipoids or non-saponifiable fats (cholesterin, serolin, castorin, and ambrein). The Section (extending over twenty-six pages) on the physiological relations of the fats, is one of the most valuable of the whole book. It does not, however, easily admit of condensation, and we must pass it over with the remark, that those of our readers who are acquainted with German, will do well to study it with care.

The NON-NITROGENOUS NEUTRAL BODIES, grape-sugar or glucose, and milk-sugar, constitute the next of Lehmann's divisions; and to these we must add inosite or muscle-sugar, recently discovered by Scherer.

The following is the best method of applying Trommer's test for the discovery of *glucose* or *diabetic sugar* in the animal fluids. Caustic potash must be added to the suspected fluid, which, if there is a great precipitate, should be filtered. An excess of potash does no harm. Sulphate of (oxide of) copper, in the form of a dilute solution, must be gradually added, which causes a precipitate, that disappears when the mixture is stirred or shaken. As the quantity of oxide of copper that is soluble, is proportional to the quantity of sugar that is present, very little sulphate of

* Handwörterbuch der reinen und angewandten Chemie. Von Liebig, Poggendorff, und Wöhler. Art. *Harn*. Dritter Band, s. 785.

copper should be added at a time, when we suspect that only a little sugar is present. If the azure-coloured solution thus obtained be allowed merely to stand at rest for some hours, we observe a deposit of a red or yellow powder (of suboxide of copper), such as is obtained at once on boiling. If we do boil the solution, it should, at all events, not be boiled for any length of time, since there are many substances, (for instance, the albuminous bodies,) which, on prolonged boiling, separate suboxide of copper from alkaline solutions of oxide of copper.

If we are examining urine with very little sugar, or searching for that substance in some other animal fluid, we must, in the first place, obtain an alcoholic extract of the solid residue, and dissolve this residue in water, before applying the potash and sulphate of copper. By this means, we can generally be successful; if, however, the quantity of sugar is extremely minute, as, for instance, in chyle, blood, the contents of eggs, &c., it is expedient to neutralise the aqueous solution, with a little dilute acetic acid, before evaporating it, in consequence of the solubility of the albuminate of soda or casein in alcohol.

In connection with the physiological relations of grape-sugar or glucose, we may observe, that it has been found, both by Trommer and Lehmann, in the chyle of horses fed on amylaceous matters; in the blood of dogs fed on potatoes (Magendie); in both the white and the yelk of eggs (Winkler and Lehmann); and in the tissue of the liver. Its presence in the liver was first recognised by Bernard and Barreswil; and, in their experiments, they included the livers of animals taking neither saccharine nor amylaceous food. Lehmann has experimented on the livers of frogs with a similar result; and we learn from Liebig and Kopp's 'Annual Report,' for 1847-8, that experiments, conducted in the Giessen Laboratory, have confirmed the statements of the Paris observers, both in reference to the livers of graminivorous and herbivorous animals.

We are not aware that any one has confirmed the subsequent statement of Bernard, that, in pricking a certain spot in the fourth ventricle of rabbits, the urine and the blood, in the course of an hour or two, contain large quantities of sugar!

There is nothing in the observations on *milk-sugar* calling for special remark. It is, however, worthy of notice, that Braconnot has, very recently, (since the publication of Lehmann's first volume,) detected this form of sugar in the cotyledons of the seeds of vegetables.

"I believe," he adds, "that I have satisfactorily demonstrated that the cotyledons of the acorn contain all the elements of milk; as we not merely find there milk-sugar, but likewise a considerable amount of caseous matter, a slightly nitrogenous extractive substance, a large proportion of phosphate of lime, and likewise the soluble salts which occur in milk; and lastly, a fatty substance, which, however, does not possess the consistence of butter; but this, as is well known, varies even in the milk of the same animal."

Scherer, in his investigations on the fluid of the muscular tissue, has discovered a crystalline substance with a decidedly sweet taste, to which he has given the name of *inosite*, (from ἴς, ἰνός, muscular fibre.) In its anhydrous state, it is represented by the formula, $C_{12}H_{12}O_{12}$; but, on solution in water, it recrystallises with 16 per cent. of water; thus corresponding with the formula, $C_{12}H_{16}O_{16}$. It does not give an indication of sugar with Trommer's or Pettenkofer's tests; nor does it undergo alcoholic fermentation under the ordinary conditions; but it enters into lactic and butyric fermentation, when flesh or casein is added to its solution. It is somewhat doubtful whether this body is actually contained preformed in the muscular fluid, or whether it is produced by the action of the chemical reagents that Scherer had employed, on any of the nitrogenous constituents.

The COLOURING MATTERS (hæmatin, melanin, bile-pigment, and urine-pigment,), and the EXTRACTIVE MATTERS, are discussed in a few pages, and need not detain us.

The NITROGENOUS HISTOGENETIC SUBSTANCES, occupy a Section of nearly one hundred pages. They are divided into the *protein compounds* (albumen, fibrin, vitellin, globulin, casein, vegetable gluten, legumin, tritoxide of protein,) and *derivatives of the protein compounds,* (glutin, chondrin, fibroin, chitin.) The chapters on Albumen, Fibrin, and Casein, abound in new chemical and physiological facts; and we only pass them over without extracting freely from them, because we believe that we shall have an opportunity of returning to the consideration of the protein-compounds in our review of the third and concluding volume.

We may remark, that Liebig* has recently shown that the idea is altogether erroneous, that the fibrin of muscular tissue is identical with the fibrin of the blood. The two substances behave quite differently when immersed in water containing a little hydrochloric acid; moreover, the fibrin of blood contains more nitrogen than the fibrin of muscle, which approximates in its constitution to albumen.

The last Section of the first volume is devoted to the MINERAL SUBSTANCES OF THE ANIMAL BODY. It commences with a notice of the investigations of H. Rose, regarding the ash left on the incineration of animal matters, and of the applications of his method to special cases by Weber, Fleitmann, Weidenbusch, and Poleck. We do not enter into this subject, because there are many points in connection with it, which, in our opinion, are not satisfactorily established. We may refer our more chemical readers, who take an interest in this subject, to an excellent paper, very recently published by Strecker,† 'On the determination of the Inorganic Constituents of Organic Bodies.'

The mineral substances are arranged in the following classes:

1. Those whose physical properties are of service in the animal body.
2. Those whose chemical properties effect certain objects in the animal economy.
3. Those which are merely incidentally present in the body, do not specially influence any single process, and are soon again eliminated.

In the first class, he places water, phosphate of lime, carbonate of lime, phosphate of magnesia, fluoride of calcium, and silica. He seems to be unacquainted with the very decisive experiments by which Dr. George Wilson has proved the existence of fluorine (most probably in the state of fluoride of calcium) in the milk and the blood.

In the second class, we have hydrochloric acid, hydrofluoric acid, chloride of sodium, carbonate of soda, the alkaline phosphates, and iron.

The third class includes alkaline sulphates, carbonate of magnesia, manganese, alumina, arsenic, copper, lead, ammonia-salts, hydrocyanic acid, and sulphocyanic acid.

* Ann. d. Ch. u. Pharm., Bd. 73, s. 125. † Ibid., Bd. 73, s. 21.

As we are desirous of placing before our readers the principal novelties contained in Prof. Lehmann's Second Volume, and as our account of it will require as much space as we have already given to the First, we find ourselves obliged to postpone our notice of it to our next Number.

ART. X.

1. *The Pathology of the Kidney in Scarlatina. Illustrated by Cases.* By JAMES MILLER, M.D., Physician to the Western General Dispensary. —*London*, 1850. 8vo, pp. 178.
2. *On Scarlatina.* By J. W. TRIPE, M.D. (From the 'Medical Times,' 1848-9.)

IN our last Number, we passed under review those portions of Drs. Miller and Tripe's works, which treat of the Pathology of Scarlet Fever. In the present article we propose to lay before our readers the result of Dr. Tripe's investigations, respecting the influence exerted by appreciable atmospheric changes on the mortality from the same disease. This is by far the most valuable portion of his papers. After ten years' observation on the daily variation of the thermometer, barometer, &c., and the prevalence of epidemic diseases, Van Swieten wrote: "Inde circa morborum epidemicorum originem doctior non evaserim." Sydenham declared his time and labour spent in this matter, lost; while Ramazzini, in 1692, said: "Let each one think what he will, and let him ask of his imagination the influence of the manifest mutations of the temperature, of the seasons, on the production of morbid constitutions: as for me I see no constant relation, and each year I am as ignorant as ever in the matter." Huxham kept a register of the hygrometrical, barometrical, and thermometrical changes in the atmosphere of Plymouth, and then compared the state of the weather with reference to those particulars, with the state of the public health, as evidenced from his general observations on disease as it occurred in his own practice. He arrived at no definite result. All attempts of the kind were necessarily futile, until a much more accurate register, not only of the atmospheric changes, but also of the prevalence of, or the mortality from, disease was kept. The Registrar-General's returns have supplied some of the necessary materials; however, even with their aid, the subject is beset with difficulties, and for correct results to be obtained, the investigations must be conducted with logical precision.—The question Dr. Tripe has undertaken to answer, is susceptible of clear definition. What is the influence exerted by barometrical, thermometrical, hygrometrical, and electrical changes of the atmosphere, on the mortality from scarlet fever in the metropolis? Scarlet fever, as we have shown (see our last Number), is eminently a typical or proper stationary fever; it exhibits the same essential symptoms in all years and in all countries, arises from the same specific cause, and requires essentially the same treatment. The disease is the same, its mortality varies; the question is to determine the influence of certain conditions of the atmosphere on that variation. By the Registrar-General's returns, we learn the periods of the year when the highest number, the lowest number, &c., of deaths from the disease occur; and by the

Greenwich Observatory Reports, the exact conditions of the atmosphere with reference to temperature, &c., at the same periods.

The problem is stated—the terms used have a definite signification—the data for its solution are given; how are they to be applied?

The first element for the solution, is the actual mortality of the disease in a given place at different periods of time; the second element is given by the changes experienced by the atmosphere with reference to temperature, weight, &c., at the same place and at the same dates. Were it possible to find each of the component parts of the second element separate from those with which it is associated, the solution of the problem would be effected without difficulty; but they never are thus separated. The pressure, the temperature, the humidity, and the electrical state of the atmosphere, although they vary in the relation they bear to each other, are yet always associated. Here, then, is a solitary element, and a certain number of invariably associated elements; and the question is to determine the influence of *each* of those associated elements on the solitary element.

In the science of medicine, numerous problems essentially identical with the one above formularised, have from time to time arisen and been solved; and numerous other problems, identical in essence with it, yet await solution. The solitary element may be, as in the question Dr. Tripe has undertaken to answer, the mortality from a disease; the associated elements, certain atmospheric conditions;—or the solitary element may be, as in Mr. Hutchinson's admirable researches,* the vital capacity of the thorax; and the associated elements, age, sex, height, weight, &c.;—or the solitary element may be, the weight of an organ; the associated elements, the age, sex, height, weight, &c., of the individual from whom that organ was removed;—or, again, the solitary element may be, the mortality from a disease; the associated elements, certain therapeutic agents, and the age, sex, &c., of the patient.

Now there are two modes in which such investigations may be conducted. From the written data before us, a series of observations may be abstracted, in which the first of the associated elements (we will suppose them to be four in number) varies greatly, the others remaining unchanged; then a second series of observations, in which the second of the associated elements varies, the first, as well as the third and fourth, remaining constant; then a third series, in which the third element varies, and the first, second, and fourth, remain without variation; a fourth series, in which the fourth of the associated elements varies, while the first, second, and third, remain fixed,—and yet further series of observations, in which all possible combinations of the associated elements occur; *i. e.*, one remaining constant, three changing; two remaining constant, and two changing, &c. &c.; and the influence which each of these alterations exercises on the solitary element be determined by comparison with its varying degree of frequency, extent, or other quality: *e. g.*, suppose that the influence of variations in the temperature, the weight, &c., of the atmosphere on the mortality from scarlet fever are to be determined, then we should have to find a series of periods in which all the other associated elements remaining the same, one, say temperature, varied considerably; and then to note the difference in

* Transactions of the Royal Medical and Chirurgical Society, vol. xxix.

the mortality from scarlet fever in those periods; and so on with the other associated elements, and with all their possible combinations.

Theoretically and practically, in certain cases, as in some of the researches of the natural philosopher, this method is perfect. But with reference to such a question as is here at issue, this objection lies against it—it is impracticable. The difficulty of finding the data for the calculation is insuperable; *e. g.*, in the case supposed, years, months, or weeks, in which all, save one, of the associated elements, are the same, the exceptional element at the same time varying considerably in degree; and *à fortiori* is the difficulty insuperable, of finding all possible combinations of the associated elements.

The second method is that of abstracting observations on each of the several associated elements, and then analysing the influence of each on the solitary element without reference to the other associated elements. By such analysis it would be discovered, whether any one of the associated elements exerted an influence so largely preponderating over the others, as to exhibit its preponderance, notwithstanding the opposing influence of one or more of the other associated elements; the sub-influence of the less influential might then be easily learned, by noting the periods in which the solitary element varied either less or more than the varying degree of the influential associated element would account for, and by noting which, how much, &c., of the other associated elements were at such times in access or the reverse. Or again, the influence of the most influential associated element being in a degree determined, an analysis of those observations might be made, in which that element remaining constant in degree, the solitary element was found to vary, and so the effect of changes in the other less influential associated elements might be determined: *e. g.*, let the object be to learn the influence of temperature, atmospheric pressure, &c., on the mortality of scarlet fever:—first, the influence of temperature would have to be calculated as though it were unassociated, then that of the barometrical condition alone, and so on of the other associated elements; thus it would be discovered if either of these exhibited a largely preponderating influence on that mortality; and if so, then, when exceptions or deviations from the seeming rule thus discovered might occur, the modifying power of the other associated elements would have to be considered.

To apply these rules for analysing observations, in order to solve medical problems, to other cases: Dr. Hutchinson wished to ascertain the influence exerted by certain associated elements—height, weight, age, &c., on a solitary element—the vital capacity of the thorax. Now, in the search for this result, he did not take one series of men, whose height, weight, and age, were the same; and another set of men, in whom one of these associated elements varied; and then note the influence exerted by the plus or minus condition of this one, over the other associated elements on the vital capacity: because, even in this case, it would not have been possible to obtain the number of men requisite for determining the question, in whom the required combinations of the associated elements existed. But what he did, was to consider each one of the associated elements separately, and calculate the influence of variations in that one on the vital capacity; and then, having discovered the extraordinary influence of height on the solitary element in question, he subsequently, by calculating the power of the less influential associated elements, accounted for any apparent want of the due relation between height and vital capacity, in particular cases.

Although theoretically less perfect than the first method, this is the only one possible with reference to the questions here at issue. The difficulties of working out problems in the mode we have laid down, are considerable; and, as a consequence, they are rarely thus solved. Vague generalisations from vague mental impressions are substituted for rigorous induction, and an *ipse dixit* is considered ample proof. Or, if numbers be used, and results are said to have been obtained by their employment, on examining those results, and the process by which they have been arrived at, it is found that the influence of one only of the associated elements has been considered; and too often, that the solitary element, even, has not been identical in every case, in which the influence of particular associated elements has been considered to have been determined. Hence the fallacies of the therapeutical statistics of the homœopathists; by the same name they do not signify the same solitary element,—the same disease in all essential particulars; and when, by numbers, they attempt to prove the influence of their remedies, they fail to separate the associated elements, age, sex, &c.

We shall now lay before our readers the method pursued by Dr. Tripe, in solving the question he laid down for himself, and the results he has obtained. So far as his method of analysing the observations at his disposal goes, it is identical with that we have stated to be the practical one. Still, he might with advantage have carried his analyses a little further, and have endeavoured to ascertain the modifying influence exerted by the several associated elements on each other, as determinators of the mortality from scarlet fever. He has ably analysed the influence of each of the associated elements, considered *per se;* but he has not entered on the modifications which the combination of these elements must have exerted; he has not compared the influence of one in diminishing the mortality with the influence of another in increasing it; moreover, as we shall subsequently point out, he has not taken into account certain sources of fallacy.

The average height of the barometer (taken from the readings of fifteen years) during the first three months of the year, is about 29·792.

In estimating the effects of *Barometrical changes* on the mortality from scarlet fever, Dr. Tripe takes 29·792 as the mean height of the mercury for the Winter quarter, and uses that number as a standard of comparison. He finds, that during January, February, and March, 1844-45-46-47-48, the least mortality from scarlatina was coincident with the highest barometrical readings. Dr. Caspar's observations (See Brit. and For. Medical Review, Vol. XXIV) showed, that with reference to *all* diseases, the reverse of this was true.

By a still closer examination of the data afforded by the observations at Greenwich, Dr. Tripe says, it will be seen that the highest daily barometrical mean for the winter quarter, in the three years 1846-47-48, occurred in 1846, when it reached as high as 30·511 in.; while in 1848, it never exceeded 30·370 in. In 1846, it never fell below 29·009 in.; in 1848 it fell as low as 28·652 in. The variation between the highest and the lowest daily means, in 1846, amounted to 1·502 in.; in 1847 to 1·462 in.; in 1848 to 1·718 in.; whilst the highest variation in one week, in 1846, was 0·743 in.; in 1841, was 1·352 in.

The deaths from scarlet fever, in the winter quarters of 1846-47-48, respectively, were 221, 196, and 615.

A high barometrical pressure in the winter quarter of these three years, contrary to the results obtained by Dr. Caspar, with reference to *all* diseases, "was not accompanied by a high mortality from scarlatina; while considerable and frequent variations have a decided influence in producing an increased mortality."

From a similar examination of the data afforded by the observations at Greenwich for the three months, April, May, and June, it appears that in the Spring quarter also, a high barometrical pressure is frequently coincident with a low mortality from scarlatina.

The influence of variations in the barometrical pressure for the Summer quarter cannot be very clearly made out. In the quarter ending September 30th, there appears to have been no very clear relation between the mean pressure of the barometer and its variations, and the mortality from scarlatina.

In the Autumn quarters of 1844-45-46-47, a low barometrical pressure appears to have tended to diminish the mortality. This influence, however, was not very strongly marked.

Between August 7th and November 13th, 1847, the deaths from scarlet fever gradually increased from 16 to 71 in the week. During the same time, the variations in the barometrical pressure were unusually great.

The conclusion at which Dr. Tripe arrives is, that—

"A high barometrical pressure does not exert that injurious influence on the mortality of scarlatina that it exerts on many other diseases, and that a low barometrical pressure is not so favorable as it is on some other maladies, while excessive and long-continued variations exercise an adverse influence on its progress."

The influence of *Temperature* on the mortality from scarlet fever has been most carefully examined by Dr. Tripe; each month in the years 1840—48, inclusive, has been separately considered, and then the several months of each of those nine years collected into quarters.

Had the Registrar-General's returns been used without correction, it is evident that erroneous results would have been obtained, seeing that the area comprehended within the Bills of Mortality for the metropolis has been greatly extended since 1840, and that the population has increased at the rate of 1·55 per cent. per annum. Dr. Tripe has accordingly corrected the reports for this increase of area and population, and has drawn up a series of tables, each of which shows, in the first column, the corrected number of deaths from scarlet fever in the metropolis, for four weeks in each month of the nine years 1840-1848 inclusive; in the second column, the number of deaths in each month corresponding to 100 in the preceding month; and in the third, the mean temperature for each month. As no separate return was made by the registrar-general for December, 1839, and consequently the comparative mortality of December, 1839, and January, 1840, cannot be estimated, the table for the latter month is imperfect, and we shall take the table constructed for the month of February, to illustrate the mode in which the results have been obtained.

	No. of deaths from Scarlatina, corrected.	No. of deaths to 100 in each preceding month, corrected.	Mean temperature.
1840	197	96	39°5
1841	54	65	35 3
1842	43	138	40 8
1843	90	77	36 0
1844	167	81	35 2
1845	147	86	32 7
1846	91	114	43 9
1847	66	104	35 5
1848	195	108	43 2
Mean	116·6	96	38 0

By this table we see, that in February, 1841, 54 deaths from scarlatina would have occurred in the metropolis, had it been as large and as densely populated as in 1848; that the proportion existing between the number of deaths that happened in the first four weeks of January, 1841, and the number of deaths in February, 1841, was as 100 for January to 65 for February; and that the mean temperature of February, 1841, was 35·3. We see, also, that the mean number of deaths from scarlatina, corrected, as before stated, for the month of February, in the nine years before referred to, was 116·6; the mean comparative mortality, 96; and the mean temperature, 38°0.

The number of deaths in the months of February of 1840-44-45-48, was above the mean; and in the same months of 1841-42-43-46-47, the mortality was below the mean. Of the five Februaries in which the mortality was *below* the mean, the temperature of that month in 1842 and 1846 was above the mean, in 1841-43-47 below the mean; while of the four Februaries in which the mortality was *above* the mean, the temperature of two, viz. those of 1840-41, was above, and the temperature of two, viz. 1844-45, below the mean.

Passing from the consideration of the actual mortality of each month, let us proceed to compare the relative mortality for each month with the mean temperature. The mean comparative mortality, as the table shows, is 95; we see that February 1840-42-46-47-48, had a comparative mortality higher than the mean; February 1841-43-44-45, below the mean; or 5 were plus, and 4 minus. Of the 5 *plus*, 4 had a temperature above the mean; and the increased mortality in the exceptional case was only 4 per cent. Of the 4 *minus* Februaries, all had a temperature below the average.

The excess in the comparative mortality, however, did not occur in the same ratio with the excess of the temperature; this want of relation was very strongly marked in 1842, when the excess of temperature was only 2°5, while the number of deaths increased from 29 to 43; but then the previous January had been unusually cold, so that the increase of temperature for this month was 7°9, the ordinary increase being 1°6.

The mean comparative mortality for the month of February in 1841-43-44-45-47, in each of which four months the temperature was below the average, was 82·6; while the mean comparative mortality for the same months of 1840-42-46-48, in each of which five months the temperature was above the average, was 114.

From this analysis it is evident, that a warm February is favorable, whilst a cold February is unfavorable, to the spread of scarlatina; or, to speak more strictly, to a comparatively large mortality from that disease.

But in order to bring out a very important fact, viz., that the mortality has reference not only to the absolute temperature of the month, but to the comparative temperature, we have constructed the following table from two of Dr. Tripe's:

Years marked + or —, according to comparative mortality.	Jan.	Feb.	Comparative temperature of February.		Comparative mortality of February, Jan. being 100.
— 1840	40°2	39°2	...	— 1°0	— 4
— 1841	33 6	35 3	+ 2°3	...	— 45
+ 1842	32 9	40 8	+ 7 9	...	+ 38
— 1843	39 9	36 0	...	— 3 9	— 23
— 1844	39 1	35 2	...	— 3 9	— 19
— 1845	38 3	32 7	...	— 5 6	— 14
+ 1846	43 7	43 9	+ 2	...	+ 14
— 1847	37 0	35 5	...	— 1 5	+ 4
+ 1848	34 5	43 2	+ 8 7	...	+ 8

If, then, we take the mean temperature for each January and February for 1840—48 inclusive, we shall see that the mean temperature of each February was in excess over that of the preceding January in 1841-42-46-48; and that in three out of the four Februaries the comparative mortality was also in excess; while in the exceptional case, viz., the February of 1841, although a rise in the mean temperature of 2°3 was coincident with a diminution of mortality, yet, that the mean temperature of the month was below the average temperature of the nine Februaries here examined, and also that the preceding January had been extremely cold, *i. e.*, 4 degrees below the average temperature of the month, in fact, the coldest January save one of the nine years. Moreover, the actual mortality in January had been considerably below the average. We also see from this table, that the mean temperature of the month of February was below that of the preceding January in 1840-43-44-45-47, and that the comparative mortality was also less in four out of these five Februaries; while with reference to the exceptional case, viz., the February of 1847, when a fall in the temperature was coincident with an increase in the mortality, it may be observed, that the fall in the temperature was only 1°5, and that an epidemic was impending.

Similar analyses of the Registrar-General's returns for January and March, give the following results:

"The number of deaths from the disease in the month of January are greatly dependent on the temperature,—a cold January having a small mortality, and a warm January a comparatively large mortality; or, in other words, a great reduction in the temperature of the month from that of December causes an unusual reduction in the number of deaths from scarlatina."

The mean comparative mortality for the four Januaries, the mean temperature of each of which was *below* the average temperature of the Januaries of the eight years, was 72; the mean mortality for the four Januaries, the mean temperature of each of which was *above* the average temperature of the Januaries of the eight years, was 86:

"When the temperature of March is higher than that of the previous February by an amount greater than the ordinary excess, the comparative mortality will also be in excess, provided that the disease has not been epidemic in the previous year; and when the temperature is below the comparative and monthly mean, the comparative mortality will also be below the mean."

Having thus discussed the three months separately, our author proceeds to consider them collectively as comprising one period, viz., the Winter quarter; constructing for the purpose a table for the quarter, having the same number of columns and headings as the table previously quoted for the month of January. The mean temperature in this table of course signifies the mean temperature of the quarter; and the comparative mortality, the proportion of deaths that occurred in any given quarter, compared with the number of deaths that occurred in the preceding quarter; the latter, for the sake of comparison, being represented by 100. Thus, 620 deaths happened from scarlet fever in the quarter ending March 31st, 1849,—1088 having occurred in the quarter ending December 31st, 1839; the latter being represented by 100, the comparative mortality of the former will be represented by 57, 100 bearing the same relation to 1088, that 57 does to 620.

The result obtained is, that the mean comparative mortality for the five warm winters amounted to 71, and for the four cold winters only to 53.

The difference, however, as Dr. Tripe suggests, is greater than it would have been, had not the two years 1841 and 1845, which followed the epidemics of 1840 and 1844, been included in the cold winters; excluding these, the comparative mortality for the warm winters would be as before, 71, while that for the cold winters would still be only 65.

In the month of April, the connection between the comparative mortality and the mean temperature becomes less defined. "Still it would appear," Dr. Tripe says, "that, on the whole, the balance is somewhat in favour of a cold April being less fatal than a warm one." The variations that occur in the mean temperature of the month of May, appear to exert no influence over the mortality:

"We can, therefore, conclude, that any temperature between 50°0 and 59°0 (the extreme means for the months of May), is equally, or almost equally, favorable to the mortality from scarlatina, and that some other active agent must be present to cause any increase."

The same want of relation exists between variations in the mean temperature of the month of June, and the comparative mortality.

Looking at the three Spring months—April, May, and June—collectively, it is quite evident that variations in the temperature exercise much less influence on the mortality of scarlatina, than they do during the Winter months, *i. e.*, January, February, and March. Still the mortality is somewhat less in a cold than in a warm spring; and, therefore, Dr. Tripe states, "a temperature plus the mean has a slight tendency to invest the disease with an epidemic form." It would appear, moreover, from the statistical inquiries before us, that "if the mortality for May becomes lower than that for April, and again rises considerably in June, the disease becomes more or less epidemic in the following months."

In July, the data render it probable that an unusually high temperature has a decided tendency to increase the mortality from scarlet fever; yet the comparative mortality of the month ordinarily increases over that of June, even if the temperature decreases; consequently, the sole agent in producing this increase cannot be a variation in the temperature.

The mortality from Scarlatina in August seems still less under the influence of temperature; and in September we find that a low temperature is attended with a greater increase in the number of deaths over that of August, than occurs in a warm September.

After carefully examining all the data for each of the Summer months, separately and collectively, Dr. Tripe concludes, that "a temperature in the Summer quarter *below* the average is rather more favorable to an augmentation of the deaths from scarlatina, than a temperature above the average."

In October, as in the Winter months, a high temperature and a high rate of mortality from scarlatina are coincident; and, *vice versâ*, in November and December the relation between high temperature and a high rate of mortality is less evident. Yet, taking the three Autumn months, October, November, and December, as a whole, "there cannot," Dr. Tripe concludes, "be much doubt but that a warm Autumn induces an increased mortality, whilst a cold Autumn causes a comparative decrease in the number of deaths."

Considering each year as a whole, it appears that scarlatina is less fatal in April, and more fatal in October, than in any other month in the year; and that the number of deaths increases from May to October, inclusive, and decreases from November to April, both included.

The following are the important conclusions which Dr. Tripe draws from his very able, careful, and lengthened examination:

1st. That a temperature below 44° 6 is adverse to the progress of scarlatina, whilst a temperature above that point increases the mortality.

2d. That the greatest mortality for the year happens in those months when the mean temperature ranges between 49° 6 and 56° 9.

3d. That the greatest increase in the comparative monthly mortality happened, in the nine years 1839—48 inclusive, when the mean temperature ranged between 49° 6 and 56° 9; and that the greatest decrease in the comparative mortality happened, when the mean temperature of the month was below 40° 0.

4th. That the increment in the mortality in no instance occurred in the same ratio with the increase of temperature; nor did a diminution of the emperature take place in the same proportion as the decrease in the number of deaths; the closest correspondence happened in the months of December, January, and February, when the mean temperature was 40° 0, or below.

In determining the influence of the *Humidity* of the atmosphere on the mortality from scarlet fever, Dr. Tripe, having ascertained the mean temperature for the month, calculates the quantity of moisture which air of that temperature would contain were it saturated with moisture; and then, taking that as a standard, he compares the actual quantity in the air, for each month, with that average.

Thus the average temperature of the air for the eight Januaries, 1841—8, inclusive, was 37° 4; had the air been saturated with moisture, it would have contained 2·80 grains of aqueous vapour in a cubic foot, which would have been indicated by 1·000; but as the mean degree of saturation was only ·911, the mean quantity actually contained in the air was only 2·57 grains in a cubic foot.

The mean comparative mortality, Dr. Tripe states, for the four months in which the mean humidity was above the average, amounted to 77 only; and for the four months in which the latter was below the average, it reached 88. "It is therefore evident," he adds, "that the ordinary decrease in the mortality from scarlatina which occurs in January is greater than usual, when an excess of moisture is present in the air, whilst it is less than ordinary when the air is unusually dry."

The following table will show the results obtained, by a similar examination of the different months in the eight years, 1841—8:

	Jan.	Feb.	March.	April.	May.	June.
Comparative mortality; the humidity of the atmosphere being *plus*	77	91	99	83	108	106
Comparative mortality; the humidity of the atmosphere being *minus*	88	100	83	93	129	137
	July.	**August.**	**Septem.**	**October.**	**Novem.**	**Decem.**
Comparative mortality; the humidity of the atmosphere being *plus*	112	105	114	120	85	77
Comparative mortality; the humidity of the atmosphere being *minus*	121	109	140	121	99	77

Thus, with the exception of December, in which the comparative mortality was the same for the months in which the degree of humidity was plus, and for those in which it was minus,—and March, in which a high rate of mortality was coincident with an excess of humidity,—"*the mortality was less in those months in which the degree of humidity was above the mean.*"

The influence of the *Electrical state* of the air on the mortality from scarlatina, is considered at some length; the conclusion drawn being, that "an active condition of the electricity of the atmosphere is much more favorable to life, as regards scarlet fever, than a passive condition of it."

For want of the requisite data, we suppose, for his calculations on this subject, Dr. Tripe limits his researches to the years 1846-47-48.

	Winter Quarters.	Spring Quarters.	Summer Quarters.	Autumn Quarters.
Mean comparative mortality with an *active* condition of the electricity of the atmosphere	88	103	127	119
Mean comparative mortality with a *passive* condition of the electricity of the atmosphere	94	119	129	102

The fact that 988 out of every 1,000,000 inhabitants of twelve large towns died from scarlatina during four years, while 478 only died in every 1,000,000 in twelve counties, we should attribute to the free intercourse which subsists among the poor of towns, and to overcrowding; Dr. Tripe thinks that it proves also the evil effects of imperfect drainage.

In his article on scarlet fever in the 'Cyclopædia of Practical Medicine,' Dr. Tweedie attempts to disprove the accuracy of the opinion expressed by Sir Gilbert Blane, and generally entertained by the profession, viz., that scarlet fever is more common before than after puberty. The Registrar-General's returns confirm the opinion of Sir Gilbert Blane; for, of 8133 deaths from scarlet fever that happened in the metropolis during the four years 1842—46, only 493 were of individuals more than 15 years of age. By far the greatest number of deaths from scarlet fever, as the tables compiled by Dr. Tripe show, happen "between the first and third birthdays, and next between the third and fifth. The next greatest mortality occurs in children under one year old, whilst in those above five years of age the mortality decreases as the age increases;" compared, however, with the total number of deaths from all causes, the mortality from scarlet fever is greatest between the ages of five and ten years.

The influence of sex on the mortality from scarlet fever is remarkable: during the first decennial period of existence, a larger proportion of males than females die from that disease; while in the second, third, fourth, and fifth decennial periods, the mortality is greatest in females.

With reference to the value of the mortality from any disease during a given period, as an index of the prevalence of that disease during the same period, Dr. Tripe remarks: "When a larger number of deaths occur in a given period, and in a given population, from a certain disease, we may be justified in assuming that the disease was more prevalent at that time."

This is true, however, only to a limited extent; especially with reference to chronic and to epidemic diseases, in which death is common from superadded lesion. For some conditions of the atmosphere favour the occurrence of local complications, such as bronchitis and pneumonia; and consequently a sudden change in the weather may raise the mortality very considerably, without having favoured the spread of the primary disease. Suppose, for instance, during the week ending the 7th of any month, 1000 persons suffered in a given population from scarlatina, and that of these 100 died; a sudden change in the weather may, by favouring the supervention, say, of bronchitis, raise the deaths during the week ending the 14th, (the number of fresh attacks being still 1000,) to 150. If, under these circumstances, the mortality were to be considered an evidence of the prevalence of the disease, a very erroneous conclusion might be arrived at; viz., that the cold and wet weather had favoured the spread of the fever, whereas, in reality, it had only increased the mortality by inducing intercurrent inflammation.

In using the Registrar-General's tables of mortality as an index of the prevalence of typical or proper contagious epidemic diseases, for the purpose of determining the influence of the atmospheric states in favouring or retarding their spread, it must also be borne in mind:

1st. That the Registrar-General's reports refer only to the deaths *registered* during any given period, and not to the deaths that have happened during that period; and the tables of the weather published by the same authority contain a statement of the atmospheric states during the period, say a week, in which the deaths were registered, and not that in which they occurred.

2dly. Supposing the largest number of deaths from scarlet fever to have happened during any given week, the weather most favorable to the spread of scarlet fever would not be that of the week in question; for of those who died during that week, the majority would have been ill more than seven days, and would have probably had the poison of the disease in the system incubating for seven days before the first symptoms of fever showed themselves. Consequently, they must have caught the disease a fortnight before death; and it is to the state of the atmosphere of that time that we must look, in order to ascertain the conditions favorable to the spread of scarlet fever. To determine, then, the influence of temperature, &c., in April, on the spread of scarlet fever, small-pox, or measles, the deaths registered between the middle of April and the middle of May should be taken, and not those registered during the month of April.

3dly. The occurrence of a large number of deaths among those suffering from any given epidemic disease, during the prevalence of particular atmospheric conditions, is no proof *per se* that such weather exerts any injurious influence over the primary disease in question; because this weather may have caused death by inducing an intercurrent disease, say pneumonia, in those labouring under the primary disease, in common with those previously in health, and not by any malign influence which it has exerted on the primary or essential blood-disease,—scarlatina, for example.

4thly. As a peculiar state of the recipient's constitution is essential for the development of the symptoms of either of the diseases in question, even though the virus of that disease exist around him in great quantity, and the constitution of the air be favorable to its propagation; and as the state of every man's general system is influenced not only by the "non-naturals" of the period in which he is existing, but also by those of the past, so it is essential that the appreciable atmospheric conditions of the months anterior to the spread of an epidemic disease should be taken into account. Thus, supposing that the same amount of the scarlet fever virus is constantly in the air, and is at all times equally active, but that it requires a state of the system, producible only by long-continued wet weather, for its poisonous effects to be manifested, then might tne epidemic commence its ravages on one occasion, if the wet weather had continued long enough to induce the supposed necessary state of constitution at the time, or shortly after dry weather had set in; and, on another occasion, supposing the wet to have continued a little longer than in the previous case, while the atmosphere was still loaded with moisture.

"Non possunt præsentes morbi cognosci, nisi ex præteritâ temporum constitutione, nec futura divinari, nisi ex præsentium consideratione."

We cannot conclude our notice of the very able examination of Dr. Tripe into the influence of appreciable atmospheric changes on the mortality, and by inference on the spread, of scarlet fever, without expressing our sense of the importance we attach to the establishment at the present time of the Epidemiological Society. It is only by the combined labour of the members of an Association, such as that presided over by Dr. Babington, that many of the questions which arise in the conduct of investigations into so difficult and complex a subject as the relation between the origin and spread of epidemic diseases and atmospheric changes, can be answered. Let any one attempt to determine with the requisite care what at first sight appears but one point, and problem after problem arises, each requiring solution before the question first raised can be answered; for the solution of each of these problems, table after table has to be compiled and arranged; in the end, perhaps, the inquirer finds some data wanting, and his time and labour will have been thrown away. Not so the time and labour of a Committee of the Epidemiological Society. Their resultless documents will be preserved in the archives of the Society, until fresh data shall be obtained; and then, although years may have elapsed, the work done will be so much saved to those who come after; or it may serve as a beacon, if not to direct others toward the attainment of the desired end, at least to warn them of the route they should *not* take.

ART. XI.

1. *A Practical Treatise on Diseases of the Urinary and Generative Organs in both Sexes.* Part I. *Non-Specific Diseases.* Part II. *Syphilis.* By WILLIAM ACTON, late Surgeon to the Islington Dispensary, and formerly Externe at the Female Venereal Hospital, Paris. Second Edition. —*London,* 1851. 8vo, pp. 693.
2. *Clinical Observations on Diseases of the Genito-Urinary Organs.* Part I. *Gonorrhœa and its Consequences.* By HENRY JAMES JOHNSON, formerly Assistant-Surgeon to St. George's Hospital, &c.—*London,* 1851. 8vo, pp. 358.
3. *On Syphilitic Eruptions, with Especial Reference to the Use of Mercury. Illustrated by Cases.* By THOMAS HUNT, M.R.C.S.E. L.S.A., &c. (In part reprinted from the 'London Journal of Medicine.'— *London,* 1851. 8vo, pp. 25.

THE three works we have placed together at the head of this article are of a very different class. That of Mr. Acton is intended as a complete systematic treatise on the subjects discussed. Mr. Johnson's is entirely devoted to Gonorrhœa and its immediate consequences, being the first of three separate publications on some of the diseases of the genito-urinary organs. The third is a pamphlet on one practical principle. Mr. Hunt believes that the non-mercurial treatment of syphilitic eruptions is more dangerous to the patient and his offspring, than even the unsparing abuse of mercury. With the happy conviction that he has discovered how this mineral may be so given as to be both harmless and effective, he presents us with his pamphlet.

Mr. Acton having found a Second Edition of his work on 'Venereal Diseases' called for, has enlarged the First Edition to such an extent, that his present production may be considered as a new work. He has added Chapters on Spermatorrhœa, Impotence, Infiltration of Urine, and Impermeable Stricture, and has accordingly altered his title. His amended title, however, by no means correctly designates the contents of his book. A 'Treatise on the Diseases of the Urinary and Generative Organs in both Sexes,' non-specific as well as specific, might justly be supposed to contain chapters on the diseases of the kidneys which lead to the formation of urinary deposits and calculi, and the presence of abnormal constituents in the urine,—on renal inflammation and abscess,—on diseases of the ureter and testis,—of the ovaries, fallopian tubes, and uterus. Yet, in Mr. Acton's work, nothing is said on these subjects, unless in connection with some venereal affection. His division of specific and non-specific diseases is one purely arbitrary. Both are supposed to be venereal diseases. Syphilis is the only disease he considers to be *specific*, or dependent upon a special cause or distinct morbid poison. By non-specific disease he means "those affections which follow sexual intercourse, reproducing themselves daily, often contagious, but not depending upon a special cause —non-inoculable. Under this general term is included *blennorrhagia*, and its consequences; *excoriations*, *herpes*, *eczema*, and every other affection the result of sexual intercourse, not included under the second order, or *specific affections*." (p. 25.)

Mr. Acton adopts the term *blennorrhagia* to express the principle that the muco-purulent discharge may arise from many irritating causes altogether independent of contagion; but he retains the term *gonorrhœa* "for the well-known discharge from the male urethra, in deference to the long recognised use of the word." (p. 27.) He admits contagion as a cause of gonorrhœa; but does not admit it to be a specific disease, unless it produce a specific ulcer on inoculation, in which case, he presumes upon the existence of urethral chancre; in this, as in most other points of theory and practice, being a follower of M. Ricord. Now, we consider, that a disease like gonorrhœa, clearly contagious, characterised by the discharge of muco-purulent matter which is capable of exciting a peculiar form of suppurative inflammation when applied to the mucous membranes of the genital organs of either males or females, is eminently entitled to the appellation *specific*. It may or may not be syphilitic, because it may or may not be dependent on, or complicated by, urethral chancre; but it is to all intents and purposes a specific disease, depending on the application of a specific morbid poison. The correctness of this view is more apparent, if, like Mr. Acton, we distinguish discharges produced by common irritating causes by the term *blennorrhagia;* a distinction which may become general, although, we think, Mr. Acton has assigned far too high an importance to these common causes. He gives a long list of predisposing and exciting causes, talks of age, sex, temperament, climate, and locality; food, clothing, and exercise; local irritation, derangements in the general health, inattention to cleanliness, worms, &c., to show that blennorrhagia may be produced independently of contagion;

and we are not disposed to deny that it is so produced occasionally, although, we think, Mr. Acton has far overrated the frequency of such production. That cancerous disease, or simple ulceration of the uterus, and some irritating varieties of leucorrhœal discharge, may excite a species of gonorrhœa in the male who has connection with a female suffering from those diseases, no one can dispute; but we doubt very much, if one per cent. of the patients treated either in our hospitals, or in private practice in London, can attribute their disease to any such cause. Indeed, it is very common for women so affected to continue conjugal intercourse, without any ill effect on the husband. And really those discharges produced by errors in diet, with all Mr. Acton's predisposing causes to assist them, are as different as they can be from gonorrhœa, as it is generally met with.

We hold the doctrine that the muco-purulent matter of gonorrhœa acts, not as a peculiar morbid poison, but (as Mr. Acton asserts) as a simple chemical irritant, to be altogether untenable. We will defy Mr. Acton, or any other man, to point out any chemical irritant, animal, vegetable, or mineral, solid, liquid, or gaseous, in any known form of combination, which, applied to the mucous membrane of the urethra or conjunctiva, will produce gonorrhœa or gonorrhœal ophthalmia, with anything approaching to the certainty with which these diseases are produced by the contact of gonorrhœal matter. We by no means agree with M. Cazenave and others, who follow Hunter in holding that chancre and gonorrhœa are the results of the same specific poison. On the contrary, we believe that there is no identity between the two poisons. The *fœda mulier*, the woman affected with gonorrhœa, was known long before syphilis was recognised. The coexistence of the two diseases is the exception, their separate existence the rule; their course, and the results of inoculation afford a mass of evidence which is absolutely conclusive against the identity of the two poisons. We object also to the doctrine of M. Ricord, and consider his inferences by no means correct, when he asserts, that in cases in which inoculation with gonorrhœal matter produces what he calls a true syphilitic pustule, there must be a *chancre larvé*, a syphilitic sore hidden in the urethra, although it cannot be detected. This is often a pure assertion altogether incapable of proof. M. Castelneau, like Ricord himself, has succeeded in producing what they call the true syphilitic pustule from gonorrhœal matter. If inoculation fails, he considers it a proof that the disease is non-syphilitic. If it succeeds, he indulges in the vision of urethral chancre. That urethral chancre is common; that M. Ricord deserves credit for pointing it out; that his demonstration of chancre deep seated in the female organs, has cleared up many difficulties from the path of the inquirer into the pathology of venereal diseases, we fully and freely admit; but we are quite as firmly convinced that he has departed from all the rules of sound logic in the inferences he has drawn from his experiments on inoculation. The inferences may be correct; but he has not demonstrated their truth, nor has Mr. Acton strengthened the position of his leader. He has been surgeon to a dispensary, and appears to have had free access to St. Bartholomew's Hospital, with all the assistance the medical officers of that noble institution are invariably ready to afford to their professional brethren when engaged in scientific inquiries; yet we find no information in his work as to the number of cases of gonorrhœa treated in either establishment, and the proportion of such cases in which urethral chancre existed, or any verification of the accuracy of Ricord's experiments, by repetition in this country. We confess that we looked for some such information from an author, who is anxious to render himself an authority on venereal diseases in this metropolis.

The following letters from Dr. Griffith to Mr. Acton, on the microscopic characters of the discharge in acute and chronic gonorrhœa, contain all the information which can be considered at all novel in some three hundred closely printed pages:

"The secretion of the mucous membrane affected with gonorrhœa, consists of muco-pus, but it varies somewhat in character according to the period of duration of the morbid action. In the very earliest stage it consists of a simple white watery mucous fluid, but in a very short time it becomes yellow,* and this condition in an unchecked gonorrhœa lasts for some time; ultimately it loses some of the yellow tint, becoming more watery, and remains as gleet. During the very earliest period it consists of simple mucus, and under the microscope exhibits epithelial scales and their debris. During the second stage it also contains these substances, but, in addition, albumen in solution, which is coagulable by heat, the precipitate not being dissolved by acetic acid; and the microscope detects very numerous pus-corpuscles, upon the presence of which the yellow colour is dependent, together with epithelial scales. In the latter and chronic stages, the number of pus-corpuscles is proportionably diminished, that of the epithelial scales increased, and the albuminous impregnation is diminished or disappears.

"The urine (excluding the gonorrhœal deposit) in gonorrhœa does not differ essentially from its normal state; it is, however, usually of lower specific gravity, and very commonly contains small crystals of oxalate of lime. The pus-corpuscles increase the density of the deposit, which subsides by repose, so that the latter appears to the eye to contain a more copious and dense deposit than in health. The pus-corpuscles are also somewhat different in appearance from those of normal pus, being rather larger, less granular, more transparent, and less rapidly acted upon by acetic acid; in some the molecular motion is seen, in others not; the former properties depend upon the imbibition of the urine, the latter upon their being surrounded by the mucus, which defends them for a time from the action of the acid. They ultimately yield the same nuclei as normal pus. The urine in gonorrhœa also contains slightly more pavement epithelium from the bladder than the natural fluid; but I have not been able to detect the cylinder epithelium from the urethra to any amount. However, the presence of the pus-corpuscles without excess of the vesical or renal epithelium, might guide in the diagnosis of the source of the pus." (pp. 26-27.)

Dr. Griffith thus describes the flocculi of mucus in chronic gonorrhœa or gleet:

"These, when examined microscopically, are seen to consist of amorphous granular shreds of mucus, with pus-corpuscles and epithelium scales. The epithelium is generally of the pavement kind, that modification of it which lines the bladder. Hence their source is probably the mucous membrane of the bladder.† The mucous flocculi above mentioned must not be confounded with the apparent shreds formed by the adhesion of pus-corpuscles and epithelium scales, to hairs or cotton fibres, such as are not uncommon in urine. The former may be found in urine the moment after it is passed." (p. 114.)

* "In some cases the discharge is milky white, or nearly so, in the first two stages. In these the corpuscles are of that kind which has been denominated 'mucous,' exhibiting the molecular motion, &c."

† "In some cases I have seen small, flattish, ciliated epithelium scales contained in them. I am not aware that ciliated epithelium has yet been detected as a constituent of any portion of the human urinary or genital tract. It probably arises from either the sinus pocularis, prostatic ducts, or the kidneys, near the malpighian bodies."

We really have neither space nor inclination to follow Mr. Acton through his long chapters on Blennorrhagia, with its sections on Gonorrhœa, Strictures, Infiltration of Urine, Urinary Fistulæ, False Passages, Affections of the Prostate and Cowper's Glands, Epididymitis and Irritable Testis, Cystitis, Spermatorrhœa, and Impotency; but one or two subjects are worthy of some remark.

First, as to the *pathology* of gonorrhœa,—not, as Mr. Acton understands the term, to imply only morbid anatomy,—but the theory of the disease. Is simple contact of morbid matter sufficient to produce it, without absorption of the poison into the general circulation? This very difficult question is one which Mr. Acton has not attempted to solve; and we can only reason from analogy, and from what we know of the laws of other morbid poisons, that it must be absorbed, must contaminate the blood, and after a varying period of latency, produce its specific suppurative inflammation in the mucous membrane to which it has been directly applied. This supposition would become an established law, if it could be proved that orchitis, and certain cutaneous eruptions, were owing, not to sympathy, but to a secondary action of the poison. Bubo is doubtless the effect of sympathy, stricture a result of local inflammation; but proofs are at present wanting to establish the relations between orchitis, various slight cutaneous eruptions, rheumatism, and gonorrhœa.

A word as to *treatment*, and we dismiss this subject. Without bringing forward our own daily experience, we could easily prove from the writings of others, and from authenticated results of practice which has been essentially "expectant," that gonorrhœa, even of a severe kind, often terminates spontaneously—that when a patient avoids wine, spirits, coffee, and beer, takes little active exercise, drinks freely of water or any simple diluent, and observes scrupulous cleanliness, the disease commonly attains its maximum in a week, continues in this state from one to two weeks, then gradually declines, and terminates about five or six weeks after its commencement. If to this natural treatment a little tartarised antimony be given when inflammatory symptoms run high, and a dose of castor-oil occasionally, the termination is hastened, especially if some simple astringent injection be used. That recommended by Mr. Johnson is a very good one. It is stronger than the solution of diacetate of lead generally used, containing two drachms of the *liquor plumbi* to six ounces of distilled water. We seldom find more than two requisite. One is a solution of alum, four grains to the ounce; the other an infusion of green tea, a drachm to half a pint. By ringing the changes upon them, we believe that all the good which an injection can afford may be obtained.

When the stomach will bear it, copaiba may be given in drachm doses for two or three days, and continued if it appear to be beneficial; but beyond this we do not believe it is either necessary or advisable to proceed, in one of fifty such cases as ordinarily present themselves. Yet, if we are to believe Ricord and Mr. Acton, we are to subject our unfortunate patients to bleeding or leeching, to cauterise their urethra with nitrate of silver, either solid or in strong solution, and to irritate their stomachs by boluses of the most filthy pastes it is possible for the imagination to conceive. We really pity the poor creature who swallows, and the stomach which is expected to digest, the elegant electuary recommended by Mr. Acton at page 100, even when enveloped in the wafer paper he uses. This delectable composition is a mixture of copaiba, cubebs, camphor, magnesia, extract of henbane, and treacle. If this system of polypharmacy is to continue, no one can reasonably complain of the reported success of hydropathy and homœopathy, or wonder at the fortunate result of cases in the hands of those who eschew such abominations, or the long list of frightful consequences of gonorrhœa detailed in the works of these authors, from orchitis, bubo, and general fever, retention of urine and urethral abscess, inflammation of the prostate and bladder, to induration of the corpora cavernosa and rupture of the urethra.

We must, however, let Mr. Acton describe his method of *abortive* treatment:

"INJECTIONS.—When a patient applies to me in the early stages of gonorrhœa, before scalding in making water has come on, or when the acute symptoms have passed, I at once employ a strong solution of nitrate of silver; but, as this efficient mode of treatment may become a very dangerous agent, I always inject the solution myself. The manner I proceed is as follows:—Having a solution of the crystallised nitrate of silver at hand, in the proportion of ten grains to the ounce, I desire my patient to make water, and placing him in an erect position against the wall, I inject a glass syringe full (about two drachms) of the solution into the urethra, and by pressure retain the fluid in contact with the mucous membrane a few seconds. It is as well, in doing this, to suddenly distract the patient's attention by some remark, otherwise the passage of the fluid along the whole length of the canal may be impeded by spasm or contraction of the organ. I then desire the patient to sit down for ten minutes or a quarter of an hour in an arm-chair, and to withstand the desire of making water, which, for the first few minutes, sometimes is very violent.

"The effect of the injection on different individuals is very striking. Some scarcely feel any pain; others suffer for a few moments most acutely, but usually the agony goes off in three or four minutes, and is replaced by mere temporary soreness, so that the patient is able to walk about. I, however, generally recommend him to lie down on the sofa for an hour or so, and keep quiet.

"The quantity of discharge, like the amount of pain, differs greatly in different individuals. Sometimes no further *discharge at all* is seen, and the patient gets perfectly well. More commonly the injection is immediately followed by a large quantity of serous or shreddy exudation, which soaks through the lint. This exudation is followed by a stringy yellow discharge. In a few hours this gradually becomes again serous, until it completely ceases, and redness of the meatus alone remains, which disappears in a few days.

"Of course, considerable pain would be felt did the patient make water immediately after the injection; but as the bladder has been previously emptied, micturition is not required, and the patient has only to combat for the first few minutes the desire to attempt it; and many hours will elapse before urine will be required to be passed, and by that time the effects of other treatment have been brought to bear, so that scalding is seldom or ever complained of by the patient.

"I have now no fear of leaving my patient, having applied a suspensory bandage, or what answers equally well, a handkerchief passed round the loins, and another tied in front and behind to support the testes, with strict injunctions to abstain from any kind of fluid whatever, so that the urethra should enjoy a few hours' repose. On the next morning the discharge has either altogether ceased, or a drop only is to be seen, and in the course of the day a mere weeping from the urethra occurs. The patient may now take fluids in moderation, consisting either of tea or soda-water, but coffee should be strictly prohibited, as well as wine, beer, and spirits. In some cases, towards the afternoon, the discharge returns again. If this becomes green or yellowish, I have no scruple in repeating the injection, with similar precautions; but I seldom have recourse to a third, and my patient is quit of a troublesome complaint in a very few days.

"The effect of nitrate of silver in a strong solution is very surprising, as may be judged of from the previous description. It appears to possess a specific action in changing the vitality of the mucous membrane, substituting for the original inflammation a new one, whose principal characteristic is its short duration; it destroys, likewise, the morbid element.

"Out of the large number of cases that have been treated on this plan, I have never yet seen any ill consequence arise, not even hæmorrhage or retention of urine. I may mention, however, that I have never employed the treatment except in private practice, where I have had the assurance that my patient would implicitly follow my directions. I should not recommend the treatment among out-patients at public institutions, nor, in fact, have any expectation that any abortive plan will generally succeed with them, but, on the contrary, be attended with such consequences as would soon prevent a surgeon from employing the treatment in private.

"I must however confess that this treatment has not, in my hands, been successful in the treatment of the first attack of clap in young men. In private practice a consulting surgeon does not attend, generally speaking, very young men with their first attack. Shyness, or not knowing where to apply for advice, perhaps, is the cause; but of the fact there can be no doubt, that I am comparatively rarely consulted for first attacks. When consulted the complaint has perhaps commenced several days, and the treatment is therefore no longer efficacious. From these circumstances, I am perhaps hazarding a wrong opinion; but, speaking from my personal experience, I would not advise caustic injection to be applied in cases of first attacks. No dangerous symptoms arise with common care; but I have not met with those certain results which have attended my treatment in persons previously affected.

"So great has been my success, that, with the precautions fully detailed above, I should strongly recommend the treatment in private practice, where, if it was not successful, a surgeon would be soon obliged to lay it aside, or be compelled to relinquish it, if it was found to be followed by ill consequences; otherwise, instead of giving it up, the surgeon would be given up by his patients.

"My own experience does not enable me to say if direct treatment, by means of injections alone, would succeed. I am always in the habit of combining it with general remedies, the modus operandi of which I am about to describe." (pp. 88—91.)

Now we have given a pretty fair trial to this treatment, and have done what Mr. Acton has not, namely, endeavoured to ascertain its value alone, not vitiating the experiments by employing other means at the same time. The result has been, that it failed about as often as it succeeded; and when it failed, it made matters worse than before. We found it also quite impossible to determine beforehand if it would succeed or fail; and as an abortive treatment found it not more successful than a plan of steeping the penis in hot water, three or four times a day, for a few minutes at a time, until some faintness is produced. In slight cases we have often known this simple measure cure the disease in one day. Mr. Johnson decidedly objects to the nitrate of silver in the early stage of gonorrhœa. He says: "I have seen several cases of gonorrhœa, of a most obstinate and serious description, after its adoption. I have also seen inflammation of the bladder, enlarged lacunæ, stricture in the anterior part of the urethra, and acute synovial rheumatism follow it." (p. 58.) He relates one case which terminated fatally, after purulent discharges from the bladder for some months; and another in which synovial inflammation of the knee-joint was severe and obstinate, the ill effects in both cases apparently depending on the nitrate of silver. M. Ricord acknowledges that his plan "sometimes produces severe accidents, inflammation or gastro-intestinal irritation, and eruptions on the skin" (Acton, p. 107):—we presume when he gives copaiba at the same time. Mr. Johnson says, that he has seen nearly fatal gastro-enteritis caused by large doses of copaiba, also inflammation of the kidneys; and he has been consulted in numerous cases of aggravated dyspepsia, in which the patients entirely attributed their complaints to over-dosing with copaiba.

Mr. Acton indulges in many sneers at those who have their doubts whether it is quite safe to suppress a gonorrhœal discharge in a few hours; and regards these doubts as superstitious remains of early prejudices lurking in old fashioned corners, constantly used to veil ignorance, or authorise safe but "old-womanish" treatment. He hopes to be the means of "completely routing out" the old-fashioned doctrine; but his arguments are a mere series of sweeping assertions. He says it is untrue, that a discharge rapidly cured will be followed by any general disease; but his denial will not satisfy those who believe with Baumés, Cazenave, and other practical observers, that they have seen morbid conditions of the general system and various cutaneous eruptions so caused. As to gonorrhœal rheumatism, Mr. Acton asserts that, "in nine cases out of ten, the rheumatic affection will not be caused by repressing a clap;" and he believes that, in many persons, the only way to avoid rheumatism coming on is to cure the discharge rapidly, the discharge being, in his own felicitous language, "the portal apparently through which rheumatism enters the constitution" (p. 47.) Yet we could find many men, old fashioned enough to have had a large practice for many years, who could quote a sufficient number of cases in which rheumatism has followed sudden suppression of gonorrhœa, to establish a closer relation between cause and effect than Mr. Acton's assertion could disprove, although he may be right in his estimate of escape in "nine cases out of ten." When M. Ricord sees general fever, rheumatism, or any other constitutional affection coinciding with a suppressed gonorrhœa, he indulges in the ingenious fancy, that the general affection has come on spontaneously, and has cured the gonorrhœa on revulsive principles, just as, according to Mr. Acton, a blister or seton would. This is a mode of defending a favourite practice, and explaining away unpleasant facts, far more easy than satisfactory; and Mr. Acton has really brought forward nothing beyond his own assertions, and those of M. Ricord, to prove that the fears of cutting short the disease are groundless,—a mode of "establishing the proposition," as he calls it, which cannot be accepted in an age when men are becoming peculiarly liable to question the influence of authority in matters of opinion, and to allow one authenticated fact to outweigh a great many general statements.

Another question suggests itself. Are certain morbid conditions of the general system, identical with or resembling those known as secondary or constitutional syphilis, especially a state of general disease characterised by certain cutaneous eruptions, the result of gonorrhœa, and altogether independent of syphilis? M. Ricord and Mr. Acton say, "No." But Beaumés, Biett, and Cazenave, with at least equal opportunities of observation, say, "Yes." Cazenave, in particular, is so convinced of the fact, that he used it as one of his most telling arguments in favour of the theory of a single virus as the essential element both of syphilis and gonorrhœa. M. Ricord says, that whenever constitutional syphilitic symptoms do follow gonorrhœa, there must have been a chancre concealed somewhere. He has certainly shown that the proportion of cases of these secondary diseases is something like that in which urethral chancre is detected; but he has often been forced to assume the existence of a chancre when he could not demonstrate it; and even admitting his assumption to be correct, it does not supply an answer to our question. It explains the occurrence of true secondary *syphilitic* affections, but it does not prove that gonorrhœal poison has no secondary actions. Gonorrhœal rheumatism is a constitutional affection now universally recognised. Sir Benjamin Brodie has drawn attention to a peculiar and not unfrequent morbid condition, in which gonorrhœa, synovitis, and ophthalmia compose the chain of diseased actions. Cases of gonorrhœal ophthalmia are occasionally met with, where the patients, perfectly aware of the danger of the occurrence, observe all necessary precautions, and declare most positively that direct contact of pus to the eye could not possibly have taken place. Lastly, the opinion appears to be gaining favour with practical men, that there is a class of cutaneous eruptions, whose characters differ from those of ordinary skin diseases, while they are wanting in the unique specific physiognomy of true syphilitic eruptions; and that there is a relation between this class and antecedent gonorrhœa. It is quite certain, that in many cases of obstinate exanthemata, vesicular, pustular, and scaly affections resisting ordinary remedies, some approach to the circular form is observed. A dull grayish brown colour is not dissimilar to the red coppery tint of syphilis, and roundish depressed cicatrices follow. The sufferers from such eruptions will often deny having ever had syphilis, but admit one or more attacks of gonorrhœa; sometimes connecting a state of generally impaired health with one of these attacks. There is no proof of urethral chancre, nor any ordinary secondary affection of the throat, skin, or bones; yet local treatment is useless, and the disease yields to sarsaparilla. Far more extended observation is required before this question can be settled; and the inquiry will not be stopped by any fear of the ridicule, which M. Ricord and his followers in this country imagine can be mistaken for reasoning.

The chapters on Spermatorrhœa and Impotence are good, and may be studied with advantage by those who have not read Lallemand's work. No allusion is made to the use of cold hip-baths in the treatment of the former affection, though they form the principal means employed by an advertising practitioner in this metropolis, who has the reputation of being more successful in this particular affection, than any of his notorious brethren of the 'Manhood' and 'Silent Friend' school, who are disgracefully assisted by the newspaper press in spreading their demoralising publications over the country. We have seen or heard of many cases in which these baths proved of undoubted and very great advantage. The patient begins by sitting in a hip-bath, the sitz-bath of Priessnitz, for five minutes three times a day, the water being brought to the temperature of 65°. The time is gradually increased and the temperature lowered, until the patient sits for twenty minutes, three times a day, in water at 50°. In some cases, the spine is sponged for three or four minutes before leaving the bath, and very often a shower-bath is used after the first daily sitting bath, the head being protected by a conical cap. Gentle exercise for five minutes before, and half an hour after, each of these processes is ordered. From what we have seen of the results of the treatment, we shall always be inclined to adopt it, before cauterising the urethra or following any other plan of treatment which may be hazardous. We have seen some cases of persons whose powers had been debilitated by long residence in hot climates, where the effect was very speedy and remarkable; and we have been informed, on credible authority, that the fecundating power of married men, whose generative energy was in other respects unimpaired, has been evidently restored by a few weeks' course of these baths.

One of the questions we have just been discussing, becomes of still greater interest and importance in its relation to the laws of the action of the poison of syphilis. Mr. Acton says, he believes "that immediately the morbid poison comes in contact with the cellular tissue, a local action is set up" (p. 367), and he denies any period of incubation or latency of the poison in the system, before the existence of this local action. He starts two questions: "Does incubation of the virus exist?" and "Is the nascent action of chancre local or general?" but he furnishes us with absolutely nothing in reply to these questions, save a general statement, that syphilitic virus produces its own special effects on the human system, these effects being in many respects similar to, but by no means identical with, those of other morbid poisons. While endeavouring to depreciate the evidence from the analogy of the laws of other morbid poisons with those of syphilis, he quotes some singularly apposite experiments, made by M. Rinault, at the Veterinary College at Alfort, which show how rapidly animal poisons are absorbed. This gentleman inoculated horses with the poison of acute glanders. He incised and cauterised the parts one hour afterwards; yet the disease was as certainly developed as if no incision or cauterisation had been practised. Similar experiments, made with the poison of sheep-pox, proved "that in *less than five minutes* absorption had taken place, when the poison had been brought in contact with any absorbent surface of the skin." (Acton, p. 369, from the 'Recueil de Médecine Vétérinaire,' 1849.) A long letter is appended, from Dr. George Gregory, in answer to the question, whether "vaccine matter acts generally before producing its local manifestation." We need not examine Dr. Gregory's *arguments*, which are singularly obscure and completely opposed by the only *facts* in his letter, the first of which, however, we do not believe to be a fact. He says: "The development of the vaccine pustule is not prevented by the most profuse bleeding of the wounded vessels. (?—REV.) It is not prevented by the use of the cupping glass applied over the wounded surface. It is not prevented by the most diligent ablution. The vessels having been once touched by the most homœopathic fraction of a drop of pure vaccine virus, the process *must go on;* nothing can stop it but the destruction of the part or the death of the child." (p. 369.) Not even the destruction of the part will stop it; for Bousquet has shown, by numberless experiments, that when once the virus has been inserted, the most active means of cauterisation, application of strong acids, and the continued use of the cupping glass fail to check the course of the disease. Can anything be more strongly corroborative of the doctrine, that the poison is rapidly absorbed and infects the blood, remaining latent, or in a stage of incubation, for a given period before the local action is manifested? We do not wish to argue, that because this holds good with respect to vaccinia and farcinoma, therefore it *must* with respect to syphilis also; but the probability is, that the action of syphilis on the human system is analogous to that of other morbid poisons. Each morbid poison has its own peculiarities; but all, from smallpox and plague to farcy and syphilis, appear to affect the general system primarily before any local disease can be detected. If there is any fact in medicine which can be considered as established, it is the rapid absorption of poisons. If a poison be absorbed and mingled with the blood, it is undeniable that the blood is contaminated. What the changes may be, we know not; but the fact of contamination or infection does not admit of doubt or question.

But there are especial reasons why we should examine the evidence which appears to us to establish the doctrine, that in syphilis the poison is absorbed, the blood is infected, and the primary and secondary symptoms are mere consequences or indications of the contamination of the blood. The proposition of Hunter, that early cauterisation of a chancre would convert it into a simple sore, anticipate absorption of the poison, and prevent the occurrence of secondary affections, has been warmly supported by Ricord, followed of course by Mr. Acton, who appears quite amazed that any surgeon in the present day can presume to entertain the least doubt on the subject. Yet when we come to inquire at what time after inoculation cauterisation can be successfully performed, we find such a variety of opinions among the advocates of the practice, that its success as preventive of the occurrence of secondary affections, becomes at once very questionable. Hunter thought it sufficient to excise or cauterise a chancre a *month* after its first formation; yet Mr. Wallace has destroyed chancres and healed the sore within ten days after the first appearance of the pustule, secondary symptoms following, nevertheless. Ricord affirms, that when a primary ulcer is destroyed within the first *five days*, no secondary affection follows. Mr. Acton appears to fix *three days* as the limit. Yet Mr. Colles quotes cases, where the secondary affection has occurred, although chancres have been excised on the first and second day; and every one in large practice must have seen many cases, where cauterisation with nitrate of silver has been performed by patients themselves, as soon as any evidence of a sore was perceived, without preventing constitutional disease. It would be too much to say, that in all such cases cauterisation was imperfectly performed. Further, many surgeons of most extensive experience in venereal affections not only believe that cauterisation is no safeguard against contamination of the system, but assert, on the contrary, that after it has been practised, secondary symptoms occur with even greater frequency. Biett and Dupuytren affirmed, that both the frequency and severity of the secondary symptoms were increased by the operation; and all M. Cazenave's enormous experience leads him to the belief, that cauterisation is decidedly favorable to the development of constitutional syphilis, especially of syphilitic cutaneous diseases. Mr. Acton does not offer us any statistical evidence as to the proportion of cases in which secondary symptoms follow chancre, treated with or without cauterisation; but he states, what is now generally acknowledged, that while secondary affections after indurated chancre are the rule, after simple or phagedenic chancre they are the exception; yet he would not cauterise an indurated chancre, and takes credit for preventing secondary symptoms by cauterising simple chancres, which, he knows, are rarely followed by these symptoms under any circumstances. Let us supply some of the facts Mr.

Acton might easily have obtained for himself, from the 'Reports on the Health of the Army.' The unindurated primary sore, the *venerola simplex*, or *superficialis*, the *simple chancre* of Mr. Acton, are synonyms for the class of sores described by the army surgeons as *ulcera penis non-syphilitica*. Mr. Abernethy never saw secondary symptoms follow these sores. Mr. Skey says, they are followed by no secondary disease whatever. Other surgeons, ourselves among the number, believe that occasionally, though rarely, secondary affections, especially iritis, are their consequences; all being agreed, however, that they form a very large proportion of the number of primary syphilitic ulcers treated in civil practice. The following table, which we have drawn up from the army returns, will show the proportion which primary and secondary symptoms bear to each other, among our troops on various stations:

	Syphilis primitiva.	Ulcera Penis non-syphilitica.	Syphilis consecutiva.
Dragoon Guards (Home)	1415	2144	335
Gibraltar	749	416	193
Malta	1806	1351	115
Ionian Islands	826	876	108
Windward and Leeward Command	342	503	97
Jamaica	284	81	11
Bermuda	31	93	3
Nova Scotia and New Brunswick	476	1087	87
Canada	852	2551	123
	6781	9102	1072

It is very probable, that what some surgeons would consider a syphilitic sore, others would not; and therefore these returns are only an approach to the truth: but it is generally understood that our army surgeons are guided by the existence or non-existence of induration; the first column including indurated, the second non-indurated chancres, while the third shows the proportion in which secondary symptoms follow both. We can find no report of this proportion separately; but it appears that in the army about one in every fifteen persons attacked with an ulcer on the penis, whether considered syphilitic or not, suffers afterwards from secondary symptoms. The unindurated sores are treated merely by rest, abstinence, cleanliness, and some simple dressing, or the black wash. It appears therefore, abundantly proved, that any one who believes he prevents the occurrence of secondary symptoms by cauterising simple chancres, labours under a delusion as erroneous as flattering. Mr. Acton never employs caustic when there is induration. (p. 393.) He acknowledges (p. 389) that, "in *nine cases out of ten*, in private practice, the appearances about which a surgeon is consulted are not of a specific character, and that the disease, if left to itself, would probably get well under simple treatment;" and then goes on to extol the powers of caustic as a "guarantee against the occurrence of secondary symptoms." This is pure nonsense. He knows that caustic is useless in the kind of chancre followed by secondary symptoms, and only employs it to sores which are very rarely followed by these symptoms under any circumstances, and then believes that his cauterisation has stayed the course of a morbid poison.

But we must return to the evidence of the absorption of the poison and infection of the blood. If the series of secondary affections, set up long after the period of contamination, be not proof sufficient, we find corroborative evidence in the infection of the fœtus in utero. Bertin ('Traité de la Maladie Vénerienne, p. 514) quotes the case of a pregnant female labouring under secondary symptoms, who died suddenly from suffocation in consequence of some food passing into her glottis. Her body was opened, and the fœtus found to be affected with syphilitic exostosis. It is not often that so direct an observation as this can be made; but abundant evidence can be obtained of the truth of the doctrine, that secondary symptoms may be transmitted from parent to child under various circumstances, if we go no further than Mr. Acton's chapter on Infantile Syphilis, which is the most valuable portion of his book.

It is easy to understand how the fœtus becomes affected when the mother is suffering under syphilis; indeed, considering the free interchange of maternal and fœtal blood in the placenta, it is surprising that fœtal infection is not invariable, when the mother is diseased. Trousseau and many other authorities state that children were scarcely ever *born* with syphilis, but that it appears soon after birth. M. Chateauneuf, however, states, that at the Hôpital des Enfans Trouvés, the proportion of syphilitic children admitted is 17 in 1000, and it is very seldom that admission is long after birth. Mr. Acton has obtained from the Registrar-General a table showing the ages at which 203 children died from syphilis in the years 1846-7-8, in the metropolis. We extract this table, as it is of considerable value: (p. 645.)

AGE AT DEATH.	DURATION OF DISEASE.					TOTAL.
Under months.	Not mentioned.	Congenital.	Under one month.	Under two months.	Above two months.	
1	18	19	3	...	...	40
2	15	31	11	3	...	60
3	11	13	4	4	1	33
4	5	17	1	3	...	26
5	6	7	...	...	1	14
6	2	7	...	...	1	10
7	2	1	...	...	1	4
8	1	1	...	...	...	2
9	1	2	...	...	...	3
12	2	7	...	...	2	11
Total	63	105	19	10	6	203

Thus it appears, that in more than one half the cases in which the fact could be ascertained, the disease was congenital. Mr. Acton supposes that many surgeons interpret *congenital* to mean *hereditary*. Some may, but we doubt the frequency of so great an error. It would be well, however, to institute further inquiries on this subject, in addition to the inquiry also suggested by Mr. Acton, as to the latest period of pregnancy at which a woman becoming infected can transmit the disease to her infant.

The infection of the fœtus by the father, the mother remaining in good health, is a fact which leads to some curious physiological problems. The reports of Dr. Mahon from the Venereal Hospital, Vangivard, prove the frequency of the occurrence; and most accoucheurs of large experience, have seen premature births when the child exhibited undoubted signs of syphilis, while the mother has not presented the least symptom of it. Indeed, it appears to be the general rule, that where a mother contracts syphilis, it must be through primary symptoms. It is quite clear, however, that there are exceptions to this rule,—exceptions which prove that the seminal fluid when it impregnates the ovum, imparting the likeness of the father, imparts also the specific disease with which his blood is infected,—and that the fœtus so diseased propagates the morbid poison to the mother. She does not receive it directly from the father, but indirectly through the unborn offspring. Here is the statement of Ricord, as quoted by Mr. Acton from the 'Lancet:'

"Supposing a female to be impregnated by an infected agency, how will *she* be affected by carrying a poisoned fœtus? According to certain well-observed facts, we may infer that the mother can receive the germs of the disease from her child, so that, in such a case, she suffers from the syphilitic infection by the instrumentality of the fœtus in utero. It had hitherto been believed that the mother received the infection directly from the father, and that she transmitted to her offspring the diathesis with which she became imbued; but this never happens except the mother has been subjected to the contagion of *primary sores,* and she herself has had an indurated chancre as well as secondary syphilitic symptoms consequent upon such chancre. I am ready to acknowledge that a woman may give birth to an infected child without experiencing any inconvenience herself; the father, in such a case, transmits the poison by reason of the secondary symptoms which are upon him at the time. If he had had *primary* symptoms, he would have diseased the mother directly, and the effect (as before mentioned) may still have reached the child. A man who has constitutional syphilis upon him, of howsoever long standing it may be, should not marry, for his progeny runs great risks; his wife, however, is by no means so much in danger, for the embryo may or may not contaminate her. I well remember a case of this description, where a gentleman with certain secondary manifestations was advised by his medical attendant to postpone engaging in wedlock; he disregarded the advice, married, and, nine months after, he had the mortification of seeing a well-defined eruption upon his child; his wife, however, escaped unhurt." (pp. 619-20.)

It would appear also from a case published by Mr. Foot, that it is not necessary that the father should be actually labouring under evident symptoms of syphilis at the time of impregnating his offspring. The disease may be altogether latent. A gentleman married soon after primary symptoms of syphilis had been cured. Within a year, a son was born healthy, and remained so. Three abortions followed. At the end of the third year a female child was born with decided syphilis. Within a month after the birth of this child, the father began to suffer from offensive discharge from the nose and palate, a node formed on the forehead, with thickening about the elbow-joint, and nocturnal pains. Both father and child were treated with mercury and recovered, but the former lost his palate.

The possibility of infection of infants through the milk of a diseased nurse has been often debated, but no satisfactory conclusion has yet been arrived at. The possibility is not disproved by the fact, that diseased women do occasionally suckle infants without communicating the disease. Such cases are quoted and authenticated, but are only negative facts. They would be no answer to a positive instance of a healthy foster-child being affected with syphilis, after being suckled by a female with sound nipples who was suffering from secondary syphilis.

The influence of a diseased infant on the nurse is another problem yet unsolved. The following case, recorded by Mr. Acton as an instance in which the mother was probably contaminated through the placental circulation, we should rather regard as an instance of infection from the mouth of the child through the nipple, as the mother had remained healthy until some time after the birth of the child:

"*July*, 1850.—A gentleman, twenty-eight years of age, came to me to-day complaining of a sore tongue. On the left side of the organ a white spot as large as a threepenny-piece, looking like a cicatrised ulcer, has broken out; on the lip there is a similar spot, but the surface is quite level.

"His history is the following:—Two years and a half ago he contracted syphilis, secondary symptoms followed. During the time he laboured under the complaint, his wife became pregnant, went her full time, and the child was born healthy; a few weeks after birth it showed symptoms of secondary syphilis, spots at the corners of the mouth, and on the palms of the hands; the mother, who had been perfectly healthy up to this time, then (some months after her confinement) had unequivocal marks of secondary symptoms, no sore breasts, but psoriasis palmaris.

"Here, then, is an instance of a father infecting the child, and the child contaminating the mother, the contamination showing itself in the mother twelve months after the embryo had been infected. This case is the more important, as it occurred in a person who had read all that had been written on syphilis of late years, and was an excellent observer." (pp. 632-3.)

Had this child contaminated a healthy wet-nurse instead of the mother, it would have settled the question. Mr. Colles, and other Irish surgeons, state, that syphilis is frequently conveyed from the child to the nurse in Ireland; and one well-observed and well-authenticated case is enough to settle the question of *possibility*. The frequency of the occurrence and the degree of danger is another question, though Mr. Acton does not seem to comprehend this; for he brings forward *one* experiment of inoculation, and *one* case in which the secretion of condylomata, applied on the sound skin of a bedfellow for a length of time, did no further harm than produce an unhealthy looking sore, as, in his own words and italics, "*showing the impossibility of infecting the system through secondary symptoms.*" We can in no way explain this contradiction after the evidence he had been quoting of transmission of secondary symptoms to the fœtus; and through the fœtus to the mother. We regard the question as unsettled; and as it is important in a medico-legal point of view, we extract the following letter from Professor Taylor to Mr. Acton:

"*The alleged transmission of Syphilis from Child to Nurse.*

"A woman, acting as wet-nurse to a child, born syphilitic, contracts what she supposes to be syphilis as a result of suckling the child, and sues the parents for damage to health, &c., thus sustained.

"Before she could recover in such action, it must, however, be clearly proved by evidence satisfactory to the court and jury:—1st. That the disease under which she was labouring was really syphilis; and, 2d, that she could not, by any possibility, have contracted the disease in any other way.

"If, as it is alleged, syphilis cannot be thus transmitted from child to nurse, and no such case has ever been met with by any authority on the subject, this would be strong evidence for the defence; and if supported by good medical opinions, it would probably lead to the non-suiting of the plaintiff (the nurse).

"If it could be shown that the disease in the nurse was not syphilis, but some other affection, or that, being syphilis, it might have been acquired by the nurse in some other way, and not as a result of the act of suckling the diseased child, then, in either case, the plaintiff could not recover damages.

"Her case may, however, be supported by good medical and circumstantial evidence. Strong medical opinions might be given that the disease in the nurse was really syphilis, and that it might be transmitted from child to nurse. Again, the witnesses for defendants (the parents), although they might not have met with a case in which the disease was transmitted by suckling, would probably, in such a novel question, find great difficulty in swearing that its transmission under the circumstances was *absolutely impossible.* As cautious men, and having a due regard to the abstruse nature of 'infection,' they would probably confine themselves to swearing that they had never met with nor heard of such a case, and to the best of their judgment and belief it could not occur. This would not suffice to defeat the plaintiff's claim, if it were otherwise well supported.

"In a conflict of medical opinions, and when direct proofs are wanting, a jury is commonly directed to look to all the circumstances irrespective of medical evidence. If the case stood as above supposed, and the plaintiff was of excellent moral character, and there was no reason to believe that she could have contracted the disease in any other way, the jury would probably find in her favour.

"The recommendation of a wet-nurse by a medical man would not, in my judgment, affect the right of the woman to claim compensation from the parents, since they are the parties who hire her, and must be responsible for the results of such hiring. Whether the parents would afterwards have a right of action against the medical man for recommending them to employ a wet-nurse, knowing the child to be syphilitic, is another question. In order to recover in an action for damages against him, it must be proved, that by such recommendation he showed himself to be grossly ignorant and unskilful in his profession. He might, however, quite innocently, and without any just imputation of ignorance or unskilfulness, make such a recommendation; because the fact of syphilis being thus transmitted is a *quæstio vexata.* He may have believed, *bonâ fide*, that the disease could not be thus conveyed from child to nurse, never having heard nor met with such a case: damages could not, therefore, be fairly claimed of him, because he had acted to the best of his judgment; and at the most it could only be alleged against him, that he was guilty of a venial mistake into which nine tenths of the profession would, under the same circumstances, have fallen.

"ALFRED S. TAYLOR, M.D. F.R.S." (pp. 656-8.)

How far a medical man is morally justified in putting a syphilitic child to a healthy wet-nurse, is another question. Mr. Acton does it because he thinks the danger of bringing up a child by hand very great. If human milk were considered absolutely essential to the welfare of the child, it might be procured by the breast-pump from a healthy person, the child being fed from the bottle. This would satisfy all parties.

Our space is nearly exhausted, but we cannot conclude without some review of the present state of opinion regarding the use and abuse of mercury in the treatment of syphilis, primary and secondary. Mr. Hunt, in the preface to his pamphlet, makes some startling assertions. He says:

"The general failure of the non-mercurial treatment has already manifested itself in a fearful increase of hereditary syphilis in every rank of life, in both sexes, and at all ages. A very large proportion of the cutaneous affections met with, both in hospitals and private practice, are unquestionably syphilitic; and if the wide-spread existence of an hereditary taint is to be attributed to the non-mercurial treatment of the parents—and if the disease has no certain tendency to wear itself out, but may be transmitted by the blood from generation to generation, a heavy responsibility certainly rests upon the medical profession. The excessive and indiscriminate use of mercury in the last century was as unnecessary as it was barbarous. It fell heavily on the culprit; sacrificed oftentimes his teeth, and sometimes his life; but it did not visit on the children the sins of their fathers. A surgeon of the highest eminence has, within the last few years, declared an opinion, that of late mercury has been too sparingly used in these diseases. If the present generation of surgeons do not become convinced of the truth of this opinion, and do not put it into practical operation as they have opportunity, it is much to be feared that posterity will re-echo the theory under accumulated evidence of its truth." (preface, pp. iii-iv.)

Again, in his introductory remarks on the treatment of syphilitic eruptions, he continues in the same strain:

"A large proportion of the cutaneous diseases met with in every station in life, are syphilitic. That a man can, without infecting his wife, transmit to his children a syphilitic taint, is an established doctrine. Several cases of this nature are related by the older writers on surgery, and by some of modern date. Several, also, the validity of which no scrutiny can impeach, have occurred in my practice. I have seen syphilis in infants and in children of all ages, whose fathers had married after the non-mercurial treatment of the disease had effected an apparent cure. After marriage, (in some cases several years after,) secondary eruptions made their appearance. These were treated mercurially, and the result was most instructive. The wife escaped, as did also the children begotten *after* the mercurial treatment of the father; but the children begotten *before* the mercurial treatment were infected. In one instance, the child suffered more than the father. The father had psoriasis plantaris in a mild form; the child, a girl of ten years of age, had iritis, sores about the nostrils, and a scaly eruption on the scalp. All these diseases, together with that of the father, yielded to active mercurial treatment, and to no other; iodine, sarsaparilla, and other remedies of that class having proved perfectly inert. Several children, begotten and born after the mercurial treatment of the father, were perfectly healthy, nor was any trace of disease to be found in the mother. In another striking instance, a married man contracted syphilis. The primary sore readily healed under simple treatment. Secondary symptoms appeared, which ultimately yielded to active mercurial treatment. In the interval between the healing of the primary sore, and the subsequent mercurial treatment, a female child was begotten. This child was born apparently healthy; but, as she grew up towards womanhood, suffered from sore throat, scaly eruptions, sores about the nose, and other unequivocal symptoms of syphilis. Subsequently to the mercurial treatment of the father, several children were begotten, but not one of these has been affected. And two elder children, born before the father contracted syphilis, have been also entirely free from the disease, together with the mother, who was fortunate enough to escape infection in the first instance." (pp. 6-7.)

Now it is very easy to assert, that by adopting a certain mode of treatment we infect large masses of the community with hereditary syphilis, and cause the transmission of contaminated blood from generation to generation; but, before so frightful a charge can be substantiated, far more proof will be required than Mr. Hunt affords. He furnishes no proof whatever of the increase of hereditary syphilis, which, he says, is observed in every rank of life since mercurial treatment has been partially abandoned; and we cannot accept such a statement as correct, without the support of an amount of numerical evidence which it would require the observation of a generation to accumulate. Still, the mere possibility of so serious an evil should lead us to examine more closely than ever, what is the real amount of power we obtain over syphilis by the use of mercury.

In simple chancre it now appears to be agreed on all sides that no mercury is required. Secondary symptoms follow so rarely that many surgeons of great experience assert that they *never* do so; and it would be absurd to subject a patient to a course of mercury, to treat a sore which will get well under the most simple local treatment, with a general treatment so mild that Mr. Skey has published his deliberate opinion, in which we entirely concur, that he is "acquainted with no form of medicine, which, as applied to the case before us, is both more efficient and less objectionable than BREAD PILL." ('Medical Gazette,' 1838-9, pp. 265-6.)

But when we come to the sore which is apt to be followed by constitutional syphilis—to the indurated chancre—the question assumes a different aspect. There can be no doubt that indurated sores have been frequently cured, especially by the army surgeons, without the use of mercury. The reports of Mr. Rose, Mr. Guthrie, Dr. Thomson, and Dr. Hennen, would fully establish this fact even without the corroboration of Mr. Carmichael. But it also appears that recovery is very slow, being much more certain and rapid when mercury is administered; and if we looked to nothing more than the cure of the primary sore, it would be well to follow the rule of Ricord, and have recourse to mercury whenever a chancre is indurated, or when induration persists after a superficial cicatrisation, the exceptions being those cases only where a debilitated or scrofulous constitution forbids the employment of the mineral. As a means of preventing the occurrence of secondary symptoms, it would appear that the influence of mercury is very uncertain. These symptoms will follow in some cases, however trifling the primary sore, and however large the quantity of mercury given. The point to decide is, whether, in a given number of indurated chancres treated without mercury, secondary symptoms are more numerous or more severe than when mercury is given. If by *treatment* we mean *abuse* of the remedy, inundation of the system with mercury after the primary sore has healed, or the practice of large inunctions, and this without much regard to the character of the primary symptoms, it appears to be proved that mercury increases both the frequency and severity of the secondary symptoms. The remarkable series of experiments quoted by Dr. Williams ('Elements of Medicine,' vol. ii, p. 134), are sufficient to establish this proposition. He states, that in Sweden, —

> "So late as the year 1814, it was the practice to heal the primary symptoms by most free inunction; and, under this system, it was calculated that the number of cases of secondary affections of the bones was no less than 54 per cent. of the whole number treated. In 1814, however, this treatment was changed, and a milder method adopted, as by fumigations, by other local applications of mercury, and by diet; and the result is stated to have been so eminently successful, that the College of Health reported, that the cases of diseased bones were reduced from the large number mentioned, to about 6½ per cent. It is added, also, as a consequence, that, instead of there being six hospitals for the reception of venereal patients, there is now only one in all Stockholm."

This is quite sufficient to prove what the *abuse* of mercury will do, in causing or aggravating what are considered symptoms of the secondary and tertiary action of syphilitic poison. It is considerably more difficult to ascertain the precise effect of its proper use, or to determine what amount of mercury can be considered necessary as a safeguard. When Mr. Acton lately visited M. Ricord at Paris, and asked him: "How long must mercury be given after the disappearance of induration?" the reply was: "*Six months, at least!* may pass, before leaving off the preparation," which ought to be kept up to nearly the same dose which has effected the cure for that time; and he added: "Even then the patient must not be surprised at seeing the disease return." Sir B. Brodie says, mercury should never be left off, "until the hard cicatrix has disappeared." But we still remain in the dark as to the amount of influence, which even this prolonged use of mercury has, in preventing secondary symptoms. Even after *six months*, said M. Ricord to Mr. Acton, the return of the disease must not be wondered at; and we have to express our regret that Mr. Acton did not ascertain something more definite from the Parisian professor, and take some pains to inform his readers, in what proportion of cases of indurated chancre mercury would be likely to stop the subsequent action of the poison. We are not able to supply the deficiency; for it has so happened that we have very seldom been called on to treat indurated chancre. This may be partly accidental, and partly from the very simple treatment we adopt in all primary sores; but the result in the few cases we have seen in our own practice, and in that of others, has been to induce us to leave off mercury a very few days after cicatrisation of the sore, convinced that a longer course is very uncertain as a preventive of further syphilitic affections, and more likely to injure the general health than the latent existence of the poison, even if it do exist, and that it in no degree assists us in the treatment of any of the secondary symptoms when they do arise. We merely give this as our own impression, not as a doctrine which could be established. It remains for some future writer on syphilis to clear up the doubts which obscure this question, by collecting accurate statistical information. Nothing but a long course of careful observation can enable us to arrive at any satisfactory conclusion. What a mine of information Duvergie's 59,620 cases would have afforded us, if he had distinguished the cases less loosely than as "primary syphilis," a designation in which continental writers include gonorrhœa, orchitis, &c., thus taking all value from their investigations. The man who aims at becoming the Pearson, Wallace, or Carmichael of his day, or wishes to earn the title of the "British Ricord," must investigate for himself the natural history of syphilis; and far from being satisfied with vague generalities, or the results of moderate personal observation, or the indefinite statements of others, must establish his doctrines and confirm his experience by at least such an array of facts and figures, as any diligent student might collect in five years in the hospitals of this metropolis.

It will become necessary to determine whether mercury prevents, or only retards, the occurrence of secondary symptoms. They have been seen eight days after the appearance of primary sores, and they have not appeared for eighteen years. In a thesis by Dr. M'Carthy, quoted by Mr. Acton, the doctor gives an analysis of 123 observations of secondary symptoms, in which the intervals between the occurrence of primary and secondary symptoms varied from two weeks to eighteen years, the average of roseola and condyloma being seven weeks, papulæ ten weeks, pustules nine months, and tubercles ten years. Dr. M'Carthy says, that Ricord's further experience confirms the opinion, that these are the usual intervals—we presume when mercury is given, for 118 of the 123 cases had been preceded by indurated chancre; and we find Mr. Acton stating on Ricord's authority, that when no mercury is given, from six weeks to two months is the average interval:

"That if a sore gets well without mercury, and the patient does not suffer under secondary symptoms within six months, they *never will occur;* he has a perfect immunity from them, provided he contract no second sore. *But,* if the sore be treated with mercury, the patient *may, it is true,* remain free from secondary symptoms some *months,*—yes, perhaps, *years.* They may be so slight as to pass unnoticed; or, lastly, such a patient may escape secondary symptoms altogether, and years will pass when the tertiary forms will break out." (p. 482.)

If the truth of this statement could be demonstrated, no argument could be brought forward with greater force against the utility of mercury as a preventive of secondary symptoms. If it lengthen the period during which the poison remains latent in the blood, and retard the local actions by which its presence can be alone recognised for months or years, such an action must be the very reverse of beneficial.

We think, also, that Ricord's doctrine, that one attack of secondary syphilis exhausts the susceptibility of the system to another, in other words, that a person once cured of secondary symptoms is not liable to them again, although he contract indurated chancre, requires investigation. Many cases might be quoted against it, but they might possibly be explained; and the subject is one well worthy of further inquiry, as is that of *syphilitic chlorosis.* Ricord finds the blood invariably altered, the red globules being diminished, often to a great extent, the sounds of the heart being modified, as in anæmia. Our hospitals afford an ample field of observation to any young chemist anxious to distinguish himself by original researches; and we hope this hint will not be lost.

The value of mercury, not in the prevention, but in the treatment of secondary symptoms, is another problem. Mr. Hunt is altogether for it in cutaneous affections; and he thinks he obtains peculiar advantages by giving five grains of blue pill night and morning, until some visible improvement occurs, then leaving it off until some return or increase appears, when another course is given; a third, or fourth, if necessary in the same way, each succeeding course being more energetic than its predecessor, until the disease is overcome. We must have further reports, however, of the results of this practice before deciding upon its success, although *à priori* it appears to be less injurious than long-continued courses of the mineral. Our own belief is, that each form of syphilitic eruption, each variety of secondary and tertiary affections, will be found to require a special treatment. Mr. Acton lays down some general rules, but does not enter into the details of treatment of the various forms of secondary and tertiary diseases. He considers mercury as the remedy in the secondary, iodide of potassium in the tertiary affections. He gives ten-grain doses of the iodide mixed with tincture of gentian and syrup, to be taken in infusion of quassia, one of the nastiest forms of administration that could be devised. He has completely misstated the claims and opinions of the late Dr. Robert Williams, with regard to the influence of the iodide in syphilis. Dr. Williams was the real discoverer of this influence, perhaps the greatest therapeutical discovery of the age, after that of the anæsthetic effects of ether and chloroform. His paper was read at the College of Physicians in 1834, five years before Ricord began his experiments; and so far from giving it indiscriminately in all cases, he took the greatest pains to investigate its real powers, and pointed out where it was efficacious and where useless; not with hesitation, but with all the open candour of his nature. In his 'Elements of Medicine,' while showing the marvellous certainty of its action in rupia and the hard periosteal node, he showed that its power was much less in roseola, purpura, and ecthyma, but still it was better than mercury; while, in lichen, lepra, psoriasis, and iritis, he proved, with equal clearness, that mercury, either locally or generally, had far more beneficial influence than the iodide. He pointed out the curious fact, that while the action of the iodide on *hard* periosteal node was as certain and evident as that of quinine in ague, when once suppuration had commenced, sarsaparilla was the remedy, the iodide being useless. In soft node and prurigo, he showed the true power of sarsaparilla; and in syphilitic angina and rupia, the invariably good effects of combining local mercurial applications with the internal administration of the iodide. Yet Mr. Acton says:

"The late Dr. Williams was a great advocate for iodide of potassium, and he seems to have given it pretty indiscriminately in all cases, in preference to mercury; but, on closely examining his cases, the reader will at once see, that the late physician to St. Thomas's Hospital admits its inefficiency, although he does it with hesitation, and believed that it cured some varieties of eruptions in preference to others." (p. 561.)

This is most unfair; and we are not sorry that the opportunity has been afforded us of doing justice to the memory of one of the most careful observers and philosophical physicians the age has produced. We witnessed many of his experiments, and for the last twelve years have been guided by his results, without ever having had cause to regret it; and after tolerably extensive opportunities of treating secondary symptoms, the only modification we have learnt to make in his practice, is the occasional use of the protoiodide of mercury in lichen and in some of the affections of ligaments and synovial membranes. We almost always give the dose recommended by Dr. Williams, eight grains three times a day in water or camphor mixture; and when using the protoiodide of mercury, begin with one grain daily in divided doses, increasing gradually to three or four grains in the day, made into pills with licorice, or with catechu, if it acts on the bowels. Opium appears to destroy its power altogether. We never saw any good done by giving a mercurial course before the iodide, as many recommend, but often much harm. On this point, and on the relative powers of iodide of potassium and mercury in syphilis, we would refer to a work in which the investigation has been made in the true spirit of science by Dr. Hassing, of Copenhagen. A notice of this book, and an abstract of some of the important results, will be found in the 'British and Foreign Medical Review' for 1845, Vol. XX, pp. 482-6.

After commenting so freely upon some of the most important questions suggested by a perusal of the works before us, some opinion may be expected from us as to their value.

We may say, then, that the pamphlet of Mr. Hunt will repay the expenditure of the half hour required for its perusal.—Mr. Johnson's book, without containing anything particularly novel or unusually important, may be considered as a very good account of gonorrhœa and its consequences. The hypercritical might notice and complain of a certain flippancy in the style, and an undue fondness for hackneyed quotations; but would allow that the work throughout was lively and readable.—Mr. Acton's handsome, well-printed volume is remarkable for an exceedingly full index, in which respect it might be advantageously imitated. In addition to the subjects upon which we have commented, it contains some very good introductory remarks on the numbers and condition of prostitutes in London, and on the means of diminishing the causes of syphilis; with some short chapters on syphilitic affections of the lungs, syphilitic cachexia, and syphilitic monomania, which are well worthy of attention. Readers of this article, however, will doubtless have perceived numerous faults in Mr. Acton's style and arrangement, with errors both in doctrine and practice. We do not think he has succeeded in the object expressed in his preface, of writing a complete practical treatise on Venereal Disease. A work to deserve such a designation, to merit a place in the study of the student and practitioner, with such special works as Mackenzie on the diseases of the Eye, Brodie on the Joints, Lawrence on Hernia, Clark on Climate, Cooper on Dislocations and Fractures, Louis on Phthisis, and Taylor on Poisons, or such general systematic treatises as Watson's Medicine, Fergusson's Surgery, Pereira's Materia Medica, Carpenter's Physiology, and Ramsbotham's Midwifery, has not yet been published. With a Lock Hospital, devoted expressly to Venereal Diseases, with large wards set apart for these diseases in our general hospitals, with thousands of outpatients annually treated both in hospitals and dispensaries, it must be allowed that ample opportunities for observation and study are supplied in London. It must at the same time be confessed, that the London School of Surgeons has done singularly little towards increasing our knowledge of these diseases. The great discovery of the value of the iodide of potassium was made by a physician, one only called upon to treat the secondary forms of syphilis; and with this exception, we should be at a loss to point out what science has gained by the writings of London surgeons on Syphilis, since the commencement of the present century. Mr. Acton does not supply the defect; for although he has assisted in the promulgation of Ricord's writings, it would be more easy than kind to define the value of his own contributions to the science and art of Medicine and Surgery.

ART. XII.

Observations relating to the Science and Art of Medicine. By WILLIAM WEGG, M.D. Cantab., Physician to the St. George's and St. James's Dispensary.—*London*, 1851. 8vo, pp. 233.

THE object of this work appears to be two-fold: first, to express the confidence which the author feels in the power of remedies when properly used; and, secondly, to inquire into the *modus operandi* of such remedies. Dr. Wegg believes, and we think justly, that from the extreme of *druggism*, we are about to fall into the deeper abyss of *expectantism;* and he has desired to express his opinion, that, in the hands of an experienced and cautious practitioner, drugs can perform all that can be reasonably expected from them. We are much inclined to agree with him in this opinion, and to believe that many men are too apt to excuse their own mistakes, and to palliate their own carelessness, by referring the unfavorable results of their practice to the imperfection of their instruments, or to the uncertainty of their art. No one can question the undoubted, and, in some cases, tremendous, power of the agents which are used in medical practice; and it appears to us equally certain that, in the majority of cases, these agents are employed usefully and well. And while the practitioner should never forget that the power of nature, that is, the never-ceasing changes which go on in the system, may, in many cases, gradually bring about the removal of an abnormal condition, it is equally his duty to know that he can assist this power, give it a particular direction, or enable it to overcome some obstacle which would otherwise impede its exercise. Uncertainty there is, and must be, in the use of drugs; but it appears to us nothing but madness to doubt, that, in many cases, to forego their exercise is to deny ourselves the use of some of the most potent means for good, which a benevolent Providence has placed in our hands. As an illustration of this, we need not refer to such obvious cases as the employment of quinine in ague, mercury and iodine in certain forms of syphilis, turpentine in some hæmorrhages, or aconite in many neuralgias; but we are prepared to say, that in common daily practice, the mercurials, antimonials, salines, diuretics, and purgatives, which are given to aid excretion, and to clear from the system substances which are wrongly retained there, are of the most incalculable service.

The rules which direct us in the employment of such agents, are almost entirely empirical. We find that under certain conditions a drug does good, and we employ it when those conditions present themselves. The *modus operandi* is often totally unknown; and though it would be very satisfactory to know it, yet we can dispense with it, and from experience alone, prescribe our remedies with very considerable success:

> "What explanation of the action of mercury," writes Dr. Wegg, "do we offer, when we say that it makes the mouth sore, affects the glands, regulates the inflammatory process, and removes the morbid product? These are but acknowledged facts, which the empirical use of the remedy has shown and observation has noticed. The use of some remedies has taught us that they will purge, and purge in different ways; that antimony, ipecacuanha, quinine, and a variety of other remedies have such and such effects, evident to our senses; and beyond this, genius and industry have never penetrated, and probably never will." (p. 23.)

If, indeed, we were to wait till physiology could trace our remedies through all the recesses of the frame, and indicate their combinations in all the intricacies of the vital laboratory, we might lay aside active practice altogether.

In speaking of the action of medicines, Dr. Wegg refers only to their visible effects; he does not attempt to trace all the links between the administration of the drug, and the manifestation of its final result; but merely considers under what conditions this result will be useful, and how, being useful, it may most readily be brought about. He considers, therefore, in as many chapters, the uses and effects of bloodletting, mercury, iodine, antimony, arsenic, iron, colchicum, opium, hydrocyanic acid, stramonium, strychnine, and quinine; and he devotes two chapters to the considerations of the alimentary canal, and of "support." He thus passes under review the majority of our most powerful remedies. There is nothing novel in his mode of treating this subject, and not much that is new in his rules and facts. He seems to have watched disease carefully; and partly from original observation, and partly from information obtained from books, he has acquired a good knowledge of the practical rules which guide us in our administration of drugs. The great fault of the book, however, is, that these are stated too generally; there is a great want of precision; and very common-place observations are given with an air of importance which is hardly necessary. Yet there is much good common sense and useful matter in the book, which we shall illustrate best by giving one or two extracts:

In speaking of the alimentary canal, Dr. Wegg thus writes:

"A hightly important action of medicines upon the intestinal surface remains to be noticed, as affecting its excretory function. By this it is not meant to express the process previously spoken of, which eliminates from the villous surface a fluid largely composed of water, containing the remains of epithelium, &c., and which almost any irritating cause may excite into unusual activity; but the excretory function of the glands, which thickly stud the surface of the bowels, and especially those of the large intestine. Although the lungs, liver, kidneys, and skin contribute largely to the depuration of the body, there is little doubt that these glands contribute greatly to the same result, though, probably, by expelling matter different from that which those other depurating organs eliminate." (p. 213.)

The following observations about iron present a favorable specimen of the therapeutics of the book:

"Though the several preparations of iron produce the same principal effects, they do not cause the same results in some minor points. Of all the forms of this agent, perhaps the most useful and active is the sulphate. More than any other preparation, it seems to check the secretion of the gastric and intestinal surfaces, and so to induce constipation. But this effect is not constant. Indeed, the sulphate sometimes purges slightly. It occasionally causes headache, though seldom if the bowels are kept regularly open. If there is a tendency to constipation, the sulphate may be advantageously given in solution with small doses of Epsom salts. Indeed, the operation of iron upon the system, particularly in cases of anæmia, is promoted by a very loose state of bowels." (p. 155.)

These extracts will enable our readers to form an idea of the contents of the book, which is undoubtedly the production of a sensible man, although we cannot compliment him on having elaborated a work of great originality or depth. There are also many observations in the book, from which we entirely dissent. For example, at page 214, Dr. Wegg states, that the affection of Peyer's glands in some epidemics "gave rise to the notion—now getting absolute—that the essence of fever lay in this lesion." This is perfectly wrong, as no one ever dreamt, except a few bunglers whose notions were not very clear, of confounding the essence of a disease with its anatomical sign. The intestinal lesion has never been considered as the essence of fever. So again, at page 14, the influence of pregnancy in arresting phthisis is referred to, although the observations of Grisolle, by far the most accurate which have been made on this point, go far to overturn this once generally received opinion.

Yet with these and many other points of difference, we regard Dr. Wegg's book as a meritorious one in many points, and it has given us pleasure to peruse it.

ART. XIII.

1. *The Correlation of Physical Forces.* By W. R. GROVE, M.A. F.R.S., Barrister-at-Law. Second Edition.—*London*, 1850. 8vo, pp. 119.
2. *On the Mutual Relations of the Vital and Physical Forces.* By WILLIAM B. CARPENTER, M.D. F.R.S. F.G.S., Examiner in Physiology and Comparative Anatomy in the University of London. (Philos. Transact., 1850.)—4to, pp. 30.
3. *Die organische Bewegung in ihrem Zusemmenhange mit dem Stoffwechsel. Ein Beitrag zur Naturkunde.* Von Dr. J. R. MAYER.—*Heilbronn*, 1845. 8vo, pp. 112.

On Organic Movement in its Relations to Material Changes. A Contribution to Natural Knowledge. By Dr. J. R. MAYER.

4. *The Philosophy of Vital Motion.* By CHARLES BLAND RADCLIFFE, M.B., Lecturer on Vegetable Physiology and Botany at the Westminster Hospital, &c. &c.—*London*, 1851. 8vo, pp. 158.
5. *The Philosophy of Spirits in Relation to Matter; showing the Real Existence of two very distinct kinds of Entity, which unite to form the different Bodies that compose the Universe, Organic and Inorganic; by which the Phenomena of Light, Heat, Electricity, Motion, Life, Mind, &c., are reconciled and explained.* By C. M. BURNETT, M.D.—*London*, 1850. 8vo, pp. 312.

No one can have watched the recent progress of Physical Science, without being struck with the great change which is taking place in its aspect, in consequence of the substitution of *dynamical* for *material* ideas and language, in expressing the phenomena of Light, Heat, Electricity, Magnetism, &c., which are now coming, by common consent, to be ranked as *forces*, instead of under the old self-contradictory designation of "Imponderable Elements," or immaterial matter. To develope the origin and growth of these ideas, and to show how they have gradually taken possession of the minds of the leading philosophers of our time, would doubtless be a very interesting topic of inquiry; but such would lead us too far away from our present object, which is to point out the alterations which these ideas will tend to introduce, in our mode of viewing the phenomena of Life, and even of Mind.

Those philosophers who have laboured most successfully to determine the laws of operation of these forces,—or, in other words, to ascertain the conditions of their action,—have been those who have been most impressed with the closeness of the relation which subsists between them. Among these, Professor Faraday stands pre-eminent for the extent and profundity of his researches; and he has on various occasions expressed himself strongly in favour of their essential unity. Thus, at the commencement of his memorable Bakerian Lecture for 1845, he thus speaks:

"I have long held an opinion, almost amounting to conviction, in common, I believe, with many other lovers of natural knowledge, that the various forms under which the forces of matter are made manifest have one common origin; or, in other words, are so directly related and mutually dependent, that they are convertible, as it were, one into another, and possess equivalents of power in their action. In modern times, the proofs of their convertibility have been accumulated to a very considerable extent, and a commencement made of the determination of their equivalent forces."

Probably the first systematic attempt, however, to *formularize* the whole series of these mutual relations, was made by Mr. Grove, who at that time held a Professorship in the London Institution, in a course of Lectures delivered before its members in 1842, and again in 1843. An Outline of this course, which constituted the first edition of the treatise that stands first on our list, was published in 1846; and this outline has been considerably expanded in the present republication, so as to make it much more intelligible to those who are not prepared, by previously-acquired knowledge, to enter into the author's views. Of these we now propose to offer our readers such a general sketch, as may, we trust, not only render them intelligible in themselves, but also show what is the method of investigation to be followed, when the inquiry is extended from the Inorganic to the Organic Universe, from the Physical Forces to the Vital.

The position which Professor Grove seeks to establish in this Essay, is,—"that the various *affections of matter* which constitute the main objects of Experimental Physics, viz., Heat, Light, Electricity, Magnetism, Chemical Affinity, and Motion, are all *correlative*, or have a reciprocal dependence; that neither, taken abstractedly, can be said to be the essential or the proximate cause of the others; but that either may, as a force, produce the others." (p. 13.) Thus Heat may, mediately or immediately, develope Electricity, as when an electric current is produced by heating a compound bar of bismuth and antimony; conversely, Electricity may develope Heat, as when that same electric current is made to pass through a very fine wire whose temperature it raises. As a mode of expressing the idea of "correlation" without any hypothetical assumption, Dr. Carpenter states it as follows:—"force A, operating upon a certain form of matter, ceases to manifest itself, but B is developed in its stead; and *vice versâ*, force B, operating upon some other form of matter, ceases to manifest itself, but A is reproduced in its stead." Now this, be it observed, is a simple statement of facts, and involves no hypothesis; and it is further to be noted, that it has all the precision of a physical law, since a certain definite ratio or equivalent always exists between the two forces thus mutually interchangeable; so that the measure of force B, which is excited by a certain exertion of force A, shall, in its turn, give rise to the same measure of force A as that originally in operation. Thus, to use Dr. Carpenter's illustrations:

"When an electric current is set in motion (to use the common phraseology) by galvanic action, the amount of chemical decomposition which it will effect bears a precise correspondence (*cæteris paribus*) with the amount of zinc which has undergone oxidation; chemical action thus exciting electricity, which in its turn reproduces the original equivalent of chemical action.—In like manner, when water at 212° is converted into steam, the heat which it receives is no longer manifested *as* heat, but mechanical force is developed in its stead, and this in a certain definite ratio; as soon, however, as the steam, losing its elasticity by condensation, returns to the condition of water, the original equivalent of heat is again developed, its mechanical force being no longer manifested." (p. 731.)

One circumstance is pointedly adverted to by Dr. Carpenter, on which

Professor Grove has not thought it requisite strongly to dwell; namely, the necessity for a certain *material substratum*, as the medium of the change in question:

"Thus, to take a familiar case, the correlation of Electricity and Magnetism is indicated by the development of magnetic attractions and repulsions in iron, when a current of electricity is made to circulate around it. In like manner, the correlation between Heat and Electricity is shown in the disturbance of electric equilibrium which ensues on the application of heat to bars of certain dissimilar metals (especially bismuth and antimony) in contact with each other. The iron, in the first case, is the necessary medium for the development of the magnetic force by electricity; as the bars of dissimilar metals are in the second, for the development of the electric force by heat. So, again, in the so-called 'magnetization of light,' by Professor Faraday, it seems necessary that the magnetic force should act through some material substratum, in order to produce any effect upon the luminous ray; the intensity of the effect varying according to the medium employed." (p. 732.)

This consideration will hereafter be found to be of great importance, when the relation of the Physical and Vital Forces comes to be inquired into. The *material substratum* may be of almost any description whatever; as when Heat is produced by the friction or retarded Motion of solids, liquids, or even gases; or when Motion (as shown in expansion) is produced by the application of Heat to any kind of material substance. But in other cases, the change can only be effected through some *special* form of matter; or if several substances may serve as its medium, there is some one which is greatly superior to all the rest, in the readiness with which a certain force manifests itself through it. Thus, iron is the substance through which an Electric current can best develope Magnetic force; the affection of Light by Magnetism, though producible through *any* transparent medium (but not through a vacuum), can be made much more obvious when the magnetism is made to act upon a peculiar glass composed of vitrified borate of lead, than through the medium of any other substance yet known. This *speciality* in the action of different substances, when subjected to the play of the same forces, is a fact of fundamental importance; and it is on it, indeed, that our notion of their several *properties* depends.

The phenomena of "correlation" have been previously viewed under a very different aspect. It has been imagined that Heat, for example, could exist in a *dormant* or *latent* form; so that, when it ceased to manifest itself *as* heat, it is still present in the substance to which it has been communicated, its change of condition being coincident with a change of condition on the part of the substance. Thus, for the conversion of water at 212° into steam at 212°, an amount of heat is required, which would suffice to raise the temperature of the water no less than 960°, if it remained in the liquid form; but, as the steam is not sensibly hotter than the water, it has been considered that the heat becomes latent. And this idea seemed to be confirmed by the fact, that when the steam is brought back by condensation to the inelastic or liquid form, the 960° of heat are given out again; so that by the condensation of one pound of steam, eight pounds of water might have their temperature raised 120°. And this doctrine of *latent heat* seemed so accordant with the phenomena which it was invented to explain, that, since the time of its complete enunciation by Dr. Black, it has been accepted more as a *fact*, than as what it really is, an *hypothesis.* For, let it be observed, it gives no account whatever of the mechanical force which is generated when the liquid is changed into elastic vapour, nor of the cessation of that force when the vapour is condensed into liquid; and the very notion of *dormant force* which it involves, is one which cannot be satisfactorily conceived, since if a force ceases to act, it is no longer a force; and no indication whatever of the presence of heat, *as* heat, can be obtained from the steam, beyond that which the boiling water itself affords. Further, it has always been found very difficult, on this hypothesis, to account for the production of heat by friction; for it has been satisfactorily shown by experiment, that there is no limit to such production; and yet it is difficult to conceive that any substance can possess an unlimited amount of latent heat locked up in itself, waiting to be liberated by the application of mechanical force. On the other hand, this production of heat by friction, to an unlimited extent, is a phenomenon which tells strongly in favour of the "correlation" doctrine; for this expresses, without any hypothesis, the simple fact that, in proportion to the retardation of motion (that is, in proportion as the force that produces motion ceases to manifest itself as such,) is heat developed in its stead.

It is difficult, we think, for any one who receives this doctrine as a general expression of the mutual relation of the different physical forces, not to go further; and recognising in these several forces the different *modi operandi* of one and the same power, to admit that they are actually transformed or converted into each other. To the minds who have been led, by their exclusive devotion to some one branch of scientific inquiry, to look rather at the distinctions than at the points of accordance between these forces, to study them in their isolated manifestations rather than in their reciprocal bearings, it may be difficult to conceive of such a conversion or metamorphosis. But the difficulty is far less to those who have been accustomed to look deeper than the phenomenal surface, and to consider the nature of Force in the abstract. In fact, there are psychologists of the present day, who are so fully possessed with the idea of the inherent unity of all power, as necessarily derived from the mental or subjective idea of causation, that they look upon the labours of those physical philosophers who have laboured in this field as entirely superfluous. We quote the following view of the subject from a recent very able writer, not because we agree with his low estimate of the value of the inquiries to which he refers, but because we wish to show our readers that there are important considerations derivable from an entirely different field of inquiry, which tend to the same conclusions with those at which our most distinguished physical philosophers are being led by their own investigations.

"Our knowledge of causation being altogether a subjective disclosure, it follows that the sole authority to which we can appeal in every question of this nature is our own idea of Cause. This one constant type of thought is to be carried by us into all experience, and to serve as the *regula naturæ.* Phenomena are to be construed by it, and can add nothing to it. They start up; and, as we notice them, we are constrained to think of them as the expression of *power:* and whenever they come, and whatever they be, this is the self-identical thing that occurs to us over and over again. In its original subjective seat of authority, therefore, all Causality is one; it lies there in undistributed unity of type. How, then, do we come to speak of a plurality of different causes, such as electricity, gravitation, and chemical action? Does this classification belong to our knowledge of causation, or to our knowledge of phenomena? Evidently to the latter. The only ground for the discrimination lies in the resemblances and differences prevailing among the effects. A family likeness running through a group of phenomena, and their observable conditions, induces us to set them apart, to think of them together, and make propositions—among others, causal propositions—about them collectively. It is because the movement of the tides, the path of a projectile, and the revolution of a planet, present, as events, certain features of similarity, that we refer to their cause under the one term—Gravitation. It is because the flash of lightning and the spark from the excited surface of a non-conductor are alike in themselves and different from the others, that we denote their source by a distinct name,—Electricity. Of these, however, as two forces different *per se*, we know nothing: the causation which we think in the one, is identical with that which we think in the other: now it is Power issuing one class of phenomena, and then Power producing another. The whole arrangement lies in the fact perceived, not in energies conceived: it is sensible, not dynamical. It is, therefore, altogether an illusion to suppose that science can take us behind phenomena, and lay out before us a series of physical, or vital, or mental forces, distinguished from one another by certain special characters; and it is a deeper plunge into the same illusion, to discuss the convertibility, the correlation, the possible identity of these forces; as if their specific difference, as δυνάμεις, was the present *datum* of our knowledge, and their sameness the future *quæsitum*. Not that we, in the least, deny the importance of such researches as those of Faraday and Matteucci, which cannot fail to give new insight into the order and analogies of natural events. Only let it be clearly understood that they are *phenomenal* investigations, prosecuted under the disguise of *dynamical* language. *In rerum naturâ*, as apprehensible by us, there are not several kinds of causes, but only several kinds of effects: and causation, while it is given to us as an object of knowledge by the testimony of our own faculties, possesses, as such, that perfect unity which belongs, as their distinctive mark, to all things cognisable *à priori*. To determine the nature of the one uniform cause, the process—since we have to do with an object subjectively known—must be altogether psychological: no physical inquiries can ever throw the least light upon the matter; on the contrary, the first aim must be to strip off the distinctions and classifications accumulated by empirical perception, and reach the primitive type,—the central nucleus of thought,—which logically constitutes the idea of Cause." ('Prospective Review,' May, 1851, p. 240.)

We must again express our conviction, that the very able writer from whom we have quoted, is led by his predilection for psychological investigations, to undervalue these direct inductions from phenomena, which constitute the strength of Physical Science. And herein do we see another example of the evil results of that isolation of Psychological and Physiological inquiry (we here use the word *physiological* in its wide sense, the study of *Nature* in general), which has led to misapprehension on both sides. It will only be, we feel assured, when the reasoner on the phenomena of the universe has been led, by a higher logic than even that of Bacon, to look for their immediate causation in mental agency, and when the seeker after the knowledge of mind studies it in its external or objective manifestations, as well as in the analysis of his inner consciousness, that the full harmony of creation shall be evolved, and the fundamental accordance between science and religion rendered obvious. Until that time arrives, we shall have, over and over again, to witness the painful spectacle of a writer, who professes to be a votary of Science, setting it up as hostile and superior to Religion, and of a writer who advocates the cause of Religion declaring the conclusions of Inductive Science to be at variance with its essential doctrines. To this topic, however, we shall recur on a future occasion; at present, we shall take a cursory survey of the ground over which the writers on 'Correlation' have struck out new paths.

Commencing with *Motion*, Professor Grove points out that, although it is the most obvious and most distinctly conceived of all the affections of matter, yet "there are certain vanishing shadows or undefined limits, at which the obvious mode of action gradually disappears; and to detect the continuing existence of the phenomena, we are obliged to have recourse to other methods of investigation, and we frequently apply other names to the effects so recognised." Thus Sound is universally admitted to be motion, although it is only in certain cases that we can clearly discern the identity; and there is such an analogy between the phenomena of transmission of sound, and those of light and electricity, that "it requires no great stretch of imagination to conceive light and electricity as motions, and not as things moving." Professor Grove argues, and we think quite successfully, both from abstract considerations and from observation of natural phenomena, that there is no such thing as annihilation of force; but that when a force ceases to manifest itself, it is merely subdivided or altered in direction or character. Thus, if it be asked, what becomes of force, when the motion of one body is arrested or impeded by the countermotion of another body, —in which case, a state of perfect rest, that is to say, entire destruction of motion, and consequent annihilation of force, are usually supposed to be produced,—Professor Grove replies, that "the *Heat* which results from friction or percussion, may be regarded as a continuation of the force which was previously associated with the moving body; and which, when this impinges on another body, ceasing to exist as gross palpable motion, continues to exist as heat." In support of this position he adduces the well-known fact, that the more completely the motion of bodies is destroyed by percussion, (as happens, of course, when very hard bodies impinge on each other,) the greater is the amount of heat developed; whilst the more the motion of the moving body is communicated to surrounding bodies, (as happens especially when it is immersed in a fluid, or when the fluid is interposed between rigid moving bodies,) the less heat is generated. "I cannot present to my mind," says Mr. Grove (p. 20), "any case of heat resulting from friction, which is not explicable by this view; friction, according to it, is simply impeded motion. The greater the impediment, the more force is required to overcome it, and the greater is the resulting heat; this resulting heat being a continuation of indestructible force, capable, as we shall presently see, of reproducing palpable motion, or motion of definite matter."

It is only, however, when the two bodies which impinge or rub together are *perfectly* homogeneous, that heat *alone* is produced; in all cases in which the slightest difference exists between them, *Electricity* also is generated by retarded motion. The most perfect accordance between the two surfaces appears to be obtained, when the two ends of a broken bar are rubbed together, and no electrical currents can then be detected by the most careful observers; whilst in all other cases they are excited, even when the substances are chemically the same, but differ only in the character of their surfaces, as, for instance, rough and smooth glass, cast and wrought iron. As yet, however, the characters by which the ratio of electricity, produced by retarded motion, is determined in different substances, cannot be stated with any precision.

"Bodies may differ in so many particulars which influence more or less the development of electricity, such as their chemical constitution, the state of their surfaces, their state of aggregation, their transparency or opacity, their power of conducting electricity, &c., that the *normæ* of their action are very difficult of attainment. As a general rule, it may be said that the development of electricity is greater when the substances employed are broadly distinct in their physical and chemical qualities, and more particularly in their conducting powers; but up to the present time, the laws governing such development have not been even approximatively determined." (p. 22.)

Not only heat and electricity, but also *Light*, may be directly generated from motion, as when light accompanies the heat of friction. It may also be produced mediately, by the aid of electricity developed by motion; the electric spark having all the attributes of common light, and differing from it in no other respect than that in which luminous spectra produced by light emanating from different sources, or seen through different media, differ from each other. *Magnetism*, likewise, may be developed mediately, through the agency of electrical currents excited by motion. So, again, *Chemical Affinity* may be developed from motion, through the medium of electricity; as when changes of composition are produced in substances, in which the terminal point of the conductors of an electric machine may be immersed. It occurs to us, however, that Professor Grove might find instances of chemical affinity developed immediately by motion, in those numerous cases in which the slightest mechanical disturbance serves to produce a new arrangement of the component elements of compound bodies; thus, the faintest friction or the slightest blow causes fulminating mercury and fulminating silver to explode; and though it may be asserted, that the effect is here produced by the intermediation of heat, it is difficult to imagine that such can be the case when the chloride or iodide of nitrogen, or the ammoniacal oxide of silver, are violently decomposed by the mere touch of a feather.

Thus, directly or indirectly, Motion may be made to give origin to all the other Physical Forces; the amount in which they are generated being proportional, *cæteris paribus*, to the amount of motor force which has been expended; that is, which ceases to manifest itself in motion. But, conversely, Motion may be again reproduced by all the forces which have emanated from it; thus, "the divergence of the electrometer, the revolution of the electrical wheel, the deflection of the magnetic needle, are, when resulting from frictional electricity, palpable movements reproduced by the intermediate modes of force, which have themselves been originated by motion."

If we now take Heat as our starting-point, we shall find that its relations to other modes of force are yet more intimate. Thus, Motion is so generally the effect of Heat, that the latter has long been viewed by many philosophers simply as a mechanically repulsive force, antagonistic to the attraction of cohesion or aggregation, and tending to *move* the particles of all bodies, or to separate them from each other. In considering this relation, it is necessary to put aside for the time the sensation which heat produces on our own bodies, and to regard it simply as to its effects upon inorganic matter; and it then becomes obvious that, with very few exceptions, the ostensible effects of what we call heat are simply an expansion of the matter acted on, the matter so expanded having the power also of communicating the same condition to all bodies in contiguity with it.

The phenomena of what is termed *latent heat* have been considered, it is true, by many eminent philosophers, as telling strongly in favour of that view, which regards heat, either as actual matter, or, at all events, as a substantive entity, and not a motion or affection of ordinary matter. But as already shown, if we admit the convertibility of heat into motion, and the reconvertibility of motion into heat, all difficulty in regard to the dynamical view of heat at once vanishes, and we get rid of the very unphilosophical and really undefinable notion of "latent matter," of a something which is not tangible, visible, or audible; but which, like a mathematical point that has neither length, breadth, nor thickness, is a subtle mental conception, not to be admitted as a physical reality. Professor Grove analyses several of the cases, which have been supposed to be strongest against the dynamical theory, and, in our apprehension, explains them satisfactorily; the following example, furnished by the well-known experiment of Thilorier, will serve as an illustration of his method of reasoning on the subject:

"Carbonic acid gas, retained in a strong vessel under great pressure, is allowed to escape from a small orifice; the sudden expansion requires so great a supply of force, that in furnishing the demands of the expanding gas, certain other portions of the gas contract to such an extent as to solidify: thus, we have reciprocal expansion and contraction going on in one and the same substance, the time being too limited for the whole to assume an uniform temperature, or, in other words, an uniform extent of expansion." (p. 34.)

It must be obvious to our readers, that our sensations of the effects of Heat upon our own consciousness cannot be taken as evidence of anything else than of its operation upon our bodily organs; and that this operation may take place in several conceivable modes, none of which is inconsistent with the doctrine here propounded. Thus we may imagine that the agency of heat may be exerted in altering either mechanical, chemical, or electrical relations of the organic substances on which it acts; but, as formerly pointed out (Vol. I, p. 233), its operation is probably more direct and specific than this, and we are rendered conscious of its agency, in virtue of its direct and specific relation to nervous force. It is most important, however, to remind our readers, before going further, that the sensation of heat bears no relation in its intensity to the amount of heat applied, and that it may be produced without any actual elevation of temperature; thus the application of an intensely cold substance (such as frozen mercury) produces the same sensation as that occasioned by an intensely hot body; and in inflammation the feeling of heat is beyond all proportion to the actual elevation of temperature, whilst the same feeling may even be experienced in the most distressing degree, without the least augmentation in the normal temperature of the part to which it is referred. Hence it is evident, in the first place, that our sense of heat is a most fallacious guide, not only as to the *degree* of that agent which gives rise to our consciousness of its presence, but also as to its actual operation; whilst, secondly, we must admit that other agencies besides heat may give rise to the *feeling* of heat,—a fact of great importance, as will be hereafter shown, in our physiological inquiries.

Of the production of Electricity by Heat, the phenomena first brought into view by Seebeck, and known under the name of "Thermo-Electricity," afford the most characteristic example. When dissimilar metals are made to touch, or are soldered together, and are heated at the point of contact, a current of electricity is set in motion, which has a definite direction according to the metals employed, and which continues as long as an increasing temperature is gradually pervading them, ceasing when the temperature is stationary, and flowing in the contrary direction whilst it is decreasing. Advantage is taken of this relation of heat to electricity, and also of the relation of electricity to magnetism, in the construction of the *thermo-multiplier*, which is by far the most delicate instrument known, for measuring slight changes of temperature. In this instrument, the electric current, set in motion by the application of heat to the united extremities of small bars of antimony and bismuth, and intensified by passing through a succession of these, is conveyed to a galvanometer, in which its existence is manifested by the deflection of the magnetic needle; the degree of this deflection thus serving as the index of the temperature.

To say that Heat will produce Light, is, as Mr. Grove remarks, to assert a fact apparently familiar to every one; but there may be some reason to doubt, he continues, whether the expression "to produce light" is correct in this particular application; for the relation between heat and light scarcely seems to be analogous to the correlation between these and the other four affections of matter. Heat and Light, in fact, appear rather to be modifications of the same force, differing in the modes in which they respectively affect our senses, than to be absolutely distinct forces; the experiments of Melloni having shown that a very close analogy exists between them, in the conformity of their laws of reflection, refraction, double refraction, and polarisation.

The only method by which Heat can be strictly said to *produce* either Chemical Affinity or Magnetism, is through the medium of an Electric current; but this agent directly affects and modifies both magnetic action and the play of chemical affinities. Thus it has recently been shown by Professor Faraday, (Philosophical Transactions, 1851,) that the magnetic condition of oxygen is greatly affected by heat, so that oxygen is a much better conductor of magnetic force at low temperatures than at high; and in this manner he has most ingeniously and satisfactorily accounted for the annual and diurnal variation of the magnetic needle, which takes place in different degrees over different parts of the earth's surface; since the lines of magnetic force which issue from the earth, will have their curvature greatly affected by the degree of facility with which they are transmitted through the atmosphere; and it is on *their* direction, that the direction of the needle depends. A relation between Heat and Chemical Affinity is indicated by the multitude of cases in which the union or separation of chemical substances is brought about by changes of temperature; as, for instance, when oxygen and hydrogen gases are made to unite by heat; or when ammonia is separated from its compounds by the same agency. And though it be true that, in many of these cases, the force of heat seems to act more as a *determining* than as a *producing* influence; yet to operate even thus, as Professor Grove justly remarks, it must have a definite relation with the force whose reaction it determines:

"Thus, although gunpowder, touched with an ignited wire, subsequently carries on its own combustion or chemical combination, independently of the original source of heat, yet the chemical affinities of the first portion touched must be exalted by, and at the cost of, the heat of the wire; for to disturb even an unstable equilibrium requires a force in direct relation with those which maintain equilibrium." (p. 45.)

Since the First Edition of Professor Grove's Essay was published, he has communicated to the Royal Society some most interesting experiments, by which an important exception to the general action of heat on chemical affinity is removed, whilst the probability of a general relation between heat, chemical affinity, and physical attraction, is proportionally increased:

"I find that if a substance capable of supporting an intense heat, and incapable of being acted upon by water or either of its elements,—such, for instance, as platinum, or iridium,—be raised to a high point of ignition and then immersed in water, bubbles of permanent gas ascend from it, which on examination are found to consist of mixed oxygen and hydrogen in the proportion in which they form water. The temperature at which this is effected is, according to Dr. Robinson, who has since written a valuable paper on the subject, =2386°. Now, when mixed oxygen and hydrogen are exposed to a temperature of about 800°, they combine and form water; heat therefore appears to act differently upon these elements according to its intensity, in one case producing composition, in the other decomposition. It seems not a far-strained theory to account for these diverse effects by the fact that the constituent molecules of the oxyhydrogen gas are at ordinary temperatures in a state of stable equilibrium, the attractive force being in excess; while at higher temperatures the equilibrium is unstable, the attractive and repulsive forces being balanced. In the former case, to destroy the equilibrium an antagonising force greater than that which maintains it must be employed; in the latter, any force either antagonising or co-operating, will destroy the equilibrium, provided it is a disturbing force, and not absolutely uniform in its action." (pp. 45-46.)

It is interesting to remark, that the only mode in which Professor Grove could succeed in producing and sustaining the required degree of heat in a metallic substance in contact with water, was by transmitting through it a powerful voltaic current. The following are the general inferences to which this experiment leads:

"Upon this view, heat has the same relation to chemical affinity as it has to physical attraction; its immediate tendency is antagonistic to both, and it is only by a secondary action that chemical affinity is apparently promoted by heat. Heat may also, upon this view, promote changes of the equilibrium of chemical affinity among mixed compound substances, by decomposing certain compounds and separating elementary constituents whose affinity is greater, when they are brought within the sphere of attraction for the substance with which they are mixed, than for those with which they were originally chemically united: thus an intense heat being applied to a mixture of chlorine and the vapour of water occasions the production of muriatic acid, liberating oxygen.

"Carrying out this view, it would appear that a sufficient intensity of heat might yield indefinite powers of decomposition, and there seems some probability of bodies, now supposed to be elementary, being decomposed or resolved into further elements by the application of heat of sufficient intensity; or, reasoning conversely, it may fairly be anticipated, that bodies which will not enter into combination at a certain temperature, will enter into combination if their temperature be lowered, and that thus new compounds may be formed by a proper disposition of their constituents when exposed to an extremely low temperature." (pp. 47-48.)

Of all the physical forces, there is no single one whose relations with all the others are so intimate, and so capable of being quantitatively determined, as *Electricity*. Our readers must be well aware, that the notion of a distinct "electric fluid," or of two such fluids, was long entertained as the most feasible mode of representing electric phenomena. At the present day, however, there are few, if any, among those philosophers who have most deeply studied this department of science, who have not altogether abandoned this hypothesis, and embraced the doctrine that Electricity, like Magnetism, is a *polar force,* and that its phenomena are due to attractions and repulsions operating in definite directions between the particles of matter. In this, as in many similar cases, the ordinary language of the science still manifests the influence of the ideas on which its doctrines were formerly based; thus, we continually speak of an "electric current," as if *a something* moved from one end of the circuit to the other; whereas the transmission is now usually attributed to the polarisation of the successive molecules of the substance forming the circuit, each molecule altering the polar state of the one beyond it, as it was itself altered by the one before it. The electric spark, the brush, and similar phenomena, which, on the old theories, were regarded as material emanations of the Electric fluid, are considered by Professor Grove as produced by an emission of the material from which it issues; a view of the subject which derives full confirmation from the variations in the colour of the electric light, which are caused by varying the metallic bodies from which it is given off; the colours produced being those exhibited by the respective metals in their ordinary combustion. And further, when an electric or voltaic discharge takes place, the particles emitted from the metallic electrodes are sufficiently large and numerous to admit of being readily collected, tested, and weighed. The phenomena of the voltaic arc are peculiarly interesting, when viewed with reference to this question; and tend, as Professor Grove justly remarks, to modify considerably our previous idea of the nature of the electric force of ignition, and also of combustion:

"The voltaic arc is, perhaps, strictly speaking, neither ignition nor combustion: it is not simply ignition, because the matter of the terminals is not merely brought to a state of incandescence, but is physically separated and partially transferred from one electrode to another, much of it being dissipated in a vaporous state: it is not combustion, for the phenomena will take place independently of atmospheric air, oxygen gas, or any of the bodies usually called supporters of combustion, combustion being, in fact, chemical union, attended with heat and light. In the voltaic arc we may have no chemical union; for if the experiment be performed in an exhausted receiver, or in nitrogen, the substance forming the electrodes is condensed, and precipitated upon the interior of the vessel, chemically speaking, in an unaltered state: thus, to take a very striking example, if the voltaic discharge be taken between zinc terminals in an exhausted receiver, a fine black powder of zinc is deposited on the sides of the receiver; this can be collected, and takes fire readily in the air by being touched with a match, or ignited wire, instantly burning into white oxide of zinc: to an ordinary observer, the zinc would appear to be burned twice; first, in the receiver, where the phenomenon presents all the appearance of combustion; and, secondly, in the real combustion of air. With iron the experiment is equally instructive. Iron is volatilised by the voltaic arc in nitrogen, or in an exhausted receiver, and when a scarcely perceptible film has lined the receiver, this is washed with an acid, which then gives, with ferro-cyanide of potassium, the prussian blue precipitate: in this case we readily distil iron, a metal by ordinary means *fusible* only at a very high temperature." (pp. 50-51.)

Of the production of Motion by Electricity, it is not requisite to cite any further examples; since a number of phenomena of this character must be familiar to the minds of our readers. The production of Heat by Electricity is shown in the familiar phenomenon of the ignition of a fine wire by the voltaic current; the heating power of which is conformable to the retardation of the transmission of the electric force, just as the amount of heat generated in friction is conformable to the retardation of the motion of the rubbed bodies. This is seen not only in the fact, that the rise of the temperature in the wire is inversely proportional, *cæteris paribus*, to its size,—a large wire allowing the free transmission of a current, which cannot pass through a small one, and the latter being consequently heated to ignition, whilst the temperature of the former is scarcely elevated,—but also in the alteration in the conducting power of the same wire, according to its relation with external heat; as is shown in the following beautiful experiments:

"Let a thin wire of platinum join the terminals of a voltaic battery of suitable power, the wire will be ignited, and a certain amount of chemical action will take place in the cells of the battery,—a definite quantity of zinc being dissolved and of hydrogen eliminated in a given time. If now the platinum wire be immersed in water, the heat will, from the circulating currents of the liquid, be more rapidly dissipated, and we shall instantly find that the chemical action in the battery will be increased, more zinc will be dissolved, and more hydrogen eliminated for the same time.

"Reverse the experiment, and instead of placing the wire in water, place it in the flame of a spirit lamp, so that the force of heat meets with greater resistance to its dissipation. We now find that the chemical action is less than in the first or normal experiment. If we place the wire in other different gaseous or liquid media, we shall find that the chemical action of the battery will be proportioned to the facility with which the heat is circulated or radiated by these media, and we thus establish an alternating reciprocity of action between these two forces." (pp. 52-53.)

A similar relation of reciprocity will be detected, Professor Grove feels assured, in all such cases of metamorphosis; and if we cannot realise it in all, it is because we have not yet eliminated interfering actions. And, as he justly remarks, it seems impossible to conceive it to be otherwise, unless we suppose that force can arise from nothing, or can be extinguished, both which propositions are equally untenable.

The production of Light by Electricity is made obvious, not only in the ordinary electric discharge, but still more in the ignition of various substances in the voltaic arc, which produces a light whose intensity cannot be paralleled elsewhere. On the production of Magnetism by Electricity, there is no need to dwell, since this is now universally admitted. As first pointed out by Orsted, these two forces act upon each other, not in straight lines, as do all other known forces, but in a rectangular direction; that is, bodies through which an electric current is being transmitted, tend to place magnets at right angles to them; whilst, conversely, magnets tend to place bodies conducting electricity at right angles to them. Hence it is, that the passage of an electric current across a bar of soft iron renders it magnetic; and that the transmission of a current through a helix coiled round a cylinder of soft iron, converts the latter into a powerful magnet so long as the current is circulating. Of the production of Chemical Affinity by Electricity, whereby we are enabled to obtain effects of analysis and synthesis with which ordinary chemistry does not furnish us, it cannot be necessary to say anything; since all our readers must be prepared to admit the existence of this relation.

As the relations of Electricity to the other Physical Forces are the most obvious, and have been the most completely developed, so those of *Light* are at present most obscure and indeterminate. The labours of *photo-*

graphers, however, have done much to elucidate the chemical actions of this force; and although some among them are disposed to attribute these to an agency distinct from light, yet, as Professor Grove justly remarks, since the actions which the different coloured rays produce in different chemical compounds, are not usually proportionate in intensity to the effects of these rays upon the visual organs, the fact that chemical effects are produced also by non-luminous rays, does not indicate more than a difference in the *degree* of effect produced by the several components of the solar beam, and it is not requisite to attribute to the chemical or actinic rays (as some have called them), a difference in kind. Although the capability of Light to produce the polar forces of Electricity and Magnetism, and also to determine crystallisation, may be suspected from the results of various experiments, yet these have hitherto been of so indefinite a character, that they can only be regarded as justifying the supposition, and not as furnishing adequate proof. The idea of the direct production of Heat by Light, however, is capable of being much more definitely substantiated; and it seems to enable us to resolve a difficulty that has been felt by all optical investigators in regard to the absorption of light, a phenomenon of which neither the "undulation" nor the "emission" theories has yet given a satisfactory account. When light falls upon a perfectly black surface, it ceases to manifest itself any longer, being neither reflected nor transmitted; and here it is impossible to apply the hypothesis of *latency,* since the light which is said to be absorbed by the black surface cannot be reproduced from it by any treatment whatever. Now, whilst light continues *as* light, even though reflected or transmitted by different media, little or no heat is developed, provided the media are perfectly transparent; but wherever light is absorbed, either in reflection or transmission, then heat manifests itself in its stead. In support of this view, Professor Grove refers to the well-known experiment of placing a series of different-coloured pieces of cloth upon snow exposed to sunshine; the black cloth absorbs the most light, and is shown to develope most heat by sinking more deeply in the snow than any others; and the other colours or shades sink the more deeply in proportion as they absorb or cause to disappear more light, until we come to the white cloth, which, reflecting the greater part of the light, remains upon the surface. This heating power of the dark cloth becomes the more remarkable, and the conversion of light into heat becomes more probable, when it is remembered that the heat which directly acts upon the different surfaces, if more readily absorbed by the black cloth, is also more readily given off by radiation; and this is accordant with the familiar experience, that blackened bodies exposed to the solar rays not only give off more heat by radiation, but also have their own temperature more raised, than objects of a lighter colour, thus showing a surplus of heat which can scarcely be accounted for, save by the metamorphosis of light.—In regard to the nature and transmission of Light, Professor Grove adopts neither the "corpuscular" nor the "undulatory" theories; but suggests a view which seems to avoid the chief difficulties of both, and to combine their excellencies. On the corpuscular theory, light is a substance, the particles of which travel in straight lines through space; and it is requisite for the explanation of Newton's rings, and other optical phenomena, to have recourse to the somewhat cumbrous hypothesis of "fits of easy transmission." On the undulatory theory, light is supposed to be transmitted by vibrations or undulations, just as sound is propagated by the vibrations of wood, or as waves are by water; but as the medium for these undulations, a hypothetical ether is required, the assumption of which is attended with many difficulties. Professor Grove adopts the doctrine of undulations; but considers that luminous, like sonorous undulations, are propagated by the matter through which they pass, and holds that matter in a finely-divided state must exist even where there is apparent vacuity. On *any* hypothesis, as he justly remarks, *some* form of matter must exist throughout space; since we cannot conceive of the transmission of light, unless (1) it be itself corpuscular, or (2) it be transmitted by the vibrations of a luminiferous ether, or (3) it be propagated by the vibrations of finely-divided particles of ordinary matter, so attenuated that we cannot recognise it by the tests of other forces, such as gravitation. Of these, the last seems to be the least gratuitous.

Magnetism differs from the other physical forces, in being itself *static* or *directive* merely, altering the direction of other forces, but not having a motive or originating power. In order to produce a dynamic force, therefore, motion must be superadded to it; and thus it is, that when magnets are moved in the direction of the lines joining their poles, electrical currents are produced in such neighbouring bodies as are conductors of electricity, in directions transverse to the line of motion. Through the medium of the Electricity thus induced, Magnetism can excite Heat, Light, and Chemical Affinity; and its presence is ordinarily made known to us by the Motion which it produces, either in other ferreous bodies, which are made to move towards or with the magnet, or in the magnet itself when this has been moved out of its position of equilibrium. Thus, as Professor Grove remarks, "by motion or arrested motion only, could the phenomena of magnetism ever have become known to us: a magnet, however powerful, might rest for ever unnoticed and unknown, unless it were moved near to iron, or iron moved near to it, so as to come within the sphere of its attraction." For a very ingenious explanation of the relation of such a *directive* force to a *motor* force, we would refer our readers to Professor Grove's treatise, (pp. 65-7,) the passage being too long to quote here. But though Magnetism cannot initiate the other forces, it may change their direction or mode of action; or, at all events, it may so affect matter subjected to these forces, that their direction is changed. Of this, we have a very remarkable example in the rotation of the polarized beam, which has been shown by Professor Faraday to take place, when the line of magnetic force is made to pass through transparent media. The same effect has been observed in regard to Heat. So, again, with regard to Chemical action; there are many experiments which show that when substances are undergoing chemical changes, and a magnet is brought near them, the direction or lines of action of the chemical force will be changed. This interesting subject is at present undergoing investigation by Mr. Robert Hunt, Mr. Wartmann, and others; and it is probable that ere long, some definite laws may be discovered, by which the results may be positively expressed.

There is this peculiarity, however, in the condition of Magnetism at the commencement and termination, or during the increment or decrement, of its development; namely, that it then exhibits a dynamic force, which indicates a molecular movement in the particles of the substance exhibiting it. The following experiment, published by Professor Grove in 1845, illustrates this, and tends to show the character of the motion impressed on the molecules of a magnetic metal at the period of magnetisation.

"A tube filled with the liquid in which magnetic oxide of iron had been prepared, and terminated at each end by plates of glass, was surrounded by a coil of coated wire. To a spectator looking through this tube, a flash of light was perceptible whenever the coil was electrised, and less light was transmitted when the electrical current ceased, showing a symmetrical arrangement of the minute particles of magnetic oxide while under the magnetic influence." (p. 73.)

So both electricity and heat may be produced in the change of a magnetic body from the magnetised to the demagnetised condition, as Professor Grove has shown elsewhere; and he considers that there is every probability that magnetism, in the dynamic state, either when the magnet is in motion, or when the magnetic intensity is varying, will also directly produce both Chemical Affinity and Light.

We have, lastly, to notice the relations of *Chemical Affinity;* a mode of force, of which, as Professor Grove remarks, the human mind has hitherto formed the least definite idea. "The word itself," he continues, "is ill chosen; its meaning, in this instance, bearing no analogy to its ordinary sense; and the mode of its action is conveyed by certain conventional expressions, no dynamic theory of it worthy of attention having been adopted." At first sight, its action appears so distinct in kind from that of the forces already named, that it can scarcely be considered surprising that a strong line of demarcation should have been drawn between Physical and Chemical agencies. The more deeply they are studied, however, the more close and definite does their relation appear. Of the conversion of Chemical Affinity into Motion, we seem to have a familiar instance in the projectile force of gunpowder; and there can be no doubt that, in this case, the chemical affinity is converted into motor power, either by direct conversion, or by the liberation through its means of other forces previously existing in a state of static equilibrium. The relation between Chemical Affinity and Electricity is of the closest kind; and though it is not capable of being clearly defined, yet examples of the conversion of each of these forces into the other are so abundant, that some philosophers have regarded them as identical. The following instance, made known some years ago by Professor Grove, presents the fact in a very elegant form. We presume that it is scarcely necessary to remind our readers, that although neither nitric nor hydrochloric acid will act separately upon gold, the metal will be dissolved by a mixture of the two; the oxygen of the nitric acid being ready to act upon the hydrogen of the hydrochloric, so as to set free the chlorine of the latter, which is the real solvent of the gold.

"Now, in order to exhibit this chemical force in the form of electrical force, instead of mixing the liquids, place them in separate vessels or compartments, but so that they may be in contact, which may be effected by having a porous material, such as unglazed porcelain, asbestos, &c., between them. Immerse in each of these liquids a strip or wire of gold. As long as these pieces of gold remain separated, no chemical or electrical effect takes place; but the instant they are brought into metallic contact, either immediately or by connecting each with the same metallic wire, chemical action takes place,—the gold in the hydrochloric acid is dissolved, electrical action also takes place, the nitric acid is deoxidized by the transferred hydrogen, and a current of electricity may be detected in the metals or connecting metal, by the application of a galvanometer, or any instrument appropriate for detecting such effect." (p. 78.)

There are, in fact, few, if any, chemical actions, which may not be experimentally made to produce electricity; and in the voltaic battery, in which the quantity of electricity generated is constantly proportional, *cæteris paribus*, to the rapidity of the chemical action, advantage is taken of this relation, when we desire to obtain electricity of low intensity, but in large quantity.

On the other hand, the electric current thus generated may be applied to the production of chemical changes; and the researches of Professor Faraday have shown, that whatever may be the use thus made of it, the amount of chemical action produced by the current, bears a direct relation of equivalence to the amount of chemical action by which the current is set in action:—

"That is, supposing the battery to be formed of zinc, platinum, and water, the amount of oxygen which united with the zinc in each cell of the battery was exactly equal to the amount evolved at the one platinum terminal; while the hydrogen evolved from each platinum plate of the battery was equal to the hydrogen evolved from the other platinum terminal. Supposing the battery to be charged with hydrochloric acid, instead of water, while the terminals are separated by water, then, for every 36 parts by weight of chlorine, which united with each plate of zinc, 8 parts of oxygen would be evolved from one of the platinum terminals; that is, the weights would be precisely in the same relation which Dalton proved to exist in their chemical combining weights. This may be extended to all liquids capable of being decomposed by the electric current, thence called *Electrolytes;* and as no voltaic effect is produced by liquids incapable of being thus decomposed, it follows that voltaic action is chemical action taking place at a distance, or transferred through a chain of media, and that the chemical equivalent numbers are the exponents of the amount of voltaic action for corresponding chemical substances." (pp. 82-3.)

Through Electricity, we may of course effect a conversion of Chemical Affinity into Heat, Light, Magnetism, or Motion; but it has nearer and more direct relations with some, at least, of these forces. Thus, Heat is an immediate product of chemical action:—"I know of no exception," says Professor Grove, "to the general proposition, that all bodies in chemically combining produce heat:"—and the researches of Professor Graham, Mr. Hess, Dr. Andrews, and Mr. Joule, all tend to show that the amount of heat produced in each case depends upon the chemical relations of the substances whose state of combination is undergoing change. Light is produced by chemical action, in all rapid combustions; as well as in some of those taking place at a slower rate, in which there is no perceptible development of heat, as in the phosphorescence of many organic substances. So, again, Chemical Action produces Magnetism, wherever it is thrown into a definite direction, as in the phenomenon of electrolysis; and this is well exemplified by the gas battery. The following ingenious arrangement of this instrument takes advantage of the *catalytic* power of platinum to occasion the union of oxygen and hydrogen, in such a mode as to generate electricity thereby:

"In a single pair of the gas battery above alluded to, one portion of a strip of platinum is immersed in a tube of oxygen, the other in one of hydrogen, both the gases and the extremities of the platinum being connected by water or other electrolyte; a voltaic combination is thus formed, and electricity, heat, light, magnetism, and motion, produced at the will of the experimenter." (pp. 86-7.)

The recent experiments of Plucker, Faraday, and others, have shown that a definite relation exists between Magnetism and the polar force which produces crystallization. Thus Plucker has shown that many transparent crystalline bodies, when freely suspended, take a position in relation to the lines of magnetic force, which is dependent upon their optical axis, or axis of symmetry; and it appears from Faraday's researches, that crystals of bismuth and other opaque substances will likewise hang in a certain definite direction between the poles of a magnet. The mineral Cyanite is influenced by Magnetism in so marked a manner, that when freely suspended it will arrange itself definitely with reference to the direction of terrestrial magnetism, so that it might even be used as a compass-needle.

The following observations on the terms and categories now in use for the nomenclature and classification of the phenomena of the Inorganic universe, will be found to have a very important bearing on the considerations to which we shall hereafter have to direct our reader's attention:

"I have now gone through the affections of matter for which distinct names have been given in our received nomenclature: that other forces may be discovered, differing as much from these as these differ from each other, is highly probable; and that, when discovered, and their modes of action fully traced out, they will be found to be related *inter se*, and to these, as these are to each other, I believe to be as far certain as certainty can be predicted of any future event.

"It may, in many cases, be a difficult question to determine what constitutes a distinct affection of matter or mode of force. It is highly probable that different lines of demarcation would have been drawn between the forces already known, had they been discovered in a different manner, or first observed at different points of the chain which connects them. Thus, radiant heat and light are mainly distinguished by the manner in which they affect our senses; were they viewed according to the way in which they affect inorganic matter, very different notions would possibly be entertained of their character and relation.

"Electricity, again, was named from the substance in which, and magnetism from the district where, it first happened to be observed; and a chain of intermediate phenomena have so connected electricity with galvanism, that they are now regarded as the same force, differing only in the degree of its intensity and quantity, though for a long time they were regarded as distinct.

"The phenomenon of attraction and repulsion by amber, which originated the term *electricity*, is as unlike that of the decomposition of water by the voltaic pile, as any two natural phenomena can well be. It is only because the historical sequence of scientific discoveries has associated them by a number of intermediate links, that they are classed under the same category. What is called voltaic electricity might equally, perhaps more appropriately, be called voltaic chemistry. I mention these facts, to show that the distinction in the names may frequently be much greater than the distinction in the subjects which they represent, and *vice versâ*, not as at all objecting to the received nomenclature on these points; nor do I say it would be advisable to depart from it. Were we to do so, inevitable confusion would result; and objections, equally forcible, might be found to apply to our new terminology." (pp. 90-1.)

In concluding this brief sketch of the series of relations between the various physical forces, it may be remarked that in many cases where one of these is excited or exists, all the others are also set in motion.

"Thus, when a substance, such as sulphuret of antimony, is electrified, at the instant of electrization it becomes *magnetic* in directions at right angles to the lines of electric force; at the same time it becomes *heated* to an extent, greater or less, according to the intensity of the electric force. If this intensity be exalted to a certain point, the sulphuret becomes luminous, or *light* is produced; it expands, consequently *motion* is produced; and it is decomposed, therefore *chemical action* is produced. If we take another substance, say a metal, all these forces, except the last, are developed; and although we can scarcely apply the term chemical action to a substance hitherto undecomposed, and which, under the circumstances we are considering, enters into no new combination, yet it undergoes that species of polarization, which, as far as we can judge, is the first step towards chemical action, and which, if the substance were decomposable, would resolve it into its elements. Perhaps, indeed, some hitherto undiscovered chemical action is produced in substances which we regard as undecomposable: there are experiments to show, that metals which have been electrized are permanently changed in their molecular constitution. Thus, with some substances, when one mode of force is produced, all the others are simultaneously developed. With other substances, probably with all matter, some of the other forces are developed, whenever one is excited; and all may be so, were the matter in a suitable condition for their development, or our means of detecting them sufficiently delicate." (pp. 92-3.)

That the same view might be applied to the mutual relations of some of the *vital* forces, did not escape Professor Grove's sagacity, as appears from the following passage near the conclusion of his essay:

"I believe that the same principles and mode of reasoning might be applied to the organic as well as the inorganic world; and that muscular force, animal and vegetable heat, &c., might, and at some time will, be shown to have similar definite correlations; but I have purposely avoided this subject, as pertaining to a department of science to which I have not devoted my attention."

It is to be remarked, however, that the forces here alluded to by Professor Grove—those of *heat* and *muscular motion*—are really *physical* in their manifestations, though generated in living bodies; that the purely *vital* operations of growth, development and reproduction, are not even named by him; and that not the slightest hint is given by him of the existence of any definite relation between the vital and the physical forces.

There have not been wanting, at any period in the history of Physiology, men who have attempted to identify all the forces acting in the living body with those operating in the inorganic universe; and we may observe the same tendency among certain chemists and physicists of the present time, as among the iatro-chemists and iatro-mathematicians of the sixteenth and seventeenth century. Among almost all modern physiologists, however, there is a distinct recognition of the fact, that,—although many of the phenomena of living bodies may be placed in the same category with those of inanimate matter, and are not otherwise affected by vital agency than as this prepares or modifies the conditions under which they occur, still, as Dr. Carpenter remarks,—

"Living bodies present a large class of phenomena which are altogether peculiar to them, and which can only be attributed to agencies of which the inorganic world is altogether independent; and hence has arisen the notion of *vital agency* as the foundation of Physiological science, just as the notion of *affinity* is the foundation of Chemistry, and that of *mutual attraction* of General Physics. And putting aside all hypothetical considerations with regard to the abstract nature of that agency, Physiologists have been aiming to determine the laws of its operation; following the same mode of inquiry for this purpose, as that which has been found successful in other departments of scientific investigation. In doing this, it has been necessary for them to *isolate*, as much as possible, those phenomena which may be regarded as Chemical or Physical, from those which must be distinguished as Vital; in order that, by the collocation and comparison of the latter, their mutual relations may be discovered. Still, after making every possible allowance for the operation of chemical and physical agencies, in the *direct* production of the changes of composition, mechanical movements, &c., which connect living beings (so to speak) with the universe around them, it is impossible for the discriminating inquirer not to see, that the influence of these agencies is *indirectly* exerted, to a yet greater extent, in the production or modification of purely vital phenomena. Thus, to take a very simple case, it cannot be for a moment doubted that heat and light exert an influence upon the vegetable germ, which is essential to its growth and development into the perfect plant, and to the performance of all the actions of the latter, whether these have reference to the extension of its own fabric, to the formation of organic compounds from the materials supplied by the inorganic world, or to the production of the germs of new individuals which are in like manner to go through the same series of phases. Hence light and heat have been designated as "vital stimuli;" the current idea being, that their agency upon the vegetable germ excites or awakens the forces which were dormant in it; and that, by enabling it thus to assimilate the new materials supplied by the inorganic world, and to give to these the structure of organised bodies, they contribute to develope the latent powers of these materials, which in their turn exhibit vital properties as they are made to form part of organised structures. Such, at least, is the doctrine of those who have most clearly expressed themselves upon the relation of the "vital stimuli" to the "vital properties" of organised bodies; and the author has not been able to find in physiological writings, any indication of a more intimate relationship between the physical forces and vital phenomena, than that just stated,—save on the part of those who have vaguely identified Heat or Electricity with the "vital principle," with about the same amount of philosophical discrimination as that which was exercised by the iatro-chemists and iatro-mathematicians of the sixteenth and seventeenth centuries." (Phil. Trans., pp. 728-9.)

The earliest endeavour, in this country, to show the existence of a connection between the vital and physical forces, analogous to that which links together Heat, Magnetism, Electricity, &c., appears to have been made by Mr. Newport, in a paper which he communicated to the Linnæan Society, 'On the Natural History of the Oil Beetle, Meloë,' in December, 1845; which contains the following passage:

"Thus the unerring influence of a great physical cause, which arouses the instinct of the newly-developed being, seems to be clearly indicated in the effects of light upon these meloës. These effects I may, perhaps, be allowed to designate *the polarisation of instinct*. The facts I have now detailed lead me, in conformity with the discovery by Faraday of the analogy of light with heat, magnetism, and electricity, to regard light as the primary source of all vital and instinctive power, the degrees and variations of which may, perhaps, be referred to modifications of this influence on the special organisation of each animal body. Matteucci already has shown, that electricity and nervous function are closely related; and now that Professor Faraday has proved that light and electricity are the same principle (?), we seem to have approached closer to a knowledge of the origin of life. The throes of parturition in the pregnant female,—the electrical shock of the torpedo,—and, doubtless, also the ejection of the poison by stinging insects,—the impressions of sensation, and the act of contraction in muscular fibre,—all seem to be concomitant with the maturation, evolution, change of form or of nature of some material constituent of organic life,—and directly connected with, or influenced by, the hitherto imponderable agent, nervous function; a too intense, or too frequent diffluence (exhaustion) of which seems to hasten the dissolution of the whole body, and diminish the intensity of those affinities by which its primary constituents are held together, and the cessation of which constitutes death."

Only the first two sentences of this passage, however, were published in the 'Linnean Transactions,' the Council of the Society having objected to the remainder; so that no other record of this enunciation of Mr. Newport's views existed, save a brief abstract of them in the periodicals of the time, until he reproduced this passage in a paper read before the British Association at its last meeting, (August, 1850,) which was published a short time subsequently, ('Annals of Natural History,' October, 1850.) We have given the entire passage, in order that our readers may form their own judgment as to the distinctness with which the doctrine was enunciated by Mr. Newport, and the completeness with which it was carried out. He seems to have taken no further pains to develope his views, until he made the communication last referred to.

In the first Number of this Review (January, 1848, p. 235,) the idea of "correlation," which had been in the meanwhile developed by Mr. Grove in regard to the Physical forces, was definitely enunciated by Dr. Carpenter, as expressive of the relation between Nerve-force and Electricity; and he extended it likewise to other physical forces, showing that Nerve-force must be related likewise to Heat, Light, Chemical Affinity, and Motion. After fully developing the relation of Nerve-force to these Physical Forces, he offered some suggestions as to the existence of a similar relation between Heat and the Organising force; and pointed out, that the progress of inquiry tends to the conclusion, "that the *vital* forces of various kinds bear the same relation to the several *physical* forces of the inorganic world, that they bear to each other; the great and essential modification or transformation being effected by their passage, so to speak, through the germ of the organic structure, somewhat after the same fashion that heat becomes electricity when passed through certain mixtures of metals."

In September, 1849, a paper was communicated to the British Association, at its Birmingham meeting, by Dr. Fowler, of Salisbury, under the following title:—"If Vitality be a Force having Correlations with Forces, Chemical Affinity, Motion, Heat, Light, Electricity, Magnetism, Gravity, so ably shown by Professor Grove to be modifications of one and the same Force." A peculiar interest attaches to such a communication, from the fact, that Dr. Fowler, the date of whose graduation was 1793, was one of the first in this country to repeat the researches of Galvani and Volta; and that, although now, we believe, some years past eighty, he continues to prosecute his scientific inquiries with all the ardour of youth. Whether Dr. Fowler had or had not seen the views which had been previously put forth in our own pages, we cannot say; but we cannot doubt the entire originality of those expressed by him, obviously proceeding, as they did, from a mind long trained to habits of philosophic thought, fully alive to the progress of scientific inquiry, and constantly directed towards this particular branch of investigation.

Of Dr. Fowler's paper, we believe that no other published record exists, than the following notice contained in the Reports of the British Association, for 1849, p. 77:

"The author, after having shown that each of these modified forces can be excited by any other, or, in its turn, be the exciter of all the rest, and consequently the antecedent or consequent, indifferently of all the others, proceeded to show that this is equally true of vitality, and that the coils in which these forces are latent, and by whose modifications in an excited state they are rendered apparent to our senses, constitute one of the differences between them. For instance, the change of temperature to which the infant is necessarily exposed at its birth, the heat rapidly going out of it, excites the motion necessary for inspiration. This gives the oxygen of the air access to the carbon of the blood by endosmosis; this, again, to animal heat. From that, electricity may be obtained—and from electricity, by an appropriate coil, magnetism. Gravity the infant acquires by its growth; and can counteract by its muscular contractility. It may be said, that an infant affords no evidence of the production of the forces, light, electricity, and magnetism; but the experiments of Dr. Faraday have demonstrated, that all these may be produced by the vitality of the Gymnotus, and rendered palpable to our sight and feeling. So much for the qualities by which vitality has correlations with all other forces. But there still remains a difference.—Vitality is the artist of its own *coils*. No other force can make an organ of either an animal or a plant, (the coil by which their vitality is evinced.) Neither a Volta nor an Œrsted could have invented an eye, or an ear, or even a graft by which the sap of a fruit-tree is so modified as to differ from that of the parent stock.

"The author added instances of the light of fire-flies, glow-worms, and some marine animals, as instances of the production of light, apparent to the vision of others, by vitality. And any person may satisfy himself of the ease with which a flash of light, the product of his own vitality, may be rendered perceptible to himself, by putting a plate of zinc between the gums and the cheek on one side of the mouth, and the broad handle of a silver spoon in the other; and then (in the dark) he will see a flash of light at every instant of contact and separation of the zinc and silver.

"That mind and vitality reciprocally excite and depress each other, must be obvious to all who are attentive to their daily feelings; and, conversant with surgical practice, must be aware of the difference in healing of wounds in a healthy or exhausted subject."

The next allusion to the subject, with which we are acquainted, is to be found in the work entitled 'Proteus, or the Law of Nature,' by Dr. C. B. Radcliffe, which was published in the spring of 1850, and was noticed by us in our Number for October last (p. 525). Of Dr. Radcliffe's treatment of this topic, it is only necessary to say, that it was merely incidentally introduced, and not at all systematically handled; and that, in fact, Dr. Radcliffe's views are not by any means so fully or precisely expressed, as those which had appeared in our own pages more than two years previously. We are quite sure, from what we know of his character, that he would have most scrupulously referred to the latter, had he derived any assistance from them; and we willingly give him credit for originality in the enunciation of views, which it is obvious to us that he had thought out for himself.

We now come to the Memoir "*On the Mutual Relations of the Vital and Physical Forces,*" communicated to the Royal Society by Dr. Carpenter, which bears date June 20, 1850, and which is published in the 'Philosophical Transactions' for last year. This, we believe, is the first *systematic* attempt that has been made, in this country at least, to work out the subject; and as it is mainly an expansion of the ideas which had been put forth in our own pages at the beginning of 1848, the author may claim priority, as regards the enunciation and development of the idea, both of Dr. Fowler and Dr. Radcliffe, although to a certain degree anticipated by Mr. Newport. We shall presently find, however, that both these gentlemen were themselves anticipated in a quarter they little guessed;

and the whole case is obviously one of a kind, of which the history of Physiology, as well as of other sciences, furnishes many examples,—in which a connecting idea, developed in another department of inquiry, struck many individuals at once as applicable to the same class of facts, and was wrought out by them in different modes, and with various degrees of success, according to their previous habits of thought.

We shall now endeavour to place our readers in possession of Dr. Carpenter's views, in the same manner as we have brought before them those of Professor Grove; endeavouring to give a condensed view of the entire Memoir, and using its language whenever it suits our purpose.

After some Introductory Remarks, from which we have already quoted, Dr. Carpenter proceeds to inquire into the *Mutual Relations of the Vital Forces*. Looking at the phenomena of Life from the same point of view as that from which we are now taught to regard Physical phenomena,—namely, as the results or manifestations of certain *forces* acting through those forms of matter termed Organised, which forces may be provisionally termed *Vital*, it should be our first object to ascertain whether these phenomena (such of them, at least, as are neither Chemical nor Physical) can all be referred to the agency of *one* Force, operating through a variety of instruments, or whether it is necessary to have recourse to the idea of a number of distinct forces. "Our clearest idea of the agencies essentially concerned in the production of vital phenomena, is derived from the study of the development of any single organism;" and the simplest vegetable cell is selected by Dr. Carpenter as presenting this series of phenomena in their least complicated form. Now in the growth of such a cell (belonging to any one of those simple Cryptogamic tribes, in which each cell may be regarded as an independent organism,) from its germ, we notice, in the first place, that it exerts a power closely allied to, if not identical with, that of ordinary *chemical transformation;* for it decomposes carbonic acid, and unites its carbon with the elements of water; at the same time decomposing ammonia, and uniting its azote with the oxygen, hydrogen, and carbon, derived from the sources just named; thus forming *organic compounds*, such as no operation of ordinary chemistry has yet been able to imitate. This process, as is well known, can only be effected under the stimulus (to use the common phraseology) of Light; but it would rather appear from the preceding considerations, that Light is the *force*, which, acting through the Vegetable cell as its instrument or "material substratum," *produces* those new Chemical attractions, which determine the formation of these new compounds. Dr. Carpenter then goes on to show, that in the application of the nutritive materials thus generated to the development of the cell, we must distinguish a force of *assimilation* or vital transformation, by which these materials are rendered plastic or organisable, and a force of *organisation* or complete vitalisation, by which they are incorporated with the solid texture, and become possessed of its properties.—Now, although we may provisionally designate these as distinct forces, on account of the diversity of their manifestations, it is impossible not to see that they are mutually dependent, and that they form the successive elements of a continuous series of phenomena belonging to the same category, that of *cell-life;* and further, we observe that they operate under the same conditions, namely, the presence of a cell-germ and of the materials of its growth, and the action of Light and Heat.—Again, in the *multiplication* of the original cell, by whatever method performed, we cannot but trace the continued action of forces of the same character; since this operation takes place as a continuation of the process of growth, and under precisely the same influences.—Further, we occasionally meet with examples, even among the simplest forms of vegetation, of very active *movement;* thus the filaments or elongated cells of the *Oscillatoriæ* are continually bending themselves backwards and forwards, with a regular rhythmical undulation; and the "zoospores" of the *Conferveæ* are propelled through the water by the rapid vibration of the cilia with which they are furnished. Now that such a production of a purely physical change is a manifestation of vital force, is obvious from this,—that it takes place only while the vitality of the organism endures, and that it is dependent upon the very same conditions as the other vital operations require. The spiral filaments, again, which have been discovered in most of the higher Cryptogamia, and which seem to perform the same function with the spermatozoa of Animals, have a similar spontaneous movement, which must be looked upon as an expression of *their* vital force. Many cases of motion produced by a change of form of certain contractile cells, might be cited from among the higher tribes of the Vegetable kingdom; these movements being sometimes rhythmical and spontaneous, as in the *Hedysarum gyrans*,—sometimes taking place only in respondence to stimulation, as in the *Dionæa muscipula* (Venus's fly-trap),—and sometimes occurring as part of the series of ordinary vital phenomena, although producible also by stimulation, as in the *Mimosa pudica* (sensitive-plant), which regularly closes its leaves at night, but will do so at any time when they are touched or otherwise irritated. These movements only take place during the life of the Plant; and it is particularly observable in the last-named species, that the facility with which they may be excited in any individual is closely related to the activity of its vegetating processes.—Thus even in the Plant, we see that the Vital forces manifest themselves, not merely in *growth*, but in *movement*.

When we examine the structure of one of the higher Plants, we find that, although the principal part of its fabric is still made up of unmetamorposed cells, yet that certain portions of it have undergone *histological transformation;* that is, its primordial cells have lost their original character, having been changed into other kinds of tissue. This transformation takes place to a much greater extent in the Animal body; in which the variety of actions to be performed is much larger, and in which we accordingly find a much greater variety of tissues developed as their instruments. But however widely these tissues may depart from their original character, we find that the process of transformation takes place under the same conditions as that of growth, and must be regarded as a continuation of it; being, in fact, the special manifestation of vital force in one set of cells, as multiplication is in another, or as motion in another. And we find, that, in proportion as this transformation takes place, do the tissues lose their proper vital endowments; for it may be stated as a general fact, that even in the most complicated and elaborate Animal organism, *all the most active vital operations are performed by tissues which retain their original cellular constitution with little or no change.*

Further, it is to be observed, that as it is the peculiar character of such organisms that each of their parts should be appropriated to some distinct office which it is specially adapted to perform, so do we find that the cells which become the instruments of some one particular kind of operation seem to lose their other endowments; *as if the expenditure of the vital force of each cell upon any one purpose, unfitted it for any other agency.* Of this, numerous examples might be cited; it will be sufficient here to refer to a few of the most characteristic.—It is necessary for every act of Secretion, that a set of cells should be formed within the ultimate follicles of the Gland which is the instrument of the function; and these ultimate follicles are really to be regarded as parent-cells, which produce the true secreting cells in proportion as the materials of their growth are supplied by the blood. Now these parent-cells themselves possess no secreting power, their vital force being entirely expended in the production of the true secreting cells. On the other hand, the true secreting cells possess no reproductive power, but die and are cast off when they have reached their maturity; as if their whole vital force were expended in the secreting process, which is nothing else on their parts than an act of growth. So, again, the cells which are endowed with the special *reproductive* power, exercised in the true act of Generation, seem to possess no other endowment; for they do not exercise chemical transformation, nor do they undergo histological change, nor do they multiply after the ordinary fashion. "That a relation of reciprocity," observes the author, "exists between the forces concerned in the growth, development, and maintenance of the individual organism, and those which are employed in the generative act,—so that an excessive expenditure of either diminishes the amount of vital force which is applicable to the other,—is an idea so familiar to physiologists, that he need not here dwell upon it, further than to point out how completely it coincides with, and illustrates, the view for which he is contending."

When we look at the cells concerned in the production of *mechanical movement*, we find the same principle holding good in a most remarkable manner; these cells being apparently incapable of performing any other function. Thus the cells which constitute the fibrillæ of Muscular fibre, and of whose change of form the contraction of the muscle is the result, exercise no power of chemical transformation, undergo no histological change, and appear to be entirely destitute of the power of self-multiplication; the expenditure of their vital force in the act of muscular contraction involves their death and disintegration; and their renewal appears to be accomplished by a production of new cells from the nucleus of the Myolemma, which, itself possessing no contractile power, retains its reproductive capacity. The ciliary action of particular epithelium-cells affords another very beautiful example of the same principle; being not merely dependent upon the vital activity of the cell, but being the *sole* manifestation of its life, when once it has attained its full growth:—

"For the *ciliated epithelium is never a secreting epithelium;* so that in tracing the one form into the other, there seems to be such a marked transition in function (the mode of production and the general conditions of development being essentially the same), as clearly indicates that the ciliary and the secreting agency, although very dissimilar in themselves, are both to be looked upon as modes of operation of the same vital force as that which is exerted in the production of the cell. And this view derives remarkable confirmation from the fact, that in the history of the 'zoospores' of the Algæ we have two distinct periods, one of ciliary action, and the other of growth and multiplication; *so long as the ciliary action continues, which is provided for their dispersion, no further vital change seems to take place in them; but so soon as this ceases and they become stationary, they begin to exercise chemico-vital transformations, and to grow and multiply as cells.*" (pp. 739-40.)

These views in regard to the mutual relationship of the different kinds of Vital Force are strikingly confirmed by the phenomena of Nervous Agency; and we shall quote Dr. Carpenter's remarks upon this subject in full, deeming them the more worthy of the consideration of our readers, since they have been adopted by Mr. Paget, as having an important bearing on the theory of Inflammation.*

"There can be no reasonable doubt that the production of nerve-force in the central organs is dependent upon the development of the peculiar cells constituting the ganglionic or vesicular substance; and, as already remarked, the progress of physiological inquiry seems to justify the belief (long since entertained and expressed by the author) that either cells or cell-nuclei are the agents in the origination of nerve-force at the peripheral extremities of the nerve-fibres. The nerve-force thus generated is not merely expended in arousing mental activity on the one hand, or in exciting muscular contraction on the other, but has an intimate relationship (there can be no doubt) with all the other manifestations of vital force which the animal organism exhibits. So intimate is this relationship, so obvious is the controlling and regulating action of the nervous system over the operations of nutrition, secretion, &c., especially in the higher animals, that many physiologists have regarded these actions as necessarily *dependent* upon the exertion of nervous force. On the other hand, it has been urged with great plausibility by Prof. Alison and others, that since the functions of organic life in Animals are performed under the same essential conditions as those of Plants, and since the acts of formation, secretion, &c. are effected by the very same agency in animals as in plants,—namely, by cell-growth,—there is no valid reason for regarding them as dependent upon nervous agency; although it must be freely admitted that they are greatly affected by that agency, being not merely accelerated and retarded through its influence, but also altered in kind. The view here advocated will, it is believed, afford a definite scientific expression for all the phenomena which bear upon this question. For, just as electricity developed by chemical change may operate (by its correlation with chemical affinity) in producing other chemical changes elsewhere,—*so may nerve-force, which has its origin in cell-formation, excite or modify the process of cell-formation in other parts, and thus influence all the vital manifestations of the several tissues, whatever may be their own individual characters.* And this expression will also be found available for the well-known influence of mental conditions upon the properties of the various tissues and secretions, since this influence can only be exerted through the medium of nervous agency. Further, it not only appears that a simple withdrawal or disturbance of the nervous force supplied to particular organs occasions a retardation or perversion of their vital operations; but there also seems evidence that an influence of *an opposite kind* may be transmitted through the nervous system, which is positively and directly antagonistic to the vital powers of the several tissues and organs;—such, at least, appears to be the only mode of accounting for the extraordinary effect of a *shock*, mechanical or mental, in at once and completely destroying the contractility of the heart, and immediately bringing to a stand the vital operations of other parts; and it harmonizes well with the fact that, in hemiplegia, the 'palsy-stroke' transmitted from the brain along the spinal cord almost invariably affects the leg less injuriously than the arm, and for a shorter duration, recovery first taking place in the leg, even when it has been at first paralysed as completely as the arm. If the nervous force be regarded as a *polar* force (as suggested by Messrs. Todd and Bowman, 'Physiological Anatomy,' vol. i, p. 237 *et seq.*), analogous in its mode of transmission to electricity or galvanism, it

* See 'Medical Gazette,' July 19, 1850, p. 95.

is not difficult to understand that the *reversal* of the usual direction of its action may produce the effects in question, regard being had to the opposite effects shown by Prof. Matteucci to be produced upon nervous excitability by the *direct* and the *inverse* electric currents." (pp. 740-41.)

Hence, then, we have reason to believe that all the truly *Vital* phenomena, however diversified, are but results of the operation of one and the same Force, whose particular manifestations are determined by the nature of the material substratum through which it acts; the same fundamental agency producing simple *growth* in one case, *transformation* in another, *multiplication* in a third, *mechanical movement* in a fourth, whilst in a fifth it develops *nervous power*, which may itself operate in a variety of different modes.—Such a view seems fully justified by the consideration, (1) that all these forces are exerted, even in the most highly-organised living being, through a common instrumentality, the simple cell; (2) that the entire assemblage of cells making up the totality of any organism, have all a common parentage, being lineally descended from the single primordial cell in which it originated; and (3) that they are manifested in connection with each other, as parts of the life of each individual cell, in those simple organisms which are the lowest members of the two kingdoms respectively, and in which there is no separation or specialisation of function.

The question next arises,—what is the source of the Vital Force, of which the phenomena of Life are the manifestations; and this is considered by Dr. Carpenter, in the second part of his Memoir, on the *Relations of the Vital and Physical Forces.* Under the guidance of the ideas derived from Physical Science, we shall have no difficulty in referring Vital Force to the operation of those external agencies, the influence of which has long been known to be essential to Vital action, and which have been usually designated by the term *Vital Stimuli.* Thus, the growing Vegetable cell cannot decompose carbonic acid, except when acted upon by Light; and the amount of this change which it effects, is in strict ratio (*cæteris paribus*) with the illuminating power of the rays which it receives. So, again, neither Plants nor Animals can maintain their activity, except under the continual influence of a certain measure of Heat; and the amount of that activity is found to bear a constant ratio, in all those tribes which have no independent power of sustaining it, to the quantity which they receive from external sources; this being true, not merely of the general rate of the Vegetative actions of growth and development, but also of those manifestations of vital power which are peculiar to Animals. Thus we may say, that Light and Heat acting upon the organic germ, become transformed into Vital force, in the same manner as Heat acting upon a certain combination of metals becomes Electricity, or as Electricity acting upon iron develops itself as Magnetism; and we shall find that this view is in complete harmony with all the phenomena of Vital action. Moreover, the Vital force thus engendered frequently manifests itself in producing Physical or Chemical phenomena; thus completing that *mutual* relationship, or correlation, which has been shown to exist among the Physical and Chemical forces themselves. Of this we have already seen an instance in the *movements* produced by muscular contraction and by ciliary vibration. The production of *heat* by certain Plants and by warm-blooded Animals, is another apposite exemplification of the same principle. But the most remarkable illustration is undoubtedly derived from the Nerve-force; which, whilst itself a peculiar form of the general Vital force, and capable of affecting all the other manifestations of the same force (as in the modifications which it produces in the processes of Nutrition and Secretion, as well as in exciting Muscular Contraction), is capable of developing Electricity as well as Light and Heat, and is also capable of being called forth by the action of Light, Heat, Electricity, Chemical Affinity, or even Mechanical Motion, on the Nervous tissue. It is a most remarkable confirmation of the views here advanced, that the nerve-force, which must be accounted, in its relations to Mind, as the highest of all the forms of Vital force, should yet be the one which is most directly and intimately related to the Physical forces, —the "correlation" even of Electricity and Magnetism not being more complete, than the "correlation" of Electricity and Nerve-force may be shown to be.*

The following are, in a somewhat abridged form, the considerations which are cited by the author, in support of his position, that, since among Plants and cold-blooded Animals *a precise relation may be traced between the vital activity of each individual and the amount of heat which it receives from external sources,* we have a right to affirm that its vital activity is dependent upon Heat; in other words, that Heat is the force by which it is sustained.

"1. According to Boussingault, the same annual plant, in arriving at its full development, and going through all the processes of flowering and maturation of its seed, *everywhere receives the same amount of solar light and heat,* whether it be grown at the equator or in the temperate zone; its *whole period of growth being in a precisely inverse ratio to the amount it receives in any given time, and its rate of growth consequently in a direct ratio.* Hence it appears that the organising force of Plants bears a relation of equivalence to the Heat and Light which act upon them.

"2. This has been separately demonstrated with regard to the special influence of Light, in producing the decomposition of carbonic acid and the formation of chlorophyll, &c.; the amount of carbon fixed by plants being *cæteris paribus* in accordance with the amount of illumination they receive. The influence of Light, it may be remarked, seems to be exerted only in this peculiar process of vital chemistry; whilst that of Heat is exercised in *all* the other operations in which growth consists; and hence it is that animals are comparatively little dependent upon light, their food being prepared for them by the agency of the vegetable kingdom.

"3. The rate of 'rotation' of the fluid within the cells of *Chara*, &c., and the rate of 'cyclosis' in the latex-vessels of *Ficus elastica*, &c., appear to be in precise relation (within certain definite limits) with the temperature to which these organisms are subjected; the movement of the fluids being accelerated by warmth, and retarded or checked by cold.

"4. In cold-blooded Animals, the same relation may be seen, between the activity of the organising processes, and the amount of Heat to which they are subjected. The production of larvæ from the eggs of Insects, like the germination of the seeds of plants, may be accelerated or retarded at pleasure, simply by the regulation of the temperature; and the time required for the last metamorphosis is precisely in the inverse ratio to the heat supplied; so that, as in the maturation of the plant,

* See 'Brit. and For. Med.-Chir. Rev.,' Vol. I, pp. 233-5.—It may be remarked, that, in his Seventh Series of 'Electro-Physiological Researches,' Professor Matteucci has formally adopted the idea of the correlation between the nervous and electrical forces, and has adduced new experimental proofs in support of it. (See 'Brit. and For. Med.-Chir. Rev.' Jan., 1851, p. 254.

each individual of the same species receives the same amount of heat, whether the intensity of its action be greater or less. Further, it has been remarked by Mr. Paget, that the processes of *development* seem to require a higher degree of Vital force than those of simple *growth;* and it harmonizes admirably with the doctrine here contended for, that there appears to be a necessity for a higher temperature for developmental operations, than for those of simple increase. Thus in the economy of the Social Bees, as shown by Mr. Newport, there is a special provision for generating heat during the last few hours of the metamorphosis, in which the tissues and organs of the imago are being completed; and in the Viper and some other ovo-viviparous Reptiles, there seems to be an unusual calorifying power, for the purpose of promoting the development of the embryo. So, again, it has been found by Dr. Edwards and Mr. Higginbottom, that the metamorphosis of Batrachia requires a larger amount of light and heat than suffices for their growth in the larva state, being retarded or even prevented by the want of a due amount of these agencies; and it has been also shown by Mr. Higginbottom, that the *development* of new limbs in the Triton, to replace those which have been lost, cannot take place at a lower temperature than about 60°, although the processes of *growth* go on under a much less degree of heat. The general propositions enunciated by Prof. Milne-Edwards,* in regard to the geographical distribution of the Crustacea, indicate the existence of this relation in the most decided manner.

"5. The influence of temperature upon the general vital activity of cold-blooded animals is no less remarkable. The facts determined by the experiments of Dr. W. F. Edwards lead to this general conclusion;—that the *rate of life* of Batrachia and Fishes, of which the activity of their respiratory process is the exponent, varies directly (within certain limits) as the temperature of the surrounding medium; so that the *duration of life,* when these animals are deprived of air, either partially or completely, or are placed in any other circumstances unfavorable to its sustenance, varies inversely with the external temperature. Thus when frogs were confined in a limited quantity of water, and were not allowed to come to the surface to breathe, it was found that they died in from 12 to 32 minutes, when its temperature was 90°; in from 35 to 90 minutes when its temperature was 72°; in from 350 to 375 minutes, when its temperature was 50°; and from 367 to 498 minutes, when it was cooled down to the freezing point. The prolongation of life at the lower temperatures was not due to torpidity, for the animals performed the functions of voluntary motion, and enjoyed the use of their senses; but it was occasioned by the diminished activity of *all* their functions, and their consequent less demand for air. On the other hand, the elevation of temperature increases the demand for air, and occasions speedier death when it is withheld, chiefly by producing a vast acceleration in the rate at which all the operations, both of animal and organic life, take place.

"6. Although the warm-blooded animals are in great degree removed, by the independent calorifying power which they possess, from the influence of external temperature, yet it is very easily shown that their vital activity is no less under the direct and immediate influence of heat, than is that of cold-blooded animals. In fact, it would seem to be for the sake of keeping up their vital energy to a certain high and uniform rate, that they are endowed with the heat-generating power; and if this power be not exercised, and the body be cooled down, its vital activity is reduced, and at last extinguished." (pp. 747—50.)

The vast mass of facts of which the foregoing are examples, seem to justify the conclusion, that Heat is something more than a *stimulus* capable of arousing a dormant vital force; but, on the other hand, they by no means justify the assumption, that Heat and the "Vital principle" are identical. That Heat, acting upon or through an organised structure, then manifests itself as Vital Force,—or that Heat and Vital Force are directly "correlated,"—seems to be the expression of their mutual depen-

* Histoire des Crustacés, tom. iii, p. 555 *et seq.*

dence, which is most in accordance with all our knowledge of the influence of heat upon organised beings; whilst, conversely (as will be presently shown), it accords with the fact of the restoration to the inorganic world—under some form or other—of all the *force*, as of all the *materials*, drawn from it for a time by the agency of living beings. The Plant forms those organic compounds, at the expense of which Animal life (as well as its own) is sustained, by the decomposition of carbonic acid, water, and ammonia, and by the recombination of their elements; and the Light, by whose agency alone this process can be effected, may be considered as metamorphosed into the peculiar *affinity*, by which the elements of these compounds are held together. The *heat* which Plants receive, acting through their organised structures as Vital force, serves to augment these structures to an almost unlimited extent, and thus to supply new instruments for the agency of light and for the production of organic compounds. The whole *nisus* of Vegetable life may be considered as manifested in this production; and, in effecting it, each organism is not only drawing *material*, but *force*, from the universe around it. Supposing that no Animals existed to consume these organic compounds, they would be all at last restored to the inorganic condition by spontaneous decay, which would reproduce the carbonic acid, water, and ammonia, from which they were generated. In this decay, however slow, heat and light are given out, in the same amount as when more evidently produced in the ordinary combustive process; and this sometimes occurs even during the life of the plant, whose vital movements, also, may be considered as restoring to the Inorganic universe a certain measure of the force they have derived from it under other forms. Thus in making use of the stores of Coal, which have been prepared for his wants by the luxuriant Flora of past ages, Man is not only restoring to the atmosphere the carbonic acid, the water, and the ammonia, of the carboniferous period, but is actually reproducing, and applying to his own purposes, the Light and Heat which were operating to produce the growth of vegetation at that remote period in the Earth's history.

But the organic compounds which the agency of Light and Heat upon the Vegetable structures has produced, are designed for a much higher purpose than that of being merely given back to the Inorganic universe by decay or combustion; and the forces which hold together their elements have a much more exalted destiny. In serving as the food of Animals, a part of them become the materials of their organised tissues, and the instruments through which the nervous and muscular forces are developed; whilst another part are applied to sustain the combustive process, by which the heat of the higher classes is maintained quite independently of the external supply of that force. The greater part of the Animal kingdom, however, is dependent, like the Vegetable, upon the Inorganic Universe, for the Heat which serves as its organising force; and it is only under the constant influence of this agent, that the operations of growth, development, and maintenance can take place. The Animal is not dependent, like the Plant, upon Light: and this is obviously because that agent is chiefly concerned in the preliminary operation, by which the organic compounds are generated as the pabulum of the growing tissues; in fact, the embryo within the germinating seed, which, like the animal, is nourished upon matter previously prepared for it, is most rapidly developed in the absence of light, up to the time when, its stores being exhausted, its further supplies must be obtained by its own instrumentality.

The Vital activity of Animals, then, may be considered as chiefly sustained by the Chemical forces subsisting in their food, which are set free when the elements are reconverted to their original state; and by the Heat which they derive from external sources, or from the combustion of a part of their food. These forces may be considered as in a state of continual restoration to the Inorganic Universe, during the whole life of Animals, in the heat, light, electricity, still more in the motion, which they develope; and, after their death, in the production of heat and light during the processes of decay. During Animal life, there is a continual restoration to the mineral world, of the carbonic acid, water, and ammonia, which have been appropriated by Plants; and the amount thus given off by the animal organism bears a close correspondence, on the one hand, with its degree of vital activity, as shown in the amount of heat and motion which it generates, and, on the other, with the amount of the organic compounds which it consumes as food. So that, on the whole, there is strong reason to believe that the entire amount of force (as of materials) received by an animal during a certain period, is given back by it during that period, provided that its condition at the end of the term is the same as it was at first; and further, that all the force (like the material) which has been expended in the building-up of the organism, is given back by its decay after death.

In order to bring this idea into contrast with the notions usually entertained, and to illustrate its application more fully, Dr. Carpenter has recourse to the speculations of Physiologists as to the forces concerned in the Development of any highly-organised being from its primordial germ-cell:

"According to the doctrine current among some physiologists, the whole 'organising force,' 'nisus formativus,' or 'bildungstrieb,' which is to be exerted in the development of the complete structure, *lies dormant in this single cell*, the germ (it has been affirmed) being 'potentially' the entire organism. And thus all the organising force required to build up an oak or a palm, an elephant or a whale, is concentrated in a minute particle, only discernible by microscopic aid." (p. 751.)

He might have added that, on this hypothesis, the whole "nisus formativus" now existing among the individuals of any one species, must have been concentrated in their first progenitors; a doctrine scarcely less monstrous than that of the *emboitement* of the germs themselves, which were once supposed to be packed one within the other, like nests of pill-boxes:

"As a refuge from this doctrine, which seems almost too absurd ever to have gained believers, other physiologists (among whom the author formerly ranked himself) have affirmed, that *vital force must exist in a dormant condition in all matter capable of becoming organised;* that the germ-cell, in drawing to itself organisable materials, and in incorporating these into the living structure, does nothing else than evoke into activity their latent powers; and thus that, with every act of growth and cell-multiplication, new vital force is called into operation, whereby the process is continually maintained. This proposition, it may be safely asserted, does not involve any manifest absurdity. It attributes to oxygen, hydrogen, carbon, and nitrogen, properties which they were not previously supposed to possess; but no one could logically deny to these elements the possession of dormant vital powers, whilst they held that a dormant magnetic power might be attributed to iron. In the one case, as in the other, (it may be affirmed,) a certain combination of conditions is needed to call the property in exercise; and the living cell, combining the elementary substances into the pabulum of its growth, and then applying this to its own nutrition, calls their latent vital properties into activity,—just as (it has been argued) an electric current, made to circulate around a piece of iron, develops the latent magnetic force of that metal." (p. 751.)

If the views of Professor Grove, however, and of those philosophers who concur with him, be correct, there is not really any such thing as "latent force;" and the Physiologist can scarcely escape from the conclusion, that when organisable materials are applied to the extension of a living structure, and are caused to manifest vital powers, this is not in virtue of a force existing within the organism itself, but that some agency external to it is the moving spring of the whole series of operations. This agency seems to be chiefly Heat; but the author by no means excludes the other Physical Forces as also concerned in the production of vital phenomena.

As Dr. Carpenter has taken care to guard himself from being supposed to identify Heat or Electricity with the "vital principle," so does he assure his readers, that he does not in the least recognise the possibility, that any action of heat upon the inorganic elements can *of itself* develope an organised structure of even the simplest kind:

"The pre-existence of a living organism, through which *alone* can heat be converted into vital force, is as necessary upon this theory, as it is upon any of those currently received amongst physiologists. And it is the *speciality* of the material substratum thus furnishing the medium or instrument of the metamorphosis, which in his opinion establishes, and must ever maintain, a well-marked boundary-line between the Physical and the Vital Forces. Starting with the abstract notion of Force, as emanating at once from the Divine Will, we might say that this force, operating through inorganic matter, manifests itself in electricity, magnetism, light, heat, chemical affinity, and mechanical motion; but that, when directed through organised structures, it effects the operations of growth, chemico-vital transformation, and the like; and is further metamorphosed, through the instrumentality of the structures thus generated, into nervous agency and muscular power. If we *only* knew of heat as it acts upon the organised creation, the peculiarities of its operation upon inorganic matters would seem as strange to the physiologist, as the effects here attributed to it may appear to those who are only accustomed to contemplate the physical phenomena to which it gives rise." (p. 752.)

In the concluding passage of his Memoir, we believe that such of our readers as have followed us through the present article, will fully accord:

"If these ideas be correct, they will be found, he believes, to afford a precision to physiological doctrines, which they have never before possessed; and to open out a vast number of new lines of inquiry, which promise an ample harvest of results, not only valuable in a scientific view, but likely to be fertile in applications to various departments of the therapeutic art. At any rate, it is very important that physiological science should be considered under the same *dynamic* aspect, as that under which the physical sciences are now viewed by the most enlightened philosophers; and he trusts that the present attempt may thus aid in its advancement, even if it should answer no higher purpose." (p. 757.)

One of the most fertile paths of inquiry to which we are thus introduced, is that which leads us to consider the dynamical relations of Nerve-force to Mental agency on the one hand, and to the several Vital forces on the other. This topic is handled both by Dr. Radcliffe and Dr. Burnett; and we had intended to conclude the present Article with an examination of their views, and a development of our own. Our exhausted space warns us, however, of the necessity of postponing this subject for the present; and we trust to be enabled, on a future occasion, to do it that justice which a cursory notice would not permit.

We must not omit, however, to give our readers some account of the remarkable production of Dr. Mayer, who seems to have arrived at conclusions in all essential respects similar to those of Professor Grove and Dr. Carpenter, previously to the publication of the first Edition of the "Correlation of the Physical Forces," though subsequently to the delivery of the Lectures in which Professor Grove first announced his views, and to the publication of the abstract of them. Of the existence of this treatise we have only recently been made aware; and we venture to affirm that Professor Grove and Dr. Carpenter were alike ignorant of it. We bring it before the public now, both as an act of justice to its author, and also because it affords additional evidence in favour of the correlation doctrine, that it should have been independently worked out by a clear and intelligent thinker.

The first part of Dr. Mayer's treatise is concerned entirely with the Physical Forces. He starts with the two axioms, "*Ex nihilo nil fit,*" and "*Nil fit ad nihilum;*" and founds upon abstract considerations his first argument for the *unity of force,* and for the convertibility of those which are commonly accounted distinct forces. Of this convertibility he then proceeds to adduce experimental proof, in very much the same mode with Professor Grove; and he at last arrives at the following scheme, expressive of their relations.

	I. Force of Gravity II. Motion	*Mechanical force* *Mechanical effect*
	A. *Simple* B. *Undulatory, vibratory.*	
Imponderables	III. Heat IV. Magnetism Electricity, Galvanic current	
	V. Chemical Decomposition of certain elements Chemical Combination of certain other elements	Chemical Force.

He then passes on to the study of Vital phenomena; and he finds, like Dr. Carpenter, the source of all change in the living Organism, as well Animal as Vegetable, in the forces acting upon it *ab externo;* whilst the changes in its own composition he considers to be the immediate source of the forces which are generated in it. He does not enter, like Dr. Carpenter, into an analysis of the phenomena of Growth and Development; but fixes his attention rather upon the production of Heat, Light, Electricity, and (above all) Motion, by living bodies; and aims to show that all these forces are developed in the course of material changes in the organism, and hold a certain definite relation to them. On these points his exposition is very full and complete; and the perusal of his Essay will amply repay any who desire to see how much may be done in imparting precision and clearness to physiological reasoning, by minds trained in the school of the exact sciences.

ART. XIV.

A Memoir on Stricture of the Urethra. By JOHN P. METTAUER, M.D. LL.D., Professor of the Principles and Practice of Medicine and Surgery in the Medical Department of Randolph Macon College, Virginia.—*Farmville,* 1849. 8vo, pp. 45.

As every one who has once passed a bougie considers himself qualified to discourse upon Stricture of the Urethra, it is not surprising that the Press should teem with publications upon the subject; and although, as a general rule, we cannot approve of this wholesale surgical inundation, we are yet quite prepared to do justice to the merits of those who, like Dr. Mettauer, write merely for the purpose of communicating the result of their own experience in the management of the disease, and without any admixture of the vice of bookmaking. This Transatlantic pamphlet is, indeed, so sensibly written, that we are tempted to wish it had been somewhat longer than it is; not that we have any liking for the peculiar style of treatment recommended by the author,—far from it,—but because we think his account of the symptoms and causes of stricture is, so far as it goes, as well worth reading as anything we have met with for a long time past.

If there is anything that characterises his views more particularly, it is the stress he seems disposed to lay upon constitutional causes as productive of Stricture, apart from local ones. The following lines, for example, express views, which, if they had been more fully worked out, might have resulted in something considerably more important than anything which, of late years, has been added to the pathology of the disease:

"It is not determined," says our author, "in the present state of our knowledge, whether most of the cases of stricture are not the product of sympathetic irritations translated to the urethra. Certain it is, that many examples of the disease have been met with, which could not be traced to a local cause operating directly on the urethra; and when stricture has followed the action of such a cause, its appearance has generally been observed to take place at a period more or less remote from its application to the urethra." (p. 7.)

We might quote other passages in a similar strain,—proving that the author is a man of thought and experience;—but we should effect little good thereby, for Dr. Mettauer disclaims any intention of writing a Treatise on Stricture, and only desires to bring before the public the benefits to be derived from the peculiar plan of treatment which he adopts; and we are bound to say that he religiously keeps to this resolution. The treatment itself is not of a character to engage our sympathies;—it consists entirely of various methods of dividing strictures by means of instruments constructed on the plan of those which are familiar to the profession, under the name of Stafford's Lancet Catheters.

The forms of these instruments and the method of using them may be learned from the pamphlet itself; and for the rest, the author's observations on the Complications of Stricture, Fistulæ, False Passages, and so on, do not present sufficient claims to ensure our dwelling longer upon them.

As a specimen of typography, this little work will not enhance the reputation of its publisher; and we may remark, "*en passant,*" that it abounds in queer expressions, the like of which are not to be found in any language of our acquaintance, and certainly not in the tongue in which it professes to be written.

PART SECOND.

Bibliographical Notices.

ART. I.—*Pathology of the Human Eye.* By J. DALRYMPLE, F.R.S. F.R.C.S. Fasciculus VII.—*London,* 1851.

THIS Fasciculus is entirely devoted to the subject of Cataract, which, we need scarcely tell our readers, is one of the most important that can engage the attention of the ophthalmic surgeon, and one of those most capable of being illustrated by pictorial delineations. It is now almost superfluous to mention, that these delineations express everything which Art (in its present state, at least,) can represent; and the part before us is consequently one of the most valuable of the entire series.

Of the origin of Cataract, Mr. Dalrymple does not tell us anything in addition to that which was previously known; his remarks, however, indicate the sound physiological and practical information which almost invariably characterise his writings. As the nutrition of the lens, he tells us, depends upon the integrity of its capsule, and as its materials are drawn by endosmose from the surrounding vascular membranes, it is obviously necessary, not only that the capsule should preserve its integrity, but that these structures should duly perform their office; and hence we understand how opacity of the lens supervenes in cases of long-continued or chronic inflammation of the choroid, ciliary body, or iris. "In whatever way the healthy state of the neighbouring capillary vessels becomes altered, the process by which the lens is kept in its proper transparent condition is often interrupted, and cataract supervenes." In speaking of traumatic cataract, Mr. Dalrymple mentions a curious case, in which the lens, with its capsule entire, was dislocated into the anterior chamber, and there remained many weeks, retaining its entire transparency, and producing no irritation until a second accident set up inflammation in the interior of the eye, when the lens began to grow opaque. This case appears to us of peculiar interest, when it is remembered that a blow or concussion which does not appear to rupture or displace the capsule, will not unfrequently induce opacity of the lens; and we are inclined to believe, that in such a case there is usually not only an interruption of nutrition by some displacement of the natural relations of the capsule with the vitreous body or with the canal of Petit (as suggested by Mr. Dalrymple), but also some inflammatory action, which generates a morbid product, whose presence renders the lens opaque. We cannot otherwise account for the fact, that a lens altogether displaced should retain its transparency whilst nourished in no other way than by imbibition from the aqueous humour, and should then become opaque as soon as inflammation in the surrounding tissues was set up. Of this case we are afterwards further told, that the lens could be made to pass either into the posterior or anterior chamber, by inclining the head either backwards or forwards.

The following observations on *Congenital Cataract* are interesting:

"In those cases of congenital opacity of the lens, not unfrequently met with, in which partial vision exists for a considerable length of time, there appears to be an arrest of development at a period, varying in different cases, anterior to the birth of the infant; for if we see the patient for the first time, in youth or early manhood, we find, on dilating the pupil, that the lens is probably not more than two-thirds its natural bulk; that, in fact, a clear ring of black may be seen between the pupillary margin of the iris, and the circumference of the opaque lens. . . . . In these cases of imperfect cataract and ill-developed lens, the opacity is seldom complete, but the aspect is that of a milky semi-transparent body, suspended behind the iris, and relieved or contrasted against the natural dark colour of the pupil. Sometimes we see a little opaque tag or fibre, which seems to attach the capsule to the vitreous body, or more probably to the anterior layer of the zone of Zinn, as if the lens, not increasing with the general growth of the rest of the eye, its former natural connections had been partially separated, leaving here and there points of adhesion which retain it in its central position."

These conditions are illustrated by highly characteristic figures.

Mr. Dalrymple adds his testimony to the fact, which we believe that none but ignorant pretenders attempt to dispute, that whenever an unequivocal opacity is formed in the lens, such opacity never disappears, although it may remain stationary, or increase but very slowly; and he altogether discredits, therefore, the wonderful statements of Mr. Howard, on which we commented upon a former occasion (Vol. VII, p. 122).

Under the head of *Soft Cataract,* Mr. Dalrymple makes some interesting observations upon the somewhat rare species, designated as "fluid" cataract. This seems to consist in a fatty degeneration of the lens; for the opacity is of that milky character, which the diffusion of oleaginous particles through a liquid would induce; and Mr. Dalrymple has had the opportunity of determining by actual observation, that the fluid of the capsule contains the debris of the lens, easily distinguishable by the microscope, with many oil-globules, and sometimes plates of cholesterine. In old subjects, there is usually a persistent nucleus, hard, and of a yellow colour. The following observations upon the consequences of the escape of the fluid contents of the capsule into the anterior chamber, in the operation of Keratonyxis, are of special interest:

"In a few hours after the operation, the patient is seized with nausea and violent vomiting, and with intense ocular or frontal neuralgia. This is so constant an accompaniment of this form of operation, that it is necessary to forewarn the patient of the probability of its occurrence. Opium and ammonia, in full doses, will sometimes relieve the patient; and if the suffering be considerable, we may even evacuate the fluid of the anterior chamber by a puncture with a broad needle. In one case, I remember to have seen the vomiting and neuralgia continue almost unremittingly for three days. At the end of a week, however, the whole anterior chamber became clear by the absorption of the opaque fluid, and vision was beautifully restored. Upon what circumstance these phenomena depend, is wholly unknown to us; that it must, however, be closely connected with the poisonous presence of the contents of the capsule in a cavity in which absorption and reproduction are always going on, does not admit of a doubt; for if such a cataract be removed by extraction, in which case the lens generally escapes entire, no such state follows."

Upon the various operative procedures required for the cure of Cataract, Mr. Dalrymple says but little, the character of his work being rather pathological than surgical; but that little is very much to the purpose. In speaking of Keratonyxis, he especially warns young operators not to be anxious to do too much at first; for, under the most favorable circumstances, the operation requires repetition; and the most disastrous results follow from opening the capsule too largely, or disuniting the lens too extensively. No displacement of the lens, or even of a fragment of it, should be permitted, by too large an aperture, until absorption have reduced its original bulk, and the admission of the aqueous fluids softened its texture. So in regard to Extraction, he very justly remarks that the safe performance of the operation itself is the least part of the duty of the surgeon; since its success must mainly depend upon the after-treatment. Of the operation of Depression, he observes, that it has fallen into unmerited neglect in this country, and points out the cases in which recourse may be advantageously had to it.

The regular progress of this admirable work deserves our highest commendation, and will, we hope, obtain for it the support of all who are not withheld by its cost from possessing themselves of it.

ART. II.—*Urinary Deposits, their Diagnosis, Pathology, and Therapeutical Indications.* By GOLDING BIRD, A.M. M.D. F.R.S. F.L.S., Assistant-Physician to, and Lecturer on Materia Medica and Therapeutics at, Guy's Hospital. Third Edition, revised and enlarged.—*London,* 1851. Post 8vo, pp. 428. With Sixty Wood-Engravings.

IT can scarcely be necessary for us to say anything of the merits of this well-known Treatise, which so admirably brings into practical application the results of those microscopical and chemical researches regarding the physiology and pathology of the urinary secretion, which have contributed so much to the increase of our diagnostic powers, and to the extension and satisfactory employment of our therapeutic resources. In the preparation of this new edition of his work, it is obvious that Dr. Golding Bird has spared no pains to render it a faithful representation of the present state of scientific knowledge on the subjects it embraces. "Anxious not to increase its bulk," he informs us in his Preface, "I have re-written rather than added;" and in every part of the volume, we see the evidence of the care which he has taken in the elimination (to use a phrase appropriate to the subject) of all effete matter, and in the substitution of new and well-assimilated materials. Although, of course, there are many topics which are open to differences of opinion, we cannot point to any well-substantiated result of inquiry which the author has overlooked. We would caution our readers, however, against placing too implicit reliance on the tables which he gives, (pp. 50-52), of the quantity of solid matters contained in the urine, as estimated by its specific gravity; it being now generally admitted that the ratio which Dr. Christison and others have sought to establish, is not sufficiently constant to be worthy of confidence. On this head, Dr. Golding Bird thus expresses himself:—

"It is true that, by formulæ of this kind, *only an approximation to the truth* can be gained, in consequence not only of the different densities of the various elements of the urine, but from their not always existing in the same proportion, and therefore are never to be relied on, where great accuracy is required, as in the chemical analysis of the urine. Yet they are of great value in the investigation of disease at the bedside, as affording an approach to a knowledge of the solids removed from the system in a given time, sufficiently accurate for all clinical purposes, either in relation to diagnosis, or indications of treatment." (p. 50.)

The great use of these tables appears to us to be, to assist us in estimating the relative amounts of solid matter passed from day to day, when we have to do with such forms of disease, as present us with some established perturbation in the condition of the urine; in which class of cases the result is not likely to be seriously affected by the disturbing causes which come into play, when the diet, exercise of body and mind, exposure, and other conditions which affect the nature and proportions of the components of the secretion, are undergoing great variations.

Dr. G. Bird has added to this edition a very useful chapter, entitled, *Remarks on the Therapeutical Employment of Remedies influencing the Functions of the Kidneys;* his purpose being to explain the apparent caprices and irregularities which show themselves in the action of these remedies, by referring the phenomena to general laws, which will enable us to predict in any particular case what will be the operation of the remedy. These laws are as follows:

"I. All therapeutical agents intended to reach the kidneys, must either be in solution when administered, or capable of being dissolved in the fluids contained in the stomach or small intestines, after being swallowed.

"II. Bodies intended to reach the kidneys must, to ensure their absorption, have their solutions so diluted, as to be of considerably lower density than either the liquor sanguinis or serum of blood (i. e. below 1·028.)

"III. If a sufficient quantity of water cannot be received into the small intestines; or the circuit through the portal system in the vena cava ascendants [ascendens?], or thence through the lungs and heart into the systemic circulation, be obstructed; or if there be extensive disorganisation of the kidneys, the due secretion of urine cannot be effected." (pp. 387—97.)

Respecting the *first* of these laws, there cannot be now any difference of opinion. The *second* is based upon the assumption of Liebig, which we believe to be well-founded, that as soluble matters are taken into the circulation by endosmose, the neutral salts which have the most powerful diuretic effects, are not taken up if they be given in a state of too concentrated solution; but act simply as hydragogue purgatives, drawing certain of the liquid components of the blood into the intestinal canal. Attention to this principle is of special importance, in the administration of those combinations of alkalies with vegetable acids, which Dr. G. Bird has shown to have such a powerful depurating influence on the system generally. The *third* law is stated in the words of Dr. Barlow, to whom we owe the discovery of the interesting fact, that whenever a stricture or other obstruction exists in the course of the small intestines, sufficient to prevent fluids from readily passing along them, the urine will be diminished in bulk in the direct ratio of the proximity of the obstruction to the pylorus; this ratio being so constant, that observation of the bulk of the urine secreted may even be employed as a means of diagnosticating the seat of obstruction in cases of incurable constipation. The same result must happen, when any obstacle materially interferes with the route thus taken by the blood in any part of its career; as, for example, when the patient has a congested or myristicated liver, or a contracted condition of either of the auriculo-ventricular openings of the heart, so as to interfere with the free circulation of blood. "In cases of this kind," Dr. G. Bird justly remarks, "no good can accrue by goading the kidneys by diuretics, unless the obstruction can possibly be lessened or removed. They may be irritated by stimulants like cantharides, copaiba, or squills, until congestion or something worse occurs, without increasing the secretion of urine, simply because the fluid elements are prevented reaching the kidneys." It is in the exercise of this kind of discrimination, that the character of the *scientific* as distinguished from the *routine* or *empirical* practitioner, is most advantageously displayed; and it is one of the most wholesome indications of progress in our art, that such distinctions should not only be drawn by the sagacious practititioner for himself, but that they should be capable of being reduced to rules, which even the least learned can apply for himself. With three such rules, Dr. G. Bird concludes the present treatise: we hope that in a future edition, he will be able to expand this most valuable chapter by the results of additional investigations, and thus add to the obligations under which the profession already lie to him, for the zeal and ability with which he has laboured to place the science of therapeutics upon a positive basis:

"1. Whenever it is desirable to impregnate the urine with a salt, or to excite diuresis by a saline combination, it must be exhibited in solution, so diluted as to contain less than 5 per cent. of the remedy, or not more than 25 grs. in an ordinary draught. The absorption of the drug into the capillaries will be ensured by a copious draught of water, or any diluent, immediately after each dose.

"2. When the urine contains purpurine, or presents other evidence of portal obstruction, the diuretics or other remedies employed should be preceded or accompanied by the administration of mild mercurials,—taraxacum, hydrochlorate of ammonia, or other cholitic remedies. By these means, or by local depletion, especially by leeches to the anus, the portal vessels will be unloaded, and a free passage obtained to the general circulation.

"3. In cases of valvular disease, or other obstructions existing in the heart and large vessels, it is next to useless to endeavour to excite diuretic action, or appeal to the kidneys by remedies intended to be excreted by them. The best diuretic will, in such cases, be found in whatever tends to diminish the congested state of the vascular system, and to moderate the action of the heart; as digitalis, colchicum, and other sedatives, with mild mercurials." (p. 400.)

ART. III.—*On the Causes, Symptoms, and Treatment of Spermatorrhœa.* By M. LALLEMAND. Translated and Edited by J. M'Dougall. Second Edition.—*London*, 1851. 8vo, pp. 411.

THE opinion we expressed upon the appearance of this translation, (Vol. I, p. 34,) that it supplied a *desideratum*, has been amply justified by the rapid sale of the first edition. Doubtless, in a work of this nature, many of the purchasers have been from among the lay-public; and this is by no means to be regretted, for as persons suffering under this class of disorders *will* peruse works relating to them, it is surely preferable that their attention should be directed to such as this, which, without being written for them, or indeed departing at all from a strictly professional character, by the graphic pictures it contains, and the sound counsel it administers, may give them a true idea of their position and the nature of the means necessary for their relief, and may save them from the consequences of the perusal of the vague, ignorant, and filthy publications so assiduously advertised and distributed, even by men who, to the disgrace of our Collegiate Institutions, are still allowed to inscribe themselves on their title-pages as members of an honorable profession. It is also to be hoped, however, that the greater diffusion of M. Lallemand's views among the members of the profession in this country, has had the effect of inducing many to undertake the charge and careful investigation of this class of cases, instead of, as heretofore, leaving them to the tender mercies of the charlatan.

The present edition is very handsomely got up; but we must confess to disappointment in finding Mr. M'Dougall not availing himself of the opportunity afforded by its issue, of giving some account of the additional experience of M. Lallemand and himself upon the subject, seeing that nearly ten years have elapsed since the appearance of the original work, and four since that of the translation. When so judicious an observer as Mr. Phillips, ('Med. Gaz.,' Vol. XLI. p. 489,) basing his criticism upon many hundred observations, taxes M. Lallemand with great exaggeration, both as respects the prevalence and consequences of the disease, any additional evidence as to the correctness of the original assertions should have been adduced. Mr. M'Dougall, in his more recent investigations, has found reason to attribute great importance to the character derived from an altered chemical condition of the urine, to which he alludes in the following passage of the preface to this edition:

"The translator wishes to express his coincidence with M. Lallemand's views, that, in many cases, disorders of the brain arise from spermatic discharges, and may be relieved by the arrest of this drain; at the same time, he finds that the most serious cases are always attended with anæmia, accompanied generally by great disorder of the nutrition of the body, and characterised by disordered urine. This generally contains oxalate of lime, and has a tendency to rapid putridity, with deposits of phosphates in some cases; while in other cases, cellular formations are rapidly developed, especially in a warm temperature. On these peculiarities the translator hopes, on a future occasion, to lay his opinions before the profession."

Mr. M'Dougall prefaces this edition by a short "historical notice" of what has appeared upon the subject both in ancient and modern times; whence it appears, that, although prior to M. Lallemand's publication, the involuntary flow of semen, under the designation "gonorrhea," was frequently adverted to, it is to him we are indebted for first laying down the true principles of the etiology and diagnosis of the affection; while all those who have subsequently written upon it have limited themselves to a reproduction of his views.

In one of the few additional notes to this edition, the translator observes, that in nearly one-third of the severer cases of involuntary seminal discharges, the patients have suffered from *varicocele;* but he is disposed to consider this rather as a complication and sign of want of power to resist disease, than as a cause of disease itself. In all these cases the varicocele was situated on the left side, with one exception in which there was also a commencing varicocele on the right.

"Many of these cases have suffered greatly from constipation, and there seems no doubt as to the bad effects of the pressure of the distended colon and sigmoid flexure on the long spermatic vein. The majority of the patients could give no account of the formation of the disease. The attention of many seems to have been accidentally called to it after it had attained so large a size as to have become troublesome; and this has generally occurred after the age of puberty, and before thirty. Although, as I have above stated, I believe the presence of varicocele to be rather a negative than a positive cause of disease, it is certainly possible that in some unfortunate cases the venous enlargement may become so great, and the pressure and retardation of the column of blood so serious, that the secretion of the testicle may be materially interfered with, and in a few rare cases the testicle itself may dwindle and waste. Of course, with a double varicocele this may occur on both sides, and the patient consequently will become impotent. Such a state of things, however, must be exceedingly rare, judging from the infrequency of enlargement of the spermatic vein on the right side. Another condition may also probably arise from a varicocele, viz.: from the constant pressure and disorder to the circulation in the testicle and epidydimis, the semen is not properly secreted, the seminal animalcules not properly formed, and thus an imperfect secretion reaching the seminal vesicles, these may become irritable, inducing obstinate pollutions, resisting, or only a little benefiting by treatment." (p. 273.)

Believing that even yet another edition will be required at no distant period, we hope that Mr. M'Dougall will occupy the interval in preparing an account of his own and M. Lallemand's more recent experience.

ART. IV.—*Portraits of Diseases of the Skin.* By ERASMUS WILSON, F.R.S. Fasciculi VIII and IX.—*London,* 1851.

Two Parts of this truly splendid work have appeared since we last directed our readers' attention to it; thus fully justifying in the rate of their issue, as they do in the perfection of their execution, the promises made by Mr. Wilson at its commencement.

Of the Eighth Fasciculus, the First Plate admirably delineates a case of *Sycosis,* which is considered by the author as being essentially a disease of the sebiparous glands, whose secretion is poured into the hair-canals; but depending upon a disorder of assimilation, of which the local affection is but a manifestation. In regard to the treatment of this most troublesome complaint, Mr. Wilson mentions that he has found the strong citrine ointment, the iodide of sulphur ointment, and the tar ointment, to be the most useful local applications; but that it is difficult to predict in any individual case, which of these will be most serviceable, one frequently succeeding where another has failed, or even proved highly injurious. At the same time he gives Fowler's or Donovan's solution internally; but he mentions a case in which a gentleman was nearly poisoned by two drops of the former.

The Second Plate represents a case of the *Molluscum contagiosum* of Bateman; to which, however, Mr. Wilson denies the contagious character, (an opinion in which we quite accord), and which he therefore designates as *Molluscum simplex.* This appellation we consider objectionable, as it has been already appropriated by Simon and others to an affection, in which the external manifestation is nearly the same, but in which the multiple tumours are chiefly composed of the elements of the skin, and not formed by the distension of the sebaceous glandulæ by accumulated secretion; and we should suggest in its place some term that should express the sebiparous origin of the affection. Mr. Wilson points out that, when it occurs in children, it is commonly associated with a strumous diathesis, and that it is benefited by treatment which tends to invigorate the system. By touching the tumours a few times with nitrate of silver, or, in adults, with potassa fusa or nitric acid, they shrivel and fall off, leaving scarcely a trace of their existence; and we may mention that the same treatment is applicable to those solitary tumours, which are of very frequent occurrence near the edges of the tarsi, and of which the structure is the same, although, as they do not appear in a "multiple" form, they are not considered as constituting Molluscum.

In the Third Plate we have an admirable portrait of a case of *Pemphigus* (*Pompholyx*) *acutus*, occurring on the arm of a delicate emaciated child of scrofulous diathesis, who had suffered six attacks of the acute form of the disease within twelve months; the appearance of the eruption being preceded for some days by constitutional disturbance, which subsided as soon as it had burst forth. It was obvious, that in this case the disorder was attributable to the general debility consequent upon the want of fresh air and of nourishing food; there being, at the same time, an unusual *waste* in the system, for the urine contained an unduly large proportion of urea. Here, again, Mr. Wilson states, that he has found Fowler's solution useful; but we must express our doubts whether this was a case fitted for its administration, and whether the patient's improvement was not rather due to the nutritive and strengthening diet on which he was placed.

The Fourth Plate gives a no less excellent representation of a case of *Lichen syphiliticus*, var. *corymbosus;* a peculiarity in which was, that the eruption appeared concurrently with an attack of rheumatism (apparently in consequence of exposure to cold) whilst the patient was under mercurial treatment for a primary sore, without the occurrence of bubo or any other indication of affection of the general system. The eruption broke out first upon his face, and then upon his legs, front of the trunk, and back. The arm is selected for the subject of illustration, as presenting the best example of the corymbose arrangement. Mr. Wilson remarks that:

"The occurrence of a sero-purulent fluid or pus in the summits of the papules of syphilitic lichen, is a common character when the eruption is severe, has lasted a long time, or has been aggravated by exposure to cold, or the undue or injudicious use of mercury. I have seen instances in which the greater number of the papules were capped by a white summit of pus. When the pus dries up, it forms a scab of some thickness; and occasionally, though rarely, on the fall of a scab, there remains behind a trifling degree of ulceration. When seen in this stage, the eruption might be mistaken for one of a primary vesicular or pustular kind."

The case was successfully treated with iodide of potassium and sarsaparilla.

In the First Plate of the Ninth Fasciculus, Mr. Wilson gives seven figures of what he designates *Lichen annulatus solitarius*, which is one of the affections usually grouped under the generic name of "ringworm." These seven figures are all disposed on the person of one individual; a departure from Mr. Wilson's usual rule of giving "portraits" of actual subjects, which seems to us peculiarly unfortunate in the present case. For the occurrence of only a single patch on any one individual is a character which he regards as nearly constant in this affection; and yet the unfortunate boy, from whom the figure is drawn, has not only his own ring to answer for, but half a dozen others from as many different subjects; one of them being transplanted from the base of the thigh of a young gentleman to the root of his neck, another from a little girl, another from a young married woman, and so on. We had much rather see these delineated in the old *square* style, than put on a "subject" in a wrong place. Of the delineations themselves, however, we can only speak in terms of the highest commendation; those who have been familiar, as we have, with the affection in question, must at once acknowledge their beauty and fidelity. Mr. Wilson does not express any decided opinion as to the contagiousness of this affection; we have ourselves no question of it, and feel sure that it cannot be justly attributed to derangement of the general health, as we have seen it in children in the best possible condition, who had been associated with others infected with it. The case narrated by Mr. Wilson confirms the view we had previously been disposed to entertain, that it may be regarded as a variety of common ringworm of the scalp, originating in the same contagion, and frequently occurring on the bodies of children whose heads are affected with the latter disease.

"The pathological seat of this curious eruption is the follicles of the skin, the pores of which are raised up into papules, which are identical with those of simple lichen. Sometimes the papules are quite distinct, or one or two are more prominent than the rest; at other times they are confluent, and form an almost even ridge, upon which the cuticle rapidly exfoliates."

The facility with which this disease may usually be checked and cured by such local applications as acetum cantharidis, or tincture of iodine, or (we may add) nitrate of silver, without any constitutional treatment whatever, is an indication of its external origin.

The Second Plate is a genuine Portrait of a disorder named by Mr. Wilson *Roseola annulata*, which he regards as Congestive Inflammation of the Derma. We should say the same of the disease here delineated, as others have said before us; namely, that it may receive three or four different generic designations, according to the symptom or appearance to which most weight is attached. In regard to the nomenclature of cutaneous affections, the old adage, *Quot homines, tot sententiæ*, is most peculiarly applicable; and we are not by any means sure that the subject has not been additionally complicated by some of Mr. Wilson's fine-drawn deductions. Our present business, however, is rather with the fidelity and artistic character of the portraits, than with the correctness of Mr. Wilson's classification.

The subject of the Third Plate is a case of *Lichen syphiliticus confertus*, which is so characteristic, that there can be no difficulty in its recognition. This plate is another of those marvels of minute workmanship. to which we have before called our readers' attention; the whole surface of the abdomen being represented as covered with thickly-set papulæ, which appear at certain spots to be enlarged, or to have coalesced, into those larger prominences which are known as syphilitic tubercles. The pathological condition to which Mr. Wilson refers this disease, is Depositive Inflammation of the derma.

The Fourth Plate is an admirable delineation of that well-known affection, *Psoriasis guttata;* which is designated by Mr. Wilson as Squamous inflammation of the derma, a term which does not seem to us to be peculiarly happy or characteristic, "squamous inflammation" not being a pathological state to which we can attach any distinct meaning. We believe, with Mr. Wilson, that the arsenical treatment is almost a specific for this disease, when the patient can persevere in it sufficiently long.

We have only to add that the execution of this part is in every respect, save that which we have named, on a par with that of its predecessors.

ART. V.—1. *Elements of Anatomy.* Compiled from the most recent Authorities, and translated into Hindustani by FREDERICH J. MOUAT, M.D., Assistant-Surgeon Bengal Army, Member of and Secretary to the Council of Education, Professor of Materia Medica and Medical Jurisprudence in the Bengal Medical College, &c. &c. (Published by Order of Government.)—*Calcutta,* 1849. 8vo.

2. *An Atlas of Anatomical Plates of the Human Body ; with Descriptive Letterpress in English and Hindustani.* By FREDERICK J. MOUAT, M.D., &c. &c. (Published by Order of Government.)—Folio, pp. 281.

THE first of these works is an original Treatise, rather than a translation, inasmuch as the matter of it has been collected from many sources. The second work is a large atlas, with an English and Hindostani text. The plates are taken from Bourgery, Quain and Sharpey, and other sources, and not a few original drawings have been inserted by Dr. Mouat himself. The English text is extremely well done; the description of parts is concise and simple, and yet as far we have gone over it, very accurate. The authorities for the anatomy are, Meckel, Quain, Ellis, Harrison, and Cruveilhier; and for the Physiology, Carpenter and Bostock. We are not able to say much of the Hindostani part from personal knowledge; but we have every reason to believe it is as good in its way as the English portion of the work. In producing these treatises, Dr. Mouat has done more to facilitate the acquirement of anatomical knowledge in India, than could have been effected in any other way. Thanks to him, the native student can now lay as extensive and as correct a foundation of Anatomy, as his European brother.

We must, however, not only speak in very high terms of Dr. Mouat as an anatomist, but we must express our satisfaction at the method he has adopted in executing his Hindostani version. This was not so easy a matter as may be imagined. Two difficulties attended him at the outset. One was as to the choice of the language. Sanscrit is the learned and classical language of the Hindus, and Arabic is the tongue of the Mahommedans. But to employ Sanscrit would be like using Latin in the present day in Europe, when some few in all countries would read it, but the great mass, who know only their vernacular tongue, would profit little by it. To use Arabic, again, would be to confine the use of the work to a comparatively small class. Dr. Mouat, therefore, wisely determined to choose that vernacular language which is common to both Hindoos and Mahommedans, and which is familiar to all native medical students. The choice of the language being settled, another point remained. This was as to the translation of the European scientific terms. It might be possible to find, for some of them, Sanscrit or Arabic synonyms; but as this was impossible in every case, Dr. Mouat determined on transposing bodily the European terms into Hindostani, giving at the same time the Sanscrit or Arabic synonyms, if such could be found. This plea has also the great advantage of forming a standard of scientific terms, which is common both to Europe and the East.

"The experience of the past," writes Dr. Mouat, "does not afford much promise of success for the future, in superseding the ignorance of the East, by translating the learning of the West, into the sacred and classic media of their learned tongue. It is through the vernacular language of the people, the vulgar mother-tongue of the mass, that the impression must be made, and the mists of ignorance and prejudice be dispelled. These are views which I know are disputed by many eminent Orientalists, whose opinions are entitled to the highest respect and consideration. It must be remembered, then, that the present work is written chiefly for the pupils of the military class, attached to the Medical College, none of whom are Arabic, and few Persian scholars. They are all taught in Urdu, and are rendered familiar with the European scientific terms used in anatomy, chemistry, medicine, surgery, and materia medica, from the very commencement of their study. To introduce, therefore, simple or compound Arabic terms in their text books, would at once render them unintelligible, as it presupposes an amount of classical Oriental learning, possessed by none of them. It would be productive, likewise, of the disadvantage of being perfectly incomprehensible to the medical officers under whom they are hereafter to serve; whereas, where the same terms with the same significations attached to them, are used by both, there is little chance of mistake or misunderstanding, and the duties of native corps may be efficiently carried on with a very moderate acquaintance on the part of the surgeon in charge with the vernacular language of the Sepoy, and of the native doctor."

In addition to these works, Dr. Mouat has published a Persian version of the London Pharmacopœia, and we are happy to see that Hindustani treatises on medicine, surgery, and chemistry, are in active preparation.

It is not a little singular to observe the West now repaying to the East interest and compound interest for the knowledge which originally flowed from India and Egypt; and it is most satisfactory to find that our empire in the East is not only one of conquest and of tribute, but is to be, under God's providence, a means of diffusing true knowledge, and its attendant happiness, through the vast regions over which floats the banner of England. We cannot too much praise the able and indefatigable men who are the agents of this diffusion; and among these men, no one has done so much as Dr. Mouat, or done it so well.

It is pleasing also to observe the modesty with which this man, who is performing a duty so important, speaks of his own qualifications for the task.

"It is to be regretted," says he, "that the task was not undertaken by a professed anatomist, as well as an Oriental scholar. I can lay claim to neither character. My only pretensions are an earnest desire to be useful to the best of my humble abilities, and an anxious wish to supply a great public want, until some one more worthy, in every sense, than myself, shall arise to execute the work in a fitting manner." ('Atlas,' p. 5.)

For our part, we doubt whether there is any one in India who could have executed both duties so well.

We must not omit to observe that the work is very well got up. The Hindostani types are very good, and the execution of the plates reflects no little credit on the Calcutta craftsmen.

Art. VI.—*Dr. Hooper's Physician's Vade-Mecum: or, a Manual of the Principles and Practice of Physic. New Edition, considerably enlarged and improved, with an Outline of General Pathology and Therapeutics.* By William Augustus Guy, m.b. Cantab., Professor of Forensic Medicine in King's College, London, &c.—*London,* 1851. Fcap. 8vo, pp. 576.

The Edition before us is the Third which has been issued since the work was placed under the charge of its present Editor; and each of these has constituted a great improvement upon its predecessor, as well as, we need hardly say, upon the original. In fact, we question whether, if the departed author could now look into his Vade-Mecum, he would recognize his own treatise. The First Part, which is an Outline of General Pathology and Therapeutics, is entirely the work of Dr. Guy; and this has been extended in the present edition from 163 to 209 pages. We do not hesitate to express the opinion that this is by far the most valuable portion of the book; and that it is the best outline of the subject that could be put into the student's hands. Our only complaint against it is its conciseness; and we trust that in a future edition, Dr. Guy may be led to omit much of what is designated the "Practice of Medicine," which cannot, within the limits of 300 pages, be treated in such a way as to be *really* useful to any class of students, and will incorporate this Part with the preceding, as "Special Pathology." Of all subjects, the *Practice* of Medicine is the one that will the least bear compression within the procrustean limits of a "Manual;" the *principles*, however, are capable of being much more concisely stated; and we feel satisfied, from the excellent manner in which Dr. Guy has hitherto performed his editorial office, that he will most benefit the readers of the Vade-Mecum and will do most justice to his own scientific character, if he will give up Dr. Hooper altogether, and stand upon his own foundation.

We must not omit to notice, that in the arrangement of diseases in this edition, Cullen's Nosology is no longer followed, but an arrangement is adopted, that is more in accordance with the present state of knowledge. It seems to us by no means perfect,—what Nosology, indeed, is?—but as it has been framed rather with a view to convenience than to scientific correctness, and is well calculated to answer that purpose, we shall not stop to criticise it.

ART. VII.—*A Catalogue of Medical and Scientific Works: to which is added, a Classified Index of Subjects, including the Names of those Authors who have treated upon them.—London,* 1851. 8vo, pp. 83.

THIS Catalogue, issued by Messrs Highley, is intended, we are informed, "to comprise books published during the last five and twenty years, excepting pamphlets and other publications of less than half-a-crown in price." Its scope, however, is nowhere clearly indicated; since its title leaves it doubtful *what* are the Sciences which it embraces in addition to Medicine; and we might be led to infer from it, that Foreign as well as British publications would be included. We believe that we are correct in stating, however, that the Scientific Works whose titles are here catalogued, are those which treat of subjects included in the Medical Curriculum, such as Chemistry, Botany and Vegetable Physiology, Zoology, and Comparative Anatomy, (Geology, however, being also introduced;) and that it has not been intended to go beyond the range of English Literature, save in the case of translations or republications of Foreign or American Works.

We cannot, of course, vouch for the accuracy or completeness of this Catalogue; but can only say, that, on looking into it for a number of works by various authors, whose titles occurred to our minds, we have found all save one,—the exception being "Prof. Owen's Memoir on the

Pearly Nautilus," which is interesting, as the work on which his reputation as an Anatomist was originally founded. And we have been made aware, on glancing through its pages, of the existence of so many other works of which we had never previously heard, and of which we are very glad to be informed, that we cannot but believe that such a Catalogue must have a a very general utility.

The Second Part of it is intended to serve as a '*Catalogue Raisonné*,' giving under each head the names of the authors who have written upon it, the titles of their works being to be found by reference to the First Part. This, we think, might have been made more complete with advantage; since it is based rather on the *titles* of the works, than on their *contents;* and it consequently makes no allusion to many authors who have written upon similar subjects under diverse titles. The difficulty of making a good 'Catalogue Raisonné of Medical Literature' is extreme, on this very account; and we do not know of any instance in which it has been fully grappled with, save in that of the 'Library of the Royal Medical Society of Edinburgh,' which occupied many zealous and able workers for several years. Still, though far from complete, this attempt of Messrs. Highley to make the profession acquainted with its own literature, deserves all commendation; and will be found, we doubt not, extremely useful, provided it be borne in mind that the "Classified Index," though indicating the authors who have *professedly* treated on each subject, does not afford references to many who have *really done so under some different heading.*

ART. VIII.—1. *The Teeth and their Preservation, in Infancy and Manhood, to Old Age.* By ALFRED CANTON, M.R.C.S.L., Dental Surgeon to Charing Cross Hospital.—*London,* 1851. Fcap. 8vo, pp. 207.

2. *Practical Observations on the Teeth: designed for Popular Use.* By HENRY JORDAN, Dentist.—*London,* 1851. Fcap. 8vo, pp. 104.

HOLDING as we do that Dental Surgery, like Ophthalmic or Aural, should only be exercised by men who have received a regular professional education, and that it will not be rescued from the hands of the quacks who at present disgrace the practice of a most valuable art, until the public shall have been enlightened on the importance of a sound scientific training to those to whom they intrust the care of their Teeth, we gladly welcome the appearance of any Treatises which communicate the results of the experience of those who deservedly rank among the ornaments of the profession, in a form adapted to benefit the public; almost every individual of which has need to be instructed in various matters, which, though apparently of very little amount, have an ultimate bearing upon his comfort, that renders it most important that he should pay timely attention to them.

Each of the treatises before us is well adapted to this purpose. Each seems free from the taint of charlatanism, and conveys good sound information in a pleasing form. Mr. Canton's is by much the fuller of the two; but we are not sure that this is clear gain; for brevity is a great merit in these days, and a great many of Mr. Canton's pages are occupied with irrelevant matter. It has the additional advantage, however, of being written by a gentleman who has received a medical education; and it enters into many topics which Mr. Jordan leaves wholly untouched. We cannot say, however, that it bears the marks of any such original research as characterises the works of Bell, Formes, and other eminent dentists; and there are several points on which we should have liked to see evidences of a greater degree of discrimination in the enunciation of principles of treatment. Thus, in speaking of laryngismus stridulus, and other convulsive diseases of the first dentition, Mr. Canton sanctions the freest possible use of the gum-lancet, and says not a word of the paramount importance of fresh air and wholesome diet, which we hold to be the most complete preventives of that irritable state of the nervous system, of which these attacks are only the manifestations.

Of Mr. Jordan's little treatise, we need only say, that it is "excellent as far as it goes;" and that it contains all that is essential to be known upon the general Management of the Teeth.

A good Monograph, however, of the Disorders of Dentition, with their hygienic and medical treatment, preventive and curative, strikes us as a desideratum.

Art. IX.—*Phthisis and the Stethoscope. A Concise Practical Guide to the Physical Diagnosis of Consumption.* By Richard Payne Cotton, M.D., Assistant-Physician to the Hospital for Consumption and Diseases of the Chest.—*London,* 1851. Fcap. 8vo, pp. 97.

This neat little Volume contains, in a slightly-altered form, the short course of Clinical Lectures, which we noticed with approbation at the time of their first publication in the 'Medical Gazette.' (See Brit. and For. Med.-Chir. Rev., Vol. V, p. 322.) Dr. Cotton's reason for reissuing them in the present form, is thus stated in his Preface:

"I had often been perplexed, and I might say, almost discouraged, in the endeavour to separate from the many excellent works we possess upon physical examination, the various points really available in the practical diagnosis of Phthisis; and it occurred to me, that having, to some extent at least, overcome the difficulty, it might be serviceable to others, were I to describe them as concisely as possible, and apart from everything either speculative or argumentative."

Believing as we do, that this little volume, whilst "not intended to supersede any of the more ample and discursive works upon physical diagnosis," is well adapted "for the use of those, whose professional duties require that such matters should be studied rather in abstract than in detail," we have much pleasure in repeating the favorable opinion we have already expressed,—that, although containing nothing very novel, it is very complete, and conveys sound practical information in an easy and agreeable style.

Art. X.—*The Physician's Report of St. Luke's Hospital for Lunatics, for the Year* 1850; *with Statistical Tables for* 1850 *and the last Century.*—*London,* 1851. 8vo, pp. 59.

We notice this Report, among the numerous *brochures* of the same kind which are continually transmitted to us from various sources, on account of the very interesting Statistical Tables it contains; which carry back our view over a period of time and a length of experience of which few other institutions can boast. Of these tables we shall avail ourselves in our Periscope, as opportunity serves.

PART THIRD.

Periscope.

ANATOMY, PHYSIOLOGY, AND ORGANIC CHEMISTRY.

On the Impregnation of the Ovum in the Amphibia. By George Newport, F.R.S., F.L.S. &c.

The author states that this communication to the Royal Society is part of a series of investigations on development, on which he has been for some years engaged, and which was commenced in a paper on that of the Myriapoda, published in 1841, in the 'Philosophical Transactions.' The plan followed in these investigations has been to combine observations on the natural history of the animals with others on the conditions which affect their development, as the best mode of arriving at correct conclusions. The history of the discovery of what can now be proved to be the direct agent of impregnation, the spermatozoon, is then traced; and it is shown, that although within the last few years an opinion has been gaining ground that the spermatozoon, and not the *liquor seminis*, as formerly supposed, is the means of impregnation, no acknowledged proof has hitherto been given of the correctness of this opinion, and no refutation afforded to the theory that the *liquor seminis* is the part of the seminal fluid immediately concerned. The question of the agency of the spermatozoon has thus remained open; and it is to this question, with a view first to supply proof from direct experiments of the fact of the agency of this body, as well as to examine into the circumstances under which this agency is exerted, influenced, or impeded, that the present communication is especially devoted.

The author then traces the changes in the ovum within the body of the amphibia, from a short time before the disappearance of the germinal vesicle to the period when the ovum is expelled before impregnation. The germinal vesicle in the ovarian ovum is found to become filled with an aggregation of secondary cells, as stated by Barry; but the author differs entirely from the latter respecting the mode of disappearance of the vesicle, and also respecting the part played by its constituents in the production of the embryo. He believes the included cells to be liberated by the diffluence of the membrane of the germinal vesicle in the interior of the yelk, not in the centre of the yelk, but much nearer to the upper or dark surface than to the white or inferior, and at the bottom of a short canal, the entrance to which is in the middle of the upper or black surface at a point already noticed by Prevost and Dumas, Rusconi and Baer; and he thinks that it is due to the diffluence of the envelope of the vesicle in this situation, that the moment of disappearance has not yet been observed. The germinal vesicle in the amphibia always disappears before the ovum leaves the ovary and escapes into the cavity of the abdomen. The mode in which the ovum, after leaving the ovary, is believed to arrive at the entrance of the oviduct is then stated, and the structure of the entrance in the intermedial space, as shown by Swammerdam, described.

The author then traces the changes in the impregnated and in the unimpregnated ovum after spawning, from the first minute, to the segmentation of the yelk in the former; and shows that the appearances in the two are almost identical during the first ten or twelve minutes, but that after that time the changes in the unimpregnated ovum cease, while further changes take place in the impregnated. The yelk at the end of from twelve to fifteen minutes invariably then rotates, so that the dark surface becomes uppermost: and it constantly afterwards returns to this position, however much or frequently this may be changed. In about three hours the yelk becomes separated on the upper surface from the vitelline membrane, and a space or chamber is formed between the two. The yelk then becomes depressed on the upper surface, but is slightly elongated to an obtuse oval form, with horizontal direction; and in about half or three-quarters of an hour afterwards, begins to divide in the margin of the central spot or orifice, from which point the division, as already known, passes outwardly and around the yelk until the mass is divided into two portions. These changes do not take place in the unimpregnated ovum, which merely becomes somewhat oval, but does not divide; so that segmentation may be regarded as a proof that the egg has been impregnated, a fact that was of great use as a test in the following experiments. The susceptibility of the ovum to become impregnated, and the circumstances which affect this, are then shown to depend on the degree of expansion of the envelopes, the imbibition of fluid, the temperature of the surrounding medium, and the degree of aëration. The envelopes expand and imbibe fluid most rapidly during the first half hour after the egg has been laid, and the susceptibility is diminished in the inverse ratio of the expansion and imbibition. It is greatest during the first three minutes, but is very feeble at the end of half an hour. These conditions are greatly modified by temperature, and in a much less degree by the aëration of the ovum. Experiments in proof of these facts are detailed, especially with reference to the number of eggs segmented and of embryos produced, and their earlier or later appearance in proportion to the higher or lower temperature of the medium. In March, 1849, the author put to the test the agency of the spermatozoa in impregnation, by an experiment long since employed by Spallanzani, and more recently by Prevost and Dumas, namely, by carefully separating the spermatozoa of the Frog from the *liquor seminis* by filtration, and employing these, with the filter-paper on which they had been collected, in experiments on some sets of eggs, and the liquor in others; and the result was, that almost every ovum became impregnated in the former, but scarcely a single ovum in the latter. The production of a very few embryos in the sets to which the *liquor seminis* was added, he attributes to the fact that the whole of the spermatozoa had not been removed. These experiments he has repeated during the present spring with still more decided results; not a single ovum becoming segmented, nor a single embryo produced, when the *liquor seminis* was completely freed of spermatozoa. The author states, that these experiments had been completed, and he was engaged in preparing the paper for presentation to the Royal Society, before he was aware that the physiologists above named, Spallanzani first, and afterwards Prevost and Dumas, had obtained similar results by filtration of frog's semen, although the fact of their observations has been almost overlooked. To them, therefore, he resigns the credit of the results; but as his own investigations have been so completely independent of theirs, from which also they differ in some respects, he has felt it to be desirable still to give them in detail in this paper.

The direct agency of the spermatozoa in impregnation being thus proved, the author proceeds to investigate its nature. He first shows that the ova are not impregnated after the motive power in the spermatozoa has ceased. The period of duration of this power he finds to be much shorter than was supposed by Spallanzani and by Prevost and Dumas; and he attributes the difference in length of time as observed by these authors and himself, to their having adopted the objectionable mode of procuring the seminal fluid by vivisection from the testes as well as the vesiculæ seminales, by which he conceives that spermatorial cells were obtained as well as mature spermatozoa, and that the former became matured only at a late period of the experiments. He differs also from Prevost and Dumas, and Dr. Martin Barry, with regard to the supposed penetration of the spermatozoon bodily into the ovum. All the observations he has been able to make on the ovum of the frog, both microscopically and experimentally, are opposed to the belief that any fissure or perforation exists in the envelopes of the ovum, as described by Dr. Barry in the ovum of the rabbit, through which the spermatozoon was supposed to enter. Neither is he able to confirm the statements of Prevost and Dumas, that the spermatozoa penetrate into the substance of the envelope of the egg, either of the frog or the newt; and he thinks these distinguished observers must have supposed that spermatozoa which they saw on the exterior of the ovum were in the interior. The author has put this question to the test in the ovum of the newt, *Lissotriton punctatus.* He extracted an ovum, which he had reason to believe had not been impregnated, from the oviduct, and placed it with seminal fluid in water, and immediately afterwards examined it with the microscope. Spermatozoa were detected upon it in less than one minute after immersion; but neither then, nor at any subsequent period, could even a single spermatozoon be seen within it, although the whole interior of the egg was brought within focus of the microscope, and distinctly recognised. This egg was preserved in a small glass capsule beneath the microscope, and watched until the embryo was produced. Spermatozoa were recognised on the exterior during the first forty-eight hours.

But although spermatozoa do not enter the interior, they are invariably found in contact with the surface of the impregnated ovum, and this contact is essential to their agency. The author also shows that the envelopes of the ovum are essential to its fecundation, and that ova taken from the ovary, or from the cavity of the body after they have left the ovary, but which have not yet entered the oviduct and acquired their gelatinous coverings, are not susceptible of being impregnated. The coverings imbibe water by endosmose, but do not usually admit solid particles of matter equal in size to spermatozoa into their texture, as was proved by immersion in solution of carmine.

The author then enters at length on an examination of the agency of the spermatozoa, as affected by chemical media. Availing himself of a fact ascertained during a chemical analysis of seminal fluid by Dr. Frerichs, that the spermatozoa are decomposed by caustic potash, he conceived the possibility of so employing this agent as to render it a test in experiment. Ova were passed from the body of a frog on a dry surface, without being in contact with water, until seminal fluid mixed with it was applied to them. After the lapse of a given time, solution of caustic potash, of sufficient strength to decompose the spermatozoa immediately, was also applied, and as quickly as possible afterwards was again diluted and removed with water, before the potash, as found by other experiments, acted prejudicially on the ova. The result was, that segmentation of the yelk usually took place even when the interval of time between the application of the seminal fluid and the solution of potash was only one or two seconds, but no embryos were produced. When, however, the interval was five seconds, a very few embryos were formed; but when the interval was fifteen or more seconds, they were produced in greater number. The conclusion deduced from these and similar experiments with nitrate of potash was, that impregnation is commenced almost at the instant of contact of the spermatozoon with the ovum; but that duration of contact, and possibly also diffluence of the spermatozoon and endosmosis of its substance, is necessary for fruitful impregnation. The experiments were varied by the application of the solution of potash before that of the seminal fluid, in which case the results were more unfavorable. With nitrate of potash, applied before as well as after the seminal fluid, the formation of embryos was not unfrequent. None however were produced when diluted acetic acid was used. This acid acts quickly and most unfavorably on the envelopes of the ovum.

The agency of the impregnating bodies was then tested in a similar way, by the application of solutions of gum-arabic and of starch, the action of which is merely mechanical. The results were similar to those with the potash.

When the gum or starch was applied, as in the case of the potash, *after* the application of seminal fluid in water, embryos were constantly produced, even when the interval between the two applications was only one second; but when either of these was applied to the ovum *before* the seminal fluid, then segmentation, if it occurred at all, took place very tardily. In general, however, no segmentation occurred, and no embryos, or but very few indeed, were produced.

These experiments, compared with those with potash, seemed to show that impregnation is commenced in a very short space of time, and that the spermatozoon is the agent immediately concerned; and that this agency is material in its operation, as seems to be shown in the fact that it can be prevented by the application both of chemical and of mechanical means to the ovum. We are thus led to infer, that although the spermatozoon does not bodily penetrate into the ovum, its first effect may have some relation to catalytic action, in inducing the segmentation of the yelk; whilst, having proof that fluids permeate the coverings of the ovum, we may hereafter find that the process is completed by the diffluence of the impregnating body, and the passage into the ovum of the substance into which it is dissolved, by endosmosis.—*Philosophical Transactions*, 1851.

On the Structure of the Membrana Tympani of the Human Ear.
By JOSEPH TOYNBEE, F.R.S. F.R.C.S., &c.

THE Author states that he has been led to make a careful investigation into the intimate structure of the tissues entering into the composition of the Membrana Tympani, from the necessity of obtaining a basis for his pathological researches. Anatomists generally have recognised three layers in this membrane; an *external* one continuous with the cuticle of the external meatus; an *internal* one, derived from the mucous lining of the tympanic cavity; and a *middle* or fibrous layer, the true membrana tympani, which has been described as muscular by some (as Sir E. Home), whilst its muscularity has been doubted by others.—Mr. Toynbee has been able to distinguish in the *external* layer not merely the epidemic cells, but a dermoid layer, chiefly composed of areolar tissue, and containing vessels and nerves. So the *internal* or mucous layer consists of a dermoid lamina, and of an epithelium; both of them, however, being so thin that the presence of this layer in the healthy ear can with difficulty be detected. The *middle* or fibrous layer, he states, may also be divided into two distinct laminæ, whose fibres run in contrary directions, and have no communication with each other, being separated by the interposition of fine areolar tissue. The external layer (or that which looks towards the external meatus) is the thicker and stronger of the two; and its fibres radiate from the malleus, to be attached to the dense, white ring (reputed to be cartilaginous) which is received into the osseous groove of the temporal bone appropriated to it; so that it may be appropriately termed the *radiate fibrous lamina*. On the other hand, the internal layer, which is firm and strong at the circumference, but becomes attenuated towards the centre, is formed of annular fibres; whence it may be termed the *circular fibrous lamina*. The fibres themselves are of essentially the same character in both laminæ, but are rather larger in the radiate than in the circular. They are translucent; and with the exception of a few transparent granules, present no peculiarity of structure. They are very dense, firm, and unyeilding; and are so strong that they can with difficulty be torn across. They do not, however, present the longitudinal parallel wavy lines, characteristic of ordinary fibrous membranes; and when treated with acetic acid, they are rendered opaque. This application sometimes, though not invariably, brings nuclei into view, whose long axes correspond to the course of the fibres. The circular lamina is stated by Mr. Toynbee to be distinctly continuous with the periosteum of the cavity of the tympanum; and he considers the radiate lamina to be in like manner derived from the periosteum of the external meatus. The membrane altogether he considers to be elastic, but not muscular; and he points out the existence of a tubular ligament, enclosing the tendon of the tensor tympani muscle, by which the membrane is constantly maintained in a state of moderate tension, that is capable of being further increased by the action of the muscle.—*Philosophical Transactions*, 1851.

Influence of physical Agents on the Development of the Tadpole of the Triton and the Frog. By John Higginbottom, Hon. F.R.C.E.

An opinion generally prevalent, that the tadpole of the frog, deprived of the influence of light, cannot arrive at its full development, has led Mr. Higginbottom to repeat the experiments; and he states, as the result of his researches, that the tadpole of the frog advances in growth equally well in the dark and in the light, and that absence of light has no influence in retarding its development. His experiments were conducted in different positions and degrees of temperature; but particularly in a rock-cellar, thirty feet deep, where no solar light ever entered. The tadpole of *Triton punctatus* was found to be more tenacious of life than that of *Triton cristatus;* and the experiments were commenced by placing a number of the ova, enveloped in blades of grass, in the manner usual with this animal, in three open shallow vessels containing water; one in a room, at a mean temperature of 60° F.; another in the open air, at 50° F.; and the third in a deep, dark cellar, at 48° F. Although, in the last-mentioned instance, the tadpoles escaped as early as those out of doors, they did not afterwards increase in growth; while those in the other vessels became more developed; those in the room at 60° F. having the anterior extremities in 39 days, those out of doors in 49 days, while those in the cellar had no appearance of extremities at the end of 62 days. In another experiment with the ova of the triton, under similar conditions with the last, save that the temperature of the cellar was 55° F., the tadpoles escaped in due time, but had no appearance of anterior extremities in 105 days, when they died for want of proper food. In the next set of experiments, 24 tadpoles of the triton were placed in water, in summer, in an earthen vessel, enveloped in four or five folds of black calico, so as to exclude the light, at a temperature of 65° F.; and a similar number also, in the deep, dark, rock cellar, at a temperature of 55° F.; in the former instance, several died, but, on examination, it was found that their branchiæ were absorbed, their opercula nearly closed; they had died, in fact, of asphyxia. This was obviated by placing a stone in the water, as a resting-place for the animals. All those placed in the rock cellar retained their branchiæ, till about 21 days; after this period, two or three had left the water, and were on the stones placed in the vessel *in the cellar;* proving that the development was merely retarded by the low temperature of 55° F. In a further experiment, three tritons were deprived, one of an anterior, another of a posterior extremity, and the third of a tail; and were placed in a vessel, enveloped in calico, in a dark room, at a temperature of 70° F. In a month the amputated limbs had undergone the reproductive process; in one a miniature posterior extremity, furnished with toes, had been formed, in another the tail, and, in fourteen days later, the anterior extremity and the toes of the third were reproduced.

A second series of experiments was next instituted by the author on the Frog, from the ovum to its full development. They in a measure corroborate those just detailed; the influence of temperature being more marked than that of light. The frog is more manageable in regard to food, and arrives at its full development in much less time, than the triton. The tadpole, the author states, exerts three modes of respiration, viz. branchial, pulmonary, and cutaneous. About a fortnight before the metamorphosis of the tadpole into the frog, its body is very large, and the cutaneous surface for respiration very considerable; but when the development takes place, the body of the frog is again small, and the pulmonary takes the place of the cutaneous respiration. The food of the tadpole consists of the gelatine of the ova, and the plants growing in the water in which they are deposited; the former (according to Brande an intermediate substance between albumen and gelatine) is the first food of the animal, and, when deprived of it, several of the tadpoles died. Grass, afterwards, is sufficient to produce the full development of the tadpole, which feeds on the chlorophyll adhering to the cells of the plant; the latter also probably yielding its influence, as a living vegetable in the water. In those vessels placed in the dark, it was necessary to add grass more frequently.

The tadpoles did not feed so well at a low temperature. The author concludes by stating that, with regard to the full development of the tadpole of the frog in the absence of light, he is enabled to assert, "that it advances in growth equally well in the dark and in the light." He has ascertained this, by frequent experiments during the last two years; at first, he was even led to think the absence of light was favorable to the development, but he found that the difference depended rather on increase of temperature.—*Philosophical Transactions*, 1850.

[These experiments do not set aside the well-known results of those of Dr. W. F. Edwards, as completely as they may seem to do. The latter were performed on a different plan; tadpoles, already arrived at nearly their full growth, being secluded from light, but well supplied with aerated water and food. We are assured by Dr. Edwards, that, under these circumstances, they continued to increase as tadpoles, doubling or even tripling their usual full weight, without undergoing any metamorphosis. Until Mr. Higginbottom, or some other equally trustworthy observer, shall have obtained a different result from *similar* experiments, the statements of Dr. Edwards really remain uncontradicted.]

Electro-Physiological Researches on Induced Contraction.
By Professor C. MATTEUCCI.

IN the present memoir, Matteucci departs from his established rule of never publishing facts relating to electro-physiology until he had previously endeavoured to connect them with those already discovered, and succeeded in reproducing them in such a constant manner as to remove the slightest doubt of their truth. M. du Bois Raymond, however, having announced a work "On the Law of the Muscular Current, and the Modifications which that Law undergoes by the effect of Contraction," Matteucci has transmitted to the Royal Society the continuation of his researches on "induced contraction," confining himself for the present to some fundamental experiments.

By forming piles with muscular elements of the frog, and making these elements contract, he endeavoured, but always without satisfactory result, to obtain from the galvanometer signs of electric disturbances by the act of contraction. The author has tried a great number of experiments, increasing the number of elements which contract at the same time, without succeeding in obtaining an evident and constant development of electricity by muscular contraction. He believes that there are sources of fallacy which invalidate the results of M. du Bois Raymond's experiments; for, operating on a circle of thirty and forty men, who all contracted the same arm at the same time,—the galvanoscopic frog, the nerve of which formed part of the circuit, being also extremely sensitive,—Matteucci failed in detecting the *slightest sign of a current* from voluntary contraction. He is led to believe, however, that the phenomenon of induced contraction is that which leads most directly to this result.

Professor Matteucci, however, gives several new experiments on induced contraction, which he thinks lend an increasing probability to the conclusion, that this phenomenon is due to an electric discharge; and that muscular contraction is in some way accompanied by a development of electricity. The results of these experiments may be thus summed up:—1st. The cause of induced contraction is an electrical phenomenon, which is developed in the act of contraction, and which consists in a different state of electricity in the different points of the contracted limb. 2. This electric phenomenon, like the contraction which produces it, lasts only for an instant. 3. These electric states developed by contraction, tend to produce electric currents, which circulate in opposite directions across a conducting arch interposed between the two limbs, which contract at the same time. 4. One of the experiments so arranged proved not only the existence of these currents, but their direction; leading the author to the ultimate conclusion, that whatever the theory of these phenomena may be, it is certain that they demonstrate the production of an electrical *disequilibrium* in the act of muscular contraction. This phenomenon Matteucci is of opinion may consist in a species of discharge, propagated in the direction of the ramification of the nerve, possibly analogous to that of the discharge in the electric fish.—*Philosophical Transactions*, 1850.

On the Temperature of Man within the Tropics. By Dr. JOHN DAVY.

IN continuation of some former researches on the Temperature of Man ('Phil. Trans.' 1845), Dr. Davy has communicated to the Royal Society the results of his subsequent observations on this subject, during a period of three years and a half, chiefly at Barbadoes, where the mean annual temperature of the atmosphere, he states, is 80° Fahr., and the range of temperature throughout the year from about 10° to 18° in the open air. The observations were made three times a day; the temperature of the body being noted, with that of the external air, the pulse, and the number of respirations per minute; all of which are duly set forth in elaborate tables. The chief general results are the following: 1. That the average temperature of man, within the tropics, is a little higher, nearly 1°, than in a temperate climate, such as that of England. 2. That within the tropics, as in cooler regions, the temperature of the body is almost constantly fluctuating. 3. That the order of fluctuation is different from that in a cooler climate; the minimum degree being early in the morning, after a night's rest,—and not at night. 4. That all exertion, whether of body or mind, except it be very gentle, has a heightening effect on the temperature; while passive exercise, especially carriage exercise, has a lowering tendency. 5. That heavy clothing, especially if tight and close, tends to raise the temperature unduly, especially under active exercise; and that close, ill-ventilated rooms, especially when crowded, have, in a marked manner, the same tendency. 6. That when the body is in a healthy state, it rapidly recovers its normal condition as to temperature. 7. That when labouring under disease, however slight, the temperature is abnormally elevated, its undue degree being some criterion of the intensity of the diseased action. 8. That within the tropics, there is comparatively little difference of temperature between the surface of the body and the internal parts; the skin is more active in its functions, and the kidneys are less active; the former state being connected, perhaps, with a rapid desquamation of cuticle; the latter, with an absence of lithic acid. 9. That the effect of wine, unless used in great moderation, is commonly lowering as to temperature, whilst it accelerates the heart's action; followed, after awhile, by an increase of temperature. 10. The tendency of sea-sickness, like that of disease, is to elevate the temperature. 11. The tendency of a sea-voyage, apart from sea-sickness, is to equalise the temperature without permanently elevating it. 12. That even at sea, with a change of atmospheric temperature, there is a tendency to change of temperature of the body, increasing towards the tropics. The most interesting facts, however, are the changes of temperature depending on changes of health or disease, and the lowering influence of wines and ordinary stimulants.—*Philosophical Transactions*, 1850.

On the Alteration of the Primitive Fibres of Nerves consequent on Section. By Dr. AUGUSTUS WALLER.

DR. WALLER has directed attention to certain alterations which take place in the elementary nerve fibres, after the severance of their connection with the brain and spinal cord. Referring to the researches of Burdach and Steinbruch, he states that the former found no alteration after ligature of the sciatic nerve in the frog; and the latter only some slight atrophy of the neurilema. On the other hand, Gunther and Schön, Nasse, and others, have found very considerable structural changes. After some five months, in the experiments of Nasse, the nerve-tubes below the section were broken up into granules and small clumps, and the nerve-tubes were strongly granulated, in some the small granules being united into oval bodies, surrounded by a pale cylindrical membrane, sometimes wanting, owing probably to disorganisation.

Dr. Waller's researches have been confined to the nerves of the papillæ and muscular fibre in the frog's tongue; which in their normal state were described in a previous communication to the society by the author.

The innervation of the frog's tongue is derived from two pairs of nerves;—one traversing a foramen in the posterior part of the cranium, accompanied by the pneumogastric nerve, and corresponding to the glosso-pharyngeal in man; this is distributed to the sensory papillæ;—the other, which is distributed to the muscles, constitutes the first cervical pair; and following the example of Burdach, Dr. Waller has termed this pair the hypoglossal.

It seems not a little remarkable, that division of the glosso-pharyngeal nerve in the frog, is soon followed by death. Dr. Waller suspects, that besides their gustatory powers, these nerves are connected with respiration in regulating the action of the tongue in closing the nares, for forcing air into the lungs. To avoid this difficulty, he had recourse to the alternative of dividing only the nerve of one side; which preserves the life of the animal, while we can observe the alterations on that side, as well as when both nerves are divided. In cases of doubt, he found it even of advantage to examine at the same time a fragment from each side of the tongue.

The first effects of section of the glosso-pharyngeal nerve at the throat, Dr. Waller found to be, decreased power of moving the tongue, diminished sensibility, generally slight on the divided side, and symptoms indicating disturbance of the nutritive function. The loss of sensation and of motion is very slight, the lesions of nutrition and circulation being variable; sometimes that half of the tongue is œdematous; sometimes the papillæ are injected and congested; in many instances no difference can be detected till the part is irritated, the papillæ then becoming congested. In the microscopic examination of the nerve, during the first two or three days, no alteration can be found; at the end of the third or fourth day, we detect a slightly turbid or coagulated appearance of the medulla, which no longer appears completely to fill the tubular membrane, this being apparently not affected. These alterations are best seen in a fragment to which a little distilled water has been added to render it more transparent. Twenty-four hours after death, the difference on both sides is well marked; commencing decomposition on the healthy side causes the nerve-tubes to swell considerably; caustic potash, which dissolves all the tissues except the nerves, renders the altered nerves more transparent, and the morphological changes thereby less apparent. About five or six days after section, the alteration of the nerve-tubes in the papillæ has become much more distinct, by a kind of coagulation or curdling of the medulla. Sometimes the coagulated particles have an uneven spongy appearance, as if the white substance and axis cylinder were mixed together; often they appear like separated particles of the medulla, with the double contour and central nucleus characteristic of the nervous medulla. The diameter of the altered tubes is about a fourth smaller than that of the sound ones; and in many the tubular cylinders are wanting, the medullary particles being merely held together by the neurilema. The disjointed condition of the medulla is greatest towards the extremities. A portion of nerve-tube is frequently so disorganised, as to be carried away among the tissues dissolved by the alkali; as we ascend towards the brain the disorganization appears to decrease, and at some places, the double contour is apparently unaffected: the disorganised nerve is more opaque. The muscular fibres lose their transverse striæ in some degree; the fibre itself also is usually paler, narrower, and more wavy; the capillaries are either much congested, or completely empty, and scarcely to be detected; the epithelium is unaltered. On the eighth and ninth days the curdled particles become still more disconnected, and in parts removed by absorption: the tubular sheath is ruptured. In other ramifications of the glosso-pharyngeal, the medulla becomes more and more disjointed and collected in masses. On the tenth day we perceive another morphological change, the particles assuming a granular texture; about the twentieth day, they are completely reduced to a granular state; the presence of the nervous element is merely indicated by numerous black granules, arranged like a necklace, still contained in the tubular membrane, though this is but faintly distinguishable. The resistance of these granular bodies to chemical agents, Dr. Waller has found to be most remarkable; they remain unaffected by acids, alkalis, and ethers: he has seen them apparently unaltered for upwards of five months.

The second series of experiments was performed on the *Hypoglossal Nerve.* When these nerves are divided at their exit from the spine, all movements of the throat and tongue are destroyed, and respiration is at an end; but when it is divided at the throat, these powers are not entirely lost to the tongue; for at the inner half the fibres are still contractile, on account of their belonging to the hypoglossus muscle, which here receives a branch from above the point of section: by this means, aided by the mylo-hyoid muscle, the inner half of the tongue still exhibits contractile powers. Respiration is hurried and laboured, and death the invariable result of either operation. Division of a single hypoglossal nerve, however, only causes paralysis of the corresponding half of the tongue, and the animals generally survive. The peripheric extremities of this nerve are best found at the inferior surface of the tongue, where they occur in a network of single tubules among the capillary network, without forming free ends. On the fifth day after section of the hypoglossal nerve, the tubes appear more varicose, and the medulla more irregular;—about the tenth day the medulla forms disorganised fusiform masses, and the white substance of Schwann is not to be detected; after twelve or fifteen days, many of the single tubules have ceased to be visible, the granular medulla is removed by absorption, and the branches contain masses of amorphous medulla.

In conclusion, Dr. Waller inquires what effect mere physical agents may possess in some of these changes; and he observes, that in summer, when the renewal of the tissues is most active, the alterations are more rapid; and, finally, he has been led to a conviction, that what are called "nervous diseases," are, in reality, owing to certain organic and physical changes in the tubular fibre, which it will be the province of the microscope to unravel. If a few days' inactivity of a nervous trunk, such as is produced by ligature, is sufficient to cause such disorganisation of the medulla, how can we refuse to admit its being altered in cases of prolonged paralysis?—*Phil. Transactions,* 1850.

PATHOLOGY AND PRACTICE OF MEDICINE.

On Eclampsia Nutans. By Dr. FABER.

DR. FABER relates two cases of this curious affection. The *first* occurred in a girl, æt. 3, who, though pale and weakly, had not suffered from any decided disease until three months before, when she complained of headache and sleepiness, began to squint somewhat, and sometimes to nod her head towards the left side. This nodding action was at first continued only for a few minutes three times a day, during which the head was making constant salutation-movements. After a while the attacks increased in frequency, and were fearfully violent. The child was much disposed to sleep; and became on waking convulsed in the extremities, this passing on to complete epilepsy. She was backward in mental development, and had an idiotic expression of countenance. The *second* occurred in a boy, æt. 6, who showed good capabilities for instruction up to the commencement of his sixth year, when he fell into the water. He remained in bed several days after in a drowsy state, and was never again so lively and quick. After a while, he was observed to nod his head for two or three minutes, and this several times in the course of the day, the motions being sometimes so rapid that eighty could be counted in a minute. They commenced at first slowly, like real salutations, but gradually increased in quickness, when the child would fall back in a passion. During the time they continued his face was distorted, and great fatigue was induced. He was aware when the attacks were coming on, and his consciousness continued during their prevalence. He was pale and feeble, and had acquired a peculiar, stupid look.

The reporter inquires whether this is a partial chorea or a peculiar form of spasm, and whether it is dependent upon morbid conditions of the brain or spinal marrow. The most careful examination of his two cases did not enable him to decide. No pain or tenderness about the head or spine were discoverable, and nothing abnormal in the general condition. Various applications and medicinal substances were resorted to, with but very little success—iron seeming the most useful among them.—*Schmidt's Jahrb.*, vol. lxvii, p. 213.

On Senile Cerebral Ramollissement. By MM. BOUCHUT and DELASIAUVE.

M. BOUCHUT has examined the condition of the arteries of the brain in several old persons who have died with cerebral ramollissement, and he has found changes of two descriptions; first, an incrustation of cartilaginous or calcareous matter; and, secondly, recent fibrinous clots, which last are so rarely met with in the arterial system. Such changes in the arteries have already been indicated by others, and are, indeed, found to have occurred in all cases that have been related with care. But it may be inquired, whether this connection of a diseased state of the vessels with ramollissement, is a mere coincidence or an example of cause and effect. M. Bouchut believes that the lesion of nutrition is a true mortification, a gangrene in all respects comparable to the senile gangrene of the extremities, and produced by similar changes in the vessels. The absence of gangrenous odour is due to the absence of air, and the consequent absence of decomposition. In corroboration of this view, he refers to the now frequent examples of ramollissement of the brain, which have ensued upon ligature of the carotid,—under both circumstances there being observed softening of substance, absence of smell, arrest of function, and hemiplegia.

In opposition to M. Bouchut's view of the gangrenous nature of senile ramollissement, M. Delasiauve related a case of general paralysis, occurring in a man, æt. 47, in which a truly gangrenous state of the brain, as shown by its dark colour and fetor was observed. The brain, at its upper part, was pale and wasted, and the gray substance had disappeared; but a large portion of the base of the cerebrum and cerebellum was occupied by a dark mass exactly resembling that seen in senile gangrene, and separated from the less diseased tissues by a line of demarcation. The gangrenous putrilage gave out the peculiar odour, and the softening extended some millimetres into the white substance. The pituitary gland was completely softened, and the bones of the *sella turcica* were slightly eroded. In the great number of cases of ramollissement that M. Delasiauve has met with, he never saw anything like this before; and he only knows of another case, that related by M. Parchappe, in the first volume of his '*Traité d'Alienation Mentale*,' in which also the patient died of general paralysis. M. Parchappe, however, informs him that he has since met with two others. It was suggested to M. Delasiauve, that from the eroded state of the *sella turcica*, a perforation might have admitted air by the nares, and thus induced the gangrenous odour; but without denying the possibility of such an occurrence, an inspection of the parts renders it highly improbable.—*Revue Medico-Chir.*, ix, p. 98; *Archives Générales*, xxv, p. 323.

Case in which the Liver contained Air.

M. PIORRY recently met with a very singular appearance in the body of a man, æt. 21, who died at La Charité of very confluent variola, the colon, after death, presenting numerous ulcerations, which only affected the mucous membrane. The circumstance that attracted attention was the fact of the liver, on percussion, transmitting a sound, as if it contained air. Neither it nor any other part of the body had undergone putrefaction. On cutting into the liver, it was found to have lost its granular appearance. On pressure, it crepitated like lung, and swam in water;

if squeezed, bubbles escaped from it, and it then sank. In other respects it appeared healthy. The *vena cava inferior* also contained air, but the *vena portæ* was not examined. There were no signs of emphysema, but some gas was observed near the pancreas and kidney. In consequence of the numerous intestinal ulcerations, which, from the hæmorrhages that occurred during life, may be supposed to have opened some vessels, gases might possibly have penetrated into the ramuscules of the vena portæ, and thus arrived at the liver. However this may be, had the organ been percussed during life, it must have been mistaken for lung; and when cut into after death, a superficial examination might easily have led to the same error.—*Gaz. des Hôp.*, 1851, No. 24.

A Case of Obliteration of the Pylorus with Preternatural Formation of a Cavity communicating with the Stomach and second portion of the Duodenum. Reported by Drs. DESTEFANIS and DEBERNARDI.

THE subject of this case died at the age of 43, having for many years suffered from severe dyspepsia, in the intervals of the attacks of which he abandoned himself to irregular habits of eating and drinking. Among his later symptoms, a sense of excessive weight after swallowing food, from which he could only obtain relief, though at the expense of exciting great pain, by procuring vomiting, may be mentioned. At last all articles of food were returned undigested, and attacks of hæmatemesis sometimes accompanied their rejection. He died exhausted, worn away to a complete skeleton.

At the autopsy the mucous membrane of the pylorus was found indurated and hypertrophied, the orifice being quite closed by a tough fibro-mucous substance, completely preventing all communication with the duodenum. The posterior wall of the stomach, at about three inches beyond the pylorus, was perforated, the edges of the ulcer being turgid and flaccid. The aperture was about the size of a crown piece, placed in the midst of a whitish, softened mucous membrane, in which were observed some very small sanguineous lines and points. This aperture led to an irregular cavity behind the stomach, extending towards the larger curvature, and exhibiting a capacity capable of containing almost a *litre*, its boundaries being formed anteriorly by the stomach, posteriorly by a portion of the pancreas and the pillars of the diaphragm, superiorly by the liver and gastric-hepatic omentum, and inferiorly by the transverse colon. Its parietes were moist, smooth, and lined by a strong newly-formed membrane, and especially strong posteriorly, being thinner as it approached the stomach, where it seemed to become confounded with the peritoneal coat of that organ. This cavity, as well as the stomach, contained a semipultaceous mass, evidently resulting from ingested food, and presenting no bad odour. It opened into the descending portion of the duodenum by an irregular aperture, of the size of the little finger, opposite the choledocus and pancreatic ducts; the mucous membrane and the intestine there and elsewhere were quite normal. The portion of the duodenum between the aperture and the pylorus was much contracted. The liver was small, colourless, and soft, and the gall-bladder was full of a white gelatiniform fluid. The vessels and tissues of the body were nearly bloodless.—*Bulletino delle Sc. Méd.*, vol. xvii, p. 380.

On Anasarca in Disease of the Heart. By M. CHOMEL.

THE progress of infiltration is ordinarily slow and progressive in affections of the heart; but, nevertheless, nothing is more common than to meet with individuals among the working-classes, who, while presenting the appearance of health, and without having manifested any sign of disease, are seized with anasarca, the physical and material signs of cardiac alteration not being present, or only, at all events, to a very slight degree. This is because there are causes prevailing in this class of society,—such as excess of labour, fatigue, watchings, misery, drinking,—which, in a measure, precipitate the course of the disease. These causes come in addition to the natural influence of the disease; and the anasarca appears at a period when without these it would not have manifested itself. So, when these causes are removed, and the patient is kept at rest, and sheltered from the unfortunate conditions that have given rise to so serious a complication, the œdema diminishes daily, and the patient soon leaves the hospital believing himself cured. New exposure to excesses, fatigue, or misery, reproduce the anasarca, which may be again dispersed, and that for several times; but after a certain number of such attacks, it in the end becomes permanent.

Frequently the appearance of an acute anasarca throws a ray of light on obscure and embarrassing cases, indicating in the great majority of cases an acute disease of the heart. Doubtful endocarditis and pericarditis are often thus revealed to the observer by general œdema. M. Chomel thus considers that in the case of anasarca coming on, when we can discover neither change in the blood nor albumen in the urine, we are authorized in admitting the existence of disease of the heart or large vessels, even when all material signs of this affection are completely absent.—*L'Union Médicale*, 1851, No. 26.

On the Composition of Nasal Mucus in Catarrh. By Dr. DONDERS.

IT is well known that, at the commencement of a catarrh, the mucus is a thin, clear, watery fluid, the mucous membrane as well as the skin below the nares being reddened by it. This, according to the analyses made by Dr. Donders, is due to the large quantity of sal-ammoniac which it contains, besides a smaller proportion of chloride of sodium. As the fluid becomes more mucous in the progress of the catarrh, its alkaline reaction becomes also much diminished. A similar reddening effect is produced by tears, though these are far less alkaline, deriving this property from the chloride of sodium.—*Schmidt's Jahrbuch*, vol. lxviii, p. 283.

On the Perforation of the Intestinal Canal by Worms. By Dr. BUCHNER.

THE perforation of the intestinal canal by lumbricoids has often been denied. In No. 1 of the 'Med. Zeitung,' for 1850, however, Dr. Köppe relates a case in which such perforation seems to have resulted; and in the present note Dr. Buchner relates another. A strongly made woman, somewhat advanced in years, died in from eight to ten hours after the appearance of symptoms of acute peritonitis. On examination, the intestinal contents were found effused, and a large *ascaris lumbricoides* was lying in the immediate vicinity of the duodenum. In this gut there was observed an exactly circular hole, having sharply cut borders, and presenting an appearance unlike any ever seen in the intestinal canal by the author. It was accompanied neither internally nor externally by any signs of inflammation or other diseased appearance. No other cause of perforation could be discovered; and within the duodenum other lumbricoids were found.—*Med. Zeitung*, 1850, No. 28.

On Dropsy after Scarlatina. By Dr. BEHREND.

DR. BEHREND describes two forms of dropsy after scarlatina, which are etiologically different. The first depends upon an inflamed state of the kidney, (*hydrops nephriticus*,) and the other is produced as a consequence of anæmia (*h. anæmicus*.) The first of these may occur at an early or late period after the appearance of the exanthem; and the following is its order of manifestation as to frequency,—œdema, ascites, hydrocardium, hydrothorax, and hydrocephalus. The œdema pulmonum and cerebri Dr. Behrend regards as part of the general œdema of the skin. The more considerable the œdema, the less is the effusion into the serous membranes, and *vice versâ*,—ascites being, however, an exception to this. The congestion of the kidney, giving rise to the *hydrops nephriticus*, is as specific an effect of the scarlatina poison, as is the congestion of the skin and eruption itself. It induces exudation and hæmorrhage from rupture of the capillaries, both effusions mingling with the urine, which becomes of a dark chocolate colour. By means of the exudation, the small vessels and tubuli of the cortical substance are in part obliterated and in part compressed, a granular appearance resulting under the microscope; the capillaries are observed in part empty, and in part filled with exudation globules, without any red blood, and hence assuming a whitish-straw colour. A yet stronger pressure of blood and urine produces within the tubuli a separation and regeneration of epithelium, analogous to the eruption and desquamation of the surface; and finally albumen is separated with the urine. The impeded activity of the cortical substance of the kidney, which so much modifies the function of the tubuli, prevents the complete passage of urea from the blood, and the retention of it in this fluid combines with the scarlatina poison in inducing the narcotic-like symptoms, as coma, delirium, &c. It is to this deterioration of the blood by the urea, and the effort of the scarlatina poison to become eliminated by the cutis, that dropsical effusions are due; the effusions into the serous membranes being due to the former cause alone. The greater the activity of the organism, the less is the danger of effusions; for it shows that the blood has, as yet, undergone no considerable deterioration. So long as the urine, besides its albuminous contents, exhibits traces of blood-extravasation, separated epithelium, and an acid reaction, the danger is less than if it were clear, phosphatic, and highly albuminous, because these last indicate a very considerable deterioration. In the blood-coloured, albuminous, and acid urine, treatment must be directed to diminish congestion and secure the elimination of the urea, for which purposes an antiphlogistic and saline diuretic treatment is indicated; but when the urine, from its large proportion of albumen, is of a whitish-yellow colour, and ceases to exhibit the acid reaction, antiphlogistics must be laid aside, and we must act against the deterioration of the blood, and endeavour to augment its plasticity—iron and tonics being now necessary means. This treatment is also required, when in an anæmic child that has suffered from scarlatina, after a very slight eruption, œdema has rapidly established itself. Even if the urine contains little or no albumen, yet so defective is the blood in plasticity, that death may follow from this cause if means be not taken to avert it.—*Canstatt's Jahrb.*, 1850, p. 200.

Case of Abscess of the Cerebellum. By Dr. CHAMBERLAINE.

THIS patient, a married man, æt. 40, was attacked, two years after a severe fall upon the back of the head, by a "fit," at the end of February, which was followed by incomplete paralysis of the upper and lower extremities. Some improvement followed the use of galvanism and strychnine, but he died in May. Numerous abscesses were found on each side of the cerebellum, their size varying from that of a pea to that of a marble,—the central portion of the organ containing none, nor suffering any compression from the others.

This case is confirmatory of the opinion held by Dr. Carpenter, that the lobes of the cerebellum are especially concerned in the regulation and co-ordination of the movements, while the vermiform process is connected with the sexual functions. The most prominent symptom was an entire inability to regulate the movements, as, *e. g.*, in attempting to carry a pill to the mouth. "On another occasion, he attempted to give me his hand; he raised his arm, and advanced it towards me, but the fingers remained straight and rigid, and would not grasp mine; after several attempts, he remarked, 'They won't come down;' and finally, he endeavoured to seize and bend them himself with the other hand. In addition to this, his motions were always slow and difficult, always slower than his will, and required his attention to be fixed on each muscle, or set of muscles, before he could move at all." There is not any positive evidence as to the state of his sexual powers; but, from the free intercommunication the author had with him, he considers that if there had been any irregularity in this respect, he would have mentioned it.—*American Journ. of Medical Science*, vol. xxi, p. 101.

On Amaurosis in Albuminuria, and Hypercusia in Facial Paralysis. By M. Landouzy.

M. Landouzy has recently made additional communications upon these subjects to the Academie. In regard to the occurrence of *disturbance of vision in albuminous nephritis*, his first publication took place in 1849, and although some observers have since failed to detect this coincidence, several others, among whom are MM. Roux, Forget, and Cunier, have been more successful; and in this Second Memoir the author believes himself fully authorised in concluding:—1. That disturbance of vision is almost a constant symptom of albuminous nephritis, constituting a new species of amaurosis, which may be termed *albuminuric*. 2. The amaurosis is not attributable to a deterioration of strength. 3. It very frequently is the initial symptom, appearing before other pathognomic signs. 4. It appears, disappears, and returns, without exactly corresponding to the phases of the albuminuria or the œdema. 5. It leads to the belief that albuminous nephritis is the result of a change in the ganglionic system. In a still more recent communication (January 28) M. Landouzy refers to additional confirmatory observations, and states that he has now had frequent opportunities of remarking this amaurosis in albuminuria consequent upon the application of large blisters.

Excessive sensibility of the organ of hearing in facial paralysis is another of the unobserved coincidences to which M. Landouzy claims the merit of drawing attention. It is true that such was observed by M. Roux in his own case many years since, but was then considered as a mere accidental anomaly. M. Landouzy has now collected several cases in which hypercusia existed, and from a consideration of these draws the following conclusions:—1. Hypercusia of the paralysed side is an almost constant symptom in facial hemiplegia, when independent of all cerebral affection. 2. It appears at the same time as the paralysis, and disappears before this. 3. It may be attributed to paralysis of the *tensor tympani*. 4. Hypercusia may be present in some cases without facial paralysis. 5. Whether coinciding with the hemiplegia, or existing independently, it disappears spontaneously and completely, in a space of time varying from a fortnight to three months. 6. In some cases, in order to verify its existence, it is necessary to impress the ear with a loud, sudden sound, and the more so the longer the affection has continued. 7. Treatment directed especially to this symptom almost always fails to procure relief.—*Gazette Méd.*, 1850, No. 47; 1851, Nos. 5 and 7. *L'Union Médicale*, Nos. 153, 155.

Two Remarkable Cases of Abstinence. By Dr. Taylor.

The *first* of these cases occurred in a man, æt. 50, who, after commercial disasters, began to manifest some symptoms of insanity, one of which was obstinate refusal of food. For *ten* days and nights he entirely abstained from food or drink, but then by stratagem was induced to take a hearty meal. This was followed by another period of abstinence for fourteen days and nights, during which period he passed stools once and urine three times. He slept well, walked about the room occasionally, and suffered neither from fever nor exhaustion; on the fifteenth day he took a little water, and in future took a little of this as well as a little milk—not above a gill per diem, sometimes refusing it for days together. This state of things continued until his death, he having lived in a state of almost complete abstinence for about 100 days, a demulcent injection having been administered on rare occasions. Independently of the emaciation, he presented no symptom of disease. He passed urine every ten days, and a slight stool every fifteen days.

Mr. ——, the subject of the second case, æt. 26, after manifesting other symptoms of mental derangement, lived during one year, eighteen weeks, and sixteen days in an almost complete state of starvation, taking nothing during the last fifty-one days but a little water occasionally. He had no fæcal evacuations for seventy-two days prior to death, but passed small quantities of urine every three or four days.—*Amer. Journ. Med. Sc.*, N. S., vol. xxi, p. 55.

On Zona. By Professor ROMBERG.

PROFESSOR ROMBERG regards this as a neuralgia, rather than an exanthem. The amount of pain may be excessive, while that of the eruption is trifling; and the pain may be just as intense where no eruption appears. Exanthemata, too, are essentially symmetrical diseases; while this, appearing only on one side (the supposed preference for the left requiring additional evidence), is asymmetrical, like other neuralgiæ. Lesions of the cutaneous nerves, likewise, are not uncommonly accompanied by exanthematous eruptions or pemphigus; and when the neuralgia even arises from internal causes, as that of the fifth pair, the course of the nerves is often reddened. In some cases the pain may persist long after the neuralgia has disappeared, until anti-neuralgic remedies have been resorted to.

The disease is hardly ever fatal; but M. Romberg has met with one case in a child in which it proved so, from the eruption becoming gangrenous. In bad cases he recommends opening the vesicles, and applying the nitrate of silver; but in mild cases, the zinc ointment is a good application.—*Revue Médico-Chirurg.*, viii, 301.

Case of Mental Embarrassment in Orthography. By Dr. FONERDEN.

UNDER the above title, Dr. Fonerden communicates a letter he has received from an individual now in his 25th year, who has found an invincible difficulty in learning to spell, notwithstanding his most strenuous efforts. At the age of seven or eight he could read very well, but he could never spell even the smallest word, except by mere chance, notwithstanding the most diligent application. On this account he was so much persecuted, that he ran away from home, but was brought back, and efforts to teach him abandoned. He went to sea, and practised hour after hour with a dictionary, but could never retain the orthography of words, although, if the incidents he read about excited his attention, he never forgot them. He is very sensible of the mortifications and impediments this defect has given rise to, and applies in this letter for any suggestions that Dr. Fonerden can furnish. The letter is full of orthographical blunders, and the writer says, that were he to write it over again from memory, a great majority of the words would be spelled differently.—*Amer. Journal of Insanity*, vol. vii, p. 73.

SURGERY.

On Bronchotomy in Œdematous Laryngeal Angina. By M. SESTIER.

THIS is a very elaborate and valuable paper, having for its object the inculcation of a more frequent recourse to operative procedures for the relief of œdematous laryngeal angina, commonly called œdema of the glottis; a term objected to by Dr. Sestier, inasmuch as the arytenoid-epiglotteal folds are much oftener the seat of the disease than the rima glottidis. The author bases his remarks upon 168 cases of the disease which he has collected, in 36 of which the operation was performed, often under very unfavorable circumstances, saving life in 13 and prolonging it in 8, although death eventually occurred. He minutely analyses the circumstances under which the disease occurred, and exhibits the amount of success to be expected from the operation, accordingly as it is applied to the different categories into which he has distributed the cases. He also compares the results obtainable from it in this and in other affections; and finds that, while the operation for œdema is less successful than when it is undertaken for erythematous laryngitis, or for the removal of foreign bodies, it is more so than when performed for croup.

From his investigations it results, that the probability of its success is very much dependent upon the prior healthy state of the larynx, and the fact of the patient not suffering from other serious disease. But he believes it should still be performed even when the œdema is consecutive to severe laryngeal disease, though it has then succeeded only in 1 out of 19 cases, during convalescence from other disease though it has failed in 4 out of 6 such cases, and when concomitant with various other diseases, though it has then failed in 15 out of 17 times. In fact, he would recommend it in all cases, save when the angina is the *ultimate phenomenon* of incurable and advanced disease, the fatal issue of which would be probably accelerated by it. In several such cases, however, the object would be only to prolong life, and this should be distinctly stated: but in others, apparently desperate, nature's resources would be by it brought advantageously into play.

As to the *epoch* at which the operation should be undertaken, this must *not* be as soon as the disease is recognised, for in 28 well-marked cases it was cured by ordinary means; and these, when energetic, have several times proved of avail even after severe suffocative paroxysms and great intervening dyspnœa have become established. On the other hand, we must *not wait* until it is absolutely certain that the patient will speedily die unless the operation be performed, as he would then often die during or soon after it. We must not be deceived by the dangerous calm which follows prolonged suffocative paroxysm, and is only the precursor of speedy death. It is difficult to fix any precise period; but it may be laid down, that if in spite of active means rapidly employed, the difficulty of respiration continues to increase, *the respiratory murmur heard by auscultation becomes more and more feeble*, and the suffocative paroxysms are on the increase, the operation is urgently indicated; and it is far more safe to operate *too soon* than *too late*, success being proportionate to the early performance.

There are four circumstances under which the performance of the operation should be hastened. 1. *The debility of the patient at the period of the invasion of the angina;* and doubtless this is the reason why it has so frequently failed, when had recourse to during convalescence from other diseases. The more enfeebled the patient prior to the invasion, the earlier should be the operation. 2. The presence of *deep-seated lesions of the larynx* prior to the invasion. As these have induced the œdema, so will they maintain it. 3. *Œdema of the interior of the larynx.* In 41 autopsies out of 107 collected by the author, partial intra-laryngeal œdema prevailed in 18, and complete in 23, the *chordæ vocales* being almost always implicated in both. In four-sevenths of 57 cases of laryngeal œdema, in which the interior of the larynx has been described, this has been present. It adds much to the danger of the case, inasmuch as it is inaccessible to direct applications, and the close texture of the cellular tissue here causes the disease to yield less readily to indirect ones. Its diagnosis is therefore important, and is derived from observing (1), that it never occurs but in patients who were already ill from some cause when the œdema appeared; (2), when found in œdematous angina in connection with inflammation of the fauces, the patients have already suffered from some other forms of disease, and especially from serous diathesis; (3), it has been found in three sevenths of the cases of œdematous angina dependent upon laryngitis, and especially when the serous diathesis was present; (4), the facility of *expiration*, as contrasted with that of *inspiration*, characteristic of ordinary œdematous laryngeal angina, was not observed in seven twelfths of the cases in which intra-laryngeal œdema was present —the obstacle to respiration being then a more fixed one; (5), in 11 out of 12 cases of laryngeal œdema in which the fauces were found infiltrated, the œdema was also intralaryngeal. 4. *Rapidly increasing œdema of the soft parts of the neck*, which renders the operation difficult or impossible.

M. Sestier recommends an operation, even if the patient seems in the *agony of death*, providing this depends upon the œdema itself, and not upon preceding irremediable disease. Cases are related of life being thus saved. Even when the patient is *apparently dead*, we must not always renounce the operation. A recovery under these circumstances occurred to M. Trousseau; and some of the patients who have died during the preparations for the operation, or just before the arrival of the surgeon, might probably have been saved. If *during the operation* the patient sink as if lifeless, the operation must be rapidly continued, and in this way two recoveries were procured.

In this disease we should always carefully watch the patient, *under the expectation of having to operate.* 1. The course of the disease is frequently excessively rapid. In more than half of 65 cases in which no operation was performed, death occurred at various periods within 24 hours. 2. Certain forms are especially remarkable for their rapid course, as those dependent on inflammation of the fauces, on anasarca after scarlatina, on cachectic diathesis; as also when consecutive to a wound of the neck, with infiltration of blood into the cellular tissue external to the larynx. On the other hand, its progress is slower when it is dependent on deep-seated laryngeal lesion. 3. When the disease assumes the continuous form, it is much more rapid in its course than is the paroxysmal. 4. Sometimes while the patient's condition seems ameliorated, he yet dies suddenly amidst paroxysmal suffering. A remarkable calm after severe paroxysm is sometimes a precursor of death. 5. The nocturnal aggravation of the disease is indubitable, and calls for watching.

Among the varieties of *operative procedure,* the author prefers *crico-tracheotomy,* by which the difficulty often arising from an œdematous state of the neck is avoided, and the penetration of air into the veins (this accident occurred twice in the 36 operations, although no mention is made of it in 333 cases of bronchotomy for other affections,) and of blood into the air-passages rendered less likely. It is far easier than tracheotomy, and fitter for the inexpert called in on emergency. He gives minute directions for the performance of the operation; but as these do not apply especially to this disease, we need do no more than refer to them.—*Archives Générales,* tom. xxiii, pp. 385, 420; tom. xxiv, pp. 35, 297, and 441. *Bulletin de l'Acad.,* tom. xvi, p. 117.

[We regret that the great length of M. Sestier's Essay prevents our reproducing some of the statistical data upon which his conclusions are formed. Although, as in all similar inquiries, the absence of records of many facts that have occurred prevents these from assuming the character of absoluteness, enough is assured to justify having a more frequent recourse to the operation in a disease so dangerous, and so often unamenable to both local and general measures. The paper also contains much incidental information of value, which we have not space to notice.]

On the Use of Collodion in Ingrowing Nail. By M. Meynier.

M. Meynier treats this affection by pressing down the fleshy portion, and pouring in between this and the edge of the nail a small quantity of collodion, which soon solidifies, induces rapid cicatrisation of the ulceration, and, when the disease does not arise from an abnormal shape of the nail, procures a cure. M. H. Larrey has recently tried the plan in five cases, and succeeded in four of these.—*Bull. de Thérap.,* tom. xl, p. 186.

On the Ligature of the left Subclavian, with Post-mortem Appearances. By J. Mason Warren, M.D.

In the Periscope of Vol. III, p. 539, will be found a short account of this operation, as performed for subclavian aneurism by Dr. Warren. The artery pursued an abnormal course obliquely across the neck, parallel to the edge of the trapezius, and in company with the cervical plexus of axillary nerves. The ligature was applied between the scaleni, and did not separate until the ninety-sixth day. The patient died of typhoid fever twelve months after the operation. The tumour at the site of the aneurism had disappeared. The subclavian, from its origin to the internal edge of the scalenus retained its normal size, but suddenly terminated there, and became converted into a flat cord of little else than condensed cellular tissue, about one inch and a half in length. The aneurismal sac had contracted to about double the size of the normal vessel, the artery regaining its natural size where embraced by the two heads of the median nerve. The supra-scapular, derived from the transverse cervical, and much enlarged, had, by traversing the external surface of the sac, given rise at one time, by its pulsations, to the fear of a return of aneurismal pulsation. The vessels of the thyroid axis were double their normal size. The internal mammary was enlarged, and given off by the thyroid; and it was through this, by inosculations of the intercostals with the thoracic, and of the posterior scapular with the subscapular, that the collateral circulation had apparently been accomplished.—*Amer. Jour. Med. Sc.*, vol. xxi, p. 53.

On the Reduction of Old Dislocations. By M. MALGAIGNE.

M. MALGAIGNE recently alluded to a case of dislocation of the humerus of four months' standing, which he had reduced with the pulleys; but the reality of which reduction M. Lenoir had doubted, from the fact of finding the head of the humerus projecting somewhat more forward than on the sound side, and considerably more than after the reduction of a recent dislocation. The woman, however, when visited five months after the reduction, was found to have the perfect use of her arm. M. Malgaigne observed, that after the reduction of old dislocations, especially of the humerus, but sometimes also of the hip and elbow, the region does not usually reassume its normal form,—the head of the humerus, *e. g.*, sometimes seeming farther removed from the acromion, and sometimes projecting forwards; while in subacromial dislocation, reduced after a long period, a slight posterior projection remains. If the dislocation has not been very old, these projections may disappear afterwards; but sometimes they persist, without, however, the reduction having been incomplete, or the limb being prevented from recovering its motions.—*Rév. Méd.-Chir.*, viii, 314.

On Ligature in Erections of the Penis. By M. GISTACH.

FOUNDING his recommendation upon ninety cases, M. Gistach directs the individual, in any case in which nocturnal erection from any cause is apprehended, on retiring to rest, to draw the prepuce over the glans, and to fasten it in front of this by a ligature applied only with sufficient firmness to prevent its retraction. He declares the method to be infallible. Diurnal erections that have commenced may be at once arrested, if the attempt to draw the prepuce over the glans be made soon enough to avoid compressing this part, which would only increase the suffering.—*Bull. de Thérap.*, t. xxxix, p. 475.

On Median Lithotomy. By Professor RIZZOLI.

THE perinæal urethra being well projected-out by a very convex sound, the operator commences his incision of the superficial coverings a few lines behind the base of the scrotum, and carries it to the margin of the anus. Passing the nail of his left thumb or finger under the bulb of the urethra in order to protect it, he feels for the grove of the sound, and penetrates the anterior part of the membranous portion of the urethra with his lithotome; the ligature or torsion being applied to any of the arterial branches proceeding towards the bulb, which, owing to their abnormal development, may bleed too freely. Having implanted the point of his bistoury within the origin of the membranous portion, in order to prevent any injury to the rectum, the surgeon now takes the sound from the assistant, and raising the handle to a right angle with the pubes, enables the instrument to slide under the pubes, rendering the membranous portion prominent, and its division easier, without injury to the rectum. The incision should be carried far enough to scarify the edge of the prostate, as urinary effusions into the cellular tissue of the anterior walls of the pelvis are much more likely to occur when it is limited to the membranous portion, and the dilatation of the part by the passage of large calculi is then more difficult. The incision completed, the operator passes his index finger into the wound, with the palmar surface upwards, guiding it along the groove of the sound into the bladder, and making it serve as a conductor for the passage of the forceps. If the surgeon discover for the first time, during the operation, that he has to do with a very large calculus, it is better to break it prior to removal, for which purposes Professor Rizzoli has contrived an instrument.

He believes the advantage attendant upon this mode of operation to consist in avoiding wounding the bladder, rectum, bulb, vasa deferentia, the great perinæal arteries, or the prostatic venous plexus. He has as yet operated in this manner only upon eight patients, but in all with success. In one of his cases there had been originally two calculi in the bladder, one of which was discharged by an aperture produced by Nature in the perinæum.—*Bulletino delle Sc. Med.* vol. xvii, 271.

On Effusion of Blood into the Interior of the Eye. By M. TAVIGNOT.

IF in these cases the eye is in a normal state, in all respects save the hypæmia, the blood will be most readily absorbed under the most simple treatment; but if the organ is the seat of severe pain, congestion, or inflammation, it may persist indefinitely until these conditions are removed. The affection may be treated of according to the nature of its cause.

1. *Essential hypæmia.* This is of rare occurrence, and consists in an effusion of blood into the eye, in the ordinary circumstances of health, and without appreciable cause, not being symptomatic of a general disorder of the economy, as scorbutus, or dependent on deviation of menstrual action. A case occurred at the Necker, in 1843, in a woman, æt. 65, presenting no sign whatever of disease, and who was unable to assign any cause, notwithstanding the most careful interrogation. She had complained for two days, rather of uneasiness in the eye than of pain, and no signs of contusion were present. The organ was in a completely normal condition, except that the lower third of the anterior chamber was occupied with blood. Mere purgatives were given, and in twelve days the blood had all disappeared.

2. *Traumatic hypæmia* is of more frequent occurrence than all the other varieties put together, and may be induced either by a cutting instrument or contusion. In itself the effusion is of no consequence, the degree of injury the different tissues have undergone being the really important point. The harmlessness of almost all lesions of the iris, except puncture, is remarkable; and thus sections, lacerations, and contusions of the part may occur without phlegmasia being induced. A case in point is related. A quarryman was struck while blasting a rock. The eyelids were ecchymosed, but both the sclerotica and cornea were in a normal condition. The anterior chamber being full of blood, the patient could not distinguish objects, but complained of a sense of distension rather than of pain. Being young and strong, he was bled and purged, and in five days no blood remained. The iris was now observed to be separated from the ciliary body on the inner side, the upper side, and the external and somewhat inferior side, so that three artificial pupils resulted from this singular lesion, the natural one still being preserved.

3. *Inflammatory hypæmia.* Von Ammon has described the escape of blood from the inflamed iris, and its accumulation in the anterior chamber, and Lawrence says the inflammation need not be very severe to produce it. It is of importance to remember that this may be one of the initial symptoms of iritis. The author relates an interesting case occurring in a lady, æt. 50, in whom, twenty-six days after a successful operation for artificial pupil, he broke up a soft lenticular cataract, 30th July, leaving the fragments *in situ*. She went on very well until the 12th August, when, after errors of diet, the iris became vascular, and a bloody dew exuded from its surface, a small collection of blood being also observed in the lower part of the chamber. On the 14th and 15th pus became mingled with this, and by the 16th predominated; but prompt salivation soon induced absorption of the deposit. M. Tavignot has met with three analogous cases.

4. *Hypæmia from spontaneous vascular rupture.* Two cases are related. The first

occurred in a patient suffering from a deformity of pupil produced by adhesion of the iris to a cicatrix of the cornea, and belladonna having been dropped into the eye for a considerable period, the pupil became rounded while the anterior chamber was found to contain blood, which had proceeded from rupture of vessels during the forced dilatation of the pupil, by means of the belladonna. It was soon reabsorbed. In the other case the effusion was consequent upon the sudden destruction of an adhesion which had taken place between the iris and an opaque capsule.

5. *Hypæmia from ulceration.* But one example of this has occurred to the author. A man, æt. 40, suffered from severe pain and obstinate vomiting, after depression of cataracts, 19th of June. On the left side the lens in part remounted, nearly obliterating the pupil, and causing severe pain. By the 20th of August the pain had become violent, and a very large collection of blood took place in the anterior chamber, but, in a day or two, was in part resorbed. The rest remaining stationary, an opening was made 14th September, and it was discharged in a fluid state. An erosion or ulceration was afterwards perceived on the inner part of the lesser circle of the iris, induced, apparently, by the pressure of the reascended lens.

6. *Hypæmia from organic lesion* is a secondary accident, consequent upon various affections of the globe.—*Gaz. des Hôp.*, 1850, Nos. 81 and 84.

On the Employment of Collodion in the Production of Artificial Ectropium. By M. Cunier.

The obstinacy with which adhesions are almost invariably reproduced after the division of the tissues, constituting *symblepharon*, is known to all surgeons; and numerous have been the operative procedures contrived to prevent this. To all these, M. Cunier prefers the employment of collodion. After dividing or dissecting away the adhesions, the eyelid is maintained everted, by connecting it with bandalettes of linen soaked in collodion to the frontal region, or the cheek, as the case may be. In three cases in which the plan has been tried, it has quite succeeded. In two only, the extroversion was maintained permanently for nine days, then for some hours during the day, and all night for three weeks, and then occasionally. In the other, a much shorter space of time was required. The bands require readjusting every morning; and the eye is protected by allowing a compress to hang down before it. In the same way, the *conjunctival bridles*, which are so common in Belgium, as a result of the incautious use of caustic, may be treated; and the management of *ankyloblepharon* is thus also simplified and rendered more certain. Dr. Cunier has also employed this mode of eversion in the management of voluminous granulations and vegetations—especially those of the upper lid; and the result of six months' observation convinces him that cauterization by the nitrate of silver, and the application of the acetate of lead, exerts a more rapid and complete effect, in proportion to the time the eyelid is thus maintained continuously everted. In this way many old cases have been unexpectedly benefited.—*Annales d'Oculistique*, t. xxiv, pp. 186-94.

On Fracture of the Thyroid Cartilage. By M. Eichmann.

In this paper M. Eichmann relates the pathological appearances found in one case, and the particulars of the recovery which took place in another. In the first, the child died with the symptoms of suffocation from *œdema glottidis*, laryngotomy having been refused. A double fracture was found,—one producing a detachment of the arytenoid cartilage from the upper edge of the cricoid, and the other penetrating the thyroid at the point of insertion of the thyro-arytenoid ligaments. Extensive œdema, consisting of sero-purulent infiltration, had very rapidly formed. The cause of the fracture is not stated.

The second case occurred in a girl, æt. 9, who fell upon the sharp edge of an iron chest. Among other injuries, the reporter found a complete fracture of the thyroid, which divided the cartilage along its middle on one side, and extended somewhat over the other. There was much displacement inwards, and so much bleeding, that an arterial branch, apparently the superior thyroid, was tied. After a while, convulsions came on, together with a violent cough, which expelled frothy blood from the mouth. Some leeches and an anodyne emulsion were ordered; but the cough recurred every few minutes, accompanied by a frightful *sifflement* and *râle*. Respiration now became difficult, the eyes projected, and the veins of the neck were distended from the increasing impediment to the return of the blood. As life was obviously in danger, laryngotomy, by means of an aperture in the crico-thyroid ligament, was resorted to, and gave rise to great relief. As, however, the space between the cartilages was unusually small in this child, a section of two lines of the anterior arched portion of the thyroid was made, and a bent polypus-forceps being passed in the dislocated portion, was carefully elevated. The artificial opening was purposely kept open for a fortnight, and air only entirely ceased to issue hence at a considerably later period. The cartilage was quite healed in six weeks, the voice being then unaffected, and the respiration only slightly embarrassed.—*Med. Zeit.*, 1850, No. 29.

On secondary Syphilitic Sores of the Penis. By Dr. GAMBERINI.

DR. GAMBERINI communicates notes of several cases, in which he believes that syphilitic sores appeared on the glans or prepuce, as a secondary, not a primary symptom. One of the chief distinctive characters is, that while a primary sore commences as a pustule, a constitutional one commences with a circumscribed, deep, almost purple redness, which is soon followed by abrasion of the epithelium, and ulceration, as in syphilitic sores of the throat. Compared with the primary sore, the secondary one is of very short duration; and it does not give rise to a venereal bubo. Thus, when we see a man with a sore on his penis, we are not at once to conclude that he is the subject of a new infection, but should inquire into the history of its appearance.

He takes this opportunity of stating, that of 100 cases of constitutional syphilis observed promiscuously in the Venereal Hospital at Bologna, some of the symptoms indicated by Ricord as *tertiary*, were developed in 52, without any *secondary* ones having intervened between them and the primary. In the other 48, too, the secondary were in several cases so rapidly followed by tertiary, that the two might be considered contemporary. He believes that mercury is the most important medicine, not only for secondary but tertiary symptoms, since three fourths of the patients are cured by it, and the other fourth, who recovered under iodide of potassium, have, for the most part, formerly derived advantage from the mercury, and now apply on account of relapse of the tertiary symptoms.

Other points, in which he differs from M. Ricord, are constituted by his belief in the possibility of the occurrence of secondary symptoms after gonorrhœa, independently of any urethral ulceration, and of the occasional production of primary venereal bubo, unpreceded by sores on the penis,—the "*bubo d'emblée*" of the French. Of both these circumstances he relates some cases in this paper.—*Bulletino delle Sc. Méd.* vol. xvi, pp. 351—381.

MIDWIFERY, &c.

On the Round Ligaments of the Uterus. By M. RAU.

IN this paper M. Rau refers, at great length, to the various opinions that have prevailed respecting the structure, functions, and diseases of the round ligaments; but we have only space to refer to his own views. In regard to their *structure*, he considers that, for two thirds of their course, they are composed of a continuation of the muscular substance of the uterus, over the anterior and posterior surface of which they are expanded fan-like, reaching to its fundus, and encompassing it on either side. The muscular fibres are chiefly in connection with the most superficial layers of the uterine structure, but likewise do extend to the middle ones. He admits the correctness of Rosenberger's description of the course of the three bundles of muscular fibres that form part of the ligament, while it is in the inguinal canal; but he objects to the statement that they quit this with the termination of the ligament. The muscular fasciculi take an exactly contrary direction towards the uterus. Some of the fibres proceed to the horizontal ramus and symphysis pubis, as observed by some older writers, though erroneously contradicted by Haller.

Among the various opinions which have been entertained as to their *functions*, that which has preponderated assigns to them the power of exerting a fixing or suspending power upon the uterus. Displacement and prolapse of this organ have been explained by their rupture or relaxation,—no one caring, however, to demonstrate the existence of these conditions, or to explain why, in cases of the absence of these ligaments, such change in the position of the uterus was not observed. Direct experiment has shown the great extent to which this organ may be changed in position, without exerting any corresponding effect upon the ligaments. In M. Rau's opinion, during the unimpregnated state, the ligaments exercise no function; and they only enter into activity when their muscular structure, in common with that of the uterus, undergoes so vast a development, that by the ninth month of pregnancy this has increased six fold. A consideration of their various points of insertion in the abdominal ring, the fascia femoris and the pubis on the one hand, and their powerful expansion over the uterus on the other, teaches us at a glance in what direction their power is exerted. The elevated uterus, containing the ovum, is drawn down towards the pelvis, the fundus being directed forwards and the cervix backwards, and the child's entrance into the superior aperture of the pelvis facilitated. The fundus uteri contracts itself laterally, in order to expel its contents; and the necessary consequence of the distribution of fibres is the raising and pointing the os uteri during the prevalence of a pain—the round ligaments furnishing a *punctum fixum* during the screw-like motion of the organ,—or this very motion may be due to the agency of the ligaments. The contractile action of the round ligaments is exerted even for weeks prior to labour, producing a painless action (*travail insensible*), the fruit of which, when it does not become too urgent and induce premature labour, is confined to the production of that sinking of the womb observable during the latter weeks of pregnancy.

There is very little original matter in M. Rau's observations upon the *diseases* of the round ligament. His references, too, chiefly relate to the older writers; and he does not seem to be aware that much has been published of a later date.—*Zeitschrift für Geburtskunde*, Band xxviii, pp. 290—354.

On Intermittent Fever in Children. By M. HEIMBROD.

WHILE the author cannot, on the one hand, agree with Neumann, that one half the cases of convulsion that occur in children under three years of age are due to intermittent fever, he believes, on the other, that the opinion expressed by several physicians, that young children are not liable to the disease, is the result of superficial observation. During the severe prevalence of the fever in Oberschleisen, in the year prior to the outbreak of the cholera, he met with twenty-five cases, occurring in children from five months to two years of age. In all of these there was a distinct preliminary stage, exhibited by loss of appetite, indigestion, sleeplessness, quick pulse, &c. In fifteen of the number, the conditions of shivering, heat, and sweating, were as distinctly separated from each other as in the adult.

The paroxysm commenced with pallor of the face, a remarkable blueness of the lips, and diminished temperature of the surface, the pulse becoming small, and the child apathetic. In from a quarter to half an hour restlessness came on, accompanied by redness of the surface and excessive thirst, the pulse being full and the urine high-coloured. Vomiting occurred; and in plethoric children the symptoms sometimes resembled those of meningitis. This stage continued in a greater or less degree for one or two hours, when the burning heat ceased, the child became sleepy, and the pulse soft; and a profuse sweat broke out, which continued for a variable period, during which the child usually slept. Even in infants this sweat was sometimes so profuse as to soak through all their linen. At the end the child would wake up, drink, and continue free from ailment until the next paroxysm set in.

Cases like the above are easy of diagnosis; but the greatest care is required in distinguishing those which are termed in the manuals *vagæ* or *erraticæ,* and the type of which observes no regular rhythm. Seldom as such a variety is met with in the adult, its occurrence is frequent in the child; and M. Heimbrod has observed it with exactitude in ten out of his twenty-five cases. After a longer or shorter period of uneasiness, a severe paroxysm comes on, usually in the *morning.* The cold stage, as a rule, is either absent or so slight as to be overlooked; but the hot one is more severe and of longer duration, and is followed by a slight degree of, or even by no sweating. The pulse retains its frequency for the whole day, the child remaining restless, excitable, thirsty, and sleepless. In the *evening* the careful observer will perceive signs of a remission, although these may be very slight, and may also be present at the same time on the ensuing morning, as the paroxysm occurs on the other day. No complete intermission occurs, and the case may easily be mistaken for one of *remittent* instead of *intermittent* fever,—especially as gastric complications are common. The prevalence of intermittents among adults, and the occurrence of the paroxysm in the forenoon, and of the remission in the evening, are the most important aids for enabling us to decide.

Although, contrary to what has been stated by some, the intermittent of children may, in its regular form, assume a tertian as well as a quotidian type, Dr. Heimbrod has always observed this erratic form to assume the quotidian. Notwithstanding some of his cases were very severe, he has never known convulsions to take the place of the shivering stage, or the disease to commence under the form of cholera. The *prognosis* in the regular form is usually not unfavorable; but in the complicated and erratic form it is less favorable, as the true nature of the disease may be overlooked. If no complications forbid it, quinine, which is not indeed contra-indicated by severe gastric affections, should be given, to the elder children in honey, and to sucklings in a teaspoonful of coffee, to which milk and sugar are added. It should be continued for some time after the fever has disappeared, although relapse in children is, upon the whole, rare.—*Med. Zeitung,* 1851, No. 43.

On the Cæsarean Section. By M. HOEBEKE.

WHILE the Cæsarean Section has never yet succeeded in a Parisian hospital, and while of more than 20 women operated upon in the Brussels Hospital by Seutin and Van Hoevel, not one escaped, it may seem surprising to find a Belgian practitioner maintaining, and at present without contradiction, that he has resorted to it *thirteen* times without ever losing a case. All his patients, however, inhabited the country; and M. Hoebeke attributes his success to his resorting to the operation sufficiently early, prior to any sign of phlegmasia being set up,—the usual cause of death being, in his opinion, the not resorting to it until the favorable moment has passed away. He believes it should be had recouse to, whenever one of the diameters of the pelvis measures but 2 inches, or less; and this, whether the child be living or dead, or whatever condition the woman may be in. Above 2, and below 2¾ inches, it is still the more fitting operation, and should be performed if the child is still alive; but if this is dead, it should be resorted to only when embryotomy presents too great difficulties. Still we must take care, and not prolong our efforts at delivery, so as to endanger the patient by exhaustion, or the induction of inflammation—*Rev. Méd.-Chir.*, t. ix, p. 172.

[M. Hoebeke has probably met with one of those fortunate series of cases which are familiar to many operators, but which must not be held forth as examples and rules of conduct. Still, although experience has, we think, sufficiently decided that this operation is invariably attended with fatal results, when performed in large towns, it remains to be ascertained how far the deteriorated conditions of vitality in persons inhabiting these, or the delay in resorting to the operation, form the principal element of failure. Several fatal cases have occurred, in which this last particular did not exist; and we are disposed to believe that the atmospheric influences are by far the most important, recoveries being by no means rare in the rural districts of the continent. While adverting to this subject, we cannot but express our regret at the acrimonious and almost persecuting tone, which discussions upon this and kindred topics have assumed of late in one of our learned Societies. Several points of obstetrical science and practice are, as yet, in a sufficiently unsettled state to call for further scientific investigation; but, in order that this may be effectual, candid appreciation, mutual forbearance, and the avoidance of all personalities are essential.]

On Mercurial Saturation in Young Children. By Dr. LEOPOLD.

WHEN calomel is administered in the acute hydrocephalus of young children, there may be signs of mercurial saturation present, even when there is no salivation The tongue, moist at its tip and red at its edges, exhibits gray stripes, proceeding from behind forwards to its middle part, intercommunicating behind and separating in front. The appetite, which may have improved, now falls off. The face exhibits great moroseness, and the child is impatient and ill-tempered. Mercurial fever is present; and these symptoms are of very favorable augury.—*Casper's Wochenschrift*, 1850, No. 37.

Case of Odontorrhœa Vicaria. By Dr. PISTOCCHI.

THIS patient, æt. 40, whose mother had been very liable to menorrhagia and epistaxis, was attacked with a violent metrorrhagia, which resisted every means for eight months, and was at last subdued by the continuous application of ice. Some years after, having had a tooth extracted, bleeding continued from the gum for several days, and then stopped of itself. More than a year after this, the menstrual discharge having become unusually spare in quantity for some months, and almost entirely arrested for a month, inducing somnolence and headache, a profuse hæmorrhage took place from the gum whence the tooth had been removed, to arrest which caustics and hæmostatics were in vain employed, mechanical means alone sufficing. The patient having been treated in reference to the menstrual irregularity during a fortnight, the plug was removed from the gum; but at the recurrence of the menstrual period the hæmorrhage returned as profusely as ever, and this continued to be the case at these periods for more than a year. Dr. Pistocchi having tried in vain every hæmostatic, and all means suited to regulate the menstrual function, prescribed the expressed and depurated juice of the *urtica urens*, which, combined with orange flower water and sugar, she took to the amount of three pounds *per diem*. She was rapidly cured, and at all succeeding periods was enabled thus to arrest the hæmorrhage in three or four days. At the age of 50 the menses entirely disappeared, and since that time she has frequently been obliged to have recourse to venesection.—*Bulletino delle Sc. Med.* vol. xvii, p. 281.

MATERIA MEDICA AND THERAPEUTICS.

On Antiseptics. By MM. AUGEND and ROBIN.

IN a memoir recently transmitted from Constantinople to the *Académie des Sciences*, M. Augend details a great variety of experiments he has been making upon the comparative antiseptic powers of ether and chloroform, which show those of the latter to be pre-eminently superior. To completely prevent the decomposition of a piece of fresh meat, $\frac{1}{200}$th part of chloroform sufficed. Its vapour traverses the densest tissues. It does not coagulate albumen like creosote, and is not decomposed by the muscular fibre. The most obvious action of the chloroform, upon muscular substance and the fleshy pericarp of seeds and fruits, is the production of an immediate contraction of the fibre or parenchyma, expelling the aqueous juices to the bottom of the vessel in which the experiment is conducted. M. Augend suggests, among various other applications, that this substance might be found of advantage when it was desired to preserve a corpse for a certain period for the purposes of legal medicine.

M. Robin has conducted numerous series of experiments upon the volatile compounds of carbon and hydrogen, which possess the power of preserving animal substances in spite of the presence of oxygen, as sulphuric ether, chloroform, naphtha, oil of pit-coal, &c. &c. These bodies and their vapours enjoy complete antiseptic power, the blood in fleshy substance being maintained in quite a fresh state. Water impregnated with the vapour of these hydrocarbonates possesses similar power.

Conducted by analogy of chemical composition, he has discovered another order of substances, possessed of a high degree of antiseptic power, viz., the binary compounds of carbon and some other metalloid than hydrogen. He has proved experimentally, that sulphuret of carbon, protochloruret of carbon, azoture of carbon, the Dutch fluid, and hydrocyanic acid, are, like the carburets of hydrogen, powerful preservatives of organic matters. The vapour of these substances disengaged at the ordinary temperature in closed vessels, preserves such for an indefinite period. For the preservation of the colour of bodies, chloroform, protochloruret of carbon, and oil of pit-coal, are very superior in efficacy to bodies hitherto employed; but they are far from equalling hydrocyanic acid in this respect, the vapour of which absolutely prevents all physical alterations whatever. Nevertheless, in reference to the price of its production, the power and rapidity of its preservative action, no substance presents such great advantages as oil of pit-coal, the rectified allowing much less change of colour than the impure. M. Robin believes it may be advantageously employed for embalment, in the preservation of bodies for dissection or of anatomical preparations, in the tanning of leathers, for the destruction of insects that attack collections of natural history, trees, or seeds, and for the conservation of cereals and seeds in general.—*Gaz. Méd.*, 1850, Nos. 46 and 48.

On Aconite in Lichen and Prurigo. By M. CAZENAVE.

BELIEVING that, in papular affections of the skin, the essential thing is to subdue the hyperæsthæsia, of which the eruption is but the consequence, M. Cazenave has been long in the habit of employing the extract of aconite with great success. The intense and irresistible itching rapidly diminishes, and afterwards ceases. He divides fifteen grains into forty pills, and gives one or two night and morning.—*Bullet. de Thérap.*, xxxix, p. 471.

On Local Anæsthetics. By M. ARAN.

M. ARAN has for some time been actively engaged in following up the experiments of the English and French observers upon local anæsthetics, and the following are the conclusions of his last paper read at the *Académie des Sciences.*

1. Local anæsthetic properties are found in all agents which have been recognised as possessing general anæsthetic ones, and in others of analogous composition, belonging chiefly to the series of chloro-hydro-carbons. 2. These local properties are not in direct proportion to the general ones, but rather to the fixity of the substance. The more volatile the body, the less is its local anæsthetic power, which explains the inferior degree in which sulphuric ether possesses this property as compared with other anæsthetics. 3. A great number of anæsthetics produce irritation of the skin, and chloroform is remarkable in this respect. 4. The agent

which is the most convenient to manipulate, the most certain in its action, and the least irritating to the skin, is the *chlorined hydrochloric ether* (*èther hydrochlorique chloré*) recently introduced by M. Mialhe as an advantageous substitute for the 'Dutch Liquid,' under which name two fluids of very different power are on sale. The *sesquichloride of carbon* may also be so employed, but while the complete effect of the former is obtained at the end of some minutes, the sesquichloride requires at least two hours. 5. In order to obtain satisfactory anæsthetic effects, small quantities of these two substances suffice. From 15 to 30 drops of the ether may be applied directly to the part, or upon moist linen, covering it over to prevent evaporation. Or an ointment may be composed of 4 parts to 20 of lard, or of 4 parts of the sesquichloride to 33 of lard. 6. Anæsthetic agents, and especially the chlorined ether, produce, after an interval of from two and a half to ten minutes, a complete cessation of pain, and after a time, varying from five to fifteen minutes, a cutaneous insensibility that may be easily estimated by means of a needle. 7. The insensibility is not limited to the point at which the application has been made, but extends to the deep-seated parts themselves; and in this way by applying it to the skin we relieve the pain of muscular organs, nerves, articular cavities, and the viscera contained within the thorax and abdomen. Moreover, the anæsthetic effect extends for a variable distance around the point of application, rarely less than to two square inches. 8. The duration of the insensibility varies according to the nature of the agent employed, the quantity applied, and the duration of the contact. It lasts only from one half to an hour when the anæsthesia is produced in the physiological condition of parts; but it continues much longer when the application has been made in order to produce insensibility to pain,—*analgesia*. 9. In a medical point of view, the number of cases in which local anæsthetic applications may be employed is truly immense. The following proposition is the result of a very multiplied experience. *Whenever there exists an acute pain in any part of the economy, whether such pain itself constitutes the entire malady, or only forms an integrant and principal part of it, we may, without any inconvenience resulting, relieve the patient of it for a period more or less long, by one or several local anæsthetic applications.* The employment of these agents in *rheumatic muscular pains*, and in *neuralgic pains*, has become too common to need additional illustration, but it may be observed that the latter do not become *cured* unless they are very recent. M. Aran is however especially desirous of calling attention to their great utility in *articular diseases.* In *subacute* and *chronic articular rheumatism*, they remove pain in a few minutes. They give great relief too in *subacute* and *chronic arthritis;* but here they are especially useful in favouring the application of certain surgical procedures, as *e.g.* compression, extension of contracted joints, &c. It is, however, in *acute articular rheumatism*, that he has derived really surprising benefit from these means. The ease obtained restores to the patient temporarily the use of his limbs and his sleep; and the duration of the disease becomes much abridged. This medication may be combined with venesection or any other mode of internal treatment. By the same means he has treated *lead colic, nervous, uterine* and *nephritic colics*, and even the pain attendant upon *puerperal peritonitis, pleurisy*, and *pericarditis;* and in all, when complete and definitive cessation of pain has not resulted, at least a degree of amelioration and relief that could scarcely have been hoped for has been obtained.—*L'Union Médicale*, 1850, No. 154.

Discoloration of the Ointment of Iodide of Potassium.

The yellow discoloration of this ointment, in consequence of the separation of iodine by the fatty acids of the lard, and the irritation of the skin consequent upon its employment, have been sought to be remedied by the addition of magnesia. According to Dr. Briegers, this fails to effect the object, which, however, is completely accomplished by the use of caustic subc. potass., two drops of *liq. pot. caust.* preserving four ounces of the ointment for a month, and restoring the white colour to that which has become yellow. Dr. Hoffman says, washing the lard before mixing the ointment suffices.—*Schmidt's Jahrb.*, vol. lxvii, p. 291.

On the External use of Ergot of Rye in Visceral Engorgements. By Dr. BARBIERI.

DR. BARBIERI having derived great benefit from the prolonged employment of ergot of rye, given internally, in cases of hæmoptysis and incipient phthisis, felt desirous of testing its powers as a remedial agent when applied externally. He deemed splenic and hepatic engorgements, due for the most part to miasmatic causes, as suitable examples of disease for its employment; and reports in this paper that great success has followed its use. He either employs the powdered ergot alone, in the proportion of ʒj to ℥j of simple ointment, or, when he wishes to produce more energetic cutaneous action, he adds 5 drops of croton oil, and in very serious cases 5 drops of creosote as well. He finds hysteria a frequent occurrence in a population inhabiting a paludal locality, and in such case the above ointments are very advantageously applied to the inguinal regions.—*Bulletino delle Sc. Med.*, vol. xvii, p. 345.

On the Disinfecting Power of Chlorine. By MM. WUNSCH and SCHAFFER.

M. WUNSCH states, that an alarming spread of cholera was, in 1849, checked in a remarkable manner, in two large towns, Herndorf and Priedmost, by the employment of chlorine as a disinfectant. He also gives an account of numerous experiments which he performed with it on avowedly contagious substances. He found that vaccine lymph, taken from fine vesicles, and exposed for from a quarter to half an hour to freshly-generated chlorine gas, lost its infectious power,—the same children being afterwards easily vaccinated by other lymph. Some gonorrhœal matter, after disinfection, was powerless when applied to the urethra; and, in the same manner, the matter of itch was rendered inoperative.

Repeating M. Wunsch's experiments, M. Schaffer found that the propagating power of vaccine lymph was in this way destroyed by exposure to chlorine fumes for only a minute. These were not required to be in a state of concentration; for he only employed the chlorine which escaped from some officinal *aqua oxymuriatica*, when poured out into a glass, and which was consequently diluted with air.—*Med. Zeit.*, 1850, Nos. 31 and 41.

Continuous Local Bleeding in Meningitis.

DR. MÉRIELLE, of the Asylum of St. Yon, relates two cases of acute meningitis, in which amendment only really dated from the period (notwithstanding the prior employment of active venesection) when bleeding was kept up by the application of two leeches behind each ear, replacing these by others as fast as they fell off. In one of the cases, during two nights, 160 leeches were thus used. M. Cruveilhier has also resorted to the same practice. It is obvious that the mere expensiveness of this plan must prevent its being frequently employed.—*Gaz. des Hôp.*, 1851, Nos. 1 and 19.

FORENSIC MEDICINE AND MEDICAL STATISTICS.

On the Characteristics of the Spots produced on Linen by Meconium.
By M. FRESENIUS.

M. FRESENIUS having, some time since, had to give evidence in a criminal case as to whether certain spots were produced by meconium, and not being aware of similar researches being in existence, has made public the results of the numerous experiments he made with this substance.

1. The spots are of a brownish-green colour, and, in consequence of the remarkably tenacious condition of meconium, contain so much substance as to be pretty easily separable in their dried state from the tissues with which they are in contact. They penetrate but little into these, but on the back of very fine linen a bright brownish-green colour is visible. 2. They do not furnish any remarkable odour even when moistened with water; but a sweetish one is developed on warming them

with dilute sulphuric acid. This odour is quite different from that of excrement. 3. If cold water is poured over the stained linen, the meconium becomes in part dissolved as a neutral slimy fluid, in which swim soft brownish masses. It is filtered with difficulty. The filtered liquor is of a yellow colour with more or less inclination to green. It is not absolutely clear, is not coagulable by heat, but is rendered turbid by vinegar, and does not become clear again on the addition of an excess of this. 4. If the spots are heated in strong spirit of wine (90 per cent.) this is not coloured, but, on the addition of some drops of water, a yellow or greenish-yellow colour is produced. The solution is precipitated in flocculi by the neutral acetate of lead. In the filtered liquor a further precipitate is produced by the basic acetate. 5. If a spot be moistened with water, in a watch-glass, and a concentrated solution of potass be added *guttatim*, at first no change occurs. Gradually, however, the mass becomes slimy; and, after a while, by the aid of warmth, and by crushing the at first undissolved brownish mass, all is dissolved, forming a somewhat turbid, brownish-yellow fluid, which, when cold, presents a feeble, and when warm, a distinct smell of ox-gall. 6. Ether does not become coloured by digestion with the spots, but it takes up fat, which is left colourless on the watch-glass after evaporation. 7. If to the spots soaked in water, dilute nitric acid is added, (see *Heintz Müller's Arch.*, 1846, p. 399,) the yellow fluid assumes first a plain green colour, and, on the addition of more acid, successively a dirty violet, a dirty red, and a dirty yellow. In these changes of colour, which prove the presence of *gallenbraun*, (Cholepyrrhin, according to Berzelius,) the green is the plainest. 8. Pettenkoffer's test for bile by means of sugar and sulphuric acid, in some examples, furnished distinct results, but not so in others. The beautiful purple furnished by bile is not produced by meconium, from which a brownish colour always results. The effect of the test is best exhibited by moistening the spots with a little water, then adding a very little sugar, and next dropping the hydrated acid carefully upon it in not too small a quantity, and allowing the whole to stand for an hour. 9. If a drop of the turbid fluid, produced by adding water to meconium, is brought under the microscope, besides many epithelial cells, more or less of the rhomboid fatty corpuscules of the bile, such as are figured in Valentin's 'Lehrbuch der Phys.,' vol. i, p. 373, are observed. 10. If a dry, separated meconium spot be heated in a glass tube, there is developed, without the mass becoming melted, a yellowish white vapour, having a smell resembling that of flour undergoing carbonization. Afterwards a strongly alkaline watery fluid and a brownish tar are produced—a pretty firm coal resulting. 11. Heated on platina, while exposed to the air, it leaves behind a grayish-white ash. This contains much pyrophosphate of soda, some sulphate, and a trace of chloride of soda, a little potass, much phosphate of lime and magnesia, and some phosphate of iron.—*Annalen der Chemiè*, vol. lxxv, p. 116.

On some points of Medical Jurisprudence. By Dr. Casper.

Dr. Casper concludes his account of his first "Century of Judicial Post-mortem Examinations" with some miscellaneous observations of interest.

1. *Signs of wounds during life.*—Every tyro knows that these are distinguished by the sugillation of their edges; but still there are wounds which present no such appearance, as gun-shot or penetrating wounds, which, by implicating a large vessel, cause almost instant death by fatal hæmorrhage, so that life and death are separated from each other by no dying agony. An example of this was found among the author's cases, where the arch of the aorta was penetrated by a knife, the edges of the external wound in which exactly resembled a wouud of the dead body, there being no trace of sugillation. In another case the lung was deeply penetrated, and in another the left ventricle; and in almost all cases of severe wounds of the neck, causing immediate death, the edges show no signs of reaction. In other cases, while the edges of such wounds are found white and without sugillation, the subcutaneous tissue is slightly sugillated.

2. *Absence of external signs of fatal injury.*—It is far too readily assumed, that the absence of external signs of injury is a proof that no external violence has been

the cause of death. Indeed, such external signs are as frequently absent as present. Rupture of the spleen and liver, the heart torn off from its vessels, and the fracture of five ribs, have occurred in this series without any such sign.

3. *Death from hæmorrhage.*—In most cases the *veins of the dura-mater* do not participate in the general bloodlessness which characterises this mode of death; and it is important to bear this in mind, for this very fulness of the cerebral veins and sinuses has been advanced as a proof that death did not result from hæmorrhage.

4. *The tongue in death from suffocation.*—Books inform us that in this form of death the tongue is found between the teeth or protruded in front of them; but although we frequently find it so placed in this form, yet it is by no means only in such that this is the case, while in some examples of death from suffocation it is found behind the teeth. It was found between the teeth in a case of death from wound of the heart, and in a case of poisoning by sulphuric acid. No great weight, therefore, is to be attached to this sign.

5. *Putrefaction of the uterus.*—Of all the soft parts, the uterus putrefies last. This is well known to anatomical teachers; but the subjects juridical physicians have to do with, *e. g.*, bodies found in the water, are long past anatomical uses. It is surprising how fresh, tense, and firm this organ remains, when all the other soft tissues of the body—from the brain that first, to the lung that last, putrefies—are in a state of decomposition. Thus, when the body is so far gone as to render all general examination impossible, the question of whether the woman died *pregnant*, often one of the highest juridical importance, may still be decided.

6. *Failure of detection of shot or ball in the body.*—Ball, and even shot, it would naturally be concluded, must be discoverable within the dead body, when they have had no issue during life; and yet, during the inspection, hours may, perhaps, be spent in a vain search for the missile which has caused death by rupturing the spleen or liver, and inducing fatal hæmorrhage. In a less degree the same observation applies to the thoracic viscera. In the vast number of cases that came under the author's care during the troubles of 1848, this statement was amply illustrated.

7. *Marks made by the funis on the child's neck.*—These may be easily distinguished from marks made during attempts at violent strangulation. In all such, a mark as broad as the funis, a true sugillation, is observed, either in the whole or part of its course, being round, excavated, and, above all, decidedly *soft*. In strangulation from violence, a mummification or yellowish-brown parchment-like condition of the skin is observed along a greater or less portion of the mark, seldom offering a true sugillation, and never so deep a one as in the strangulation from the funis.—*Casper's Wöchenschrift*, 1850, *No.* 35.

Question of Post-mortem Burning.—In another paper, Dr. Casper gives an account of the investigation of the cause of death in an old woman found partially burned. The assassin confessed felling her to the ground with a paving stone, but denied having employed fire in accomplishing his crime. As, however, the body exhibited signs of death by suffocation, and *phlyctenæ* with surrounding redness on various parts of its surface, Dr. Casper was enabled to declare with certainty that burning had been perpetrated prior to death. The experiments which he has performed in reference to the question entirely confirm the conclusions of Orfila, Devergie, and Christison, that *phlyctenæ* and redness are never producible by heat applied even only a few minutes after death. He observes that it is psychologically curious, that the assassin, while he at once confessed to the commission of the murder, resolutely denied, even to the time of his execution, that he had burned his victim—the denying the commission of the crime of arson being one of those peculiar "points of honour" which are so frequently met with in the annals of crime.—*Wochenschrift*, 1850, No. 29.

[The question of *post-mortem* burning has excited great attention in Germany, since the murder of the Countess of Görlitz; and there have been interesting publications upon the subject of the so-called spontaneous human combustion by Bischoff and Liebig, to which we may have occasion hereafter to advert.]

On the Comparative Liability of Males and Females to Insanity, and their comparative Curability and Mortality when Insane. By Dr. JARVIS DORCHESTER, Massachusetts.

OF 125,028 insane persons admitted into 250 European and American establishments for the insane, Dr. Jarvis finds that 64,786 were males, and 60,242 females, whereas Esquirol found among the inmates of insane hospitals 38,701 females to 37,825 males. As the latter authority remarked twenty years since, the sexual proportion varies in different countries, or even at different periods. According to Dr. Jarvis's tables, the insane males were 121 to 100 females in America, 102 to 100 in Britain, 114 to 100 in France, but only 93 to 100 in Belgium. Belgium is the only country that furnishes data for the comparison of the numbers of insane admitted into these hospitals, with those of the entire insane population. It thence results that of 2744 male lunatics, only 1338, or 48 per cent., were sent to establishments, while of the 2361 female lunatics, 1436, or 60 per cent., were so sent. In order to ascertain the relative prevalence of insanity in the sexes, it is necessary to compare the numbers admitted with those of the entire population of the same age; and from an examination of the population returns Dr. Jarvis is of opinion that the proportion of insane above twenty years of age, in Britain and Ireland, may be stated at the ratio of 100 females to 112 males, and in the United States as 100 females to 115 males. Although the general result of his investigations gives a predominance of males, this varies. "The males predominate in the asylums of England, Scotland, Ireland, and France, and among the people of Belgium. The females predominate in the asylums of Belgium, among the people of Norway and of Paris, and among the paupers of England and Wales."

Taking the united returns of 189 European and American establishments, as a means of estimating the *comparative curability of the sexes*, we find that, of 57,794 male patients, 23,677 are returned as cured, as are 23,704 of 53,946 female patients. The recoveries in the English and Irish asylums present an excess of about 9 per cent. in favour of the females, the difference not being quite so great in the Scotch, Belgian, and French asylums. In the American there is a slight preponderance in favour of males.

In respect to the *mortality*, also, the balance is in favour of the females, they being less liable to die while insane than males. Of 57,980 male patients, 12,311 died, and of 54,163 females, 8147, furnishing a ratio of 21 per cent. on the admissions in the former, and 15 in the latter.

The above conclusions are corroborated by the returns of the *comparative mortality of the sexes from all diseases of the brain and nervous system.* Dr. Jarvis finds, upon examination of the Registration Reports of England and America, that of 326,072 deaths from these diseases, 178,255 occurred in males, and only 147,817 in females. Comparing them with deaths from all causes, 16·15 per cent. of male deaths, and only 13·85 of female deaths, were due to this class of diseases. Palsy and chorea, indeed, were the only diseases coming under the class of neuroses that proved more fatal to females than males. These returns, then, combine with those from the asylums "to overthrow the common notion that woman is especially subject to nervous disorders, and that man is comparatively exempt from them; and more than this, they show that the reverse is true,—that man is more exposed to, is less frequently cured of, and falls more under the attacks of this class of diseases than woman."—*Amer. Journ. of Insanity*, vol. vii, pp. 142—170.

A Case of Feigned Insanity.

A young shepherd, named Specht, æt. seventeen, of previous good health, lively manners, and great intelligence, residing at Hegelensheim, on the Upper Danube, violated and then shot a young girl; but avowed his crime, declaring the devil had incited him to it. Next day, when brought before the magistrate, he seemed to have lost every physical and mental faculty, could not stand without great effort, and only answered in incoherent and isolated words. He pretended to be deaf and to have lost his memory, could recognise no one, and afforded no satisfactory answer to any interrogatory. MM. Windler and Zinc, the experts employed to examine into the state of his mind, were both of opinion that the condition was simulated, inasmuch as the degree of imbecility he pretended to exhibit could only be congenital, and there is no example of a person of such excellent parts, as he possessed prior to the occurrence, becoming suddenly imbecile: for imbecility that is not congenital, only comes on and increases gradually.

The prisoner was in consequence secretly and constantly watched, and as no change could be detected he was subjected to various tests. Fire-arms were discharged suddenly near him in the middle of the night; douches of water were let fall upon him; the building which he was in was set fire to, &c. &c. All in vain; his imperturbability remained unchanged, and at last, after *fourteen months' surveillance*, he was brought to trial, the experts still maintaining his sanity. He was carried into court completely lost in utter stupidity and apathy; and the result was, that in consequence of the impression his appearance made upon the jury, he was only condemned to three years' imprisonment.

He was carried back in the same state of helpless lethargy; but he had hardly entered the cell, when he began leaping for joy at having escaped capital punishment. He stated that he had been perfectly well the whole time, and that the part he had played had been suggested by a fellow-prisoner. Had he feigned a little longer, he would doubtless have received a free pardon.—*Annales Med. Psych.*, N.S., vol. ii., p. 661.

INTELLIGENCE.

HOSPITAL FOR SICK CHILDREN.

Our readers will, we are sure, be glad to learn that a most favorable commencement has been made, in the establishment of this much-needed Institution. The Committee has been fortunate enough to meet with a house in a very central situation (Great Ormond Street), which affords almost every accommodation that could be desired, and which is furnished with that most indispensable requisite, a large garden. The house is extremely spacious, and will at once afford adequate accommodation for sixty children; while the rooms are so arranged as to admit of the due classification of the patients, and one of the apartments, being 47 feet in length, will serve as an excellent day-room for convalescents. There is ample space for additions to the house, to enable it to accommodate 100 patients, without trenching on the garden. In short, the premises furnish almost every convenience that could be obtained in a building erected expressly for the purpose.

The only drawback now experienced is the want of adequate funds; and we can scarcely think that these can fail to be supplied, if the peculiar claims of this Hospital to public support be made generally known. A feeling is rapidly extending, we know, amongst many of the most enlightened members of our Profession, against the extension of the present system of Medical Charities; and this feeling we would not wish to check. But it is not so much the benefit of the individual patients who may be received within its walls, that *we* would urge as the motive for establishing and supporting such an Institution. It is the opening a field for the Clinical Observation of Children's Diseases—in regard to the knowledge of which, British practitioners, for want of opportunities of systematic study, are lamentably behind their Continental brethren,—that we regard as the most important of its advantages, and this alike to the Profession and to the Public.

We trust, therefore, that such of our readers as feel able to assist this important undertaking by pecuniary contributions, will not be backward in doing so; but still more would we urge upon them to advocate the cause with the public, and to prevail upon them to give such effectual assistance, as shall enable the objects of the Institution to be efficiently carried out. And let it be remembered, that although the immediate benefits of the Hospital, as regards the patients received into it, must necessarily be confined to the Metropolis and its neighbourhood, yet indirectly its advantages will be felt, not merely by the Profession, but also by the Public, throughout these Kingdoms.

We trust that the Medical Officers will do their utmost, by the publication of Reports of their practice, to substantiate the claims we are advancing; for the continued support of the Profession will depend, we feel assured, upon the hold which the Institution thus acquires upon its estimation.

BOOKS RECEIVED FOR REVIEW.

On Neuralgic, Rheumatic, and other Painful Affections: with Notices of Improved Modes of Treatment. By James Arnott, M.D. London, 1851. 8vo, pp. 38.

An Inquiry on the Subject of Vaccination; addressed to the Royal Medical and Chirurgical Society. By Benjamin Ridge, M.D. M.R.C.S.L., &c. &c. London, 1851. 8vo, pp. 30.

On Bubo and Perinæal Abscess. By John L. Milton, M.R C.S.L. London, 1851. 8vo, pp. 32.

What is Mesmerism? An Attempt to explain its Phenomena on the admitted Principles of Physiological and Medical Science. By Alexander Wood, M.D., &c. &c. Edinburgh, 1851. 8vo, pp. 31.

The Mesmeric Mania of 1851; with a Physiological Explanation of the Phenomena produced. A Lecture. By John Hughes Bennett, M.D. F.R.S.E., Professor of the Theory of Physic in the University of Edinburgh. Edinburgh, 1851. 8vo, pp. 21.

On Syphilitic Eruptions, with especial Reference to the Use of Mercury. Illustrated by Cases. By Thomas Hunt, M.R.C.S.L., &c. London, 1851. 8vo, pp. 25.

Portraits of Diseases of the Skin. By Erasmus Wilson, F.R.S. Fasciculi VIII and IX. London, 1851. Folio.

Pathology of the Human Eye. By J. Dalrymple, F.R.S. F.R.C.S. Fascic. VII. London, 1851.

Clinical Observations on Diseases of the Genito-Urinary Organs. Part I. Gonorrhœa and its Consequences By Henry James Johnson. London, 1851. 8vo, pp. 352.

Annual Reports of the Grant Medical College, Bombay, for 1845—1849. Bombay, 1849. 8vo.

On the Threatenings of Apoplexy and Paralysis; Inorganic Epilepsy; Spinal Syncope; Hidden Seizures; the Resultant Mania, &c. By Marshall Hall, M.D. F.R.S., &c. &c. London, 1851. 8vo, pp. 90.

Principles of Physiology, General and Comparative. By William B. Carpenter, M.D. F.R.S., &c. &c. Third Edition, entirely re-written; with 321 Wood-engravings. London, 1851. 8vo, pp. 1098.

On the Mode of Origin and Propagation of the Epidemic Cholera in Huddersfield and the Neighbourhood, in the Autumn of 1849. By John Taylor, M.D. London, 1851. 12mo, pp. 36.

On the Prevention and Cure of Spinal Curvatures and Deformities of the Chest and Limbs; being the Result of many Years' Experience. By Mrs. Godfrey. London, 1851. 8vo, pp. 90.

Practical Remarks on some Exhausting Diseases. By Sir James Eyre, M.D. Second Edition. London, 1851. Post 8vo, pp. 110.

Nice et son Climat; avec des Notices sur le Littoral de la Méditerranée, de Marseilles a Gênes. Par Edwin Lee. Paris, 1851. 12mo, pp. 132.

The Teeth and their Preservation, in Infancy and Manhood, to Old Age. By Alfred Canton, M.R.C.S.L. London, 1851. Fcap. 8vo, pp. 207.

Practical Observations on the Teeth: designed for Popular Use. By Henry Jordan, Dentist. London, 1851. Fcap. 8vo, pp. 104.

Actstykker ·ngaaende Cholera, navnlig Epidemien i Christiania i 1850. Besörgede ved den kgl. Cholera-Central-Commission. Christiania, 1851. 8vo, pp. 128.

A Translation of the New London Pharmacopœia, including also the New Dublin and Edinburgh Pharmacopœias; with a full Account of the Chemical and Medicinal Properties of their Contents; forming a complete Materia Medica. By J. Birkbeck Nevins, M.D., &c. &c. London, 1851. Post 8vo, pp. 780.

Practical Remarks on the Treatment of Aneurism by Compression. By Joliffe Tufnel, M.R.I.A. F.R.C.S.I. Dublin, 1851. 8vo, pp. 154.

Observations on the Diseases of the Rectum. By T. B. Curling, F.R.S., Surgeon to the London Hospital, &c. London, 1851. 8vo, pp. 123.

The First Step in Chemistry. By Robert Galloway, F.C.S. London, 1851. 12mo, pp. 91.

On Diseases of the Mucous Membrane of the Throat, and their Treatment by Medication. By William R. Wagstaff, M.A. M.D. London, 1851. 12mo, pp. 182.

Lectures on the German Mineral Waters, and on their Rational Employment for the Cure of certain Chronic Diseases. By Sigismund Sutro, M.D. London, 1851. Fcap. 8vo, pp. 432.

The Wisdom and Beneficence of the Almighty, as displayed in the Sense of Vision. The Actonian Prize-Treatise for 1851. By T. Wharton Jones, F.R.S. London, 1851. Fcap. 8vo, pp. 135.

A Compendium of Materia Medica and Pharmacy, adapted to the London Pharmacopœia, embodying the New French, American, and Indian Medicines; and also comprising a Summary of Practical Toxicology, with the Abbreviations used in Prescriptions. By J. Hunter Lane, M.D. F.L.S., &c. &c. Second Edition. London, 1851. 12mo, pp. 310.

The Laws of Health, in Relation to Mind and Body: a Series of Letters from an Old Practitioner to a Patient. By Lionel John Beale, M.R.C.S. London, 1851. 12mo, pp. 306.

THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

OCTOBER, 1851.

PART FIRST.

Analytical and Critical Reviews.

Art. I.

1. *Practical Treatise on the Diseases of the Lungs and Heart; including the Principles of Physical Diagnosis.* By Walter Hayle Walshe, M.D., Professor of the Principles and Practice of Medicine in University College, London, Physician to University College Hospital, Consulting Physician to the Hospital for Consumption.—*London*, 1851. 8vo, pp. 574.

We were happy to see the announcement of this book, as we felt it was one much needed. The want of a special work on Diseases of the Chest has been acknowledged by every one; some of the best Treatises are out of print, and their authors exhibit no indication of an intention to supply new editions; others are unfinished; and those which are complete, and are still procurable, are not the best of their class. The well-known reputation of the Professor of University College seemed to warrant us in assuming, that a work on a class of diseases to which he has paid special attention, would be a boon to the practitioners of this country, and an honour to our national literature. In this belief we have not been disappointed; Dr. Walshe's work is all we expected; and we can earnestly recommend it, as giving a most accurate account, not only of diseases of the Lungs and Heart, but also of the method of investigation by which these diseases are principally detected.

The work is divided into Two Parts: the First is appropriated to a description of the physical methods of examining the organs of the Chest; and the Second details the diseases of these organs. In the First Part, therefore, the subjects of Auscultation, Percussion, Inspection, Mensuration, &c., are fully considered; and in the Second, these subjects receive their practical application.

We shall not follow Dr. Walshe, *seriatim*, through the first division of his work; but shall select for comment various original points, and one or two subjects which, from their difficulty or importance, demand especial notice.

The study of the *physical signs* of diseases, though as old as Hippocrates, has, within a few years only, attained its present importance and consideration. In fact, before the time of Avenbrugger and Laennec, these most valuable methods of investigation afforded only a few rude and imperfect indications. It may be said, indeed, that by far the most precise and characteristic signs of some diseases, as, for example, those of the lungs and heart, are derived from modes of observation which have not been in use for more than fifty years, have been only generally employed for about twenty, and are even now to be considered as in an early and rudimentary condition. To learn these methods is the most difficult task of the student's clinical career; and touch, sight, and hearing, must each be trained to seize the several modifications of physical conditions which are consequent on anatomical changes. There is, however, some difficulty in learning these methods; and it is to be feared that many students, even when they have been formally licensed to treat the diseases of their fellow creatures, are very imperfectly acquainted with the arts of percussion, auscultation, mensuration, or others which are employed to elicit what are now commonly called "physical signs." Yet we are well convinced that no more valuable knowledge can be acquired within the walls of an hospital, than that which may enable the student to recognise, rapidly and correctly, this class of symptoms.

"Interpreted by the observer and not by the patient (writes Dr. Walshe), incapable of being feigned or dissembled—estimable in degree and extent, often with mathematical precision—susceptible of indefinite refinement—physical signs, like the whole class of objective phenomena of disease, are of materially greater clinical value than its subjective symptoms. Physical signs are, then, the true indices of the nature, extent, and degree of organic textural changes, and may be regarded as instruments of pursuing morbid anatomy on the living body. But just as their significance is sure and precise, so is the difficulty of mastering their theory and practice positive and great; and hence it is that Physical Diagnosis has gradually acquired for itself the importance of a special art." (p. 2.)

After an introductory chapter on "Clinical Topography," Dr. Walshe discusses, in as many sections, the *Physical Examination of the Lungs*, under the following heads, viz.:—I, Inspection; II, Application of the Hand; III, Mensuration; IV, Percussion; V, Auscultation; VI, Succussion. On these subjects the author's views are so well known to the profession, through the medium of his excellent work on 'Physical Diagnosis,' that it is unnecessary to do more than allude to some of the most striking and novel points. Under the head of *Inspection* are some very useful remarks on the alterations of the *expansion* and *elevation*-movements of the ribs, and on the varying conditions of the thoracic and abdominal movements. We observe that Dr. Walshe mentions, that from clinical observation, Dr. Sibson's statements, that the five upper ribs *converge* during inspiration, cannot be proved. On the contrary, he states that when a finger is placed in any upper intercostal space, it is compressed during expiration, and relieved of all pressure during inspiration. But, after death, if the integuments are removed and artificial respiration practised, the five upper ribs *can* be seen to converge, although Dr. Walshe states (Appendix), that the amount does not exceed one-sixteenth or one-twelfth of an inch at the outer edge of the costal cartilages. In some diseases alterations occur, as in tubercle of the apex with pleuritic agglutination, when the elevation-movement may still be perceptible in the infra-clavicular region, but the convergence of the ribs is notably increased.

In speaking of circular mensuration, Dr. Walshe recommends "a very simple plan, suggested by Dr. Hare, that of joining two tapes at the commencement of their scales, and fixing them, as the patient reclines, at their line of union to the spine; each side of the chest has thus its separate measure." (p. 27.) Dr. Sibson's ingenious "chest measurer" is spoken of in terms of commendation, for ascertaining the changes in the antero-posterior diameter of the chest or abdomen. Dr. Hutchinson's spirometer is useful, also, for ascertaining the expansibility of the lungs in a different manner; and the indications it gives may be advantageously checked by the signs afforded by circular measurement, or by the chest-measurer. In practice, however, the use of the spirometer is limited; and we are happy to find Dr. Walshe expressing opinions on this point which agree with some we had previously formed. After stating that, although there are numerous exceptions, the general law still holds good, that the mean volume of air capable of being expelled, at the fullest expiration, by an individual five feet in height, amounts to 174 cubic inches, and that it increases by eight cubic inches for every inch of stature, from five to six feet, Dr. Walshe continues:

"A general standard of health being admitted to exist, the question arises, what amount of deficiency below that standard actually indicates disease. It is said that a deficiency of 16 per 100 is suspicious, but may possibly arise from physiological peculiarity; that beyond this the deficiency is morbid. But in practice it turns out that the general standard of height is often valueless—that the individual healthy standard occasionally varies widely on either side of the general one. So much so, that a great fall may have taken place, from disease, in the breathing volume of an individual, at a time when he expels a quantity of air above the average standard of men of his height; according to the general standard he is more than healthy, he is extra-capacious; according to his own, he is diseased. For certainty of observation, the individual standard is required; the present man must be compared with the past man, and not with other men. Again, a fall below the general average is a surer indication of disease, than the maintenance of the general average, or even a slight excess, is of health.

"But of what disease? Obviously of any disease, whether situated in the lungs and appendages, the heart and great vessels, the abdomen, the encephalon or cord, which interferes, on vital or mechanical principles, with the expansion or retraction of the lungs. The spirometer indicates imperfect expansibility of the lungs, but neither marks the seat nor the nature of its cause, unless observation should prove (and of this there seems no present probability), that special scales of reduction of capacity obtain in particular diseases. In this point of view (as well as, of course, in indicating which side of the chest is solely or most diseased), spirometry appears to me inferior to semicircular mensuration of the chest." (p. 37.)

An excellent account of the method of percussing, and of the signs afforded by the percussion of the chest in health, follows. We shall remark only, that Dr. Walshe notes the fact, previously observed by himself and others, that the right infra-clavicular region sometimes sounds duller than the left, without thickening of the pectoral muscles, or other obvious cause; and that in some cases, the sound on percussion at any part of the thorax may be clearer or duller than the corresponding point on the other side, without any appreciable anatomical difference existing between the two points. But such cases, though they sometimes occur, can lead to no mistake in practice; as the dullness is slight, and is unaccompanied by any other physical signs of disease.

In disease the statical changes in the percussion-sound are arranged under the four following heads:

"1. Diminution of clearness passing into perfect dullness—the duration of the sound being proportionably shortened, and the sense of resistance increased.

"2. Increase of clearness and of duration, with decrease of resistance.

"3. Increase of clearness and of duration, with increase of resistance.

"4. Alterations of quality." (p. 57.)

In speaking of the "sound of the cracked pot," Dr. Walshe confirms the statement of Dr. Stokes, that in cases of bronchitis in children, "a metallic resonance somewhat analogous to the cracked jar sound of cavities, but more diffused," is occasionally to be found. Skoda, as is well known, states that he has never observed this.*

The chapter on Auscultation occupies eighty-two pages, and embraces every topic of practical importance. In speaking of healthy respiration, Dr. Walshe corroborates the opinion of Louis, that in a certain number of persons, and particularly in females, the expiratory murmur is longer and louder at the apex of the right lung, both in front and behind, than at the corresponding points on the left side. We entertain no doubt whatever that this is correct, and that the inspiratory murmur in the same situations is also louder and harsher. We have also frequently observed, that in some persons the expiratory murmur is particularly loud and long over the whole chest, even at the very bases; and in such cases, if the apices of the lung only were examined by a practitioner who placed great reliance on long expiration as a sign of early tubercle, a mistake would probably be committed.

Pulmonary Auscultation in disease is considered under the following heads:

"A. 1. Modified respiratory murmurs.

"2. Adventitious sounds produced by the act of breathing.

"B. Modified vocal resonance.

"C. Resonance of the cough.

"D. Phenomena common to the respiratory murmurs, to rhonchi, to vocal and cough-resonance.

"E. The sounds of the heart and vessels as transmitted through the lungs." (p. 84.)

In speaking of sibilant rhonchus, Dr. Walshe notes the interesting fact, that even when this rhonchus is dependent on a permanent lesion, as, for example, the pressure of a tumour on the bronchus, it is not necessarily persistent, but will sometimes disappear after coughing. Dr. Walshe infers, therefore, that temporary accumulation of mucus may have more to do in the production of the murmur, than any alteration in the shape of the bronchus.

Dr. Walshe holds very decided opinions on the characters and significance of the crepitant rhonchus, herein agreeing with the views most generally held in France, and differing considerably from those maintained by Skoda and many German writers. In the first edition of his celebrated

* Abhandlung über Perkussion und Auscultation. Von Dr. Joseph Skoda. Vierter Auflage. Wien, 1850, p. 21.

work, Skoda entered at great length into this subject, and questioned with unnecessary acerbity the doctrines of Laennec. In the fourth edition, the polemic against Laennec has disappeared; but the conclusions arrived at remain the same. These conclusions are:—that crepitation is simply a moist rhonchus, arising from the same causes as the admitted moist rhonchi; that it passes into these by imperceptible gradations, and indicates, not the first stage of pneumonia, or, more rarely, œdema or pulmonary apoplexy, as Laennec supposed, but simply the existence of thin fluid in the air-cells and smallest bronchi, and is, therefore, present in many diseases. So far from crepitation being pathognomonic of pneumonia, Skoda holds, that although it may occur in the first stage, it is more usually absent. "Laennec's crepitation is not only not constant in pneumonia, but if his description is adhered to literally, it is even rarely found."* Barth and Roger, on the other hand, in the latest edition of their well-known treatise,† distinguish the crepitant rhonchus from all other signs, although they admit the difficulty of always defining it from sub-crepitant rhonchus, and from some forms of pleuritic friction. The crepitant rhonchus, they say, is heard in pulmonary congestion, in œdema, and in apoplexy of the lung, as well as in the first stage of pneumonia, but in these cases its characters are not so defined as in pneumonia, and approach those of the subcrepitant and muco-crepitant rhonchi. They sum up their conclusions in these words:

"By reason of the extreme frequency of inflammation of the lung, opposed to the comparative rarity of œdema and of apoplexy, crepitant rhonchus, especially when its characters are well defined, is a sign *almost* pathognomonic of pneumonia in the period of engorgement." (p. 151.)

Dr. Walshe, after describing the characters of crepitation, its fineness, the perfect similarity to each other of its little crackles, its dryness, its persistency, its rapidity of evolution, and its *almost* exclusive manifestation during inspiration, thus concludes:

"The crepitant rhonchus, thus characterised, is absolutely distinctive of the first, or congestive stage of pneumonia, and fixes its seat with precision; there are certainly rhonchi and pseudo-rhonchi which possess some of its properties, but none that possess them all combined." (p. 102.)

The remarkable differences of such great authorities on this point will be best seen by a recapitulation.

1. According to Skoda, the crepitant rhonchus cannot be separated from the moist rhonchi, is rarely heard in pneumonia, and is often heard in other affections.

2. According to Barth and Roger, the crepitant rhonchus, although very similar to undoubted, moist rhonchi, such as the sub-crepitant, is yet *perhaps* distinguishable,‡ and is, *from the rarity of other affections in which it is heard, almost* pathognomonic of the first stage of pneumonia.

3. According to Dr. Walshe, the crepitant rhonchus has characters which are absolutely distinctive, and when heard, is *absolutely* pathognomonic of the first stage of pneumonia.

* Op. cit., p. 118.

† Traité Pratique d'Auscultation. Par M. Barth et Roger. 3me Edition. Paris, 1850, p. 144.

‡ We say "*perhaps*," for Barth and Roger do not express themselves with very great precision. In one place, one would think they considered it easily distinguishable; in another place, absolutely indistinguishable from sub-crepitation and some kinds of pleuritic friction.

That these great differences of opinion are not confined to the three authors whom we have quoted, and who may be taken as the latest representatives of the German, French, and English schools of auscultation, could be easily shown, did our space permit us to allude to the opinions of other eminent writers. It would then be seen that, with the exception of those who have servilely followed Laennec and his first pupils, almost every independent observer has held different opinions regarding the characters, the significance, and the mode of production of the crepitant rhonchus.

Confining ourselves simply to a practical view of this question, our own experience leads us strongly to concur in the statement of Skoda, that crepitant rhonchus is frequently absent in pneumonia. It is often not heard in central pneumonia, and the first physical sign may be deep-seated bronchial respiration; it is sometimes not heard in superficial pneumonia, or is covered by sub-crepitation or fine pleuritic friction. On the other hand, we have occasionally known fine sub-crepitant rhonchi, whose non-pneumonic origin might have been reasonably inferred from the absence of all other symptoms and the subsequent progress of the case, unhesitatingly called crepitation by good auscultators, and considered as sufficient proof of the presence of inflammation of the lungs. It is well known, indeed, that many of the reputed cases of pneumonia, are nothing but capillary bronchitis.

These facts, however, prove nothing as to the main question, viz., the existence of a special and distinguishable crepitation, which, when heard, is pathognomonic of pneumonia; nor can we disguise the great difficulties which attend its solution. We are inclined to believe that Dr. Walshe's view is the most correct; and it is a strong argument for the distinctiveness of this rhonchus, that so excellent and cautious an observer should have adopted opinions so decided.

With reference to the *cause* of the crepitation of pneumonia (admitting its existence and peculiarity), Dr. Walshe combats the common notion of its production by liquid effusion, and advances arguments in support of the opinion, that each crepitus or click is produced by the forcible expansion of an air-cell surrounded with recent exudation. If this be the case, it must be wrong to say that crepitation is a sign of the *congestive* stage of pneumonia; it is a sign of the stage posterior to this, viz., that of exudation and commencing hepatization. This would explain the reason why, in many intense congestions of the lung, as, for example, in some cardiac affections, there is no crepitation, nothing but (comparatively speaking) coarse rhonchi; and it would also tally with the fact, that, when crepitation has once been heard, signs of more or less complete solidification may be expected in a few hours.

Leaving the subject of crepitant rhonchus, we must observe that Dr. Walshe has heard, in a few rare cases, a sound described by Laennec, but whose existence has been often denied since, viz., the "dry crepitant rhonchus with large bubbles." Skoda also has heard this uncommon sound; and its real existence must henceforth be admitted. But, as Skoda remarks, nothing would have been lost, had it never been heard or never distinguished.

Under the head of *pseudo-rhonchi*, Dr. Walshe described certain sounds which are of great importance, as liable to lead to error if their existence is unknown:

"If individuals, whose lungs are healthy, or diseased only at the apices, and whose breathing is habitually calm, are made suddenly to respire deeply, a peculiar, fine, dry crepitation, accompanying inspiration only, may often be detected at the bases posteriorly. But after two or three, or, at most, five or six, acts of respiration, it totally disappears. This pseudo-rhonchal sound seems to depend on the sudden and forced unfolding of air-cells, which are unaffected by the calm breathing habitual to the individual, and its only importance arises from the possibility of confounding it with crepitant rhonchus. Here we have a minor degree of the phenomenon observable in the same regions, when any considerable portion of the base of the lung is under the influence of persistent pressure from tumours or enlarged abdominal organs. I mean the pulmonary, pseudo-rhonchus, which consists of a series of fine, very dry crepiti, evolved at a peculiarly slow and drawling pace, variable in number, but generally very numerous, and commencing towards the close of inspiration, or, in some cases, apparently when this movement has ceased." (p. 113.)

These pseudo-rhonchi are produced in the interior of the lung; but Dr. Walshe also points out that pseudo-rhonchi, which may be very similar to true rhonchi, as, for example, to the sub-crepitant, have also a pleural origin. Whenever adventitious tissue within the pleura is infiltrated with serosity, "squashy" and "crackling" sounds are present, which are known to be pleural principally by their superficial characters, and also because they are unaltered by coughing, and are often attended by undoubted friction and other undoubted signs of pleural disease.

Moreover, sometimes "crepitation, inspiratory and expiratory, of variable degrees of fineness, abundance, and size, audible in forced respiration only, or in calm breathing, constant or intermittent, disappearing after a few chest expansions, or continuing through a long examination, is sometimes to be discovered over the sternum, generally or partially, while it is completely wanting over the contiguous portions of lung. No symptoms of any kind necessarily attend this stage." (p. 121.)

The chapter on Bronchophony is a very interesting one, and we recommend it to the careful consideration of our readers. For the first time in England, Skoda's theory of consonance has received the attention it deserves; and the facts noticed by Skoda, that the variable resonance of the voice is not explicable by reference simply to the better or worse conducting power of the lung, and that there is often *intensification* of the voice at some part of the chest, so that the resonance is even greater there than at the larynx where the sound is produced, are fully admitted. Dr. Walshe, however, thinks that consonance alone will not account for the phenomena, and suggests that, in addition to consonance, the vibrations of the air may be reflected towards a focus.

"The conditions of reflection are fulfilled in the hepatized lung; the tubes along which the voice is transmitted from the larynx are surrounded by semi-solid material, proper (when compared with healthy tissue) to reflect and concentrate the sound; the air-cells and minute bronchi are closed to a variable distance, and present its divergence. The tubes resemble so many speaking trumpets, and just as in these instruments, the augmentation of sound is produced by reflection from their quivering walls; as this reflection tends to propagate vibrations (otherwise divergent) in the same direction, intensification of sound must be the result. But if the reflected vibrations chance to be brought to a focus within a large tube, then echo will occur, and, as under ordinary circumstances, the sound may be materially louder than the original sound." (p. 129.)

This explanation seems very probable, and will, we think, account for some cases, which even Skoda's theory of consonance did not quite explain.

We observe that Dr. Walshe thinks that Skoda "excludes the tracheal and bronchial walls from all participation in the phenomenon." We have always interpreted the theory of consonance to imply consonating vibrations of the walls of the trachea and bronchial tubes.*

The increased resonance of the voice at any point of the chest depends, then, on three conditions: the conduction of laryngeal sound, intensification of this within the chest by consonance and by reflection of vibrations to a centre, and conduction of this intensified sound to the ear. Any of these conditions may vary; and according to the degree in which they coexist, the amount of resonance of the voice will be greater or less. It is of great importance to understand clearly the causes of intensification of sound within the thorax, as these causes affect not only the voice, but all sounds which are audible within the thorax, no matter in what manner they may be produced, whether externally to the thorax, as in the case of the voice or the pharyngeal sounds, or internally, as in the case of tubular respiration or mucous rhonchus.

The application of these principles affords us a ready clue to the variable phenomena presented by cavities or by hepatised lungs, which have proved so puzzling to the followers of Laennec. On Laennec's theory of increased resonance being simply caused by improved conducting power, the astonishing differences in the amount of vocal resonance derived from different cavities, or from solidified lung, remained unexplained. Why, over one cavity, we should hear the voice transmitted completely through the stethoscope to the ear, as in Laennec's "perfect pectoriloquy;" while, over another, the resonance of the voice is hardly increased, as in the various grades of "imperfect pectoriloquy;" and why, under certain circumstances, there should be intenser resonance from a solid lung than from a cavity, was, before the publication of Skoda's first edition, quite unexplained, or referred simply to differences in the conducting power of the lung, although post-mortem examination constantly proved that such differences did not exist.

We now know that a cavity may afford all the conditions necessary for the intensification of sound, or may afford almost none of these conditions. We may have a round cavity with solid walls and a smooth lining, having the bronchial tubes leading into it blocked up, with the exception of one or two large ones by which it communicates with the outlet, and which drain off its contents directly they are formed. In such a cavity, the laryngeal sound augments itself by consonance of the tense walls, and probably, as surmised by Dr. Walshe, by reflection of all the vibrations towards the centre. If such a cavity is connected with the surface by dense lung, all the conditions for the greatest amount of resonance are present. The voice may be heard *louder* over such cavity, than over the larynx itself.

But there may be a cavity of an irregular shape, with flaccid and rough walls opening into many tubes, half filled with fluid, and surrounded by tolerably healthy lung; and although such a cavity may be many times larger than the former one, the resonance of the voice may be scarcely, if at all, increased. A large, empty bronchial tube, on the other hand, surrounded by solidified lung reaching to the surface, may give, especially if the smaller tubes opening into it be blocked up, as is so often the case in

* See pp. 38, 39, 40, et seq. of Skoda's fourth edition.

pneumonia, an amount of resonance which is greater than that of many cavities, and which passes up through the stethoscope and vibrates on the ear, as in well-marked examples of Laennec's "perfect pectoriloquy."

It follows from these facts, that the amount of vocal resonance given by a cavity may be greater or less than that given by lung condensed merely, but not excavated; consequently, Laennec's attempt to distinguish, by a special term ("pectoriloquy"), the resonance of a cavity from the resonance of condensation (bronchophony), was not only erroneous in principle, but led to great errors in practice; as it implied, 1st, that the resonance of a cavity was peculiar, and was always greater than that of solid lung, which is not the case; 2dly, that pectoriloquy was an unfailing sign of a cavity, which is also erroneous; and 3dly, that the absence of pectoriloquy implied absence of cavity, a conclusion which daily experience proves to be a mistake.

In order to avoid the use of a term which was associated with such errors, Skoda proposed to abandon the word pectoriloquy altogether, and to include all the varieties of increased vocal resonance under the terms of "strong and weak bronchophony." Dr. Walshe's opinions are nearly the same, only he proposes to retain the word "*pectoriloquous*," as an adjective, to designate the highest degree of vocal resonance or bronchophony.

We must protest against the term which MM. Barth and Rogers have lately sought to introduce instead of "pectoriloquy," viz., "cavernous voice." Although they appear so sensible of the inconvenience of the word "pectoriloquy," it appears to us that they have succeeded in substituting a term which is much more objectionable in every way. We must state, indeed, that the chapter on the Voice in the last edition of their treatise is quite unworthy of the rest of the work, which is one of great merit.

Leaving the subject of bronchophony with this notice, we may observe that on the much debated subject of *Ægophony*, Dr. Walshe adopts, in the main, the opinions of Laennec, and believes that, although "some conditions may lead to a simulation of Ægophony, the pure quality described by Laennec never exists without the interposition of fluid." (p. 139.)

The following remarks on the production of metallic tinkling will be read with interest by all who have taken a share in the controversies on this difficult subject:

"The mechanism of metallic tinkling and amphoric echo has been sought after with all the eagerness of curiosity, but observers are far from having come to a uniform conclusion on the subject. I believe, as just mentioned, that the two phenomena are one and the same fundamentally,—echoes of different properties from the walls of a large space, more or less favorably disposed for reflection and concentration of sounds, produced either within the area, at the outlet, or in the close vicinity of that space. It appears, too, that the low pitched buzzing echo only requires the presence of air in the hollow space, though water, in moderate proportional quantity, may be present therein; while the high-pitched tinkle requires fluid for its production (not that such tinkle is physically impossible unless fluid be present, but that in the chest the conditions, independent of fluid, which are capable of generating it, do not coexist). In experimental support of this statement, it may be observed, that if we blow, cough, speak, or sing into an empty glass decanter, a low-pitched buzzing amphoric echo only will be produced; metallic and ringing in quality it is true, but never of the tinkling pitch. Let a little water now be placed in the decanter, and the result will be exactly the same, so long as the fluid is not agitated. But agitation of the fluid changes the quality of the echo. Thus, let drops of water slowly and at distinct intervals fall on the surface of the fluid in the decanter, and the ear, applied to the surface of the vessel, recognises the most perfect imitation of metallic tinkling, just as in certain instances the phenomenon occurs within the chest, independently of respiration, rhonchus, voice, or cough, when a patient, with a very large cavity, or with hydro-pneumo-thorax, suddenly changes from the recumbent to the sitting or erect posture, and when, in all probability, a drop of fluid is precipitated from the roof of the cavity to the fluid on its floor. Or, again, breathe into the water by an elastic tube, and the bubbling will be found to produce a perfect tinkle. It is probable, too, that sounds generated in fluid on the close confines of a cavity, itself free from fluid, may be echoed into metallic tinkle by that cavity. Metallic tinkle seems to be the echo of a bubble, or, at least, of a sound generated in fluid.

"The morbid states in which these phenomena have been observed, are hydro-pneumo-thorax with and *without* bronchial communication, simple pneumo-thorax, and large tuberculous excavations in the lung-substance." (pp. 143-4.)

It will be observed that Dr. Walshe agrees with Skoda, and the majority of auscultators at the present day, in considering that metallic tinkling and amphoric echo may occur without any communication between the pleural cavity and the bronchi. In fact, in the majority of cases, after perforation of the lung, the opening closes.

We must, however, pass over these and many other important topics, the next section on Succussion, and a short but very useful chapter on the "Determination of the Situation of surrounding Parts and Organs," in which the various alterations in the position of the heart, the mediastinum, the diaphragm, the liver, the spleen, and the stomach, are detailed,—in order to arrive at the second grand division of this portion of the work, viz., *The Physical Examination of the Heart and great Vessels.*

As in the case of the lungs, this Part opens with the clinical topography of the heart and vessels, and the subsequent sections are arranged in the same order as in the case of the lungs, viz., under the heads of Inspection, Application of the Hand, Percussion, Auscultation, &c. All these chapters are full of the most important facts, often either novel or placed in a new light. They are so condensed, however, that it would be impossible to make an abstract of them; and we therefore proceed, as in the case of the lungs, to notice only two or three prominent points.

In speaking of *valvular thrill* (the "purring tremor" of Laennec), Dr. Walshe refers with doubt to the opinion of Skoda, that contraction of the left auriculo-ventricular orifice is attended by thrill, and states that he has never observed cardiac thrill synchronous with ventricular diastole, nor has he even seen it in cases of simple nervous palpitation of the heart.

The extent of *transmission of the heart's sounds* during health is thus laid down:

"One great mistake (writes Dr. Walshe, in speaking of the extent to which the heart's sounds are audible in health,) commonly committed by authors who attempt to define it, is not considering separately the first and second sounds. From this omission, the ordinary starting proposition, that 'the heart's sounds are heard at their maximum in the præcordial region,' becomes an error; the second sound is in truth heard in nine people out of ten, more clearly at mid-sternum, on the level of the second interspace, than at any point of the præcordial region, even limiting that region to the space in which the heart is uncovered by lung during tranquil breathing. The thickness of the soft parts, the form of the chest, and many other physical conditions, perfectly independent of disease of any of the thoracic organs, modify the extent of propagation so variously, that there can be no practical utility in laying down rules subject to perpetual exceptions. But the lines of propagation of the two sounds severally agree in most healthy persons, whatever be their absolute intensity at their seat of production; changes in these lines point positively to some modifying cause, and hence their establishment is clinically valuable. Now, the first sound passes slantingly upwards to the left acromial angle, growing weaker and weaker on the way; it loses much more on the way to and at the right acromial angle; its propagation backwards is clearest and fullest to the left, so that, while audible at the left back, it may be inaudible at the right. The second sound with the base-region for its centre, radiates to the right and left acromial angles; with greater clearness to the left than the right posteriorly, it reaches the surface at the right side less clearly than at the left." (p. 192.)

Dr. Walshe discusses briefly the causes of the *sounds of the heart*. As causes of the first sound, besides muscular action of the ventricular walls, tension of the auriculo-ventricular valves, and some occasional subsidiary forces, he includes the shock of the blood at the orifices of the great vessels, and singularly enough he has arrived at this conclusion nearly on the same clinical grounds as Skoda, viz., that a sound is often audible in the arteries, under circumstances which exclude the idea of mere conduction from the heart; and that in certain cases of mitral regurgitant disease, where the systolic sound at the left apex is completely destroyed by murmur, a healthy first sound may be joined at the aortic base. With regard to the second sound, while he attributes it principally to the closure of the sigmoid valves as usually taught, he yet thinks it possible that, as Skoda supposes, there may be also a ventricular second sound. The great argument for this is a clinical one, viz., that in some cases the second sounds are scarcely audible at the base, while they are well heard at the apex. "There are cases," says Skoda, "where the second sound is not heard at the base, or is very weak, while at the apex it is loud and clear." (p. 188.) Dr. Walshe also states that he has seen cases of aortic regurgitant disease, in which, at the base, a murmur only was heard, while at the apex a distinct second sound was audible, which was decidedly not an intensified pulmonary second sound. Consequently this second sound at the heart's point must have been derived from some other source than the play of the aortic or pulmonary valves.

This certainly would seem to settle the point, although we cannot but believe such cases to be very rare. We have been for some time constantly on the look-out for a healthy or diseased heart, in which we could unequivocally decide that the second sound at the apex was louder than at the base, and we have never been able to find an instance in which we could affirm that the apex sound *could not be* one simply transmitted from the sigmoid valves.* But positive observations overrule negative ones, and Skoda and Walshe are men not likely to be mistaken.

Admitting, then, that a second sound may have a ventricular origin sometimes, is such an origin normal and constant, or is it abnormal and unusual?—that is, is the second sound over the ventricles always in part ventricular, as Skoda believes, or is it ordinarily a transmitted sound from the sigmoid valves, whose place, during disease, may be taken by a ventricular murmur simulating it? Dr. Walshe, without expressing any

* We put aside for the moment any arguments derived from reduplication of the second sound at the apex,—we are referring to a single sound only.

decided opinion, seems to think that there may be always a diastolic sound produced in health by the recedence of the heart's point from the side of the thorax; and that, also, during aortic regurgitation, the sudden re-entrance of the blood through the patent valve into the ventricle may generate sound, which, of course, in this case, would be diastolic, and might imitate the second sound. We observe that Weber, in his very useful little work,* seemed disposed to admit that the separation from each other of the flaps of the auriculo-ventricular valves may give rise to sound, just as their union also produces it. In either case, he presumes that there must be friction of valves on each other, and consequently more or less sound in proportion to its amount.

Leaving, however, the subject of the natural sounds of the heart, and passing to the consideration of the alterations of these sounds during disease, we find that Dr. Walshe divides these into two series:

1st. Modified sounds.

2dly. Adventitious sounds or murmurs.

The account of the modifications produced in the heart's sound without murmur, is the best we have seen, and we are sorry that we are not able to transfer it entire to our columns. We can extract only the summary of the reduplications of the sounds, as it refers to what we have just said on the second sound.

The phenomena of reduplication of the sounds, most commonly of the second, sometimes of the first, sometimes of both first and second at the same time, have long been known to auscultators; but they have never yet been systematically considered, as far as we know, except in the work before us. They are, however, of little practical value, but their real interest, as Dr. Walshe remarks,—

"Arises out of their bearing on the theory of the heart's sounds. Thus, the second sound may be continuously doubled at the base, and perfectly pure and single at the apex. How is this explicable on the sigmoid theory of the double sound? A double sound does not become single by conduction over so short a space.

"The first sound may be single at the left apex and at the base, while it is distinctly reduplicate at the right apex. Here the ventricular and arterial portions of the first sound seem to be separated on the right side of the heart.

"The second sound may be double at the base, and single at the aorta, double at the pulmonary cartilage (or *vice versâ*). This cannot arise from want of synchronism of the two sets of valves, but of the three divisions of one set.

"The second sound may be single at the base, and double at the left apex; now, according to the pure sigmoid theory, the arterial valves are the sole source of the second sound: how come the two sets to divide their compound sound at the apex? Splitting into two, as a result of conduction from the base to the apex by ventricles of different conducting powers, cannot be admitted; for the reduplication may be present at the left apex, absent at the right. This is the strongest fact I know of in favour of the second sound being, in some cases, partly of ventricular origin." (pp. 211-12.)

We cannot pretend to explain altogether these remarkable facts, but something may be said to qualify the result to which they lead. In the first case, in the majority of our cases of reduplication of the second sound at the base, the reduplication has been audible at the apex, though much more faintly. In other cases, in passing down from the base, the double second

* Ueber Perkussion und Auskultation. Kieu, 1849.

sound has gradually become fainter and fainter, until one of the sounds has ceased altogether, the other (apparently transmitted to a greater extent) has been carried down quite to the apex. In this case, the single sound at the apex has been referable to exhaustion of one of the two sounds which were produced at the base. Whether this explanation will always apply, we do not know.

In the second case mentioned by Dr. Walshe, the first sound is reduplicate; the second is not necessarily altered.

In the third case, a new and interesting form of reduplication is pointed out, which does not, however, bear on the ventricular origin of the second sound.

In the fourth case, which Dr. Walshe considers so strong an argument for the occasional ventricular origin of the second sound, we are not informed whether the observations were made on diseased or on healthy hearts. We have no doubt whatever, that in some cases of mitral and of pericardial disease, a sound is produced during the diastole, which is audible only at the left apex, and which resembles a second sound. If this is not exactly synchronous with the normal second sound transmitted from the base, it gives rise at the left apex, but there only, to what is termed a reduplication of the second sound, but which, in reality, is a morbid sound conjoined with the normal one. If, however, Dr. Walshe's observations refer to healthy hearts, then this explanation fails, and his conclusion, it appears to us, must be the only correct one. The whole subject is well worthy of more attention than it has hitherto received.

The next section, on *Adventitious Sounds and Murmurs,* is one of the best in the book. Our space will scarcely permit us to do more than extract the following enumeration of the most common valvular lesions:

"In respect of relative frequency, I should be disposed to place intra-cardiac murmurs of *organic* origin in the following order, commencing with the most common:—mitral regurgitant; aortic constrictive; aortic regurgitant; mitral constrictive; tricuspid regurgitant; pulmonary constrictive; pulmonary regurgitant; tricuspid constrictive.

"These murmurs may be variously associated; the following combinations are those I have observed most frequently:—aortic, constrictive, and mitral regurgitant; aortic, constrictive, and regurgitant; mitral regurgitant and aortic regurgitant; mitral regurgitant; aortic, constrictive, and regurgitant; mitral, regurgitant, and obstructive; mitral regurgitant, and tricuspid regurgitant; mitral, regurgitant, and constrictive; aortic constrictive, and aortic regurgitant." (p. 228.)

The various organic endocardial murmurs are very closely described, and Dr. Walshe's conclusions are, in the main, those of other observers. The doubt which Canstatt raised in the last work published before his death, as to the relationship of mitral regurgitant disease and systolic murmur at the left apex, appears to be unfounded. The additional sign given by Skoda, in cases both of mitral contraction and regurgitation, viz., accentuation of the pulmonary second sound from augmented recoil of the column of the blood, is not, according to Dr. Walshe, of much value, and, in some cases, an apparent intensification of this sound is caused simply by diminution in the aortic second sound.

The next chapter is on the method of examining the great arteries, by *Inspection, Application of the Hand, Percussion,* and *Auscultation.*

By Inspection, we discover pulsation of arteries, which may depend "on general or local excitement of the circulation, on special disease of the heart, or on disease of the vessel itself." (p. 238.) "Highly marked and extensive visible pulsation," (being, of course, permanent,) has never been observed by Dr. Walshe without coincident aortic regurgitation. Nor has Dr. Walshe ever seen well-marked aortic regurgitation without such pulsation. If, however, arteries are calcified and tortuous, there may be a certain amount of movement, particularly if the left ventricle be hypertrophied, but the cause of this is easily detected. Arterial thrill, also, may accompany such pulsation, if the artery be dilated, and especially if it be calcified ; a spanæmic condition of the blood also tends to make this more considerable. Spanæmia, without organic disease, may produce thrill, which, then, is generally more diffused. We have observed, also, that contraction of an artery, particularly if the lining membrane is rough, will give rise to great thrill. We have seen the thyroid apparently in this condition; at least, although there was no examination after death, the artery was small, intensely thrilling, and there was extensive old valvular disease of the heart, with the signs of dilatation and roughness of the aorta, over which thrill was also perceptible.

The organic and inorganic arterial murmurs are well described, but we do not find anything which need detain us. We pass on, then, to the subject of *venous murmurs.*

Murmurs in the veins are said to possess one invariable character, that of continuousness. But, although thus continuous, they are not equable, but, as Dr. Walshe remarks, are sometimes "modulated," and consist "of a series of separate tones, capable of musical notation, recurring at tolerably regular intervals, and accompanied by a low hum, which gives the continuous character to the whole." (p. 249.) They are liable, also, to intensification, and to changes in quality at each moment from various causes. One cause of this intensification is acceleration of the current during inspiration in the large veins bordering the thorax; and this is most marked, as pointed out first, we believe, by Hamernjk, in the case of the right internal jugular vein. The following are Dr. Walshe's remarks on the causes of venous murmur.

"Venous murmurs are so intimately connected clinically with a spanæmic state of the blood, that they constitute its most positive sign; why that state of the blood should engender them, is a mystery. Physically the vessels are imperfectly filled, loose, and vibratile; the blood is thin, and the friction attending its movement (according to a law of Poisseuille's), thereby proportionably increased—one element of sonorousness. So, too, external pressure, or muscular action, intensifies the sound by similarly affecting the friction of the current; though, if the condition of the blood be highly favorable, no pressure is required, especially when the arrangement of the vessels is such (*e. g.* in the torcular Herophili) as to promote forcible collision of currents arriving from different directions at a conflux. M. Andral has attempted to establish the exact relationship between the amount of spanæmic change and the constancy of venous murmur, as follows: if the red corpuscles fall below 80 per 1000, murmur is constant; if they range between 80 and 100, pretty frequent; if between 100 and 115, occasional; if between 115 and 126, murmur is sometimes heard, never if they reach the average of health.

"There are some facts difficult to reconcile with the ordinary notions in this matter. Thus it is well known, that in cases of chlorosis treated with iron, colour returns to the tissues long before venous murmur disappears. On the other hand, Becquerel gave analyses of the blood of two chlorotic girls presenting well-marked venous hum, with a mean proportion of 125·1 per 1000 of red corpuscles; certainly an amount falling within the limits of health. It is affirmed, too, by the London Heart Committee, that murmur may be produced in the veins by pressure in a state of robust health; and I have heard it in women of florid complexion, who, as far as I could ascertain, had never been *symptomatically* anæmic. It is averred by Skoda that hydræmic blood has been drawn from persons perfectly free from venous murmur; it was probably not carefully sought for. There is no proof that mere diminution of the mass of the blood will produce venous hum; such diminution, indeed, never takes place without change in composition. Plethora, especially of that kind in which the proportion of the red disks is raised, is an asserted cause of venous murmur. In all probability the proportion of white corpuscles may have more to do with the murmur than has been suspected. They are increased in many cases of chlorosis, and (as shown by Remak) augment by the repetition of bleeding; now their increase must entail great increase of friction and labour in the circulation." (p. 252.)

With this quotation we must take our leave of this portion of the work, which, though only a sort of introduction to the Second Part, is the best treatise on the art of Physical Diagnosis that has yet appeared in the English language.

The second division of the work is devoted to the special diseases of the Lungs, Heart, and Appendages. The description of each disease is clearly and forcibly written, and the treatment recommended seems highly practical and useful.

The diseases of the Lung are first discussed; neuralgia of the lung, pleurodynia, intercostal neuralgia, receive brief but sufficient notice, and then Bronchitis is considered at length. The following remarks on the diagnosis of dilatation of the bronchial tubes, will be read with interest:

"The addition of dilatation of the tubes to chronic inflammation of their mucous membrane, seriously increases the gravity of the latter disease. Here it is that the expectoration is most abundant, most opaque and solid, most thoroughly purulent, and most wasting to the system. The aeration of the blood is rendered difficult by the altered structure of the mucous membrane; hence lividity of the face commonly exists to a marked amount. The long continuance of the disease tends to produce enlargement and thickening of the right ventricle, but I have never observed hæmoptysis, unless there was coexistent mitral disease, or pulmonary tubercle. Some degree of night-sweating occasionally occurs, and the weight of the individual may fall very considerably below the standard of health.

"This symptomatic state, far from dissimilar to that of phthisis, may coexist with physical signs so like those of excavation, that it is next to impossible sometimes to affirm with certainty whether a given case be one of tubercle with cavity, or of globularly dilated bronchus with surrounding induration. The distinction of the two kinds of cavity may, however, in the majority of cases, be established through the following points. In phthisis, percussion is dull *above* the clavicle; not so in dilated bronchus; below the clavicle, too, the dullness is greater and more extensive in the former than in the latter case. The signs of tuberculous excavation are found at the apex; those of dilated bronchus, generally lower,—say at the union of the upper with the middle third of the chest. When tubercle has reached the excavation stage, flattening of the surface is habitually more marked under the clavicle than it ever is in bronchial dilatation. I have never known hæmoptysis produced by chronic bronchitis, with dilatation above; if hæmoptysis, and there be no evidence of mitral disease, the inference that the excavation is tuberculous, becomes matter of necessity. Extreme emaciation does not, as far as I have seen, come of the bronchial disease alone. The course of the physical signs will avail us also, if the case continue for a time under observation. In phthisis, the signs are, as a rule, constantly increasing in degree and extent; in bronchial dilatation, they may remain unaltered for months in both these respects; dullness under percussion, as remarked by Dr. Stokes, precedes the signs of cavity in phthisis, and does not occur till after them in bronchial dilatation; to the latter clause, however, I have seen exceptions." (pp. 267-8.)

The effect of bronchitis in producing falling-in, and collapse of tissue, beyond the inflamed part, by obstruction or obliteration of bronchial tubes, as pointed out by Carswell and Stokes, and as developed lately in the ingenious observations of Dr. Gairdner, is alluded to, but is not discussed at length. We should have been glad to have known Dr. Walshe's opinions upon this and allied points, as it has appeared to us that this collapse of lung-substance beyond the portion of tube obstructed, is a matter of great interest, and is connected, not only with bronchitis, but with almost all the diseases of the lung.

After bronchitis and its varieties and effects, such as narrowing and obliteration, or dilatation, of the bronchial tubes, Pleurisy is considered. We turned with much interest to the section on treatment, to observe Dr. Walshe's opinions on the subject of paracentesis thoracis, and we gather that the following are his views:—In acute pleurisy with effusion, paracentesis is inadmissible, unless the fluid accumulates with such rapidity as to threaten asphyxia, as in some rare cases. In empyema, however, the operation is recommended, after the employment, in the first place, of the usual remedies. The exact period when it should be employed, is thus laid down by Dr. Walshe:

"The determination of the period of the disease most favorable for operating, is a point of very serious importance. Experience proves, as might have been anticipated, that when performed at an advanced period, paracentesis is rarely successful; the local changes which have then had time to arise in the pleura, pernicious as these are, are even less subversive of success than the deep constitutional distress entailed by the disease. And yet to this period, puncture of the chest is often, I might almost say commonly, postponed; it can hardly be matter of surprise that, in some such cases, it has appeared to do little more than hasten death. There can be no question that the fitting time for operation has come, when a tendency, insuperable by medical means, exists, either to increase or to non-absorption of the fluid. The practical difficulty is to determine the precise period at which such tendency may be considered to be developed." (p. 305.)

Agreeing in the main with these sensible remarks, we yet doubt whether the operation is not regarded too much as an unwelcome expedient after a course of unsuccessful treatment. Yet, if paracentesis can be generally performed without risk, as stated by some of its late advocates, there seems some reason to believe that it will come to be employed as an auxiliary to our ordinary methods, and not as a sequence to them. Every one knows that, in a great number of cases, pleuritic effusion is not only of long continuance and of difficult absorption, but that it produces considerable, and sometimes permanent, organic mischief, both in the lung it compresses, and in the sound lung whose respiratory play is doubled. If the fluid can be drawn off without danger, not only will the mercurials and iodides, which are now so often uselessly employed, have a much greater chance of reducing the inflammation, and of causing the absorption of fibrinous effusion, but there will be much greater probability of the lung regaining its pristine expansion, even should it be again compressed by a subsequent temporary exudation of fluid. The operation, unnecessary in many cases of pleurisy, in which speedy absorption may be looked for with tolerable confidence, and hurtful in very old cases of empyema, when the constitution is originally diseased from tubercular or from renal disease, or has been secondarily and profoundly injured by the long continuance of the empyema itself, appears to be particularly adapted for cases in which no great increase of effusion occurs, but no power of absorption appears to exist, and in which the constitution is still tolerably sound. Now it is in these cases, as far as we have seen, that the practitioner goes on from day to day with counter-irritation, mercurials, and such like means; and has only recourse to paracentesis when other appliances fail. Or, possibly, in these cases, after a lingering illness, absorption commences, and a cure is effected, at least such a cure as can be hoped for with a compressed and half-obliterated lung. How much better would it be, if paracentesis is a safe operation, to employ it at a much earlier period, and to trust to active treatment to prevent subsequent effusion.

The whole argument turns on the facility and safety with which paracentesis can be performed; and although the cases are not sufficiently numerous to allow us to recommend it as in all cases practicable and useful, they yet warrant us in stating that this operation is one of which practitioners have too great a dread, and that when *skilfully performed,* it may be practised with very little hazard to the patient, and with a result, in the majority of cases, that is satisfactory to the practitioner. We cannot pretend to give a decided opinion on a point of practice so difficult, so important, and so little known; but we should not be surprised to find, in a few years, tapping of the pleura much more frequently resorted to than at present. This subject is, however, of so much consequence, that we shall leave it for the present, in the hope of returning specially to it on another occasion.

In speaking of the varieties of Pleurisy, Dr. Walshe mentions the "pulsatile empyema," and a modification, in which pus finds its way out of the thorax, and forms a subcutaneous swelling, witnessed by himself in two cases, in which, without perforation of the costal pleura, there was pulsation simulating aneurism at the inner part of one infra-clavicular region, close to the sternum.

After pleurisy, Pneumonia is described with an equal amount of care and minuteness. In speaking of the dyspnœa of pneumonia, the following observations are made on an important sign, which has been chiefly illustrated by Dr. Walshe himself, viz., the perversion which arises between the number of respirations and the beat of the heart per minute.

"When the respiration reaches 70 or 80, suffocation seems threatened, and speech is obstructed; 30 or 40 respirations per minute may exist without the patient being conscious of particular dyspnœa. Their frequency does not depend on any particular seat of the pneumonia, nor even, singularly enough, on its extent,—at least necessarily. I have known double pneumonia attended with a less number of respirations than inflammation of a limited portion of one lung. Although marked acceleration of breathing is an unfavorable sign, it is not, even to the highest degree, of fatal augury; recovery may take place when the respiration has reached 80 per minute. Now the circulation does not increase in frequency in the same proportion as the respiration; hence the ratio of the two becomes more or less perverted. Thus, for 100 pulsations per minute, there may be 60 respirations (I have seen this), numbers giving a ratio of 1·7 : 1, instead of 4·5 : 1, that of health. The same perversion exists in those exceptional cases in which the pulse maintains a low frequency throughout. This perverted pulse-respiration ratio may, as I have found in several instances, *prove the first sign of pneumonia, appearing before crepitation, or rusty expectoration*, as *per contra*, a return to or towards the healthy standard *may announce resolution some days earlier than the rhonchus crepitans redux*." (p. 324.)

The remark made by Dr. Walshe, that the quickened respiration does not necessarily bear any relation to the amount of lung affected, is very interesting. In fact, as we have had occasion to note several times, the respirations are often quicker before the least physical sign of pneumonia can be detected, than they are two or three days later, when consolidation-signs are rapidly showing themselves. It is, indeed, not to the lung, so much as to the blood, that we are to look for an explanation of the dyspnœa; and when the blood is partially purified (and it is no doubt so purified in a certain number of cases) by the deposition into the lung-substance, then, in spite of the blockade of aerating surface produced by the hepatization, the dyspnœa may be, and generally is, less than when the diseased blood was with difficulty forcing its way along the delicate pulmonary capillaries, through which its altered condition did not permit it easily to pass. We have seen this extreme augmentation of respiration-movements, and consequent perversion of pulse-respiration ratio, in cases of pyohæmia after operation, in which no other symptoms of pneumonia existed, but in which, no doubt, the state of the blood was such as to lead to pneumonia, although, in the cases to which we refer, the actual stagnation in the lung was happily averted.

The treatment of pneumonia is judiciously laid down. Bloodletting within just bounds, leeching or cupping, and antimony, are the chief measures. Dr. Walshe most truly says of antimony, that "it stands next in importance to bloodletting in the treatment of pneumonia; were I, indeed, henceforth, in the management of this disease, forced to surrender either, on the one hand, venesection, or, on the other, cupping and tartarised antimony, I should not hesitate to relinquish the former. In what manner this important agent produces its beneficial effects on the lung, is matter of the loosest speculation—that it does produce such effect is the really important point, and one of which scientific proofs abound." (p. 330.)

We must pass over some other diseases of the lung, such as emphysema, hæmoptysis, &c., hydrothorax, pneumo-thorax, &c., in order to glance at the subject of Phthisis. In accordance with the plan of the work, the morbid anatomy of phthisis is merely alluded to, but the symptoms, diagnosis, and treatment are detailed at great length. Dr. Walshe does not seem to attach much importance to prolonged expiration as a sign of early phthisis, unless it be attended with alteration of quality, such as harshness of both inspiration and expiration; "even coupled with slight harshness and coarseness of quality, it must be cautiously received as evidence in females, and at the right side." (p. 375.) "Jerking rhythm" is a sign to which we have attributed very little importance; we have found it in so many cases, when no disease of any kind, either local or general, was present, that unless backed by some much more decided indications, we should be inclined to pay very little attention to it. Dr. Walshe, however, ranks it higher than we do. If other known causes, particularly pleurisy, can be excluded, this "condition of rhythm, when limited to one apex, becomes a really important sign of tuberculisation. My opinion on this point has yearly grown more positive. It is to be remembered that it occurs at a period of the disease, when the physical signs generally are few in number, not so decisive as might be wished, and when, of course, every addition to their number is really important." (p. 375.) The murmur in the pulmonary artery which Dr. Latham described some few years ago, and regarded as an indication of phthisis, is considered of doubtful value.

The fact is, that the early diagnosis of phthisis will never be made from any single symptom; it is from their combination, from their mode of succession, and mutual relation to each other, that the approaching disease is to be recognised. One sign derives significance from another; and after some attention to this subject, we will venture to say, that the desire to find out some single physical sign of the earliest stage of phthisis, has led to many erroneous conclusions and mistakes in diagnosis. Nor, practically, is the diagnosis of early phthisis of such importance as has been stated; since the rule is uniformly acted upon, that the very suspicion of phthisis ought to be treated in the same way as if the symptoms were unequivocal.

In speaking of the diagnosis of phthisis, Dr. Walshe lays down the following highly practical and useful propositions:

"(*a.*) A young adult who has had an obstinate cough, which commenced without coryza, and without any obvious cause, a cough at first dry, and subsequently attended for a time with watery or mucilaginous looking expectoration, and who has wandering pains about the chest, and loses flesh, even slightly, is, in all probability, phthisical. (*b.*) If there be hæmoptysis, to the amount of a drachm even, the diagnosis becomes, if the patient be a male, and positively free from aneurism or mitral disease, almost positive. (*c.*) If, in addition, there be slight dullness under percussion at one apex, with jerking, or divided and harsh respiration, while the resonance at the sternal notch is natural, the diagnosis of the first stage of phthisis becomes next to absolutely certain. (*d.*) But not absolutely certain; for I have known every one of the conditions in *a*, *b*, and *c*, exist (except hæmoptysis, the deficiency of which was purely accidental,) when one apex was infiltrated with encephaloid cancer, and no cancer had been discovered elsewhere to suggest to the physician its presence in the lung. (*e.*) If there be cough such as described, and permanent weakness and hoarseness of the voice, the chances are very strong (provided he be non-syphilitic) that the patient is phthisical. (*f.*) If decidedly harsh respiration exist at the left apex, or at the right apex behind; if the rhythm of the act be such as I have called *cogged-wheel*, and there be dullness, so slight, even, as to require the dynamic test for its discovery, there can be little doubt of the existence of phthisis. (*g.*) If, with the same combination of circumstances, deep inspiration evokes a few clicks of dry crackling rhonchus, the diagnosis of phthisis, so far as I have observed, is absolutely certain. (*h.*) If these clicks, on subsequent examination, grow more liquid, the transition from the first to the second stage may be positively announced. (*i.*) If there be slight flattening under one clavicle, with deficiency of expansion-movement, harsh respiration, and slight dullness under percussion, without the local or general symptoms of phthisis, the first stage of tuberculisation cannot be diagnosed with any surety, unless there be incipient signs at the left apex also; the conditions in question, limited to one side, might depend on chronic pneumonia or on thick induration matter in the pleura. (*k.*) The existence of limited though marked dullness under one clavicle, with bronchial respiration and pectoriloquy, so powerful as to be painful to the ear, the other apex giving natural results, will not justify the diagnosis of phthisis. I have known this combination when the apex of the lung was of model health, and a fibrous mass, the size of a walnut, lay between the two laminæ of the pleura. I would even go farther and say, that the combination in question is rather hostile than otherwise to the admission of phthisis, as, had tuberculous excavation formed at one side, the other lung would, in infinite probability have been affected in an earlier stage. (*l.*) Pneumonia limited to the supra- and infra-clavicular region on one side, and not extending backwards, is commonly, but not always, tuberculous. (*m.*) Subcrepitant rhonchus, limited to one base posteriorly, is not, as has been said, peculiar to tubercle; it may exist in emphysema, and in mitral disease. (*n.*) Chronic peritonitis, in a person aged more than fifteen years, provided cancer can be excluded, involves, as a necessity, the existence of tubercles in the lungs. To this law of Louis' it is necessary to add the qualification, provided Bright's disease be also absent. (*o.*) Pleurisy with effusion, which runs a chronic course in spite of ordinary treatment, is, in the majority of cases, tuberculous or cancerous: the character of the symptoms, previously to the pleurisy, will generally decide between the two. (*p.*) Double pleurisy, with effusion, is not, as has been said, significant of tubercle; for it may depend on Bright's disease. If the latter disease can be excluded, carcinoma and pyohæmia remain as other possible causes. (*q.*) If a young adult, free from secondary syphilis and spermatorrhœa, and not dissolute in his habits, speedily lose flesh without clear cause, he is, in all probability, phthisical, even though no subjective chest-symptoms exist. (*s.*) But he is not by any means certainly so, for he may have latent cancer in some unimportant organs, or he may have chronic pneumonia. (*t.*) Nay, more, he may steadily lose weight, have dry cough, occasional diarrhœa, and night sweats, and present dullness under percussion, and bronchial respiration under both clavicles, and yet be non-phthisical. I have known all this occur in cases, both when the lungs were infiltrated superiorly with primary encephaloid cancer, and when they contained secondary nodules of the same kind. (*u.*) Failure of weight becomes less valuable as a sign of phthisis, the longer the thirtieth year has been passed. (*r.*) The discovery of cardiac disease with marked symptoms, deposes against, but does not exclude, the existence of active tuberculisation. (*w.*) The existence of cancer in any organ is unfavorable to the presence of tuberculous disease, but tubercle and cancer *may* coexist, even in the same lung." (pp. 393-5.)

Under the head of *Treatment*, Dr. Walshe speaks encouragingly of the modes of treatment which have recently come into use; and it is satisfactory to know, that the experience of two years, since his 'Report on Phthisis,' has confirmed the opinion he then formed of the utility of cod liver oil. While, however, he has no doubt, that "it more rapidly and effectually induces improvement in the general and local symptoms than any other known agent," he thinks that its power of *curing* the disease, that is of preventing subsequent attacks, is undetermined, and that the mean amount of permanency of its good effects remains yet to be fixed. The good effects of the oil are also said to be, "*cæteris paribus*, directly as the youth of those using it."

After Phthisis, Cancer of the Lung receives a brief but sufficient notice; and then Spasmodic Asthma and Hooping-Cough are discussed. An interesting chapter on Intra-thoracic Tumour concludes this portion of the work.

The next chapter is occupied with the Diseases of the Heart and Great Vessels, and is an admirable summary of our knowledge of this subject. Our space is, however, so nearly exhausted, that we can only employ another page or two in the discussion of some points which have particularly interested us.

Dr. Walshe devotes some few pages to the consideration of the efficient causes of Cardiac Dropsy, a subject of great interest and some intricacy. He concludes that dilatation and tricuspid regurgitation are the cardiac and mechanical conditions most frequently connected with dropsy, and that it is rare for either of these states to exist for any length of time without its supervention. But, in addition to these lesions, he thinks that coincident changes in the blood, and in the walls of the capillaries and small veins, are accessory conditions of importance. Tricuspid regurgitation does not, he thinks, necessarily produce dropsy; but when it is joined to this condition of the blood, dropsy then results. If we understand aright, the first step in the morbid process is held to be the cardiac disease, either dilatation, in which case stagnation recurs from weakness of the left ventricle, or tricuspid regurgitation, producing reflux of blood in the large veins and impediment to the return of blood to the heart. Then on this cardiac disease ensue changes in the blood and capillary walls, which aid transudation of fluid. We presume, however, that it is not denied, that when stagnation of the general circulation occurs in a marked degree, either from dilatation or obstruction to the flow of the blood through the right side of the heart, dropsy may be the immediate consequence of this mechanical obstruction, without any intervening stage of blood-alteration. On the other hand, the presumed alteration in the blood and in the walls of the vessels, is a usual consequence of the stagnation, but does not necessarily follow. So that, if we interpret Dr. Walshe aright, the three following propositions express his opinions:

1. Dropsy may result from the two cardiac affections above named, when they are of such a nature as to give rise to great stagnation. Such a dropsy is entirely mechanical.

2. Dropsy may result in cases of the same diseases, even when they do not give rise to stagnation sufficient, *per se*, to cause dropsy by alteration of the blood and of the walls of vessels. In this case, the cause of the dropsy is partly mechanical, partly chemical.

3. Dropsy may be absent in cases of the same cardiac diseases, when the stagnation, *per se*, is not sufficient to cause it, and when, from some cause or other, the blood-disease does not ensue.

The change in the blood which (it is supposed) thus frequently forms one of the links in the production of dropsy, is, of course, hypothetical. Its existence is, however, presumable on two grounds; first, as an explanation of the cases in which the mechanical, that is, the cardiac, cause of dropsy exists, without giving rise, as usual, to dropsy; and, secondly, because chemical changes in the blood are known to give rise to dropsy, as in most cases of Bright's disease, and in some splenic cases.

There can be little doubt, we presume, that in many cases of cardiac dropsy, it is unnecessary to seek for any other cause than the mechanical one. The readiness with which the dropsy, dependent on some additional obstructive cause, added temporarily to tricuspid regurgitation (such as an attack of subacute bronchitis occurring in emphysematous lungs), may be cured when the accessory cause (the pulmonary disease, for example,) is removed, seems to indicate that the blood can, in such a case, be but little altered from its normal condition, and that the actual difficulty of transit through the systemic vessels, in consequence of the pulmonary and cardiac conditions, is the only cause of the transudation of fluid. That such difficulty of transit may, after a time, give rise to chemical changes, or to textural alterations in the walls of vessels, is, however, very probable. The nature of such changes and alterations remains, of course, to be determined.

The *Diseases of the Aorta* are treated very fully; but we have only room for the following extract on the distinctions between aneurism of the arch and mediastinal tumour.

"Tumour in the anterior mediastinum presents the greatest number of positive points of similarity to aneurism. Now, if there be highly-marked pulsation, a broad-based prominence with conical elevation in the centre, the murmurs most distinctive of aneurism, and a sensation of the flow of liquid beneath the integuments, there can be no doubt that, whatever other grounds for diagnosing tumour may exist, aneurism is really present. But every one of these things may, in cases of aneurism, be absent; then observe how like the two things are—a sac filled with fibrin, and a solid tumour. In truth one is a tumour *inside*, the other *outside* the arch; and obstruction from without may have the same effect as from within on its circulation. Common to the two things are dullness and non-resilience (it may be, extending across the middle line), all the signs of concentric, and all the signs of excentric pressure. Under such circumstances, the question becomes one of pure probabilities. The conditions in favour of aneurism would be these: situation in the course of the arch, vibratile thrill above or below the clavicle, gradually increasing, nearness of pulsation to the surface, (but from stratification of fibrin, the pulsation of an aneurism may grow deeper, and that of a tumour grow more superficial,) absence of œdema of the arm and chest, dysphagia, and great pain, especially of the dorsal spine. The circumstances in favour of tumour, and against aneurism, would be the facts of the patient being a female,* and under twenty-five years of age; great superficial extent of percussion-dullness, especially if there were no marked attenuation of the walls of the chest; absence of any heaving motion in the affected spot; want of accordance between the sides of maximum-dullness and of pulsation, and currant-jelly expectoration, (common with tumour, rare with aneurism)." (p. 562.)

With this quotation we must take leave of Dr. Walshe's book; but we beg to assure our readers, that we have by no means exhausted, or even alluded to, all the original matter which is interposed through the volume. The work, too, is so extremely condensed, that it is really difficult to give a good idea of it by the selection of isolated passages. In a volume not so large as Hope's well known treatise, Dr. Walshe has managed to give every essential fact connected with the diseases of the lungs, as well as of the heart, and also to include one of the best treatises we have ever perused on the art of physical diagnosis. He has thus given us one of the most practical and useful works with which we are acquainted; and we have little doubt that our verdict will be re-echoed by that court before which we, equally with Dr. Walshe, stand, and which is composed of those whose daily practice will enable them to test the value of Dr. Walshe's instructions.

ART. II.

Lehrbuch der physiologischen Chemie. Von Prof. Dr. C. G. LEHMANN. Erster Band. Zweite gänzlich neu umgearbeitete Auflage, pp. 464; Zweiter Band, pp. 463.—*Leipzig*, 1850.

A Treatise on Physiological Chemistry. By Prof. Dr. C. G. LEHMANN. First Volume. Second Edition, entirely revised, pp. 464. Second Volume, pp. 463.—*Leipsic*, 1850.

IN our last Number, we gave our readers a general account of this most valuable work, and directed their attention to some of the most important novelties contained in its First Volume. We shall now bring under their notice a few of the most interesting topics embraced in the Second.

* But, as Dr. Walshe remarks, this is of little value; for the excess of aneurism of the arch in males is by no means so great as that of aneurism of all arteries "indiscriminately."

We now proceed to the Second Volume—to the consideration of the Animal Fluids, confining our remarks, for the most part, to such investigations in this department of chemistry, as have appeared since the publication of the Sydenham Society's edition of Simon's 'Animal Chemistry.'

We commence with the *Saliva.* In the consideration of this fluid, we must always bear in mind, that the saliva discharged from the mouth is not the mere secretion of the salivary glands, but a mixture of the pure secretion with the secretion of the buccal mucous membrane. Hence, in treating of this fluid, we must distinguish between *pure* and *mixed* or *ordinary* saliva.

The specific gravity of ordinary saliva is liable to considerable fluctuations; its density being partly dependent on the amount of mucus which is intermingled with it, and partly on the degree of dilution of the true salivary secretion. According to Lehmann it varies in man from 1004 to 1006, but may, even in the normal state, rise to 1008 or 1009, or sink to 1002. Jacubowitsch,* in an excellent inaugural dissertation on the saliva, makes these numbers somewhat lower; according to him, the mean specific gravity of saliva, before and after filtration, is 1002·6 and 1002·3, respectively. Normal saliva has a more or less distinct alkaline reaction, and exerts no deleterious influence on plants or animals, as has been asserted by Wright. Lehmann and Jacubowitsch agree in denying the accuracy of Wright's assertions regarding the taste of normal saliva, and suggest that the saliva experimented on by the English physiologist might not have been perfectly fresh; and as few fluids undergo change more rapidly than the saliva, this is probably the true explanation of the apparent discrepancy.

Most recent observers agree in the observation, that the alkalinity of the saliva increases during, and soon after, eating; and that, in the fasting state, it very much diminishes, or altogether disappears; indeed, in many apparently healthy persons, the saliva, when the stomach is empty, assumes a faintly acid reaction, which, however, at once disappears when any solid food is taken. The experiments of Wright and others show, that the alkalinity of this fluid is liable to great variations. Frerichs† found that 100 grammes of saliva, secreted by a man smoking tobacco, were neutralised by 0·15 of a gramme of sulphuric acid.

The ordinary saliva being, as we have already observed, derived from several distinct sources, we shall take a rapid glance at the nature of the two principal fluids entering into its composition.

The *parotid saliva* first claims our attention. Human saliva from this source has, we believe, been analysed by no chemists, with the exception of Mitscherlich and Van Setten. The parotid saliva of dogs and horses has, however, been frequently examined. It is perfectly limpid and colourless, has no taste or smell, is not so viscid as to admit of being drawn out in threads, and has a decidedly alkaline reaction. In dogs, Jacubowitsch found that its specific gravity ranged from 1004 to 1004·7; and in horses, Lehmann found it to vary from 1005·1 to 1007·4. The following may be regarded as invariable constituents of this secretion:

a. Potash, soda, and lime, in combination with an organic substance. This compound is one of the most important constituents of the saliva, and

* De Saliva. Diss. Inaug. Dorpati Livon., 1848. The experiments and analyses of Jacubowitsch were conducted under the superintendence of C. Schmidt and Bidder.

† Wagner's Handwörterbuch der Physiologie. Art. *Verdauung*, p. 760.

one on which several of its properties depend. We shall therefore notice it somewhat fully. It is very similar to, but not identical with, albuminate of soda, and partly corresponds to the salivary matter or ptyalin of Berzelius. Jacubowitsch and other chemists maintain that the saliva contains alkaline carbonates. Lehmann, however, remarks that their quantity in the fresh secretion must be extremely small, and that the alkaline carbonates which have been observed are formed during the process of analysis from the action of the atmospheric air. The formation of carbonate of lime, in this manner, in the parotid secretion of the horse, may be easily seen,—very beautiful crystals of carbonate of lime being formed. The organic matter, ptyalin, although difficult of solution, is not insoluble in water, after its separation from the alkalies or lime by carbonic or some stronger acid. Hence it is, that saliva is sometimes rendered turbid, and at other times remains apparently unaffected, on the addition of an acid. The separated matter falls in amorphous flocculi, which are difficult of solution in pure water, but dissolve readily in water to which either an alkali or an acid has been added. We find this substance—in part, still combined with an alkali—both in the aqueous and in the spirituous extracts; but it is from the latter that we can obtain it in the purest form, when, after extraction with alcohol and ether, it occurs as an almost gelatinous, colourless substance, very slightly soluble in water. As much confusion has arisen from the discrepant statements of chemists regarding ptyalin, we insert the following observations regarding its reaction. On the addition of a little acetic acid to its alkaline solution, there is a flocculent precipitate, which however readily dissolves in an excess of the acid; when boiled with hydrochlorate of ammonia or sulphate of magnesia, the alkaline solution of ptyalin becomes very turbid. The alkaline solution is precipitated by tannic acid, bichloride of mercury, and basic acetate of lead; but not by alum, sulphate of copper, &c. The acetic acid solution is strongly precipitated on the addition of ferrocyanide of potassium; boiled with nitric acid, it yields a yellow solution. From the above reactions, we see how closely this substance resembles albuminate of soda and casein, though it is by no means identical with either of them. In no other animal fluid can a substance perfectly identical with this ptyalin be found.

b. An extractive matter, soluble in alcohol and water, precipitable by tannic acid, but not by alum.

c. Sulphocyanide of potassium has been found in the parotid saliva of man, the horse, and the sheep, by Mitscherlich, Jacubowitsch, and Gmelin; but Lehmann observed no reddening on the addition of perchloride of iron to the parotid saliva of a horse.

d. The potash-salt of a not very volatile acid of the butyric acid group, probably caproic acid. The crystals under the microscope resembled the tufts of margaric acid.

e. A little epithelium and some mucus-corpuscles.

f. The chlorides of sodium and potassium.

g. A very small amount of phosphates.

h. A trace of alkaline sulphates.

In the parotid saliva of the dog, the solid residue, according to Jacubowitsch, amounts to 0·47 per cent., in which the organic is to the inorganic matter as 29·8 : 70·2, the latter consisting of 44·7 of alkaline chlorides, and 25·5 of carbonate of lime. In the corresponding fluid of the horse, Lehmann found, as the mean of six analyses, 0·708 per cent. of solid residue, in which the organic was to the inorganic matter as 46·1 : 53·9. The mean amount of ptyalin was 0·140 in Lehmann's experiments.

The saliva of the submaxillary gland, as obtained from dogs, has been recently examined by Bernard and Jacubowitsch. According to the latter observer, its specific gravity is 1004·1, its reaction is less strongly alkaline, and it contains much less combined organic matter than the parotid saliva. In almost all other respects, including its amount of sulpho-cyanide of potassium, it resembles that secretion.

Our subsequent observations apply to *mixed* or *ordinary* saliva. The first point to which we shall advert is in reference to the occurrence of sulphocyanogen in this secretion.

Jacubowitsch found 0·006 per cent. of sulpho-cyanide of potassium in his own saliva; while Lehmann, in similar experiments, found the quantity to vary between 0·0046 and 0·0089. Both chemists consider that Wright, in fixing the limits for human saliva at 0·51 to 0·98, has assigned far too high numbers for this salt. As some writers have suggested that the occasionally poisonous properties of morbid saliva may be due to an augmentation of this salt, it is deserving of mention, that Wöhler and Frerichs* have proved, by direct experiments, that it possesses no noxious properties. There are two ways, by which we may, with great accuracy, determine the quantity of sulphocyanogen. The method adopted by Van Setten, Jacubowitsch, and Tilanus,† is to dissolve the alcoholic extract of the saliva in water, and then to remove the fat by filtration; to concentrate the filtrate, add phosphoric acid, and distil; to saturate the distillate with baryta, and to evaporate the filtered fluid; the residue is then to be boiled for a considerable time with fuming nitric acid or aqua regia, and the amount of sulpho-cyanide of potassium estimated from the sulphate of baryta which separates.

The other method is, to precipitate the aqueous solution of the alcoholic extract of saliva with nitrate of silver, and, after well washing the precipitate, to treat it with water containing nitric acid, which does not dissolve the chloride of silver; the silver is precipitated from the acid solution by hydrochloric acid, a little chloride of barium is added, and the solution is evaporated, a little nitric acid being repeatedly added. We thus, also, obtain sulphate of baryta, from which the sulphocyanogen is to be calculated.

Lehmann directs attention to the circumstance, that many mineral and organic substances which have been introduced into the system, pass off far more rapidly by the salivary glands than by the kidneys. Thus, if iodide of potassium be taken in the form of pill, iodine may be detected in the saliva in the course of ten minutes; whilst it cannot be found in the urine till a space of time, varying from half an hour to two hours, has elapsed. A similar law holds good in reference to bromine, mercury, and probably several other substances, which cause an increased secretion of saliva. As Wright and several other physiological chemists have failed in detecting any indications of mercury in the saliva during mercurial salivation, it is worthy of notice, that Lehmann always found this metal

* Ann. d. Ch. u. Pharm., Bd. 65, s. 344.

† De Saliva et Muco. Dissert. Inaug. Amstelod., 1849.

in such cases, both by dry distillation of the residue of the saliva, and by the simple application of an extremely minute pair of plates of copper and zinc to the slightly acidulated saliva. The reasons why the search for mercury in these cases has been so often unsuccessful are:—1, that frequently the examined fluid is merely the buccal secretion, for, in the first stage of salivation, this constitutes almost the entire bulk of the sputa, and it is not till subsequently that the salivary glands are affected; and 2, that unless the evaporation be conducted with much precaution, the mercury readily volatilises with the aqueous vapour.

We extract the following observations on the occurrence of acid saliva:

"According to Donné, the saliva is acid in inflammatory affections of the *primæ viæ*, in pleuritis, encephalitis, acute rheumatism, intermittent fever, and uterine diseases; and, according to L'Heritier, also in cancer of the stomach. Wright lays down four varieties of acid saliva, namely: (*a*) that which occurs in idiopathic affections of the salivary glands; (*b*) that which presents itself when there is a predominance of the acids in the organism generally, from constitutional or other causes, as in scrofula, phthisis, rachitis, amenorrhœa, inflammatory rheumatism, &c.; (*c*) the form occurring in sub-acute inflammation of the mucous membrane of the stomach and intestines; and (*d*) the form presenting itself in dyspepsia. In affections of the nervous system, the saliva is, on the other hand, never acid, but often very strongly alkaline. In catarrhal affections of the gastric and intestinal mucous membranes, and in the round ulcer of the stomach, I have very often, but not invariably, found the saliva acid; in cancer of the stomach and diabetes I have, however, invariably found it acid. In inflammation of the thoracic organs, in acute rheumatism, typhus, &c., I have very often found the saliva alkaline, or perfectly neutral. According to Donné and Frerichs, the acid reaction always depends on the buccal mucous membrane, which, when in a state of abnormal irritation, always yields an acid reaction." (Vol. ii, pp. 25-6.)

As we shall take an early opportunity of reviewing the chemistry of the digestive process in an independent article, we shall, for the present, omit any notice of Lehmann's views regarding the uses of the saliva.

We proceed to the consideration of the *Gastric juice*. This fluid, when in a state of purity, is perfectly clear and transparent, almost colourless, but sometimes exhibiting a very faint tinge of yellow, and possesses a very faint but peculiar odour, and a scarcely appreciable acid-saline taste; the few morphological elements which are observed on making a microscopical examination, are unchanged cells from the gastric glands, the nuclei of those cells, and fine molecular matter arising from the disintegration of these elements. It has a very acid reaction, and is not rendered turbid by boiling; but sometimes becomes slightly opalescent when neutralised with an alkali. It differs from most other animal fluids in remaining undecomposed for a very long time; and even when its surface becomes covered with mould, it still retains its characteristic digestive property. Pure filtered gastric juice contains only a very small amount of solid matter,—not more than from 1·05 to 1·48 per cent.,—and very little is even yet known regarding the nature of the organic portion of this residue. We have already shown (p. 158) that the ordinary free acid in the gastric juice is lactic acid, and we have noticed the occasional presence of acetic and butyric acids; we may add, in relation to this subject, that Frerichs has recently discovered the presence of butyric acid in the stomachs of fasting horses and sheep, thus confirming a statement made long ago by Tiedemann and Gmelin.

Lehmann, to whom, indeed, we are indebted for the conclusive establishment of the long-suspected fact, that lactic acid is *the acid* of the gastric juice, states that in six experiments the quantity of acid varied from 0·098 to 0·132 per cent. The solid residue, in addition to lactic acid, contains a large amount of metallic chlorides, especially of chloride of sodium, and smaller quantities of the chlorides of calcium and magnesium, with traces of protochloride of iron, and small quantities of phosphate of lime; but gives no indications of alkaline sulphates or phosphate of ammonia. In addition to these mineral substances, the gastric juice also contains certain organic matters, which, however, in consequence of the small quantity in which they occur, have been very imperfectly examined; namely, a substance soluble in water and in absolute alcohol (commonly known as osmazome), and a substance soluble only in water, and which is more or less completely precipitated by alcohol, tannic acid, corrosive sublimate, and the salts of lead; the latter substance, which is possibly a mixture of several substances, loses its property of modifying the protein-compounds and gelatigenous substances when its solution is boiled, but does not, like albumen, coagulate on heating.

We must refer our readers to this portion of Lehmann's second volume for an excellent sketch of the labours of Eberle, Schwann, Wasmann, Pappenheim, Valentin, and Elsässer, in relation to artificial digestive fluids, and for an account of the mode by which he obtained a pure digestive fluid for his own experiments.

In connection with the chemical nature of the digestive principle, we must not omit to notice an ingenious idea, propounded a few years ago by C. Schmidt, the distinguished Professor of Chemistry at the University of Dorpat. He regards this substance as a conjugated acid, whose negative constituent is hydrochloric acid, which has, as its adjunct, Wasmann's non-acid or coagulated pepsin, and he conceives that this conjugated acid possesses the property of forming soluble compounds with albumen, gluten, chondrin, &c.; this pepso-hydrochloric acid becoming decomposed at a temperature of 212° F., into Wasmann's coagulated pepsin and hydrochloric acid, which cannot again unite. If this complex acid come in contact with an alkali, the adjunct is precipitated. Because an artificial digestive mixture, which has lost its digestive power, again obtains its solvent and digesting powers on the addition of a free acid, Schmidt thinks that we may conclude, that, by the addition of hydrochloric acid, the pepso-hydrochloric acid is liberated from its combination with albumen, &c., and thus regains its former properties; while the hydrochloric acid, which has been added, unites with the albumen, to form a soluble compound.

By the repeated addition of hydrochloric acid, a digestive mixture, or this assumed pepso-hydrochloric acid, may for ever regain its digestive power; unless, on the one hand, the fluid become gradually saturated with dissolved matters, or, on the other, the conjugated acid cease to be decomposed.

Ingenious and probable as this view appears to be, certain facts seem opposed to it. The existence of this pepso-hydrochloric acid has not as yet been determined by any analysis of a combination, either with a mineral base or with an albuminous substance; and the numerous experiments which have been made by Lehmann regarding the digestive agents and the substances to be digested, indicate no definite proportion between the acid and the substance to be digested, such as this hypothesis would require.

In his remarks on the abnormal constituents of gastric juice, Lehmann observes that he has twice found urea in vomited matters, when none of the symptoms of uræmia were present. This vomited matter had a distinctly urinous odour, and also contained uric acid. The patients were hysterical girls who simulated retention of urine, and had been drinking their own renal secretion. We suspect that a similar explanation is applicable to most cases of alleged urinary vomiting.

Assuming the correctness of Lehmann's experiment, that 100 grammes of fresh gastric juice (of the dog) do not dissolve more than 5 grammes of coagulated albumen, we may infer that as a man assimilates on an average 100 grammes (from 3 to 4 ounces) of albuminous matter in 24 hours, the daily secretion of gastric juice amounts to 2000 grammes, or 4 pounds.

The *Bile* next claims our attention. According to recent investigations of Strecker, to which we have already had occasion to advert in an earlier part of this article, every kind of bile contains *two essential constituents:* namely, a resinoid and a colouring constituent.

The *resinoid constituent* is the soda-salt of one of the conjugated acids already noticed (p. 161), having either glycine or taurin for its adjunct.

The *colouring principle* is *biliphæin,* which exists in the bile in combination with an alkali.

In addition to these we may mention *cholesterin* (which is always present in small quantities), and fats, and combinations of the alkalies with fatty acids. Mineral salts, consisting principally of chloride of sodium, but partly also of phosphate and carbonate of soda, phosphates of lime and magnesia, with very minute quantities of iron and manganese, are also present, but no alkaline sulphates. We are indebted to Bensch for the singular observation which has been since confirmed by Strecker, that the bile of salt-water fishes consists almost entirely of potash-salts, while that of the herbivorous mammalia consists almost entirely of soda-salts; the very reverse of what we should, *à priori,* have expected from the nature of their food.

Finally, there is always more or less mucus in the bile, with a comparatively small quantity of epithelium mixed with it. With the exception of this epithelium, which is of the cylindrical variety, fresh normal bile presents no morphological elements.

We cannot at present understand why it is that the bile of different animals presents different proportions of alkaline taurocholates and glycocholates. In the bile of fishes the resinoid constituents consist almost entirely of taurocholates with mere traces of glycocholates. In the bile of dogs scarcely anything but taurocholate of soda is found; and the same remark holds good in reference to the bile of serpents. Experiments instituted by Strecker appeared to show that, in the case of the dog, the nature of the food exercises no influence on the composition of the bile. Sheep's bile contains a great preponderance of taurocholate over glycocholate of soda; while the bile of the goose, according to the investigations of Marson, contains almost entirely taurocholic acid.

The peculiar bile-pigment is present in all classes of animals: in the carnivora and omnivora, including man, it is of a brown colour,—the cholepyrrhin of Berzelius; while in birds, fishes, and amphibia, it is usually of an intense green colour,—the biliverdin of the same chemist. The cholepyrrhin is always combined with soda or lime, most commonly with the former. Its combination with lime is insoluble, and the compound occurs in brown granules, which may readily be detected in the bile by a microscopic examination.

As none of the recent results obtained by the chemical examination of the bile have as yet found their way into any of our standard works, we shall enter somewhat fully into the details bearing on the point. According to Frerichs,* normal human bile contains about 14 per cent. of solid constituents. Gorup-Besanez† found 9·13 in the bile of an old man, and 17·19 in that of a boy aged 12 years; but further observations are required before we can decidedly say whether the bile is more dilute at an advanced age than in childhood and adult life. The organic constituents average about 87 per cent. of the solid residue, both in man and the lower animals. The alkaline taurocholates and glycocholates are the preponderating organic constituents, forming, at least, 75 per cent. of the solid residue.

In most animals whose bile has been yet examined, the taurocholate of soda is the principal constituent. This salt, whose composition is expressed by the formula $NaO{\cdot}C_{52}H_{44}NO_{13}S_2$, contains 6 per cent. of sulphur; and hence, by determining the amount of this element in that portion of the residue which is soluble in alcohol, we very readily calculate the amount of taurocholic acid which is present. In purified serpent's bile, that is to say, in the alcoholic extract, Schlieper found 6·2 of sulphur; in that of the dog, Bensch found 6·2, and Strecker 5·9; in that of the fox, Bensch found 5·96; and in that of the sheep, Strecker found from 5·7 to 5·3. In the bile of all these animals, taurocholic acid preponderates to almost the entire exclusion of glycocholic acid; but in the bile of the ox, whose alcoholic extract contains only 3 per cent. of sulphur, taurocholic and glycocholic acids occur in nearly equal proportions.

We have no trustworthy data regarding the amount of pigment, of cholesterin, and of fats and fatty acids in the bile.

The same remark is almost equally applicable to the mineral ingredients of the bile. The most trustworthy analysis of the bile-ash is that of Weidenbusch, who found 27·70 per cent. of chloride of sodium, about 16 of tribasic phosphate of soda, 7·5 of tribasic phosphate of potash, only 3·025 of basic phosphate of lime, 1·52 of basic phosphate of magnesia, 0·23 of peroxide of iron, and 0·36 of silica.

Lehmann has convinced himself by satisfactory experiments, that bile—at all events, ox-bile—contains preformed alkaline carbonates.

The mucus in human bile amounts, according to the same chemist, to 0·158 per cent.

Amongst the abnormal constituents which present themselves in the bile, we may notice *albumen,* which sometimes occurs in fatty liver, in Bright's disease, and in the embryonic state; *urea,* which occurs in the bile after extirpation of the kidneys, and has been found in this fluid in Bright's disease and in cholera; and sulphuret of ammonium, which was once detected in considerable quantity by Lehmann, in the bile of a child

* Hannov. Ann., Bd. 5, H. 1, s. 2.

† Untersuchungen über die Galle. Erlangen, 1846, s. 44.

that had died suddenly, but of whose previous history he could obtain no account.

We proceed to the consideration of gall-stones, which, according to our author, are more common in England, Hanover, and Hungary, than in other countries:

"Most gall-stones," he observes, "are so rich in cholesterin, that all other constituents may be regarded as very secondary; they all, however, contain one or more nuclei, consisting of traces of mucus and earthy phosphates, and very frequently of an insoluble combination of lime and bile-pigment. A large number of biliary concretions are formed of a mixture of cholesterin and the above-named combination of lime and pigment, which latter is sometimes uniformly distributed through the concretion; in other cases we may observe distinct strata of cholesterin and brown pigment; and in others, again, we find only a little cholesterin in the dark-brown compound of pigment and lime.

"There is a third variety of concretions, which are black or of a dark green colour; these contain another modification of pigment, which is also in combination with lime. This variety is very poor in, or altogether free from, cholesterin.

"Biliary concretions consisting principally of carbonate and phosphate of lime, are very rare.

"It is singular that uric acid has been occasionally found in gall-stones. (Stöckhard,* Marchand.†)" (vol. ii, p. 69.)

Much has been written regarding the means by which cholesterin is held in solution in normal bile; the recent investigations of Strecker have, however, satisfactorily shown, that both the cholesterin and the lime-and-pigment compound are chiefly dissolved by the taurocholic acid or the taurocholate of soda. Glycocholic acid and cholic (Strecker's cholalic) acid possess the same property in a far less degree. Hence, it appears probable, that gall-stones are formed either when the taurocholic acid is deficient in relation to the substances which it dissolves, or when it undergoes decomposition in the gall-bladder.

There is at present very little accordance amongst the statements made by different physiologists, in reference to the quantity of bile secreted in twenty-four hours. While the lowest estimate in the case of man is *one* ounce, the highest is *twenty-four* ounces. Blondlot found that a dog, in whom he had established an external fistulous opening, secreted from 40 to 50 grammes in twenty-four hours; and hence he inferred, that an adult man secreted about 200 grammes, or 7 ounces. The most accurate and ingenious experiments on this subject are those of Bidder and Schmidt, who ascertained from numerous observations on cats, that an animal of this nature, weighing one kilogramme, (or 2·2 avoirdupois pounds,) secreted ·765 of a gramme of fresh, or ·05 of a gramme of dried bile in one hour, when the flow was most abundant; and only ·094 of a gramme of fresh, or ·0076 of a gramme of dried bile during the same period, after ten days' starvation. From an extensive series of statistico-analytical investigations, instituted by these physiologists and chemists on dogs with biliary fistulæ, on 40 cats, 13 geese, and several sheep and rabbits, with the view of comparing the quantities of secreted bile and of expired carbonic acid, they were led to the conclusion, that "only $\frac{1}{10}$th to $\frac{1}{40}$th of the carbon excreted by the lungs is, in equal times, secreted in the form of bile by the liver, so that, at the least, $\frac{8}{9}$ths to $\frac{9}{10}$ths of the burned

* De Cholelithis. Diss. Inaug. Med. Lips., 1832.
† Journ. f. pr. Ch., Bd. 25, s. 39.

and expired combustible material does not pass through the intermediate stage of bile-formation, but remains in the circulating blood, and there becomes perfectly oxidised."

Our limited space compels us to pass over Lehmann's observations on the formation of the bile, on the changes which it undergoes in the intestinal canal, on its functions generally, and especially on its importance in the digestive process; we shall, however, have an opportunity of reverting to some of these points in a review of the recent works on the chemistry and physiology of digestion.

The recent investigations of Bernard,* Frerichs,† and Bidder and Schmidt,‡ although yielding results which, in some respects, are discordant, have thrown very much light on the function of the pancreas.

The *Pancreatic fluid* is a colourless, clear, very slightly viscid fluid, without smell or taste, having a specific gravity of 1008 or 1009, an alkaline reaction, forming, when heated, a very slight coagulum, and becoming a little turbid on the addition of acids and alcohol. It is a secretion that becomes very rapidly decomposed; for even after a few hours' exposure to the air, it gives off a decidedly putrid odour. In the pancreatic fluid of an ass, Frerichs found 1·36 per cent., and in that of a dog 1·62, of solid constituents.

The principal constituent of the pancreatic fluid, is an albuminous or casein-like substance, which, however, is not perfectly identical with albuminate of soda, with casein, or with ptyalin. It is only imperfectly coagulated by heat, probably in consequence of its being combined with an alkali; is precipitated by acetic acid; but slowly dissolves in an excess of the reagent and on the application of heat; and is precipitated by ferrocyanide of potassium from an acetic acid solution; nitric acid precipitates it; and if ammonia be added and it is boiled, it assumes an intense yellow colour. Frerichs found 0·309 of this substance in the pancreatic fluid of an ass. It is to this substance that the fluid owes its principal chemical and physiological properties.

A butter-like fat was found by Bernard in larger, and by Frerichs in smaller quantity (0·026 per cent.)

In the case of the ass, the organic matter soluble in alcohol was only 0·015 per cent.

Neither Frerichs nor Bernard could find any trace of sulphocyanides.

The mineral substances in the fluid from the ass amounted to 1·01 per cent., of which 0·12 were insoluble salts,—carbonate and phosphate of lime and magnesia; and 0·89 soluble salts, consisting of chloride of sodium and alkaline phosphates and sulphates.

The observations of Frerichs seem to prove, that the pancreatic juice is only secreted during the process of digestion; after prolonged abstinence the gland was pale and anæmic, and the duct empty.

The next point to which we shall advert, is the *Fæces*. An adult, living on a mixed diet, discharges in the course of twenty-four hours, from four to six ounces of semi-solid brownish matter, whose unpleasant odour seems, from the researches of Valentin, to depend rather on decomposed constituents of the bile, than on the remains of the food. As this matter contains

* Archiv. Gén. de Med., 4 ser., t. 19, pp. 68-87.

† Wagner's Handwörterbuch der Physiologie, art. *Verdauung*.

‡ Private communication to Lehmann.

about 25 per cent. of solid constituents, a healthy man on ordinary diet passes daily an ounce or an ounce and a half of solid dry material. We are indebted to Liebig for the observation that the excrements contain only a small amount of soluble salts. In the ash of normal human excrement, Lehmann found only 23·067 per cent. of soluble salts; while, after an abundant meat-diet, Fleitmann found 30·58, and Porter 31·58. According to the last-named chemist, dried normal excrements contain, on an average, 6·69 per cent. of mineral substances. The ash of human fæces contains, according to Fleitmann, 30·98, and according to Porter, 36·03, of tribasic alkaline or earthy phosphates; the former found only 1·13, and the latter 3·13, of sulphuric acid. It is worthy of remark, that in each analysis the potash preponderated extremely over the soda; for, deducting the chloride of sodium, the ratio of the soda to the potash in the ash was 1 : 40 according to Fleitmann's analysis, and 1 : 12 in that of Porter; this difference being obviously dependent on the nature of the food. According to these chemists, the magnesia is to the lime in the ratio of 1 : 2 or $2\frac{1}{2}$; the alkaline chlorides occur in only very small quantity (from 1·5 to $4{\cdot}4\frac{8}{0}$), and carbonates are always found in the ash. They confirm the observation of Berzelius, that silica is always present in the fæces.

Salts of easy solubility only pass into the solid excrements when diarrhœa is present; Laveran and Millon have observed this circumstance in relation to sulphate of soda and acetate of potash, and Lehmann in relation to phosphate of soda.

Many of our readers may recollect that, some eight or ten years ago, Schönlein and his school maintained that the presence of phosphate of ammonia and magnesia in human fæces was a sign of typhus; more recent and correct observations have, however, proved the fallacy of this view. Crystals of this salt sometimes occur in perfectly normal fæces; and in those cases when the secreted juices and the contents of the intestine readily undergo decomposition, as in typhus, cholera, and certain forms of dysentery, we find them in large numbers and of considerable size.

We extract the following observations on the grass-green pultaceous stools so frequently occurring after the use of calomel:

"My own observations show that mercury is constantly to be found in the stools after the use of calomel, whether those stools be green or black, or of their ordinary colour; this fact had been previously established by Herrmann,* and more especially by Merklein;† and Höfle has likewise convinced himself of the presence of mercury in such cases. By a process of rincing with water, the sulphuret of mercury may be separated from the fæces, and its chemical nature easily recognised. The dark colour of the finely-comminuted sulphuret of mercury may certainly, like sulphuret of iron, communicate a light-green colour to animal matters, especially when yellow bile-pigment is present; indeed, according to Herrmann, powdered calomel, rubbed up with yellowish-brown excrement, produces a greenish colour. But, notwithstanding these facts, we should not deny the presence of slightly changed bile in calomel-stools; for when we have carefully prepared the alcoholic extract of such fæces, we may easily convince ourselves, by the addition of nitric acid, that it contains bile-pigment, and by Pettenkofer's test that it contains the resinous biliary acids. Any one examining such stools must, at all events, arrive at the subjective conviction, that a portion of the green and light colour is, in these cases, dependent on the bile-pigment. In connection with this, we may remark,

* De Rationibus Dosium Calomellis, &c. Diss. Inaug. Hauniæ, 1839.

† Ueber die grünen Stüble nach dem Gebrauche des Calomels im typhösen Fieber. Inauguralabhandlg. München, 1842.

that Buchheim has recently convinced himself by the establishment of fistulous openings between the gall-bladder and the outer surface of dogs, that the administration of calomel gives rise to an augmented secretion of bile, and a more abundant secretion of mucus. If occasionally (and indeed not very rarely), after the administration of calomel, the stools do not become green, but retain their normal colour, or are impressed with the characters of some special morbid process, this affords no cogent argument against Merklein's view; for it is obvious that in abnormal states of the intestinal canal, the conditions necessary for the formation of sulphuret of mercury may not always be present; and, on the other hand, this would as little be an argument against the participation of the bile-pigment in the production of the colour, since the action of calomel on the secretion of the liver may be modified and entirely checked by the most varied circumstances.

"The case is different with the dark, often black, but frequently also green-coloured, stools, which occur after the prolonged use of ferruginous preparations, or chalybeate mineral waters, such, at least, as contain sulphate of soda with carbonate of protoxide of iron. Karsten* was the first to prove that, in these cases, the green colour is dependent on sulphuret of iron. His only error was, that he ascribed the colour to bisulphuret of iron, being led astray by the analogy with the formation of the prismatic sulphuret, $Fe\ S_2$, which, as is well known, forms on stagnating waters, when organic substances mixed with oxides of iron and alkaline sulphates undergo decomposition. In three cases in which I analysed these green and black excrements, which were voided by persons who had long used the Marienbad chalybeate water, I found 3·163, 1·039, and 2·100 per cent. of protosulphuret of iron." (Vol. ii, pp. 138-9.)

It must be recollected, that many vegetable substances communicate a more or less green or black tint to the excrements, as, for instance, indigo, bilberries, charcoal, &c., in the same manner as rhubarb, gamboge, and saffron, give them a light yellow colour.

Fat is found to abound in the excrements after the use of fatty foods, and of cod-liver oil,—a result which is quite in accordance with the observations of Boussingault, which have been lately confirmed by Bidder and Schmidt, that only a definite quantity of fat can be resorbed by the intestinal mucous membrane. Amongst the morbid conditions in which the fat is found to be increased in the fæces, Lehmann mentions pulmonary phthisis, Bright's disease, and diabetes mellitus; he observes, however, that this augmentation of fat is constant in none of these diseases. He seems unacquainted with the researches of Dr. Percy, on the fæces in diabetes and other disorders.

Coagulable albumen sometimes occurs in normal fæces; it is, however, secreted in largest quantity by the intestine, in cases of dysentery; when, on the addition of nitric acid, or on boiling, after neutralisation with ammonia, the whole fluid solidifies. We do not recollect to have previously met with the observation, that the pultaceous or fluid evacuations, which sometimes occur in Bright's disease, very often contain coagulable albumen. It is always present in considerable quantity in the liquid stools in typhus and in cholera.

We must pass with comparative brevity over the 126 pages devoted to the consideration of the *Blood*. The first point specially worthy of notice, is a table extracted from Schmidt's recent work, 'On the diagnosis of suspicious spots in Criminal cases.'† In consequence of the variations which the corpuscles undergo in size, owing to the varying density of

* Walther und Ammon's Journ. f. Chirg., th. 3, s. 180.

† Die Diagnostik verdächtiger Flecke im Criminalfäller. 1848.

the serum from evaporation, &c., Schmidt has made all his measurements with blood dried in very thin layers on glass slips. He thinks that, by a reference to the following table, we may ascertain with tolerable precision the source of the blood we are examining.

		Mean.	Maximum.	Minimum.
Human blood		0·0077mm.	0·0080mm.	0·0074mm.
Blood of the Dog		0·0070	0·0074	0·0066
„	Rabbit	0·0064	0·0070	0·0060
„	Rat	0·0064	0·0068	0·0060
„	Pig	0·0062	0·0065	0·0060
„	Mouse	0·0061	0·0065	0·0058
„	Ox	0·0058	0·0062	0·0054
„	Cat	0·0056	0·0060	0·0053
„	Horse	0·0057	0·0060	0·0053
„	Sheep	0·0045	0·0048	0·0040

We proceed, without comment, to another table, in which the chemical constituents of the blood are arranged in a new and original manner. Regarding the blood as essentially composed of two elements or factors, namely, the blood-corpuscles and the fluid in which they swim, he gives, in a collateral table, the chemical constituents of each.

1000 parts of Blood-corpuscles contain		1000 parts of Liquor Sanguinis contain	
Water	688·00	Water	902·90
Solid residue	312·00	Solid residue	97·10
Specific gravity	1088·5	Specific gravity	1028·0
Hæmatin	16·75	Fibrin	4·05
Globulin and cell-membrane	282·22	Albumen	78·84
Fat	2·31	Fat	1·72
Extractive matters	2·60	Extractive matters	3·94
Mineral substances (exclusive of iron)	8·12	Mineral substances	8·55
Chlorine	1·686	Chlorine	3·644
Sulphuric acid	0·066	Sulphuric acid	0·115
Phosphoric acid	1·134	Phosphoric acid	0·191
Potassium	3·328	Potassium	0·323
Sodium	1·052	Sodium	3·341
Oxygen	0·667	Oxygen	0·403
Phosphate of lime	0·114	Phosphate of lime	0·311
Phosphate of magnesia	0·073	Phosphate of magnesia	0·222

In reference to the density of the blood-corpuscles—a point which cannot be determined by direct observation—Lehmann observes, that it must, *à priori*, be concluded, that it varies according to the constitution of the fluid which surrounds them; since a continuous diffusion-current is passing between the contents of the cell and the liquor sanguinis. The density of the blood-corpuscles differs, then, both according as they contain a greater or less amount of the highly ferruginous compound, hæmatin, and according to their loss or gain from the surrounding fluid by endosmosis; the latter being by far the more powerful cause of variability. The density of the corpuscles in healthy men's blood fluctuates from 1088·5 to 1088·9; and in women's blood, from 1088·0 to 1088·6. In cholera, Schmidt found an augmentation of the corpuscles to from 1102·5 to 1102·7; while in dysentery there was a diminution to 1085·5; in albuminuria, to 1084·5; and in certain dropsies, to 1081·9.

The following paragraph will show the uncertainty that exists in the minds of even the best chemists regarding the nature of the blood-corpuscles:

"Most of the French chemists have maintained to the present time, that the cell-wall of the red corpuscles is composed of fibrin. Denis and Lecanu have attempted to demonstrate the presence of fibrin in the corpuscles by triturating them with salts, as, for instance, nitrate of potash and chloride of sodium; Virchow, who has repeated these experiments, has, however, convinced himself that the membranes observed by these authors are nothing more than the cell-walls of the corpuscles lying in folds, and adhering to one another, which, under the microscope, often present the appearance of Nasse's fibrinous discs, when submitted to pressure. Virchow, however, very correctly remarked, that the solubility of this membrane in a solution of nitrate of potash, and the circumstance of its swelling in acetic acid, afford no proof of its identity with fibrin; moreover, in my own experiments on the blood-corpuscles of the horse and the ox, I failed in obtaining from the cell-walls, after long digestion in a solution of nitre, even a trace of soluble matter which was coagulable or precipitable by acetic acid. Mulder regards the cell-wall as composed of binoxide of protein; but the properties of the fragments of the cell-walls, obtained by treating blood with water, do not by any means coincide with those of Mulder's binoxide of protein, for they are far more difficult of solution, both in acetic acid and the alkalies, than this substance; moreover, the binoxide of protein contains sulphur, of which I could detect no trace in these membranes. Indeed, Mulder has not attempted to prove the presence of this substance by direct experiments, but was merely led to this assumption by the following considerations: in their passage through the pulmonary capillaries, the blood-corpuscles become invested with a thick layer of this binoxide, through which (as through ground glass) the blood-pigment appears of a lighter red colour; and hence the brighter redness of arterial blood: moreover, the central depression of the coloured corpuscles seems to accord with this view; since the inflammatory crust, which, as is well known, contains much binoxide of protein, possesses a great tendency to form a similar depression or concavity.

"The cell-membranes of the corpuscles of one and the same blood have probably not a perfectly identical composition; at least, we observe that the coloured cells of the same blood are often very unequally affected by the same reagents: if we allow water, dilute acids, ether, or dilute alkaline solutions, to act on the blood-corpuscles, they are destroyed in very unequal degrees; some do not disappear even when the blood is very much diluted with water; and these we regard as the younger cells, while those which are easily destroyed are looked upon as older corpuscles; for it is believed that the capsule of the colourless blood-cells, from which the coloured cells at all events partly take their origin, retain for some time their former chemical nature, even after pigment has been formed in the cells. The cell-wall which disappears so rapidly from view, under the microscope, is actually dissolved in very few cases; it usually is only transformed into a gelatinous or rather a mucous state, in which the co-efficient of refraction is nearly the same as that of the plasma. We are entitled to draw this conclusion, not merely from the circumstance that saline solutions, tincture of iodine, &c., render the capsule again visible either in an integral state or in fragments, but also from the viscid and glutinous condition of the blood after the addition of certain substances, as, for instance, dilute organic acids, alkaline carbonates, iodide of potassium, hydrochlorate of ammonia, &c. On saturating with acids or alkalies blood which has been thus changed, or on adding to it a solution of iodine or of sulphate of soda, the cell-walls again become visible, and the blood at the same time loses its viscidity. Moreover, the above-named reagents do not reduce either the liquor sanguinis or the serum to that peculiar viscid state in which the fluid can be drawn out in threads; hence, this property must depend on the blood-corpuscles." (Vol. ii, pp. 174-5.)

We have already mentioned that a considerable part of the fat contained in the blood exists in the corpuscles. Berzelius many years ago expressed the belief, that the phosphorised fat existed principally in these cells, and this view has, to a certain degree, been confirmed by an analysis instituted by Lehmann, who found that the fat extracted by ether from the corpuscles of ox-blood yielded about 22 per cent. of ash, which had an acid reaction, and consisted essentially of acid phosphate of lime. It is worthy of remark, that the blood-corpuscles of arterial are poorer in fat than those of venous blood; thus, for instance, in the corpuscles of the arterial blood of the horse, Lehmann found only half as much fat as in the venous blood; the fat in the latter amounting to 3·595, and in the former to only 1·824 per cent. of the dried corpuscles.

In relation to the mineral constituents of the blood, the observations of Schmidt have furnished a reply to the important question:—Do certain salts especially accumulate in the blood, and if so, which are they? This chemist has shown, that the fluid contained in the blood-cells, (that is to say, the water within the corpuscles,) in addition to organic matters, contains a great preponderance of phosphates and potash-salts, so that the phosphate of potash, and the greater part of the chloride of potassium, pertains, as it were, to the blood-corpuscles, while, on the other hand, the chloride of sodium, and only a little chloride of potassium and phosphate of soda, are found in the plasma or liquor sanguinis (serum + fibrin). While in the plasma the organic matters are combined only with soda, in the blood-corpuscles the fatty acids and the globulin are combined both with potash and soda.

Schmidt has examined the relations between the potassium and sodium, and phosphoric acid and chlorine, in the blood-cells and in the plasma of various classes of the mammalia. His most important results are included in the following table, calculated for 100 parts of inorganic matters:

	Blood-corpuscles.		Plasma.		Blood-corpuscles.		Plasma.	
	K	Na	K	Na	PO_5	Cl	PO_5	Cl
Man (mean of 8 analyses) ...	40·89	9·71	5·19	37·74	17·64	21·00	6·08	40·68
Dog	6·07	36·17	3·25	39·68	22·12	24·88	6·65	37·31
Cat	7·85	35·02	5·17	37·64	13·62	27·59	7·27	41·70
Sheep	14·57	38·07	6·56	38·56	8·95	27·21	3·56	40·89
Goat	37·41	14·98	3·55	37·89	9·41	31·73	5·90	40·41

A glance at this table shows us, that the general differences between the inorganic constituents of the corpuscles and plasma are extremely marked; in the Carnivora the difference is most striking in relation to the acids, and in the Herbivora in relation to the alkalies. Schmidt believes that these relations are beyond the temporary influence of a change of food.

The earthy phosphates occur within the blood-cells, although in far less quantity (both relatively and absolutely) than in the plasma. While Schmidt found only 0·086 parts of the phosphates of lime and magnesia in the corpuscles of 1000 parts of blood, he found 0·332 parts in the plasma; or, in 1000 parts of blood-cells, the earthy phosphates amounted to 0·218, and in 1000 parts of plasma to 0·550.

The following quotation contains an excellent summary of the present state of our knowledge regarding the gases in the blood:

"The blood-corpuscles contain the principal portion of the gases in the blood; namely, carbonic acid, nitrogen, and oxygen. It has been proved by Davy, Nasse, Scherer, Van Enschut, Magnus, and others, that the capacity of the serum for absorbing oxygen and carbonic acid is far inferior to that of defibrinated blood; and I have convinced myself that at least twice as much air is developed from a volume of defibrinated blood as from an equal volume of serum which has been vigorously stirred, or shaken with atmospheric air. Van Maack has found that the solution of hæmatin possesses a considerable power of attracting oxygen, and Scherer has not only convinced himself of the accuracy of this observation, but has likewise ascertained that, after the absorption of oxygen, there is a slight development of carbonic acid.

"The question regarding the presence or absence of free gases in the blood was so far settled some ten years back by the experiments of Van Enschut, Bischoff, J. Davy, and especially of Magnus, that it was then regarded as proved, that it existed in a state of solution both in arterial and venous blood, when perfectly fresh. Magnus has recently, by more simple experiments, confirmed his former observation, that there occur, in addition to free carbonic acid, both free oxygen and nitrogen gases. According to his former experiments, arterial and venous blood contain nearly equal quantities of nitrogen; in the former, the volume of oxygen is to that of carbonic acid as 6 : 16, and in the latter, scarcely as 4 : 16; hence, relatively, there would be more oxygen in arterial than in venous blood. His more recent experiments determine not merely the relation of the volume of the gases to one another, but also to the volume of the blood: they show—at least in the blood of calves, oxen, and horses,—that from 10 to 12·5 per cent. (by volume) of oxygen, and from 1·7 to 3·3 per cent. of nitrogen are held in solution. According to an experiment of Magendie's, venous blood contains 78, and arterial only 66 per cent. of carbonic acid, calculating by volumes. The oxygen of the blood may also almost entirely be expelled *in vacuo*, or by means of other gases, as, for instance, hydrogen and carbonic acid; a fact affording sufficient evidence that it is only mechanically absorbed, and not chemically combined in the blood. Since the blood, according to the experiments of Magnus, is capable of absorbing one and a half times its volume (or 150 per cent.) of carbonic acid, it might, at first sight, appear strange, that the circulating blood is not found to be more impregnated with carbonic acid; and that in respiration the amount of oxygen which is absorbed only slightly exceeds the carbonic acid which is excreted; but when we consider, that in respiration the relations of the concurrent gases are altogether different from those in our experiments, (when we shake pure atmospheric air or pure carbonic acid with the blood,) this seeming discrepancy will at once disappear.

"It is an important question, in connection with the occurrence of free oxygen in the blood, and one, indeed, which is still not perfectly cleared up, whether, at all events, a portion of the oxygen which enters the blood through the lungs does not form chemical combinations in the arterial system with some of the constituents of the circulating fluid. Marchand attempted by certain experiments, and Magnus by a calculation founded on ascertained facts, to decide this question. Marchand thought, that if blood, free from carbonic acid, developed no carbonic acid under the direct influence of oxygen, the oxygen could only be mechanically absorbed; and, in point of fact, he found that neither fresh blood, blood-serum, the albumen of eggs, nor a solution of blood-corpuscles, after being freed from carbonic acid, developed the slightest trace of carbonic acid when a current of oxygen was passed through them; but, putting out of view the circumstance that Van Maack and Scherer have actually observed an exhalation of carbonic acid from a solution of hæmatin that had absorbed oxygen, Marchand's experiment merely proves that oxygen may be absorbed by the blood without necessarily giving rise to the formation of carbonic acid; it is, however, quite possible, that one or more of the constituents of the blood may be highly oxydised, without any separation of carbonic acid; for a development of this gas does not follow every oxydation of an organic body. The calculation made by Magnus would be more convincing, if the numbers on which it is based were not liable to such fluctuations. If, namely, in an adult man, *about* 13 cubic Paris inches enter the blood in one minute; and, further, if *about* 10 pounds of blood pass through the lungs in the same space of time; and, finally, if the blood of the horse contains *about* 11 per cent. of oxygen; it then follows, that *about* half the quantity of the oxygen of the venous blood in the lungs is absorbed, which Magnus had found in arterial blood; so that the latter has always lost about half its free oxygen in the capillaries. These facts prove, however, that the greater part of the oxygen absorbed in the lungs circulates in an uncombined state in the blood; but it does not appear to be indisputably proved that no portion whatever of the absorbed oxygen enters into chemical combination with one or more of the constituents of the blood in the heart and in the arteries; especially as we know that such a combination ensues immediately on entering the capillaries." (Vol. ii, pp. 180-2.)

We find nothing new regarding the colourless corpuscles. Lehmann simply observes, that the most recent investigations show that they are perfectly identical with lymph and chyle corpuscles; and, indeed, that no marked difference can be observed between them and mucus and pus corpuscles.

Passing over his remarks on the coagulation of the blood, the clot, the buffy coat, and the various appearances presented by the blood after death, we shall notice a few of his observations on the chemical constituents of the serum.

The albumen is the first of the constituents which he considers; and the first question to which he directs attention is, whether the albumen in different vessels, under different physiological relations, and in different pathological states, is always identical. He clearly shows, in his first volume, that there may be many modifications of albumen, consequent on its being associated with different quantities of alkali or salts, but that the group of atoms constituting the albumen always remains the same under these circumstances. These modifications, induced by an augmentation or diminution of alkali—that is to say, neutral, basic, and acid albuminate of soda—occur in the normal state in the blood of different vessels. The solution of neutral albuminate of soda becomes turbid on the addition of water; this compound occurs not merely in diseased blood, where it was first noticed by Scherer, but in the blood of certain vessels, as, for instance, the splenic vein. Independently of the other changes which the blood undergoes in the spleen, a part of the basic albuminate of soda is saturated by the free acid occurring in the pulpy matter of that organ, and the neutral compound is thus formed. The serum of the blood of the portal vein does not become so turbid, on the addition of water, as that of the splenic vein; while, on the other hand, that of the hepatic veins becomes extremely turbid, in consequence of a portion of the alkali being abstracted from the albumen of the portal blood to aid in the formation of bile. In a similar manner, we may satisfactorily explain how it is that albumen from different sources, and under different conditions, coagulates, on the application of heat, in very different forms.

Very little free fat is found in the serum; but much saponified fat, and the crystallisable lipoids, cholesterin and serolin, are always present. Phosphorised fat has not been detected with certainty in the serum.

Our knowledge regarding the extractive matters of the serum is still very imperfect, and our author has contributed nothing new on this head. He refers to the circumstance, that Schmidt has recently proved that sugar is one of the ordinary constituents of the blood of cattle, dogs, cats, and healthy and diseased men. Other substances occurring in the serum of normal blood are urea, uric acid, and hippuric acid; which latter has been found by Verdeil and Dollfuss in the blood of cattle. Since creatin and creatinine simultaneously exist in muscular flesh and in urine, it is obvious that they must occur in the blood, although their presence there has not yet been directly demonstrated. We have, as yet, no chemical means of ascertaining whether the biliary acids exist preformed in the blood; the probability, according to Lehmann, is, that they do not.

In relation to the blood-cells or corpuscles, our author shows, that in normal blood, under analogous physiological relations, they are liable to only slight variations. In an adult healthy man, we find, on an average, 512 parts of moist corpuscles in 1000 parts of blood; and, as neither the variations in excess or in deficiency exceed the number 40, we may take 552 and 472 as the limits within which the moist corpuscles must lie in the healthy blood of man.

The dried corpuscles have been estimated, by Prevost and Dumas, at 129 parts in 1000; by Lecanu, at 132·5; by Andral and Gavarret, at 127; by Richardson, at 134·8; by Becquerel and Rodier, at 141·1; by Nasse, at 116·5; by Popp, at 120; and by Scherer, at 112.

In the blood of women there are generally far fewer blood-corpuscles than in that of men; and the number sinks still lower during pregnancy, before the occurrence of menstruation, and invariably after its cessation during the critical or climacteric period. While Becquerel and Rodier found, (as we have just observed,) that in 1000 parts of man's blood, there were 141·1 of corpuscles, they found in an equal quantity of woman's blood only 127·2.

There are physiological reasons for believing that the blood in different vessels contains different quantities of corpuscles: thus, for instance, when the secretion of urine is proceeding with activity, a relatively larger quantity of blood-corpuscles should be found in the blood of the renal veins than in that of the renal arteries; and, in the same way, as the blood-corpuscles undergo essential changes in the spleen, it is natural to suppose that the corpuscles occur in different relative proportions in the blood of the splenic artery and the splenic vein. The general results of the analyses of Mayer, Hering, and Nasse, tend to show, that arterial blood contains fewer corpuscles than venous blood. C. F. Schmid found a much smaller quantity in the blood of the portal vein than in that of the jugular veins. Lehmann, on the other hand, found in the blood of the hepatic veins not only far more than in that of the portal vein, but also than in that of the jugular vein, the vena cava, or the splenic vein. Experimenting on the blood of a horse, which was fed four hours before it was killed, he found 743 per 1000 of moist corpuscles in the contents of the hepatic veins, 592 in those of the external jugular vein, 664 in those of the vena cava, 573 in those of the portal vein, and only 322 in those of the splenic vein. It is worthy of remark, that Popp found the number of the blood-corpuscles, and especially of the colourless cells, to be increased after the prolonged use of cod-liver oil.

The proportion of albumen in the serum is diminished in the following diseases: in simple ephemeral and remittent fevers (only slightly diminished), in severe inflammatory attacks supervening in the later stages of typhus, in scurvy (very considerably), in malaria, puerperal fever, dysentery, Bright's disease, and in dropsy from various organic affections. It is increased in intermittent fevers, in cholera, and after the abuse of drastic purgatives.

No subsequent chemists have added anything to the results attained by Becquerel and Rodier respecting the variations in the amount of fat in the serum in different diseases. Their results were, that, at the beginning of every acute disease, the fatty matters, especially the cholesterin, are increased; and that in chronic diseases, especially in those of the liver, inducing icterus and dropsy, and in Bright's disease, and tuberculosis, there is also an increase of the fat, and particularly of the cholesterin.

In his remarks on the salts contained in the serum, Lehmann notices the importance of accurately determining the relations in which the quantities of the mineral substances obtained by incineration stand to the quantities of the salts preformed in the blood, and of ascertaining in what manner the acids and the bases of the ash are grouped in the fresh serum. He gives the following as the average composition of the ash of the serum:

Chloride of sodium	61·087
,, potassium	4·054
Carbonate of soda	28·880
Phosphate of soda (2 NaO . PO_5) . .	3·195
Sulphate of potash	2·784
	100·000

The serum of man's blood contains usually a little more salts than that of woman's blood, the former averaging 8·8, and the latter 8·1 per cent. Moreover, arterial serum contains rather more salts than venous serum. Schultz, Simon, and Schmid, found far more salts in the blood of the portal than in that of the jugular vein (Schmid at least one half more); moreover, the serum of the portal blood contains much more salts than that of the blood of the hepatic veins; for while, in the case of the horse, they average ·850 (= 10 per cent. of the solid residue) in the former, they amount only to ·725 (or 7 per cent. of the solid residue) in the latter.

The observations of Poggiale and Plouviez show, that the use of food containing a large quantity of chloride of sodium increases the amount of that salt in the blood. Zimmermann observed, in five experiments on men and one on a horse, that during the same venesection, the last portion of blood contains a larger quantity of soluble salts, especially of alkaline chlorides, than the first portion. Little is known with certainty regarding the variations of the alkaline salts in the blood in different diseases, except that in severe inflammations they are very much diminished; and that, in the acute exanthemata and in typhus, they are very much increased: and that (as Schmidt has especially shown), they are much diminished in the serum of cholera blood, and increased in dysentery, Bright's disease, and all forms of dropsy and hydræmia; and, finally, (as the researches of Leonard and Salvagnoli show us,) that they are often doubled in quantity in various endemic diseases, as dysentery, malaria, malignant forms of intermittent fever, scurvy, &c.

The following observations are of high physiological value in relation to the chemistry of secretion:

"*Arterial blood* differs from *venous blood* in containing a smaller quantity of solid constituents pertaining to the blood-cells; they contain relatively more hæmatin and salts than the corpuscles of venous blood, but much less fat. The plasma of arterial blood is somewhat richer in fibrin than that of venous blood, and the serum of the former contains rather more water, and, consequently, rather less albumen. If we compare the solid residue of the serum of both kinds of blood in reference to their quantity of albumen, we find this very nearly equal in both, but there is a great difference in the quantities of the fats, extractive matters, and salts; the fats being very much diminished in the arterial fluid serum, and, consequently, in its solid residue, while the extractive matters are very decidedly, and the salts only slightly increased. Moreover, arterial blood contains relatively more free oxygen than venous blood.

"The *blood of the portal vein* differs in composition according to the stage and progress of the digestive process; during digestion, if drink has also been taken, it is richer in water and liquor sanguinis, and hence the number of the blood-corpuscles is comparatively small; the fibrin is a little, and the fat very considerably increased, and there is a moderate augmentation of the albumen, extractive matters, and salts; moreover, the fibrin during digestion behaves in just the same manner as that from other vessels, while at other periods it is less tenacious than usual, and forms merely a loose, easily disintegrated clot.

"As compared with the blood of the jugular vein, that of the portal vein is poorer in blood-cells as well as in solid constituents generally: the blood-cells present a flocculent appearance, and after their removal from the body seem jagged and distorted; they are richer in hæmatin and poorer in globulin, and contain twice as much fat as the cells of the blood of the jugular vein. The plasma contains fibrin which is rich in fat, but whose quantity is much smaller than in the blood of the jugular vein; the serum contains, on an average, less albumen, but more fat, extractive matter, and salts. The biliary substances cannot be found in this blood, and sugar only seldom.

"The blood of the hepatic vein presents a very different constitution from that of any other vessel: it is far poorer in water than that of the portal vein; assuming that the solid constituents were equal in both kinds of blood, the quantity of water in the latter is to that in the former, as 4 : 3 during digestion, and when not much drink has been taken; it is, however, often in the ratio of 12 : 5 after digestion has been fully accomplished. The clot of the blood of the hepatic vein is bulky, and readily breaks down; when 34 parts of serum are separated from 100 parts of portal blood, only 15 are separated from 100 parts of the blood of the hepatic vein. The blood of the hepatic vein is far richer in blood-cells, both coloured and colourless, than that of the portal vein; the colourless corpuscles occur in the most varied shapes and sizes; the coloured are seen in heaps of a distinct, violet colour, and their capsules are less readily destroyed by water than those of the blood of other vessels; while in the blood of the portal vein there are 141 parts of moist blood-cells to 100 parts of plasma, in the blood of the hepatic vein there are 317 parts of moist blood-cells to 100 of plasma. The cells in the blood of the hepatic veins are poorer in fat and in salts, and especially in hæmatin, or at least in iron, but somewhat richer in extractive matters. Their specific gravity is higher than that of the cells of the portal blood (notwithstanding the diminished quantity of iron). The plasma of the blood of the hepatic veins is far denser than that of the blood of the portal vein, for it contains a much larger amount of solid constituents generally, although little or no fibrin is to be found in it. While 8·4 parts of solid matter correspond to 100 of water in the serum of portal blood, there are 11·8 parts of solid matter to an equal quantity of water in the serum of the blood of the hepatic veins. If we compare the solid constituents of the serum of both kinds of blood, we find less albumen and fat, and far less salts, in the blood of the hepatic veins, while the quantity of extractive matter, including sugar, is perceptibly augmented. In three analyses of the solid residue of the blood of the hepatic vein in the horse, I found 0·635, 0·893, and 0·776 per cent. of sugar (the sugar [$=C_{12}H_{12}O_{12}$] being calculated from the carbonic acid that was developed when fermentation was established in the alcoholic extract by a little yeast), while in the residue of the corresponding portal blood I could only once detect sugar, which then amounted to 0·055 per cent.

The blood of the splenic vein, which as yet has only been analysed by Béclard, and compared with the jugular blood in horses and dogs, contains more water than the blood of the last-named vein. The mean amount of water in 14 analyses of the blood of the splenic vein in dogs=77·815 per cent., the limits being 74·630 and 82·681; the corresponding jugular blood contained, on an average, 1·608 less water than that of the splenic vein; in two parallel investigations on the blood of the horse, the contents of the splenic vein contained 0·4 to 0·5 more water than the jugular blood; the blood-corpuscles are somewhat diminished, but the fibrin and the residue of the serum are a little augmented in the blood of the splenic vein. Moreover, Ecker, in examining the blood of the splenic vein, found the cells containing blood-corpuscles, which were discovered by Kölliker in the fluid expressed from the spleen; they are abundant in the blood of the splenic vein in the horse, and from 1 to 5 blood-corpuscles, or small yellow granules, may be seen enclosed in a single capsule." (Vol. ii, pp. 248-50.)

In his remarks on the influence of venesection on the character of the blood, Lehmann notices the experiments of Nasse on healthy animals, and the observations of Becquerel and Rodier, and of Zimmermann on patients with various disorders. The specific heat, and the specific gravity of the blood are diminished, and the colour is of a brighter red; the blood coagulates sooner, but does not press out the serum so perfectly; the serum is turbid, and has a reddish or whitish tint; the red corpuscles are much diminished in number, and show a strong tendency to lie together in rolls; the colourless corpuscles, on the other hand, are increased in number; every bleeding causes a greater diminution in the amount of blood-corpuscles than in that of the solid constituents of the serum; in healthy animals venesection scarcely increases the quantity of fibrin; in disease its quantity appears to be altogether independent of the abstraction of blood. The blood-corpuscles are poorer in globulin, and consequently are comparatively richer in hæmatin. In reference to the differences presented by different portions of the blood during the same venesection, Zimmermann has made the singular observation, that after the loss of the first portion (say about four ounces), the solid constituents in the second portion are in no case increased, but always diminished in a nearly uniform manner; while on the other hand a third portion in many cases contains a larger amount of solid constituents than the second. The diminution in the amount of solid substances depends, according to Lehmann, on the active resorption of lymph, fluid exudations, and parenchymatous fluid, all of which have a lower specific gravity than blood.

Our waning space warns us that we must proceed without even a passing notice of our author's various sections on the constitution of the blood in different diseases, on the quantity of blood in the body, and on the formation, functions, and final destiny of the blood-corpuscles; nor can we do

more than simply mention, that he has contributed two excellent chapters on the *chyle* and the *lymph*.

We now arrive at a chapter on *Transudations*, from which we extract the following observations on the varying amount of albumen which they contain:

"The quantity of albumen in different transudations is extraordinarily variable; in some transudations, the amount is so small, that certain observers have believed it to be entirely absent, as, for instance, in the tears, in the aqueous humour of the eye, in the liquor amnii (in the last stage of pregnancy), in the fluid in the lateral ventricles and the spinal canal (in the normal and in the dropsical state), and in the fluid of the cellular tissue in œdema of the extremities. If, however, albumen is never entirely absent in these fluids, its amount in other freshly transuded fluids never reaches that contained in the serum of the blood. It now becomes a question, whether there are any conditions inducing a more copious or a diminished transudation of albumen through the walls of the capillaries, so that certain general rules, if not laws, can be established, in accordance with which there may be an augmentation or a diminution of the albumen in the transudation.

"The following is one of these rules:—*The quantity of albumen contained in a transudation is dependent on the system of capillaries, through which the transudation occurs.* We are indebted to the admirable investigations of Schmidt for our knowledge of this rule, which is equally important in the elucidation of the mechanical metamorphosis of matter and in the explanation of morbid processes; and at which he arrived by a series of carefully-conducted parallel analyses of normal and abnormal transudations. Schmidt assumes, that every group of capillaries transmits a definite and constant quantity of albumen into each transudation. He found the transudation in the pleura to be richest in albumen (= 2·85 per cent.); that in the peritoneum considerably poorer (= 1·13); that within the cranial membranes yet more deficient in this constituent (0·6, or at most 0·8); and that of the subcutaneous cellular substance the poorest (= 0·36). Schmidt found this relation of the albumen to the effused fluid in the transudations of the same individual, who was suffering from Bright's disease, and convinced himself by further investigations regarding the normal transudation of the cerebral capillaries and hydrocephalic effusions, that not only does the quantity of albumen always remain tolerably equal when there is an excess of the transudation, but also when, after the removal of the older effusion, a new transudation occurs through the same capillaries. In the normal cerebro-spinal fluid of a dog, Schmidt found 0·24 per cent. of organic matter; in chronic hydrocephalus of a child, before and after paracentesis, 0·18, and in acute hydrocephalus 0·37, 0·694, and 1·040; in a pleuritic transudation, obtained by paracentesis, 2·61; in that obtained after death from the same person, 2·85; in the first peritoneal transudation, obtained by paracentesis, 0·365; in the second, from the same person, 0·395. I collected 33·8 grammes [a little more than an ounce] of transudation from the pericardium of a perfectly healthy criminal within three minutes after his decapitation; it contained 0·879 per cent. of albumen, with 0·093 of other organic matter, and 0·089 of salts. In the fluid, in a case of hydropericardium *ex vacuo*, (in pulmonary tuberculosis,) I found 1·543 of albumen. In examining the dropsical transudations in the dead body of a drunkard with true and well-developed granular liver, I found, in the fluid of the pericardium, 1·063 per cent. of albumen; in that of the pleura, 1·852; in that of the peritoneum, 1·044; and in that of the cerebral ventricles, 0·564. In a peritoneal transudation, in a case of cancer of the liver, (where the liver extended two inches below the navel,) I once found 4·351 of albumen, besides 0·598 of extractive matters, and 0·890 of salts; while, on the other hand, in hydræmia, (consequent on chronic ulceration of the follicles of the large intestine,) I have found only 1·127 of albumen, with 0·448 extractive matters, and 1·014 salts. In the transudation from the cerebral capillaries, in hydrocephalus *ex vacuo*, (cerebral atrophy of an old man,) I found 0·144; in congenital hydrocephalus, 0·102; and in hydrocele, 6·283, 4·982, 4·055, and 3·410 per cent. of pure albumen.......

"If we compare the results of different analysts, it might seem at first sight that they are opposed to Schmidt's postulate, that the transudation of each individual group of capillaries has a special and a constant composition; but a closer examination of the relations accompanying these transudations, renders it tolerably evident that this proposition is certainly established; but that, like all natural laws, it is modified in its results or actions by other valid laws, and that thus its direct recognition is not very obvious. We can, therefore, only demonstrate this proposition, when we compare with one another the simultaneous transudations of different capillary groups *under identical conditions*...... The transudation is not the result of merely a single factor; it depends not only on the thickness or the delicacy of the capillaries, but on the rapidity of the current of blood, and on the constitution of the blood itself. Even if there were not sufficient positive facts to establish the position that the composition of the transudation from the same capillary system varies under different conditions, we might *à priori* conclude that, on the one hand, when the current of blood in the capillaries is very slow, and there is great distension of their walls, the composition of the transudation will be very different from what it would have been under opposite conditions, and that, on the other hand, its composition, and consequently its amount of albumen, will vary extremely with the varying physical and chemical characters of the blood." (Vol. ii, pp. 308-10.)

The following propositions, bearing on this point, may be regarded as established.

The capillaries appear to yield *a transudation which is rich in albumen in proportion to the slowness with which the blood traverses them.* In illustration of this principle, we may refer to the fact, that when the circulation in the abdominal veins is impeded by any large tumour, there is more albumen in the transudation than when there is only a slight mechanical impediment. When the disturbance of the blood in the capillaries is so great as to fall under the category of inflammatory hyperæmia, we have a transudation which is very rich in albumen; for, as a general rule, all fibrinous exudations are far richer in albumen than serous ones.

In reference to the constitution of the circulating fluid, it may be laid down, that *the poorer the blood is in albumen, the less albumen do we find in the transudation.* Thus Schmidt has found that in the accumulations of serum in Bright's disease, when the blood is always poor in albumen, in consequence of that substance being carried off in the urine, there is less albumen than in the transudations in dropsy from other causes.

Finally, when a transudation has stagnated for a very considerable time in a serous cavity without being resorbed or being artificially removed, as for instance often occurs in hydrocele and ovarian dropsy, the aqueous and saline constituents become partially resorbed, and thus the fluid becomes more concentrated and richer in albumen than such transudations usually are.

Passing without comment over the chapters on Milk, Semen, the Fluids of the Egg, Mucus, and the Cutaneous Secretion, we arrive at the chapter on the Urine, extending over between seventy and eighty pages, and terminating the Second Volume.

As we have already entered at some length into the consideration of several of the most important ingredients of the urinary secretion, our present remarks and extracts will be brief.

Normal urine contains fewer morphotic elements than any other animal fluid. A careful examination always reveals the presence of a little pavement epithelium; and a few mucus-corpuscles with a single lenticular nucleus may usually be detected. The *tubular or cylindrical bodies* so frequently met with in morbid urine, may be arranged, according to our author, under three classes. The tubes of the first class are those which appear to consist of the epithelial investment of the tubes of Bellini; they are tubes of a tolerably regular form, in which small cells and nuclei present somewhat of a honey-comb arrangement; these cylinders are seldom observed, except in the desquamative stage of acute exanthemata, and at the commencement of inflammatory affections of the kidney. The tubes of the second class consist of fresh exudation, which is formed in the tubes of Bellini, and retains their shape; they are cylindrical granular masses, enclosing blood-corpuscles and pus-corpuscles, and formed of fibrin; they are true croupous exudations, occurring in all the inflammatory diseases of the kidney which are included in the too general name of the acute form of Bright's disease. The tubes of the third class are hollow cylinders, with such transparent walls that they readily escape observation under the microscope, and are best observed by the use of a diaphragm; they are frequently broken, present fissures, and appear twisted round their axis; they occur only in the chronic forms of Bright's disease when there is decided fatty kidney; when treated with potash they disappear, leaving only a little granular matter; occasionally we observe in them an epithelial cell, or a fragment of one. Lehmann regards these tubes as consisting of the *membrana propria* of the urinary canals. They cannot be confounded with the croupous, fibrinous tubes of the second class.

Spermatozoa are frequently found, according to our author, in the urine of patients with typhus; in this disease they appear sometimes to make their way from the urethra into the bladder, for, after death, they are occasionally found on the vesical mucous membrane.

The *sarcina ventriculi*, discovered by Goodsir in the fluid ejected from the stomach in certain cases of pyrosis, has once been detected in the urine by Heller.

The following observations on the variations of the salts in the urine will, we believe, prove new to most of our readers. There are certain pathological conditions in which there is a great diminution in the quantity of the alkaline chlorides, namely, in all cases where copious transudations or exudations have been given off in a very short period from the blood; the diminution is often only perceptible when we examine the urine of twenty-four hours, and not when we test merely a single specimen. This condition of the urinary secretion is observed in acute dropsy, in the acute form of Bright's disease, in severe diarrhœa, in cholera, and in typhus. The diminution of the chlorides in inflammatory diseases with considerable exudations is often so great, that the addition of a solution of nitrate of silver scarcely induces more than a slight turbidity; it seldom, however, lasts for more than three days.

In regard to the alterations in the amount of the sulphates, Heller maintains, that whenever the number of the respirations is increased (especially in inflammatory diseases), there is a corresponding augmentation of the sulphates in the urine. Lehmann, however, in three sets of experiments which he instituted on the twenty-four-hours' urine of two persons with pneumonia and one with pleuritis, did not obtain results in accordance with Heller's statement. In these experiments the urine certainly contained a relative augmentation of the sulphates; that is to say, in 100 parts of the dense inflammatory urine, there were more sulphates than in the normal urine of lighter specific gravity, after recovery; but the whole amount of the alkaline sulphates excreted in twenty-four hours, was less in the former than in the latter case. Heller found the sulphates diminished in chlorosis, in neuroses, and in chronic diseases of the kidneys, and of the spinal cord; it is possible, however, that his observations refer to the relative and not to the positive diminutions. As the urine is always very aqueous in these forms of disease, there would, doubtless, be a smaller quantity of sulphates in 100 parts of such urine, than in an equal quantity of healthy urine; but as it is also generally abundant, it does not follow that there should be a positive diminution in the collected twenty-four hours' secretion; indeed, in a case of chlorosis, Lehmann found that the sulphates of potash and soda excreted in twenty-four hours, amounted to 103 grains (rather above than below the average).

The phosphates of lime and magnesia occur in healthy urine in very variable quantities; on a mixed diet about 18 grains are excreted daily by the kidneys. During a twelve-days' strict animal diet, Lehmann found that the earthy phosphates averaged no less than 3·562 per cent. of the solid residue; hence it is obvious that the quantity of this salt in the renal secretion varies extremely with the nature of the food. He confirms Donné's statement, that the amount of phosphate of lime is often, although not invariably, diminished during pregnancy, the decrease being the most marked between the sixth and the eighth month. According to Heller, the earthy phosphates are increased in rheumatism, and diminished in acute and chronic diseases of the spinal cord, in neuroses, and in acute and chronic diseases of the kidneys.

The following paragraph contains a summary of all that is known regarding the acid reaction of the urine:

"The cause of the *acid reaction* of normal urine long remained obscure. This acid reaction was formerly attributed to the presence of lactic acid, and even of acetic acid; Liebig has, however, investigated the subject, and shown that it can only be dependent on acid phosphate of soda. If we dissolve ordinary phosphate of soda in water (thus forming a solution with an alkaline reaction), and gradually add uric acid (which has no reaction on vegetable colours), and apply heat, we obtain a fluid which reddens litmus, and which, on cooling, deposits a white, crystalline powder, presenting under the microscope the most beautiful groups of prismatic crystals of urate of soda. Now, if so weak an acid as uric acid can abstract a portion of its base from phosphate of soda, there can be no doubt that stronger acids, as hippuric, lactic, and sulphuric acids, directly on their formation in the metamorphosis of animal tissue, convert the neutral phosphate of soda into an acid salt, in which form it passes with the sulphate, lactate, and hippurate of soda into the urine. If the acidity of normal urine generally can be explained in this manner, the fluid should never saturate more base than corresponds to its quantity of phosphate of soda. The experiments which have been made to elucidate this point, are, however, not so easy to conduct as might at first sight appear, for after treating urine with an alkali till there is neither an acid nor an alkaline reaction, it will still contain acid phosphate of soda in solution; for the neutral phosphate of soda has an alkaline reaction, and, therefore, the acid salt (if the urine exhibits no reaction on vegetable colours) is still not neutralised. I have endeavoured to ascertain the quantity of free acid in the urine in the following manner:—the urine was precipitated with an excess of chloride of barium, the precipitate boiled with water containing sulphuric acid, and the weight of the sulphate of baryta determined; an equal quantity of urine was then digested with freshly precipitated carbonate of baryta, till all acid reaction had disappeared; the filtered fluid was then acidulated with a little acetic acid, and precipitated by chloride of barium; this precipitate also was washed with water containing sulphuric acid, and weighed; the quantity of the latter was far less than that of the first-weighed sulphate of baryta, the difference of the weights corresponding to a quantity of sulphate of baryta whose base had been exactly sufficient to saturate the free acid contained in the urine; hence we can readily calculate from the chemical equivalents the quantity of the free acid or of the acid phosphate of soda. Now, if the calculation thus made did not give more acid phosphate of soda than was shown by a differently-made analysis to be actually contained in the urine, the acid reaction of the urine would be *alone* dependent on the acid phosphate of soda. This was certainly often the case; but both in healthy and in morbid urine I frequently met with the opposite condition, that is to say, in comparing the baryta-salts, the quantity of acid phosphate of soda which was calculated was more than that found by direct analysis; hence, in the majority of cases, in addition to the acid phosphate of soda, the urine must contain a free organic acid, or another acid salt capable of reddening litmus. We must not, however, arrive at our conclusions too rapidly, for the acidity of the urine after its discharge often increases so rapidly from the formation of lactic or acetic acid, that the excess of the free acids found in the above experiments might depend on lactic acid developed in the urine after its excretion. In morbid urine, however, we often find so great an excess of free acid over the phosphate of soda, that to these cases the above objection cannot be applicable. The acid reaction of the urine depends, therefore, in many cases, not only on the presence of acid phosphate of soda but also on the presence of hippuric and lactic acids. If there were only acid phosphate of soda present, the phosphates of lime and magnesia in the urine could only be held in solution either as acid phosphates or by another free acid. If in the above calculation of the free acid from the precipitated baryta-salts, the earthy phosphates are included in the weighing, the result always remains the same, that is to say, there is more free acid than could be derived from all the acid phosphates of the urine. The water-extract of the urine usually has an acid reaction after repeated washings with alcohol, and solely on account of its containing acid earthy phosphates; these must, however, also be present when lactic or hippuric acid is the acidifying principle of the urine." (Vol. ii, pp. 398—400.)

We now proceed to consider those constituents of the urine which have their origin out of the body, those, namely, which are not derived from effete tissues, or a superabundance of nitrogenous matter to the blood. Wöhler, many years ago, devoted much attention to this subject, and ascertained that, as a general rule, those substances only passed into the urine which were readily soluble in water, and had no tendency to form insoluble compounds with the organic or inorganic materials of the animal body; as for instance, nitre, borax, iodide of potassium, bromide of sodium, the alkaline silicates, chlorates, carbonates, &c. In order that a substance may pass unchanged into the urine, it must, however, not only possess the properties we have mentioned, but likewise that of being either perfectly oxidised or having no tendency to oxidation and decomposition; thus, for instance, sulphuret (or as it is now the fashion to call it *sulphide*) of potassium is a very soluble substance, and has no tendency to form insoluble compounds with the materials of the animal body, but it is so easily oxidised that it does not enter the urine as sulphuret of potassium, but as sulphate of potash, when it is not given in excessive doses. Many substances which form comparatively insoluble compounds with animal matters, as for instance the albuminates, can only be detected in the urine when they have been given in very large doses. Orfila, who performed a large series of experiments on the lower animals, ascertained that metals, which under ordinary circumstances are not eliminated by the kidneys, as for example, gold, silver, lead, bismuth, antimony, and arsenic, may be traced in the urine after the administration of very large doses.

It has long been known that the quantity of oxalate of lime in the urine is increased by the use of drinks abounding in carbonic acid; we do not, however, recollect that we have ever seen it remarked, that there is so great a simultaneous augmentation of free carbonic acid in the urine, as appears to be the case from the following experiments. After taking champagne, Lehmann found that it evolved 53 per cent. of its volume of this gas, and after the use of very effervescing beer, 68 per cent.

The alkaline carbonates appear unchanged in the urine, although a portion of them must have been saturated by the acid secretions of the stomach and intestines. It has not been definitely ascertained how much alkaline carbonate is necessary to render the urine neutral or alkaline; from some experiments made by Buchheim, it appears that the nature of the diet and the general mode of life modify to a great extent the amount required. Iodine rapidly combines with the alkalies, and usually appears as iodide of sodium. The soluble baryta-salts, although so easily decomposed by sulphates, carbonates, and phosphates, were detected by Wöhler when given in sufficiently large doses. Ferridcyanide of potassium appears in this urine as ferrocyanide. Sulphocyanide of potassium, when given in small doses, is soon detected unchanged. Most of the organic acids, when given in a free state, pass unchanged into the urine; this has been established in the case of oxalic, citric, malic, tartaric, succinic, gallic, and salicylous acids. Tannic acid is converted in its passage through the organism into gallic acid, and benzoic and cinnamic acid into hippuric acid. It is singular that cuminic acid ($C_{20}H_{11}O_3 . HO$), which is so closely allied to benzoic acid ($C_{14}H_5O_3 . HO$), does not, like that substance and cinnamic acid ($C_{18}H_7O_3 . HO$), enter into combination with nitrogenous matters in the body, but passes unchanged into the urine.

Wöhler and Frerichs have proved by experiments on men, dogs, and rabbits, that uric acid, when introduced into the stomach or injected into the veins, undergoes precisely the same changes in the animal body as when decomposed by peroxide of lead; the urine after every such experiment becoming far richer in urea and oxalate of lime.

Every one is (or ought to be) acquainted with the most important of the discoveries made by Wöhler in his earlier series of researches; namely, that the neutral salts formed by the alkalies with vegetable acids, become oxidised in the animal organism, exactly as if they had been burned in oxygen. Lehmann has investigated this subject with considerable attention, as he finds that in many persons living on a mixed diet, the urine becomes alkaline in two or three hours after taking half a scruple of acetate of soda, whilst in those living on a purely vegetable diet, two drachms of the same salt never rendered the urine alkaline. From numerous experiments on healthy persons, and observations on patients who were taking this class of remedies, he has arrived at the conclusions, that if these salts exert a purgative action, the urine only becomes very slightly alkaline, and, indeed, most commonly remains acid; that on an animal diet the urine does not become so readily alkaline as on a vegetable (or antiphlogistic) diet; and that when patients with febrile affections pass a very acid urine, this fluid does not very readily become alkaline from the use of these medicines. In the same person living on the same food, equal doses may render the urine alkaline when he remains quiet, but have no apparent effect on the reactions of the secretion if he take strong bodily exercise.

Quinine may be easily recognised in the urine, if given in a tolerably full dose. The experiments of Wöhler and Frerichs show that urea passes unchanged into the urine. Theine and theobromine could not be detected in the urine; but they caused a considerable augmentation in the quantity of urea, probably in consequence of their stimulating effect on the vascular and nervous systems. Aniline could not be detected. These, as far as we know, are the only organic bases with which experiments of this nature have been made.

Alloxantin appears, from the investigations of the above-named chemists, to be converted in the animal body into urea and other substances; they could find neither this substance nor alloxan in the urine of persons who had taken five or six grains of it. Allantoin does not pass into the urine, nor does it augment the quantity of oxalate of lime, as might have been expected from its artificial conversion by alkalies into oxalate of ammonia. Amygdalin and asparagin have not been detected; salicin undergoes the same change as if it had been submitted to the action of oxidising agents, being converted into salicylous acid (hydride of salicyl.) Volatile oil of bitter almonds (free from prussic acid) appears to change (without giving rise to any symptoms of poisoning,) first into benzoic acid, and, finally, to appear as hippuric acid in the urine. Benzoic ether also increases the quantity of hippuric acid.

In his consideration of the substances which only occur in morbid urine, Lehmann duly notices the presence of albumen, fibrin, casein, fat, sugar, abnormal pigments, biliary acids, bile-pigments, xantheine (uric oxide,) cystine, sulphuretted hydrogen, butyric acid, the ammonia-salts, (hydrochlorate of ammonia, carbonate of ammonia, ammonio-phosphate of soda, and ammonio-phosphate of magnesia,) &c.; this Section is followed by three others, on the analysis of the urinary secretion:—on the influence of physiological conditions on the urine; on the urine of animals; and the modification which the urine undergoes in different forms of disease. In each of these Sections there is much valuable matter to which we would direct the attention of our readers, did our limits permit of our doing so.

In concluding our notice of the first two volumes of this work, we can conscientiously assert that it presents as complete a view of the general subjects of which it treats, as the present state of our knowledge permits. Our author informs us that the third and concluding volume is almost ready for publication, and we look forward with much interest to its appearance. It will treat "of the relation of vital agencies to chemistry, of digestion and respiration, of the mechanical and chemical metamorphoses of tissue, of nutrition, &c.," and will conclude with "the consideration of pathologico-chemical processes." We are happy to learn that this important treatise will shortly appear in an English dress, under the auspices of the Cavendish Society; which is establishing a strong claim on the support of the medical profession, by the issue of publications like the present.

ART. III.

1. *Pharmacopœia Collegii Regalis Medicorum Londinensis.—Londini,* 1851. 8vo, pp. 231; 32mo, pp. 255.
2. *The Pharmacopœia of the King and Queen's College of Physicians in Ireland.—Dublin,* 1850. 8vo, pp. 236.
3. *A Translation of the New London Pharmacopœia; including also the New Dublin and Edinburgh Pharmacopœias; with a Full Account of the Chemical and Medicinal Properties of their Contents; forming a complete Materia Medica.* By J. BIRKBECK NEVINS, M.D. Lond., M.R.C.S., and L.A.C., &c.—*London,* 1851. 8vo, pp. 780.
4. *A Compendium of Materia Medica and Pharmacy, adapted to the London Pharmacopœia; embodying the New French, American, and Indian Medicines, and also comprising a Summary of Practical Toxicology; with the Abbreviations used in Prescriptions.* By J. HUNTER LANE, M.D. F.L.S. F.S.S.A. Second Edition.—*London,* 1851. 12mo, pp. 341.
5. *Medicines, their Uses and Mode of Administration; including a complete Conspectus of the Three British Pharmacopœias, an Account of all the New Remedies, and an Appendix of Formulæ.* By T. MOORE NELIGAN, M.D. Edin., M.R.I.A., &c. Third Edition.—*Dublin,* 1851. 8vo, pp. 582.
6. *The Elements of Materia Medica and Therapeutics.* By JONATHAN PEREIRA, M.D. F.R.S. & L.S., &c. Third Edition, enlarged and improved, including Notices of most of the Medicinal Substances in Use in the Civilised World, and forming an Encyclopædia of Materia Medica. Vol. II, Part I.—*London,* 1851. 8vo, pp. 662.
7. *The New London Pharmacopœia, translated and arranged in a Tabular Form, with the Edinburgh and Dublin Pharmacopœias, showing at one View the Differences in the Formulæ of the Three Colleges, together with the Tests given by each College for the Purity of several Preparations; with Practical Remarks.* By PETER SQUIRE.—*London,* 1851. Royal 8vo, pp. 199.

ALTHOUGH we consider it to be our special vocation to direct the attention of our readers to those facts and principles which constitute the *science* of our Profession, or which may serve as its foundation, yet we would by no means neglect those matters of detail which are of importance in its practice; and there are occasions on which it becomes necessary to bring these into prominence. Such an occasion is furnished by the recent publication of new editions of two out of three of our national Pharmacopœias; and we believe that we shall be doing our readers an essential service, as well as discharging our own critical duty, if we subject these to a careful examination, and point out the chief modifications which have been made in them; whilst at the same time we may notice certain other works which are more or less closely related to them.

London Pharmacopœia.—As the London College of Physicians had shown much disposition, in the formation of their Pharmacopœia of 1836, to keep up with the rapid strides then recently made in chemistry, expectations were raised that many advancements and improvements would be found in the present edition. These, however, have not been realized, for the work before us is not found to be at all in advance of the former, when considered in respect to the chemical processes contained in it; but although no progress has been made in this direction, yet it by no means follows that the new Pharmacopœia is not an improvement on that of 1836. Let us first consider what is the object of a Pharmacopœia, and what changes we should expect to be made in successive Editions; and then, after passing the alterations in review, we shall be able to judge of its merits and defects.

A century ago, or even less, when Chemistry was in its infancy, when a knowledge of the science was confined to few, and when the vendors of drugs were comparatively ignorant of the nature and composition of the remedies they sold, it was essential that a Pharmacopœia should contain minute directions for the preparation of almost every substance which was not found in nature in a condition fit for administration; and full and explicit details were, therefore, exceedingly desirable. But at the present time,—when almost every druggist has the means of acquiring a competent knowledge of Chemistry, Botany, Materia Medica, and Pharmacy,—when it is expected that he shall possess such knowledge,—when many remedies are prepared on a large scale by manufacturing Chemists, of much better quality, and at a cost much lower, than those which the retail Druggist or the General Practitioner would prepare for himself,—when many proximate principles are employed, which a General Practitioner has neither the time nor the facilities for preparing, even if he possess the knowledge and tact, which can frequently be acquired only by long practice,—when, too, the dispensing of medicines is often left, even by the Apothecary, to the Druggist,—a Pharmacopœia should possess qualifications very different from those which were formerly essential to it. We believe that, in general, the processes given in the Pharmacopœia for such preparations, are of little use to the Medical Practitioner; and to be of service to the Druggist or Manufacturer, they require to be such as the College of Physicians have no power of imparting. We do not mean this assertion as disparaging to the College; for it cannot be expected that Physicians, unless they have specially devoted themselves to the subject, should be in possession of such knowledge; nor do we think it at all necessary for the successful practice of their profession.

The remarks we have just made refer more especially to the preparation of Alkaloids, Æthers, and like bodies; and not to the common metallic salts, which are of comparatively easy formation, and require little previous initiation. We think, therefore, that in a Pharmacopœia at the present day, unless its authors are able to give processes really valuable to the manufacturer, these substances should be placed in the list of the Materia Medica; that the work should contain directions for making the various compounds, as Tinctures, Pills, Powders, Plasters, Extracts, &c., so that they may be prepared, as nearly as possible, of uniform strength; that certain directions or tests should be given, (whenever a drug is liable to adulteration, intentional or otherwise, or to undergo change,) by which the dispenser may be enabled to ascertain the purity of the preparation he is compounding; lastly, in a country where different weights and measures are employed, it is necessary that it should specify what weights and measures shall be used, both in the compounding and dispensing of medicines. Were we to possess one Pharmacopœia for the United Kingdom, constructed on this principle, we cannot help thinking that we should be far better off than we are at present, in the possession of three Pharmacopœias, with different weights, (formerly different measures,) with preparations bearing the same name, but of different strengths, (so that a harmless dose of a medicine, made according to one Pharmacopœia, may be almost poisonous, if prepared according to the directions of another,) with the same substances differently named in the three Pharmacopœias, and placed in the Materia Medica of one, and among the Preparations of another; and, lastly, with classifications entirely dissimilar in character.

The same weights and measures are employed in the new as in the old London Pharmacopœia; and the directions for preserving medicines from injury, by contact with the vessels in which they are kept,—for saturating acids and alkalies,—for filtration,—for measuring the degree of heat to be employed,—for taking specific gravities,—for the use of crucibles, water and sand-baths,—remain the same, in effect, in the new as in the old edition. The only addition to this part, is a short direction as to washing and drying precipitates, and avoiding the use of excess of the precipitant, lest the precipitate should be redissolved.

At the commencement of Part I, there is a description of the method of gathering and preserving vegetable substances, very similar in effect to that which, in the last edition, was included in the section *Vegetabilia*.

We next come to the Catalogue, or list of substances to be used in medicine, and which are either found native, or of which the preparation is not given in the Second Part of the work. Here more additions have been made, than omissions. On minute examination, however, it will appear, that the increase does not depend so much on the augmentation of the number of remedies, as on the removal of many from among the Preparations, and placing them in the Materia Medica.

List of Substances contained in the List of Materia Medica of the Pharmacopœia of 1836, *but removed from that of* 1851.

Acetosella
Acorus
Allium
Althææ folia
Amygdalæ amaræ
Argentum
Asarum
Aspidium
Aurantium
Aurantii flores
Barytæ carbonas
Bergamii oleum
Brominum
Calamina (*replaced by* Calamina præparata)
Cardamine
Centaurium
Conii fructus
Contrajerva
Creta (*replaced by* Creta præparata)
Cupri sulphas (*replaced by* Cupri sulphas venalis)
Curcuma (*put in Appendix*)
Dauci fructus
Digitalis semina
Euphorbium
Ferri sulphas (*replaced by* Ferri sulphas venalis)
Ferri percyanidum
Juniperi cacumina
Lacmus (*put in Appendix*)
Lactucarium
Lavandula
Lauri folia
Limones
Malva
Marmor
Marrubium

Menyanthes
Mora (*replaced by* Mori succus)
Olibanum
Opoponax
Origanum
Plumbi carbonas
Porrum
Potassæ carbonas impura (*replaced by* Potassæ carbonas)
Quina
Rhamnus (*replaced by* Rhamni succus)
Rosmarinus
Rumex

Sabadilla
Senna (*replaced by* Senna Alexandrina *and* Senna Indica)
Simarouba
Sodæ acetas
Sodæ carbonas impura (*replaced by* Sodæ carbonas)
Spigelia
Stannum
Succinum
Terebinthina canadensis
Testæ
Toxicodendron
Tussilago.

From the above list, it will be seen, that from fifty to sixty substances have been removed from the Pharmacopœia. We cannot regret the disappearance of the greater number of these, as they were but seldom employed; many, as Acetosella, Cardamine, Allium, Porrum, Opoponax, Olibanum, Succinum, Testæ, &c., being mere incumbrances, and others we could name being little better. There are a few, however, of whose removal, until further trials had proved their inutility, we question the propriety. The use of male fern (Aspidium) has recently been revived, and by some practitioners it is considered to be of very great value, as an anthelmintic; equal, even, to the now fashionable Kousso. In Dr. Neligan's work on 'Medicines,' under the head of Aspidium, it is stated: "The powder of the male fern root is, perhaps, one of the most efficacious anthelmintics we possess in the treatment of tænia; and, as an indigenous remedy, it is especially worthy of attention."

Again, the omission of Bromine and Bromide of Potassium from the Pharmacopœia was rather a hasty step; these substances having enjoyed but a short elevation to the rank of remedies deemed worthy of notice by the London College. They were introduced, we presume, on account of the statements of Dr. Williams, of St. Thomas's Hospital, as to their efficacy in removing enlargements of the spleen; perhaps their introduction into the work was premature; but being there, it would have been well to have allowed them to remain for a time longer, in order to give a greater chance of their real action on the system being more thoroughly investigated. Dr. Williams's name has, to the sorrow of the profession, disappeared from the College list of Fellows; and his remedy has now been removed from the list of their Materia Medica. *Sic transit gloria mundi!*

List of Substances added to the List of Materia Medica in the present Pharmacopœia.

Those marked with an asterisk were contained among the Preparations of the Old Pharmacopœia.

*Acidum aceticum
* — benzoicum
* — citricum
— gallicum
* — hydrochloricum
* — nitricum
— tannicum
* — tartaricum

*Æther
Aloe barbadensis
— hepatica
*Ammoniæ liquor
* — sesquicarbonas
*Anethi oleum
*Anisi oleum
*Anthemidis oleum

*Aqua distillata
*Argenti nitras
Atropia
*Aurantii floris aqua
*Calcii chloridum
*Calx chlorinata
*Carui oleum
*Cornu ustum
*Fœniculi oleum
Granati radix
*Lactucæ
*Lavandulæ oleum
*Menthæ piperitæ oleum
* — viridis oleum
*Morphiæ acetas
* — hydrochloras
Morrhuæ oleum
Panis
*Pimentæ oleum
*Plumbi acetas
*Potassæ bicarbonas
* — sulphas
* — tartras
*Potassii iodidum
* — sulphuretum
*Pulegii oleum
*Quinæ disulphas
*Rosmarini oleum
Rutæ oleum
Sabinæ oleum
Silex contritus
Sinapis alba
*Sodæ bicarbonas
* — sulphas
* — potassio-tartras
*Strychnia
Sulphur præcipitatum
*Veratria
Viola
*Zinci sulphas.

It appears from this table, that the additions made by the College to the number of our remedies are very few indeed, but for the most part valuable. No one who has employed Tannic and Gallic acids in the treatment of hæmorrhages, will doubt of their efficacy, and of their superiority over the crude drugs containing them. Atropia, also, is a good addition; for, as an elegant substitute for Extract of Belladonna, in eye affections, it is well worthy of being introduced into the Pharmacopœia; and we doubt not but that its use will be extended much further. Granati radix has been introduced as an anthelmintic; it is contained also in the Edinburgh and Dublin lists.

Oleum Morrhuæ has claims which it would have been difficult to pass over; but we think that some test of its quality should have been given, as it is very liable to adulteration. It exhibits a peculiar reaction, not possessed by other oils, at least not by any but fish *liver* oils; we allude to the effect of Oil of Vitriol in producing the beautiful lake colour. A few other remedies might, perhaps, have been introduced with advantage; as one or more Valerianates, perhaps Berbeerine, &c.

Having investigated the changes in Part I, we shall now consider the second portion of the work, or that treating of the Preparations and Compounds. No alteration in the arrangement has been made; nor do we think that a more useful one could have been adopted, the different divisions or sections being placed in alphabetical order.

Acida.—Among these, Acidum Aceticum Dilutum is an addition; whilst Acidum Aceticum, A. Benzoicum, A. Citricum, A. Hydrochloricum, A. Nitricum, A. Tartaricum, have been removed to Part I. Dilute Acetic Acid has been introduced, on account of the alteration in the preparation of Acetum Colchici and Acetum Scillæ, these being made with this preparation in place of distilled vinegar. We find, also, that the dried corm of the colchicum, and the dried bulb of the squill, have been ordered, instead of the corresponding parts of the plant in their fresh state; but, of course, in smaller quantities.

The removal of the six acids was judicious, as they can be procured very cheaply, and sufficiently pure for medical use; tests to ascertain their quality are given in Part I.

In examining the dilute acids, as Acidum Sulphuricum Dilutum and A. Nitricum Dilutum, we notice some alterations in their strength; but the difference is not important, and by no means so great as might, at first, be imagined from the change of the formulæ. Thus, in Acidum Nitricum Dilutum of 1851, there are three fluid ounces of nitric acid in twenty of the dilute acid; in the dilute of the old Pharmacopœia only two ounces were contained in the like quantity; but the specific gravity of the present strong acid is 1·42, and 100 grains saturates 161 grains of crystallized carbonate of soda; whilst that of the Pharmacopœia of 1836 was 1·50, and 100 grains of it saturated 217 grains of the same salt.

In the process for the preparation of Acidum Hydrocyanicum Dilutum, from Ferrocyanide of Potassium and Sulphuric Acid, the amount of the acid is much diminished; the product, however, is of the same strength, and contains 2 per cent. of real acid. A second process for obtaining this preparation extemporaneously, by the action of Dilute Hydrochloric Acid upon Cyanide of Silver, is altogether omitted.

Ætherea, Ethers.—The changes under this head are as follow:—the introduction of a process for Chloroformyl or Chloroform; the transfer of Sulphuric Ether to the Materia Medica; the change of name of Spiritus Ætheris Sulphuricus Compositus into Spiritus Ætheris Compositus; and the diminution of the amount of Sulphuric Acid in the process for procuring the Oleum Æthereum. The introduction of Chloroform into the Pharmacopœia was imperatively called for; experience having proved, far beyond doubt, its real efficacy as an anæsthetic agent, and the value of its administration in surgical operations, and in the treatment of disease. The process given for its preparation is a good one; and it is ordered to be purified by shaking with Chloride of Calcium for an hour, and then redistilling; Sulphuric Acid is generally employed for this process, and by some considered necessary. A few tests of its purity are added. It is stated that its specific gravity should not be less than 1·48; and that it should not redden litmus paper, &c. It might have been advantageously ordered to be kept from the influence of light; as, by exposure to this agent, it is rendered liable to decomposition, and is then unfit for use as an anæsthetic agent. This decomposition is indicated by the litmus paper showing a strong acid reaction.

The omission of the process for making Ether was judicious, as this body cannot be advantageously prepared by the medical practitioner or retail druggist, and it can readily be procured, and of sufficient purity,—some tests for which are to be found in the Materia Medica. Why the process for the Oleum Æthereum is retained, we are at a loss to imagine, as it is a product more difficult to procure than Ether; and we are informed that the process now given is by no means an advantageous one. The change of name of the Compound Æther we have already alluded to. In the process for Spiritus Ætheris Nitrici, the proportions of the ingredients are rather different from those of the previous formula; but the product remains the same.

Alkalina, Alkaline remedies.—Under this head we find very considerable changes in the new Pharmacopœia, arising from the omission of the processes for preparing several alkaloids, and the transposition of Liquor

Ammoniæ, Ammoniæ Sesquicarbonas, Morphiæ Acetas, Morphiæ Hydrochloras, Quinæ Disulphas, Strychnia, and Veratria, to the list of the Materia Medica. Aconitina and Morphia are removed altogether from the Pharmacopœia. Liquor Ammoniæ Citratis, Liquor Morphiæ Acetatis, Liquor Morphiæ Hydrochloratis, and Atropiæ Sulphas, are introduced; and Liquor Ammoniæ Acetatis is now ordered to be made with Dilute Acetic Acid, in place of distilled vinegar.—With regard to the omission of the processes for making the alkaloids, we think the College has acted wisely; for the difficulty of their preparation is very great; and we question the ability of the College to give methods sufficiently good for their advantageous separation. The details of the processes are not unfrequently kept secret by the manufacturers; and it is an acknowledged fact, that many of those contained in the last edition of the Pharmacopœia were far from being such as could be advantageously employed.

Why has Aconitina been omitted, and, at the same time, another alkaloid, Atropina, been introduced? If the former was omitted on account of its intense energy, why should another, also possessed of very great power, have been introduced? Aconitina was used chiefly, almost entirely, we believe, as an external agent; its intensity, therefore, was not against its being placed in the Pharmacopœia; and it is well known, that its use has been of late years gradually increasing, the limitation of its employment depending only on the very high price of the article. Even this objection will soon be overcome; for it could be produced in a very pure form, and at a much more moderate price, if the demand should continue. The value of the alkaloid, as an external agent, in painful nervous affections, has been placed beyond doubt; and many of the first practitioners of this country are in the habit of frequently prescribing it. Atropia has been placed in the Materia Medica; and its sulphate is ordered to be prepared by neutralising Dilute Sulphuric Acid with the alkaloid, evaporating and crystallising. Atropia itself can be procured in a crystalline form, and is sent over to this country in a state in which it would be difficult to adulterate it; whereas the Sulphate is a salt difficult to crystallise, and, in fact, is generally procured in the form of a powder made from the evaporated gummy mass. We think, therefore, that it would have been sufficient to have given directions to dissolve the alkaloid with the aid of some acid, so as to form a solution, rather than have placed in the Pharmacopœia a salt of the character above described. If a salt of Atropia is required, why not one of Strychnia? which is far less soluble in water than Atropia, and the sulphate of which readily crystallises.

Solutions of Acetate and Hydrochlorate of Morphia are very properly introduced, each solution containing eight grains of the salt to the fluid ounce of the preparation. We think that a solution of Strychnia would have been exceedingly useful, as the dose of the alkaloid is small, and the probabilities of its being accurately dispensed (a consideration of the highest moment) would have been thus much increased. Liquor Ammoniæ Citratis is an elegant form of administering a salt of Ammonia, being much more palatable than the corresponding Acetate.

The division *Animalia* is now omitted: of the three preparations formerly contained in it, Carbo Animalis Purificatus and Testæ Præparatæ are banished, and Cornu Ustum removed to the Materia Medica.

Aquæ, Waters.—Aqua Distillata is removed to the List; Aqua Fœniculi and Aqua Florum Aurantii are omitted; a second process is given for preparing the Aqua Anethi from the oil; and powdered *silex* is ordered to be employed in place of Carbonate of Magnesia, when the oils are used in the preparation of the waters. This substance possesses the advantage of not absorbing the oil, but only acting as a mechanical agent.

Cataplasmata, Cataplasms.—Cataplasma Carbonis and Cataplasma Sodæ Chlorinatæ have been introduced, and boiling water substituted for the vinegar in the preparation of the Cataplasma Sinapis. Wood charcoal is employed in the Cataplasma Carbonis; but, as we suppose it has been introduced on account of the absorbing powers of carbon for noxious matters, would it not have been better to have ordered Carbo Animalis, which possesses, in a very exalted degree, all the valuable properties of wood charcoal? The substitution of water for vinegar, in the Cataplasma Sinapis, is a good alteration; the generation of the essential oil of the mustard, which is the active agent in the cataplasm, being favoured by the former more than by the latter fluid. We question the propriety of using boiling water, as the catalytic action of the albuminous principle of the mustard is apt to be injured by too high a temperature.

Cerata, Cerates.—Ceratum Saponis is called Ceratum Saponis Compositum; and Ceratum Sabinæ of 1836 is now elevated to the rank of an Unguent.

Confectiones, Confections.—The alterations here are confined to the names. Confectio Piperis Nigri and Confectio Rosæ Gallicæ are now called Confectio Piperis and Confectio Rosæ.

Decocta, Decoctions.—Decoctum Cinchonæ Cordifoliæ is simply Decoctum Cinchonæ; and the Decoctum Cinchonæ Lancifoliæ and Decoctum Cinchonæ Oblongifoliæ have their names altered to correspond with the changes in the names of the barks. In Decoctum Dulcamaræ, ten drachms of woody nightshade are ordered to be employed for each pint, in place of eight. Decoctum Gallæ, Decoctum Granati Radicis, Decoctum Pareiræ, and Decoctum Taraxici, have been introduced; and Decoctum Malvæ Compositum and Decoctum Veratri have been omitted. These changes are, doubtless, of advantage, if Pareira is valuable, and we have statements of a most eminent surgeon in its favour; the Decoction is one of the best forms of administration; and, therefore, the replacement of the Infusion by the Decoction is a good change. The same remarks apply to the other new Decoctions; and few will regret the omissions that have taken place.

Emplastra, Plasters.—In Emplastrum Ammoniaci, dilute acetic acid is ordered in place of distilled vinegar; in Emplastrum Belladonnæ, the resin plaster is replaced by soap plaster, and the amount of Extract of Belladonna is made equal to that of the plaster employed, instead of being half the amount. Emplastrum Ceræ is omitted; and in Emplastrum Cantharidis, the ingredients of the old Emplastrum Ceræ are separately employed in the same proportions. In Emplastrum Galbani, a little less turpentine is used; in Emplastrum Opii, the extract of opium is ordered instead of powdered hard opium; and the amount of this drug is more than doubled. In Emplastrum Saponis a little resin has been added. We find that three new plasters have been introduced; viz. Emplastrum Cumini (a revival from an old Pharmacopœia), Emplastrum Ferri, and Emplastrum Potassii Iodidi. The object for reintroducing the first of these, composed of Cummin, Carraway, and Bayberries, with Burgundy pitch, wax, and oil, we cannot imagine; for surely any effect which it can produce might be obtained by the employment of Emplastrum Galbani or Emplastrum Ammoniaci, or even by the Emplastrum Picis. The Emplastrum Ferri is similar to that in the Dublin and Edinburgh Pharmacopœias, which formerly, by the Dublin College, was called the Emplastrum Thuris. If the Iodide of Potassium, applied in the form of a plaster, is able to produce any of its peculiar resolvent effects, then the introduction of the Emplastrum Potassii Iodidi into the London Pharmacopœia is a valuable addition.

Enemata, Enemas.—In Enema Colocynthidis, half a drachm of the simple Extract of Colocynth replaces two scruples of the old Compound Extract; in Enema Tabaci, the tobacco is diminished one third; and in Enema Terebinthinæ, the yolk of one egg is ordered in place of a sufficiency. One new preparation has been added, Enema Assafœtidæ, made by mixing a drachm of prepared assafœtida with half a pint of decoction of barley; this is a useful preparation in hysterical cases, and an officinal formula is therefore valuable. The diminution in the amount of tobacco, in the Enema Tabaci, is a safe precaution, as most dangerous depression has occasionally occurred from this mode of exhibiting the drug, and all the useful effects may be produced by the infusion of the above strength.

Extracta, Extracts.—Extractum Digitalis has been omitted; Extractum Colocynthidis Compositum changed in name, and now found under the name of Pilula Colocynthidis Composita; Extracts of the Barbadoes Aloes and of Nux Vomica have been introduced; and a liquid Extract of Sarsaparilla substituted for the ordinary one. The change of name of the Compound Extract of Colocynth is apt to lead to some confusion in dispensing, as the Edinburgh and Dublin preparations have been known under this name, and the preparation has all the characters of an extract. The form of extract has always been considered a bad one for administering Digitalis, and the powdered leaf answers all its purposes. The Extract of Barbadoes Aloes is ordered to be prepared in the same way as the Extractum Aloes formerly called Purificatum. The Extractum Nucis Vomicæ is an alcoholic extract, made by first softening the beans by the aid of steam, then slicing, bruising, and drying them, digesting in rectified spirit for many days, and distilling the tincture to the consistence of an extract: it is similar to the Edinburgh preparation of the same name. We consider it a very useful addition to our Pharmacopœia; for the use of this drug, in some affections of the alimentary canal, as habitual constipation, diarrhœa, and tympanitis, has been gaining ground of late, and some fear is often felt in ordering the Alkaloid Strychnia, for fear of any inaccuracy in the dispensing of the medicine. Its dose, in the affections above alluded to, may be from half a grain, gradually increased. This preparation can also be employed in place of Strychnia, for producing the peculiar action of the drug in paralysis. The College might with advantage have changed the formulæ for preparing Extractum Stramonii, which is a watery extract of the seeds, a bad preparation, inelegant, and very liable to become mouldy; another objection is that an extract bearing the same name is contained in the Edinburgh Pharmacopœia, and that, as the latter is made with spirit, the strength of the two are very dissimilar. In some trials of these Extracts, prepared according to the directions of the two Colleges, we found that the effects of the drug on the throat, eyes, &c., were made very manifest with doses of the Edinburgh preparation, which, when the London Extract was employed, produced no apparent effect.

Infusa, Infusions.—Many alterations have taken place in this division. Infusum Diosmæ is now called Infusum Buchu (its old name); Infusum Cinchonæ of 1836, made with the pale bark, is now named Infusum Cinchonæ Pallidæ; and the present Infusum Cinchonæ is ordered to be prepared with the yellow bark. Infusum Pareiræ, Infusum Simaroubæ, and Infusum Scoparii are omitted; and in Infusum Rosæ, maceration is to be made for two hours in place of six. Two inspissated infusions are also introduced, Infusum Cinchonæ Spissatum, and Infusum Cinchonæ Pallidæ Spissatum; these are ordered to be prepared by macerating the coarsely powdered barks in the manner directed under the Extract of the same, then evaporating the infusions to a fourth part, and allowing the dregs to subside, the strained fluid is afterwards to be further evaporated, until the specific gravity is 1·200, and to this, when cool, spirit is to be gradually added, in the proportion of three drachms to the ounce of concentrated fluid; lastly, the liquor is to be set aside for twenty days, to cause the entire subsidence of the dregs. For many years preparations have been employed in medicine, and by some considered of great value, called Liquor Cinchonæ, Liquor Opii Sedativus, &c., made by Mr. Battley, of Fore Street, who considers the specific gravity of an infusion the best criterion of its strength; and it is in imitation of these, we suppose, that the present inspissated infusions have been introduced. When made according to the directions of the Pharmacopœia, they must necessarily be expensive preparations; and it would be well to ascertain, by careful clinical observation, whether they really possess any medicinal value beyond the alkaloids Quina and Cinchona, obtained from the barks. In the concentrated infusions, the alkaloids remain united with their natural acids and certain other ingredients of the barks, which may possess considerable value; and they certainly have much advantage over the powdered barks, when given in equivalent quantities, in not being so liable to disturb the stomach. It is stated by some manufacturers, that although an imitation, these infusions are not the same as the Liquors of Mr. Battley.

Linimenta, Liniments.—Linimentum Hydrargyri Compositum is now called Linimentum Hydrargyri; the formula of Linimentum Camphoræ Compositum is altered; the Spiritus Lavandulæ has been omitted, and a substitution is made of three fluid ounces of Liquor Ammoniæ Fortior, sp. gr. ·882, in place of seven and a half ounces of Liquor Ammoniæ, sp. gr. ·960. In Linimentum Saponis, less soap is ordered and a little water, the spirit of rosemary being rather stronger than before. A Linimentum Calcis has been introduced, composed of equal parts of lime-water and olive oil, shaken together.

Mellita, Honeys.—In Mel Rosæ, less water is used to infuse the petals, and this is divided into two portions; moreover, infusion is ordered to be made for two hours in place of six. In Oxymel, the acid is diluted with rather more than an equal volume of water, as the preparation was previously too strongly acidified.

Metallica, Metallic Preparations.—The Aluminous preparations remain as before. In the process for Oxysulphuret of Antimony, Liquor Sodæ is ordered in place of Liquor Potassæ, the resulting products remaining the same. The process for Tartar Emetic is altered, the new one having the advantage of being more simple, and superior in all respects. In place of burning the Sulphuret with Nitre, and adding Hydrochloric Acid occasionally during the combustion, it is made by mixing the Sulphuret with Sulphuric Acid, burning until the Sulphur is driven off, and then the remaining Oxide of Antimony, after being well washed and dried, is to be boiled with the Bitartrate of Potash for half an hour, the solution filtered and set aside to crystallise. The latter part of the process is the same as that given in the Pharmacopœia of 1836; but the proportions of Tersulphuret of Antimony and Bitartrate of Potash are slightly different.

In making the Antimonial Wine, crystals of Tartar Emetic are to be used.

The Argenti Nitras Fusa is transferred to the Materia Medica; and no preparations of silver are ordered, with the exception of a solution of the Nitrate, which is put in the Appendix. Cyanide of Silver has been omitted altogether: it was used only for the extemporaneous formation of dilute hydrocyanic acid.

Liquor Potassæ Arsenitis remains as before; and another solution, called Liquor Arsenici Chloridi, has been introduced, made by dissolving arsenious acid in dilute hydrochloric acid, and then diluting the solution, so that thirty grains of the arsenious acid are contained in the pint. Arsenious acid is more soluble in dilute hydrochloric acid than in water, but remains unchanged in composition; and therefore the Pharmacopœia preparation is simply a solution of arsenious acid in acidulated water, and not, as it is styled, a solution of the chloride of arsenic. A chloride of arsenic can, however, be prepared, but by a very different process. It is difficult to imagine that arsenic, given in this form, can have any effect beyond that of arsenious acid; for, suppose that ten minims of this preparation were administered, which would be about a medium dose, the amount of hydrochloric acid in it would not exceed a quantity so insignificant, that we can scarcely think that it could influence the action of the drug, especially as the gastric fluid probably contains the same acid in a free state; and no Chloride of Arsenic or new combination is contained in the solution. It would be interesting to ascertain whether a patient, intolerant of the ordinary arsenical solution, could take the same amount of the metal in the new form without inconvenience. We think not; for an ordinary dose of Fowler's solution is probably more than neutralised when it reaches the stomach, more especially as it is generally given soon after a meal, when the gastric secretion is most abundant.*

Baryta preparations are omitted from this part of the work; the Chloride of Barium and its solution are now put in the Appendix. Bismuthi Trisnitras is again called Bismuthi Nitras. Why is its name so altered? Whatever may be its composition and the mode of arrangement of its elements, it certainly has the characters of a sub-salt; its very mode of preparation shows this; and therefore a name, expressing such a composition, is preferable to the one which it now bears. Besides, another Nitrate can be formed, which has more right to the title given to the Pharmacopœia compound. We think that the College is not called upon to alter a name of a preparation, simply on account of the equivalent of a metal having

* Since the above remarks were written, we have had a good opportunity of ascertaining, that where there exists intolerance of the Liquor Potassæ Arsenitis, the same is exhibited when the Liquor Arsenici Chloridi is administered.

been recently supposed by some chemists to be three times as great as it was formerly considered.—Among the preparations of Lime, the following changes have taken place: Calx, Calx Chlorinata, Creta Præparata, and Calcis Chloridum, are transferred to the Materia Medica list, and Liquor Calcis Chloridis is omitted altogether; Liquor Calcis being the only remaining compound of Lime.—Cupri Sulphas is ordered to be prepared from the commercial sulphate, by dissolving in boiling water, filtering the solution, and allowing crystals to form on cooling; then evaporating the mother-liquor, to obtain a further crop.

Ferri Sulphas is directed to be made from the commercial sulphate, which is put in the List, by dissolving in water acidulated with sulphuric acid, adding iron wire, and heating until solution takes place; then filtering, and putting aside to crystallise. Ferri Iodidum is omitted, and its place supplied by Syrupus Ferri Iodidi, which is prepared by first making a solution of the Iodide, by heating Iodine and iron wire with water, until union has taken place, evaporating, and adding the sugar. The amount of iodide contained in the preparation is four grains to the fluid ounce,—a quantity greater than that usually found in the iodide syrups of the shops; but this is no objection, and a formula for a uniform syrup is therefore a valuable addition to the Pharmacopœia. The solid Iodide of Iron was of very little value in medicine, being very liable to decomposition; when, however, the syrup of this salt is properly prepared, it becomes much more permanent. The College have also introduced the Carbonate of Iron, combined with sugar, in the form of the Ferri Carbonas c̄ Saccharo, prepared by precipitating the sulphate with carbonate of soda, washing the precipitate with water, then mixing it with syrup, and evaporating until the powder is dry. It is ordered to be kept in a well-closed bottle. This preparation, if properly prepared, (and care should be taken to avoid much contact with air before the addition of the sugar,) will be found a valuable remedy. The Protocarbonate, in the form of Griffith's Mixture and the Pilula Ferri Composita, has been for a long time used with success; and the objection to the process is, that the frequent washing of the precipitate ordered by the College may cause the conversion of much of the precipitate into a sesquioxide,—a change which it is very desirable, if possible, to prevent.—A formula is also given for the preparation of Ferri Ammonio-Citras, now first introduced into the Pharmacopœia. This salt has for many years been used extensively in medicine, and is a good addition. Ferri Potassio-Tartras is ordered to be made by a new and better process. Vinum Ferri again appears as officinal, and is directed to be made as in the year 1824, by digesting iron wire in sherry wine for thirty days. It is usually prepared by dissolving the Potassio-Tartrate of Iron in wine; and it is an important objection to the Pharmacopœia process, that the strength of the solution must depend much on the quality of the wine, since the more acid the wine, the more iron will be dissolved. A little variation in its strength, however, will not be of any serious inconvenience.

Many Mercurial Salts are omitted in the present Pharmacopœia; as Hydrargyri Oxydum, Hydrargyri Binoxydum, Hydrargyri Biniodidum, Hydrargyri Bicyanidum, and Hydrargyri Sulphuretum cum Sulphure. Most of these salts can be well spared, as the number of our mercurial remedies was unnecessarily large, and the black and yellow oxides were scarcely ever employed, except when made extemporaneously, in the form of black or yellow wash. The processes for making Hydrargyri Chloridum and Hydrargyri Bichloridum are still retained; and the ground for so doing is thus stated in the Preface, after giving reasons for the transference of many substances to the Materia Medica list, and the consequent omission of the formulæ for their preparation:

"If, on the other hand, certain other compounds, just as carefully prepared, may be procured from the same chemists, we have nevertheless thought it better to give the formulæ for their preparation (such as the Chloride and Bichloride of Mercury); for, notwithstanding their power and importance, they can be prepared without difficulty; and it is very necessary that a person should know how to prepare them, in case he should be unable to buy them, or should suspect those offered for sale."

In making these Chlorides, the sulphuric acid is ordered by fluid measure; and the same is the case with the nitric acid in the process for the Hydrargyri Nitrico-Oxydum. Had the College followed the same rule with the Chlorides of Mercury as they have done with the Nitrate of Bismuth, they would have called Calomel a subchloride, and Corrosive Sublimate a chloride; the arguments for the halving the equivalent of Mercury being stronger than for tripling that of Bismuth. The objections to this change would not have been great, as it would rather diminish the chance of a poisonous substance being given for one comparatively inert.

There are no material alterations in the Magnesian Salts. In the preparation of the Carbonate, the solutions are to be boiled for two hours, instead of a quarter of an hour; the amount of Carbonate of Soda is also slightly increased.

Plumbi Acetas is removed to the list; Plumbi Chloridum and Plumbi Oxydum Hydratum are omitted; and, in the process for Plumbi Iodidum, eight ounces of the Acetate of Lead are ordered in place of nine.

Potassæ Carbonas, Potassæ Bicarbonas, Potassæ Sulphas, Potassæ Tartras, Potassii Iodidum, and Potassæ Sulphuretum, are removed to the Materia Medica list; Potassæ Bisulphas, Potassii Bromidum, and Liquor Potassæ Effervescens, are omitted.

Liquor Sodæ is newly introduced; it is made in the same way as Liquor Potassæ, with the substitution of Carbonate of Soda for Carbonate of Potash. Sodæ Sesquicarbonas is transferred to the list, under its correct name of Sodæ Bicarbonas. Sodæ Sulphas, Sodæ Potassio-Tartras, and Sodæ Carbonas, are likewise removed. Liquor Sodæ Effervescens is omitted. The Liquor Sodæ is now ordered in place of Liquor Potassæ, in some of the Pharmacopœia processes; and it will doubtless be more frequently used as a medicine. It would be well to have the relative actions of potash and soda on the system accurately determined by clinical observation: both bases are constituents of the body, although the relative amounts found in different parts of the body are very different.

Among the Zinc Salts, the changes are as follows:—Calamina Præparata and Zinci Sulphas are transferred to the Materia Medica; and a process is introduced for the preparation of Zinci Chloridum, by dissolving the metal in dilute hydrochloric acid, evaporating the salt to dryness, then fusing and pouring the liquid out on a flat and smooth stone, afterwards breaking the mass up, and preserving in a well-stoppered bottle. This salt has been much employed of late, both in its solid and liquid states, as an escharotic, astringent, and disinfectant; and it is the active ingredient of Sir W. Burnett's solution.

Misturæ, Mixtures.—Distilled water is ordered in place of common water in the preparation of Mistura Acaciæ and Mistura Ammoniaci. Mistura Assafœtidæ, Mistura Cascarillæ Composita, and Mistura Moschi are omitted.

The division *Olea Distillata* is omitted. Of the Oils contained in it, those of Anise, Chamomile, Carraway, Juniper, Lavender, Peppermint, Spearmint, Pennyroyal, Pimenta, Rosemary, and Turpentine are found in the Materia Medica; and Oleum Origani, Oleum Sambuci, and Oleum Succini are omitted from the Pharmacopœia.

Pilulæ, Pills.—Pilula Ipecacuanhæ Composita is now called Pilula Ipecacuanhæ cum Scilla; and the Compound Extract of Colocynth is placed here as Pilula Colocynthidis Composita. Pilula Hydrargyri Iodidi and Pilula Sagapeni Composita are omitted; and Pilula Aloes cum Sapone introduced. Soft soap is substituted for hard in the formulæ for Pills; and treacle for syrup or gum. The Pilula Aloes cum Sapone consists of equal parts of extract of Barbadoes aloes, soft soap, and extract of liquorice, rubbed up with the necessary quantity of treacle.

Pulveres, Powders.—No omissions, additions, or alterations have been made in the powders.

Spiritus, Spirits.—In this section, Alcohol is omitted, and removed from the Pharmacopœia. Spiritus Ammoniæ and Spiritus Lavandulæ are also left out. In Spiritus Ammoniæ Aromaticus, the relative proportions of some of the ingredients are slightly altered, and the specific gravity should be ·918 in place of ·914.

Spiritus Anisi is to be made with the oil and proof spirit; so are Spiritus Carui, Spiritus Cinnamomi, Spiritus Juniperi Compositus, Spiritus Menthæ Piperitæ, Spiritus Menthæ Viridis, and Spiritus Pulegii. Tincture of Camphor of 1836 is placed in this section as Spiritus Camphoræ.

Sulphureum, Sulphur Preparation.—This is a new heading, but at present contains one preparation only, viz. Sulphuris Iodidum, made by heating together, in a glass vessel immersed in boiling water, one part of sulphur and four parts of iodine, afterwards breaking the iodide into fragments, and keeping these in a well-stoppered bottle. The proportions ordered are such as would produce a diniodide (but it has not been yet carefully analysed), and are the same as in the New Dublin Pharmacopœia. In this compound, the elements are very feebly united, boiling water being sufficient to separate them. It has enjoyed some reputation on the Continent, as an external remedy in certain cutaneous affections, especially those of a tubercular and scaly character. An ointment containing it will be found in the Pharmacopœia.

Syrupi, Syrups.—Less sugar is used in Syrupus Simplex, which is an improvement. A small quantity of rectified spirit is ordered to be added to many of the syrups; and two new syrups have been added, viz. Syrupus Cocci and Syrupus Violæ, simply for the purpose, we presume, of colouring mixtures.

Tincturæ, Tinctures.—Tinctura Cardamomi and Tinctura Guaiaci are omitted, and Tinctura Camphoræ has become Spiritus Camphoræ, as in 1824. Tinctura Balsami Tolutani is called Tinctura Tolutana; and Tinctura Catechu is changed to Tinctura Catechu Composita. In Tinctura

Ammoniæ Composita, the oil of amber is omitted. In Tinctura Cardamomi Composita, the amount of cochineal is increased from ʒj to ʒijss to the two pints of the tincture. The cardamoms are omitted from the Tinctura Conii. In Tinctura Cubebæ, the cubebs are increased from five to twelve ounces, and proof spirit is ordered in place of rectified. The oils of lavender and rosemary are ordered in place of the spirits of the same, in the Tinctura Lavandulæ Composita; and in Tinctura Jalapæ the amount of the tuber is diminished one-half.—Tinctura Aconiti, Tinctura Belladonnæ, Tinctura Cinchonæ Pallidæ, Tinctura Ergotæ Ætherea, Tinctura Limonum, Tinctura Lobeliæ, Tinctura Lobeliæ Ætherea, and Tinctura Quinæ Composita have been added.

The tinctures omitted could be well spared; the changes of names are of little consequence; but the alterations should be taken notice of, as the cubebs are much increased and the jalap halved. The additions claim most attention. Tinctura Aconiti is made by macerating for seven days fifteen ounces of coarsely-powdered aconite root in two pints of rectified spirit. In the Dublin Pharmacopœia, ten ounces (avoirdupois) are used to each pint; in Dr. Fleming's Tincture, that frequently ordered, sixteen ounces are percolated with rectified spirit, and twenty-four ounces of tincture obtained. We think that it would, perhaps, have been better to have had two Tinctures of Aconite, one of which might have been called Tinctura Aconiti Fortior, for external use only; the other for internal employment. The present Pharmacopœia tincture is scarcely strong enough, if we wish to produce the topical benumbing effects of aconite; and for internal administration the dose must be very small. If any fear existed, as to the possibility of error arising in administering one for the other, we think that a tincture of the strength of Fleming's would have been better. Tinctura Belladonnæ (four ounces of dried leaves to the two pints of proof spirit) we consider a valuable addition, as previously we had no opportunity of administering this remedy in a fluid form, except by dissolving the extract. The dose will be from about ♏ v upwards. In Tinctura Quinæ Composita, a little sulphuric acid should have been ordered, because the amount of the disulphate is more than the Tincture of Oranges will dissolve: we are told to digest for seven days, or until the quinine is dissolved. We have seen some samples made according to the formulæ of the College, but a large quantity of the salt has remained undissolved, although kept for much longer than the seven days: possibly some Tannate of Quina may be found precipitated.—The time for the preparation of the tinctures, when specified, has been altered in the present Pharmacopœia from fourteen to seven days.

Vegetabilia Præparata, Prepared Vegetable Substances.—We find in the present Pharmacopœia that certain substances are ordered to be "prepared:" these are Ammoniacum, Assafætida, Cassia, Galbanum, Pix Burgundica, Prunum, Sagapenum, Tamarindus, and Storax. This last substance was ordered in the former edition to be treated with spirit and strained. Some, such as the Gum Resins, are to be strained through a hair sieve, after being boiled with water, and then evaporated till they become hard on cooling. The rest are to be prepared in a manner somewhat similar.

Vina, Wines.—There are no alterations in this section, except that the time of maceration has been changed from fourteen to seven days. We have already alluded to Antimonial and Iron Wines under the heading "Metallica."

Unguenta, Ointments.—Six ointments have been introduced, three omitted, one altered in name, and four slightly changed in composition. The new ointments are—Unguentum Belladonnæ, prepared by rubbing together a drachm of the Extract with an ounce of lard, a useful application in certain painful affections; Unguentum Conii, made by boiling equal weights of fresh hemlock leaves and lard, and then straining; Unguentum Hydrargyri Nitratis Mitius, one eighth of the strength of the old preparation; Unguentum Potassii Iodidi, containing about one ninth of its weight of the salt, and Unguentum Sulphuris Iodidi, containing a sixteenth; and, lastly, Unguentum Opii, with a scruple of the powdered drug to the ounce of lard. The old Ceratum Sabinæ also appears here as an ointment.

The ointments omitted are—Unguentum Hydrargyri Mitius, Unguentum Hydrargyri Biniodidi, and Unguentum Veratri. Unguentum Hydrargyri Fortius of 1836 is simply Unguentum Hydrargyri; Unguentum Cetacei and Unguentum Plumbi compositum have had slight and immaterial alterations made in their composition; and the oil of Bergamot has been omitted from the Simple and Compound Sulphur Ointments.

In the present Pharmacopœia an Appendix has been added, containing a List of the Tests to be employed in ascertaining the purity of medicines. Its contents are Hydrosulphuric Acid, Oxalate of Ammonia, Nitrate of Gold, Silver, Copper, Chloride of Barium, Turmeric, Isinglass, Litmus, Solution of Sulphate of Indigo, Bichloride of Platinum, Iodo-Cyanide of Potassium and Mercury, and Protochloride of Tin. In this part we also find formulæ for preparing the Solutions of the Nitrate of Silver and Chloride of Barium, of the same strength as those of the Pharmacopœia of 1836, or with a drachm of either salt to the ounce of distilled water, together with a process for making Chlorine Water. We think that it would have been as well to have stated something about the preparation of the Iodo-Cyanide of Potassium and Mercury; for it is not a salt often kept by druggists, and we suspect that not a few are ignorant even of its existence. At the same time, it is one very readily prepared, simply by the addition of a concentrated solution of Iodide of Potassium to one of the Bichloride of Mercury. It is introduced for the purpose of testing dilute Hydrocyanic Acid, which, according to the College directions, should not redden on the addition of this salt; but its value is exceedingly questionable, as the presence of a little foreign acid, which is the cause of any change of colour that may take place, has been shown to have considerable power in preserving Prussic Acid from decomposition. Chlorine Water is used as a test for Quina and Morphia; a solution of the first alkaloid, when treated with this fluid, will, on the addition of Ammonia, strike a beautiful emerald green colour. The College have introduced this test, rather recently discovered, into their Pharmacopœia; we think that others might have been also advantageously given, such as the reaction of Strychnia with Chromic Acid or Peroxide of Lead, and of Brucia with Nitric Acid, by which means the freedom of Strychnia from the latter alkaloid might have been determined.

Solutions of Sulphate of Indigo and Protochloride of Tin are to be used to ascertain the freedom of Hydrochloric Acid from free Chlorine, the former solution being discoloured if such be present, and the latter giving a purple precipitate after the acid has been first boiled with gold-leaf.

As in the old Pharmacopœias, so in the present edition, an Index has been added, showing the changes which the names of certain drugs have experienced. Many of these we have had occasion to notice already, and but few of the remainder call for any remarks. Aloe of 1836 is now Aloe Socotrina, on account of the introduction of Aloe Hepatica and Aloe Barbadensis; and the names of the three Cinchona barks have been changed from Cinchona Cordifolia, C. Lancifolia, and C. Oblongifolia, to Cinchona Flava, C. Pallida, and C. Rubra. The College have also changed the references, and have adopted Weddell's views regarding Yellow Bark as derived from Cinchona Calisaya, the Pale Bark from Cinchona Condaminea, and leaving the origin of Red Bark undetermined. The references in 1836 were undoubtedly erroneous; in the present edition the Yellow and Pale Barks are probably referred to their true source.

We have dwelt thus at length on the alterations which have taken place in the different sections of the present work, in order that those of our readers who are acquainted with the old Pharmacopœia may be enabled, by the perusal of our article, and with little trouble to themselves, to form a correct idea of the contents of the new edition. We shall defer giving an opinion of the respective merits of the two editions, until we have reviewed the changes which have been effected in the Pharmacopœia of the Dublin College.

Dublin Pharmacopœia.—Until the publication of the recent edition of this work, at the end of the year 1850, no edition had appeared since 1826, a period of twenty-four years, during which time the College had been contemplating a revision of its work, and the publication of a new and improved edition:

"This task it accordingly undertook in 1847, and since that period experimental and other inquiries of various kinds have been steadily carried on under its direction, having for their object to correct whatever might be defective in the processes previously given, and to introduce such new medicinal agents as would seem called for by the rapid progress made, in modern times, by those sciences from which our knowledge of therapeutics is chiefly derived. These researches have not only occupied much time, but have been conducted with the attention and care necessary to ensure accurate results. The formulæ, therefore, to which they have led, are given with considerable confidence; and though in the progress of discovery, some of them will, no doubt, be replaced by others of a superior description, they are, at all events, such as may be successfully repeated by any person who attends strictly to the directions given in them. They will also, it is believed, be found to be recommended generally by their economy, simplicity, and the comparative purity of the products which they yield. The question of economy, however, the College considers of very subordinate nature, and not at all to be entertained, should the purity of a medicinal agent be endangered by a diminution in the cost of its production."

Many and important changes will be observed on comparing the new Pharmacopœia with its predecessor: in the first place, the English language is employed instead of Latin, the same change having been adopted in the last edition of the Edinburgh Pharmacopœia. This we consider a useful alteration; as the object had in view in *prescribing* in the Latin tongue by no means holds good, when considered in reference to a Pharmacopœia; the work is thus made more intelligible and useful to the mass of those who have to use it, and many errors in compounding and dispensing drugs are thereby avoided.

Next we find that, in the new Pharmacopœia, a complete change, both in the weights and measures, has been introduced. In the edition of 1826, the old wine gallon, consisting of eight wine pints, each pint having sixteen fluid ounces, was employed; now the imperial gallon, that used both by the London and Edinburgh Colleges, is substituted. There can be no doubt as to the advantage of such an alteration, for it makes the measures of the three British Pharmacopœias alike, and the same as the standard measures of the country. The Dublin College have also altered their weights; formerly they used the Troy, or apothecaries pound, containing 5600 grains, the same as that ordered by the London and Edinburgh Colleges; but they have now returned to the Avoirdupois pound of 7000 grains, and have subdivided it as follows:

The pound	contains	16 ounces,	or 7000	grains Troy,
„ ounce	„	8 drachms,	or 437·5	„
„ drachm	„	3 scruples,	or 54·68	„
„ scruple	„	„	or 18·22	„

With respect to these alterations, the College states:

"There are many reasons justifying such a change, but it will be sufficient to glance at a few of the more important of them. At present two systems are in use with the apothecary, for his purchases are made in Avoirdupois, and his sales generally, but not always, in Troy weight. A practice so inconvenient and arbitrary surely requires to be reformed. The manufacturing chemist, too, by whom the avoirdupois pound and its subdivisions alone are used, is compelled, in preparing any medicine according to the directions of the Pharmacopœia, to have recourse, as a preliminary, to tedious arithmetical reductions; so that unnecessary labour is thus created, and the chances of error greatly augmented. Besides, between the imperial measures and Troy weights there is no simple relation which would abridge calculation, and enable us to determine with facility the proportions by weight which the liquid materials of a process, when given in measures, bear to the solid, or *vice versâ*. In fact, as respects the important point just adverted to, nothing would appear to have been accomplished by substituting the imperial for the wine gallon. It is otherwise, however, when with the imperial gallon and its divisions we combine the avoirdupois system of weights adopted in the present work; for as the fluid ounce and ounce by weight are both similarly subdivided, and as the avoirdupois ounce of 437·5 grains is the weight of a fluid ounce of water, and the avoirdupois drachm of 54·68 grains is the weight of the fluid drachm of water, when we have the volume of any liquid in imperial ounces or drachms, its weight is obtained by simply multiplying this by its specific gravity. Thus the weight of 11·5 fluid ounces of proof spirit is $11{\cdot}5 \times {\cdot}92 = 10{\cdot}58$ ounces; and 7·75 drachms by measure of *pure sulphuric acid* weigh $7{\cdot}75 \times 1{\cdot}846 = 14{\cdot}3$ drachms. The weights of liquids may obviously be converted with equal simplicity into measures, viz., by dividing instead of multiplying by the specific gravity."

The College appear fully aware, that many objections would be raised to the alteration made in the weights, and have anticipated most of them.

To the objection that the new drachm and scruple are not multiples of a grain by integer numbers, they state that they consider this of no importance; nor do they regard as of consequence a circumstance which will not unfrequently occur, viz., that a practitioner, not taking the trouble of consulting the work, will have administered to his patient rather less than he intended; for example, 437·5 grs. in place of 480 grs., if an ounce of any substance is ordered, and so on in proportion for the drachm and scruple. Another objection which would occur to almost every one studying the alterations, would be this: that in the avoirdupois ounce, sixteen drachms are contained, and therefore the new Dublin drachm would not correspond to any known weight, so that mistakes might occur from the same word being used, to express very different quantities. The answer which the College make to this objection is, that "the drachm, whose value is the sixteenth of an ounce, has nearly gone out of use; and even though it were common, it would, it is considered, be very unlikely to be mistaken for a weight, between which and it there is so great difference." They confess, however, that "the fact of there being an avoirdupois drachm different from that which it is proposed to institute, is certainly an inconvenience." But still they affirm, that "it is also one which does not admit of being removed; for subdivision of the ounce into eight parts was indispensable, in order that the drachm should retain nearly its original pharmaceutical value, and that drachm measures should be convertible into weights by multiplying by the specific gravity." However advantageous this change may appear in the eyes of the Dublin College, it will, we believe, be generally considered that it must tend greatly to bewilder both prescribers and dispensers; and, considering that Irish prescriptions have frequently to be dispensed in England and Scotland, and English and Scotch prescriptions in Ireland, it may not always be unattended with danger. It is said, that "the chance of error from this source has been materially diminished by Mr. Donovan, of this city (Dublin), who has undertaken to supply at a moderate cost, and without the slightest view to personal profit, the weights adopted in the present work;" but this will not apply either to England or Scotland.

In the new Pharmacopœia, there will also be found a considerable change in the classification. The following statement in the preface will at once explain this:

"In the present work, the various formulæ are grouped under thirty-seven headings or sections, distinguished by different names, and these are placed consecutively in alphabetical order, this being obviously the method of arrangement which best secures facility of reference. Several of the headings are the same as in the Pharmacopœia of 1826; but there are also some important changes, the principal of which is the suppression of the Metallics of the last edition, and the distribution of the numerous preparations of this class under sections headed respectively—Acetates, Arseniates, Carbonates, Chlorides, Citrates, Iodides, Metals, Nitrates, Oxides, Phosphates, Sulphates, Sulphurets, Tartrates, and Valerianates. According to the old arrangement, the *Sulphurea* appeared as a distinct section; and while some oxides were included amongst the *Metallica*, others, as the alkalies and earths, were not. Such incongruities are avoided by the method of subdivision of the metallic preparations now adopted; and this method has the additional recommendation of being made to rest upon the electro-negative, rather than the electro-positive, constituent, and thus coming, in its principle, to coincide with the most approved classification of the objects of the mineral kingdom. In a single instance, however, it has been found expedient to deviate slightly from the arrangement which has been sketched; and, as it became necessary to erect the alkaloids into a distinct section, seeing that they are not metallic-oxides, it was thought better to arrange with each alkaloid its saline compound, rather than transfer such to the sections characterised by their respective acid or electro-negative constituents."

As one objection to this alteration in the classification, we would urge, that it tends to cause a greater difference between the Dublin and the two other British Pharmacopœias; but this difference would not be of very great moment. A second is, that, as a rule, the therapeutic effects of metallic compounds have a closer relation to the Metal or Basyle than to the Acid or Salt-radical; and we find from the above quotation, that in the case of the salts of the alkaloids, the Dublin College has itself departed from the arrangement it advocates, of making the electro-negative constituents the basis of the classification.

On reviewing the details of the work, and comparing it with that published by the London College, we find that one of its great features consists in the number of its processes. When new remedies have been introduced in the London Pharmacopœia, they have generally been placed in the list of the Materia Medica; but here we find processes given for their preparation, and those for the most part good. This is the case with glacial Acetic, Gallic, Sulphuric, and Tannic acids. The process for making Acidum Sulphuricum Purum, contained in the edition of 1826, appears to be very troublesome, yet inefficient, for obtaining a pure product; —this is still retained. Acidum Prussicum, now called Acidum Hydrocyanicum Dilutum, is ordered to be made, not from the Bicyanide of Mercury, as in 1826, but by a process similar to that employed by the London and Edinburgh Colleges, viz., by the action of dilute Sulphuric Acid upon Ferrocyanide of Potassium; the strength of the product is to be measured by its specific gravity (·997), a test by no means so accurate as that given in the London Pharmacopœia. In the preparation of Morphia, Gregory's process for obtaining the Muriate by the use of the Chloride of Calcium, is first ordered, and the resulting Muriate afterwards decomposed by Ammonia. Processes for Quina and Strychnia are also given, and Muriates of these bases ordered to be prepared; the latter (Muriate of Strychnia) is a valuable addition, on account of the very slight solubility of the alkaloid itself.

Liquor Arsenicalis has 72 grains of arsenious acid to the pint; its specific gravity is given as 1013; but this, as we stated above, is a dangerous mode of ascertaining the correctness of its composition, as it contains Carbonate of Potash and Compound Tincture of Lavender, as well as Arsenic. Among the carbonates, a formula for preparing a precipitated Carbonate of Lime, and also a heavy Carbonate of Magnesia, will be found. Preparations formerly called Succi Spissati are now styled Extracts, as in the other Pharmacopœias. Among the Iodides, Donovan's solution has been introduced, under the name of Arsenici et Hydrargyri Hydriodatis Liquor, (a preparation much employed but not contained in the Edinburgh or London Pharmacopœias,) which is ordered to be prepared as follows:

"Take of Pure Arsenic (metallic), in fine powder, six grains;
,, Mercury, sixteen grains;
,, Iodine, fifty grains and a half;
,, Alcohol, half a fluid drachm;
,, Distilled Water, nine ounces, or a sufficient quantity.

"Rub together the Arsenic, Mercury, Iodine, and Spirit, until a dry mass is obtained; and having triturated eight ounces of water with this in successive portions, let the whole be transferred to a flask, and heated until it begins to boil.

When cool and filtered, let as much distilled water be added to it as will make the bulk of the solution exactly eight fluid ounces and six drachms."

This solution has enjoyed considerable reputation in the treatment of certain chronic skin affections, and appears to be a valuable combination of alteratives for such cases. We consider, therefore, that a formula for its preparation has been very properly given. Iodide of Sulphur has also been introduced.

Among the Liniments we find—Linimentum Crotonis, made by agitating one part of Croton Oil and seven parts of Oil of Turpentine, a useful counter-irritant; its odour, however, would in many cases be objectionable. Under the head of Metals, there is a process for procuring Arsenicum Purum, or pure Metallic Arsenic, which, as we have seen, is used in the preparation of Donovan's solution; it is made by deoxidising arsenious acid by passing its vapour through charcoal, previously heated to redness; this process is ordered to be performed in a combustion-tube of German glass, and with the aid of a furnace used in organic analysis. Ferri Pulvis, or Metallic Iron in fine powder, is ordered to be made by passing Hydrogen Gas through Peroxide of Iron heated to redness in a similar tube, and preserving the reduced metal in an accurately stopped bottle. If iron is ever employed as a medicine, in its metallic state, this is certainly the best form, as it would be easily acted upon by the acid secretion of the stomach; it constitutes the "Fer reduit" of the French.

Considerable simplification has been effected in the preparation of the Ointments; the Unguentum Albæ Ceræ, or Ointment of white Wax, being made the basis of most of them. Unguentum Creasoti has been added, made with one drachm of creasote to seven drachms of ointment of white wax; this is a very useful external application; we have found it exceedingly valuable in that most troublesome affection, Pruritus Ani. Several other ointments have also been introduced; as Unguentum Plumbi Iodidi, Unguentum Hydrargyri Iodidi Rubri; and some omitted, as Unguentum Scrofulariæ.

Liquor Ammoniæ is placed among the Oxides; but considering that it is the solution of a gas (NH_3) in water, and that we have no proof of its being in the state of oxide of ammonium, this cannot be looked upon as being a very philosophical classification; it is so placed to bring it into the same division as Potash, Lime, &c.

Pulvis Antimonialis is ordered to be made by precipitating a mixed solution of tartar emetic and phosphate of soda, by means of a solution of chloride of calcium and ammonia. The resulting precipitate consists of phosphate of lime and oxide of antimony. The London and Edinburgh Colleges order it to be prepared by the combustion of the tersulphuret of antimony and horn shavings. It would appear probable that the moist method insures a more definite compound than the other. By the old methods, the composition was very liable to vary; the preparation at one time containing much oxide (SbO_3), at another, much antimonious acid (SbO_4), a very inert substance. We question, however, whether any antimonial salt but the tartar emetic need be employed in medicine; certainly every useful effect of antimony can be produced by this salt, and its activity is always the same, not depending on the condition of the stomach for its introduction into the system. It is well known that both Pulv. Antimonialis and James's Powder are liable to produce different effects upon the same patient at different times; the variation depending, probably, on the state of the primæ viæ.

Essences, made by dissolving the various essential oils in spirit, in the proportion of one part of the oil to nine of the menstruum, have been substituted for distilled spirits. With regard to this alteration, it is stated by the College, that "a class of medicines, the distilled spirits, has, with a few exceptions, been suppressed, while their place has been supplied by the *Essences*, preparations which, while they exert an equivalent action to that of the *Spirits*, are necessarily of definite strength, much more easily made, and better suited to medicinal use."

In the prepation of the Tinctures, when a time is specified, fourteen days is ordered; formerly seven days was usually the time appointed for their maceration. We thus find that, in this respect, the London and Dublin Colleges have changed in contrary directions.

Under the section Valerianates, are found several new preparations; viz., the Valerianates of Iron, Quina, Zinc, and Soda. The first three are ordered to be made from the last, by decomposing this with the sulphate of iron, muriate of quina, or sulphate of zinc; and the Valerianate of Soda is prepared from the Fusel Oil or Alcohol Amylicum, a preparation placed in the Supplement, and made by distilling Fusel Oil, an after-product obtainable at any large distillery. Fusel Oil is an alcohol, or a hydrate of an oxide, not of Ethyle (C_4H_5) but of Amyle ($C_{10}H_{11}$); it has, therefore, the composition ($C_{10}H_{11}O+HO$); when this substance is acted upon by oxidising agents, it is converted into an acid, in the same way as alcohol; and this acid (the vinegar of the Amyle series) is identical with that which occurs in the Valerian plant, and is called Valerianic Acid, having the composition $C_{11}H_9O_5+HO$, and forming, with oxides, salts called Valerianates. These salts had gained considerable celebrity in the treatment of nervous diseases, but their expense was a great drawback to their employment; besides which, they were often found to be adulterated; so much so, indeed, that some samples consisted only of other salts of the bases, mixed with a little of the oil of Valerian. Since the publication of the Dublin process for producing this acid, the price of these salts has become very much less, in some cases not more than a sixth of that previously charged. As this process may be unknown to many of our readers, and as it is now introduced for the first time into a British Pharmacopœia, it may be interesting to give it in detail.

"*Sodæ Valerianas.*—Take of bichromate of potash, reduced to powder, nine ounces; fusel oil, four fluid ounces; oil of vitriol of commerce, six fluid ounces and a half; water, half a gallon; solution of caustic soda, one pint, or as much as is sufficient. Dilute the oil of vitriol with ten ounces of water, and dissolve with the aid of heat the bichromate of potash, in the remainder of the water. When both solutions have cooled down to nearly the temperature of the atmosphere, place them in a mattrass, and, having added the fusel oil, mix well by repeated shaking, until the temperature of the mixture, which first rises to about 150°, has fallen to 80° or 90°. The mattrass having been now connected with a condenser, heat is to be applied, so as to distil over about half a gallon of liquid. Let this, when exactly saturated with the solution of caustic soda, be separated from a little oil that floats on its surface, and evaporated down until, the escape of aqueous vapour having entirely ceased, the residual salt is partially liquefied. They should now be withdrawn; and when the valerianate of soda has concreted, it is, while still warm, to be divided into fragments, and preserved in a well-stopped bottle."

From this Salt, those of Iron, Quina, or Zinc, can be made by double decomposition.

The Distilled Waters are now ordered to be prepared by agitating the essences of the plants with water, and then filtering; except in the case of the Aqua Lauro-Cerasi, which is obtained by the distillation of the fresh leaves of the plant.

The Supplement, besides the Fusel Oil, which we have just had occasion to notice, contains Carbo Animalis Purificatus, Chloroformum, Elaterium (which is not now called an Extract), and Sodæ Causticæ Liquor. The process given for Chloroformum differs in several points from that of the London College; in the first place, slaked lime is mixed with the chlorinated lime used to decompose the spirit, and the impure chloroform is ordered to be purified by agitation with half its volume of pure sulphuric acid, and lastly shaken with peroxide of manganese, and rectified from off this with a very gentle heat. We have already noticed, that according to the London process, it is simply distilled from Chloride of Calcium.

We have, as yet, not noticed the List of the Materia Medica of this Pharmacopœia. When compared with the one contained in the old edition, many omissions of useless drugs will be observed, together with not a few valuable additions; amongst the most important of which are found Chiretta Matico or Artanthe elongata, Cannabis Indica, Creasote, Glycerine, Hemidesmus Indicus, Lactucarium (omitted by the London College), Lobelia, Lupulina, Cod Liver Oil, and Pyroxylic Spirit. Some changes have also taken place in the names of the plants to which certain drugs are referred; and with regard to the Cinchona barks, we find the Crown or Loxa bark referred to Cinchona Condaminea, Cinchona Flava and Rubra to unascertained species of Cinchona (the Dublin College seem to be unacquainted with Weddell's work); and they have introduced the Grey or Huanuco bark, derived from Cinchona Micrantha, which is also made officinal by the Edinburgh College. Hepatic Aloes is the only species mentioned, and stated to be the inspissated juice of undetermined species of Aloe.

We have no hesitation in affirming, that we consider the present editions of the two Pharmacopœias, which have just been passed under review, superior to the last; the merits, however, of these works are of very different quality, those of the London College being of a rather negative, and those of the Dublin of a more positive character. In the London work, the improvement consists chiefly in its greater simplicity, arising, as we have seen, from omissions both of drugs and of the processes for their preparation; but in it we have also noticed many useful additions, both of articles in the Materia Medica, and of Galenical compounds. The chief excellence of the Dublin Pharmacopœia, on the other hand, depends on the many valuable processes with which it abounds; not mere theoretical processes, but such as are really useful to the manufacturer; we also remark, throughout the whole work, a great simplicity and uniformity, although, as we have already said, we by no means approve either of the alterations in the weights, or in the principle of classification. In concluding our review of the above works, we cannot help expressing our regret, that our prospect of having but *one* British Pharmacopœia appears as remote as ever; and yet we cannot see that any great obstacle exists, as far as the nature of the works themselves is concerned.

If the Fellows of one College were Homœopaths, and those of another Allopaths, or if they differed in any degree with regard to the nature of the remedies which should be employed in medicine, then, indeed, we could understand the impossibility of a coalition or fusion of the Pharmacopœias of the three Colleges; but such is not the case,—their chief difference, and the one which is most serious, consists in the want of uniformity in the strength and composition of their Galenical preparations. Surely it can be of no moment to any body of men, whether the Solutio Morphiæ Acetatis is of one strength or another, although of the greatest importance in the dispensing of the drug; want of this uniformity being liable to lead to serious errors. The same remarks apply to very many other preparations, as Hydrocyanic Acid, Sol. Morphiæ Hydrochloratis, Acetum Opii, Acetum Colchici, Acetum Cantharidis, Acidum Aceticum, &c. &c. Any difference of opinion as to the best methods of preparing salts, active principles, &c., might easily be determined by comparative experiments, and need form no impediment to the formation of a triune Pharmacopœia. The only obstacles to this amalgamation seem to result from petty jealousies existing between the three Colleges; the distance of the respective capitals has been urged, but this cannot reasonably be brought forward in an age when it may, indeed, be said, that "Many run too and fro, and knowledge is increased."

The *title* given by Dr. Nevins to his work, indicates, to a certain extent, its character; but from it we should hardly be led to expect a book of such extensive dimensions.

In the first place, it contains a translation of the Pharmacopœia of the London College of 1851, "the list of which is indicated throughout by being printed in large type," and is made the basis of the work; and, whenever any important difference exists between the directions of the London College and those of the Edinburgh and Dublin, the latter have been introduced, but not otherwise. Dr. Nevins states, in his Preface, that "he believes nothing of any practical importance has been omitted." The translation is correct and elegant, and nothing more could have been desired in this respect.

Under the head of Weights and Measures, &c., Dr. Nevins has introduced some remarks on the various kinds of Thermometers employed in this country and abroad, describing the air, spirit, and mercurial thermometers; and has shown, also, the relation between the different scales, viz. the Fahrenheit, Centigrade, and Reaumur; this is illustrated by a useful woodcut, indicating, at a glance, the corresponding points in each scale. Methods are also given for reducing the degrees of one scale to those of any other. A short account of the Pyrometer, for measuring high degrees of temperature, and of the differential thermometer, are also introduced. This section contains, likewise, the methods of ascertaining the specific gravity of gases, liquids, and solids; and lastly, some tables, not in the Pharmacopœias, showing the weights and measures employed in different countries, their relations and differences. With regard to this small part of Dr. Nevins's work, we may remark, that we consider the additions which he has made to the Pharmacopœia list judicious, and the matter such as every medical practitioner should be acquainted with.

In Part I, or the Catalogue, the substances in the Materia Medica are arranged in the Pharmacopœia as follows:—in one column, the short names of the substances are placed, or such as are commonly employed to designate the drugs; in the other, when a vegetable substance is under consideration, the botanical designation, according to Linnæus or De Candolle, is adopted; when an animal product, Cuvier's nomenclature is employed; when a mineral substance, the modern chemical appellation. Dr. Nevins has arranged the substances of the Materia Medica in the same manner; but, in place of confining himself to two columns, has employed ten, giving, besides the common and scientific names of the drugs, the natural order, the Linnæan class and order, the place whence obtained, the properties, active principles, and preparations.

To illustrate the manner in which Dr. Nevins has executed this part of his work, we shall select a specimen from each kingdom, and give their mode of arrangement and the remarks under each head, reversing the direction of the tabulation, in order to admit them into our page.

Names for Prescription	Nux Vomica	Aloe Barbadensis, Barbadoes Aloes	Plumbi Acetas Acetate (Sugar) of Lead
Scientific Name or Description	Strychnos Nux Vomica (seeds)	Aloe Vulgaris (inspissated juice of cut leaf)	Plumbi Acetas, crystals
Natural Order.........	Apocynaceæ	Liliaceæ	—
Linnæan Class.........	Pentandria	Hexandria	—
Linnæan Order	Monogynia	Monogynia	—
Whence obtained	East Indies, Ceylon	Barbadoes	—
Properties	Tonic, excitant of motor nerves	Purgative, emenagogue	Astringent
Active Principle	Strychnia	Aloesin and Resin	—
Preparations	Ext.	Enema, Ext., Pil. Al. cum Sapone, (Pil. Al. et Ferri, E), (Pil. Coloc. et Hyoscyami, E)	Cerat. (Pil. Plumbi opiat. E)
Description	Alk. Strychnia	Decoct. Aloes Co.	Metals, Plumbi Acet.

In the last column, under the head of Description, is indicated the preparation in Part II, where an account of the drug is to be found.

We find, in the above list, not only the substances made officinal by the London College, but also those contained in the Edinburgh and Dublin Catalogues, together with a few important drugs not officinal in any British Pharmacopœia, as Kousso, Bebeerina, &c. Those not contained in the London list are in smaller type. The remarks on the characters and purity of the articles in the Materia Medica are embodied in the Second Part of the work, under the different heads where the drugs are described. We consider this Catalogue, which occupies forty-two pages of the work, to be very carefully and judiciously arranged, with few and trifling inaccuracies, altogether forming a very valuable table of reference for those engaged in prescribing.

Dr. Nevins commences the Second Part of his work with a description of the properties of the few tests which are applied for the purpose of ascertaining the purity of medicines, and are now contained in the Appendix of the London Pharmacopœia. To these are added, a few from the Edinburgh. His remarks are short, but to the point. Under Acidum Hydrosulphuricum and Ammoniæ Hydrosulphuretum, we find the mode of preparing these tests, and the colour of the precipitates which they produce with the various metals, such as Arsenic, Antimony, Lead, &c. Under Potassii et Hydrargyri Iodo-Cyanidum, we find its mode of formation stated, its use (if it has any) in ascertaining the freedom of Hydrocyanic from any foreign acid, and the explanation of its action as a test. To these points we have already alluded (p. 349).

Next follows the description of the various preparations, under the heads of Acids, Æthers, Alkalies, &c. The arrangement followed is that of the London Pharmacopœia, except that the heads *Animalia, Sapones,* and *Trochisci* have been introduced. The first, discarded in the new London Pharmacopœia, is here retained for the purpose of describing Carbo Animalis Purificatus of the Edinburgh and Dublin Colleges, Cornu Ustum, Hirudo, Ovum, in the London Materia Medica, and Spongia of the Edinburgh. Under *Sapones,* we find the process of saponification explained, and an account given of Soaps, Adeps, Serum, Glycerine, together with Almond, Olive, Castor, Croton, and Cod-Liver Oils. Under *Trochisci,* the composition of the Edinburgh Lozenges.

Our author, as we have before said, states in the table or catalogue of the Materia Medica, contained in Part I, the preparation under which the description of the properties of the drugs is to be found; and he generally selects the most important one, or that most frequently employed in medicine. Thus, Opium is described under Tincture of Opium; Nux Vomica and Cinchona Barks under the heads of the alkaloids Strychnia and Quinæ Disulphas; Rhubarb under Pilula Rhei Composita, and so on. In his description of these vegetable substances, he makes the following divisions:—Taking Colchicum for an example, we find it placed under Acetum Colchici, the preparation of which is given according to the London Pharmacopœia, and the differences between it and the corresponding Edinburgh and Dublin formulæ indicated; after some remarks on this compound, an account of the officinal parts of Colchicum Autumnale is to be found, the characters of the corms and seeds, at what time the former should be collected, and how dried and prepared (Lond. Pharm.); then we have a mode of distinguishing the corm and seeds from other substances, the corm from onions and squills, and the seeds from black mustard and digitalis; afterwards the composition, the medicinal properties, uses in medicine, and diseases in which they are employed; lastly, the doses, modes of administration, and officinal preparations. We have selected Colchicum for an example, but the same remark applies to almost any vegetable remedy; and to give some idea of the manner in which this portion of the work is executed, we will shortly review the various divisions under which the description of such remedies is arranged.

The remarks on the preparations are limited usually to the physical characters, the reasons why certain parts of the plant are employed in preference to others, the value of the solvent of a liquid preparation, the cause of certain admixtures, the changes, if any, which it undergoes by time, and its chemical nature. These remarks are generally valuable and sufficient; and, under Acetum Colchici, Dr. Nevins gives an illustration of what we have dwelt upon in an early part of this article—the absolute need of more uniformity in strength between the corresponding preparations of the three British Pharmacopœias. The Acetum Colchici of the Dublin College being three times the strength of that of the London and Edinburgh, we cannot fail to see that great danger might readily arise from this cause; for many gouty patients can take a drachm of the London preparation with advantage, but the administration of a drachm of the Dublin Acetum Colchici might be attended with very unpleasant consequences.

In the description of the parts of the plants, little, often nothing, is said of their botanical characters. This has been an intentional omission, for in the preface the following remarks occur:—

"He has passed over the botany and zoology of various substances very briefly, indeed, in many cases having scarcely alluded to them. He has, however, been careful to mention everything which is necessary for students in preparing for their examinations; and more than this, his experience has taught him, they never read, even if provided. His object has not been to write a book on natural history, but upon the properties and uses of medicines, to which branch of his subject he has devoted by far the largest proportion of attention, in the hope that not only the student, but the practitioner also, may find himself in possession of a complete compendium of the effects and uses of medicines, so far as our present knowledge extends."

In these remarks we find a very good excuse for the absence of a part of the subject, which is, we think, by far too much dwelt upon by many writers on Materia Medica; and, although not complimentary to the medical student, yet we believe them to be correct, as far as this branch of medicine is concerned. The introduction of the botanical characters of the medicinal plants would have enlarged the work too much, and, unless fully and correctly given, it is much better to refer the student to some of the many excellent Botanical works in which they are to be found.

Dr. Nevins has not, however, omitted such when important, and has dwelt at some length upon the mode of distinguishing the roots, leaves, seeds, &c., of one plant from those of another; these differences being sometimes illustrated by drawings and diagrams. On certain points an apparently unnecessary space is devoted to this subject, and substances differentially diagnosed, which we could hardly have imagined that the most obtuse student would confound. But we have our author's word that, under this head, "he has not mentioned anything which he has not known to cause mistake or difficulty;" and he also gives to the student a piece of advice in which we fully concur, namely, that "he would strongly advise the student, whilst engaged upon the subject, to place at the same time on the table before him the different things which may be mistaken, by which plan he will fix them in his memory more effectually than by any other means."

The composition of the various drugs is accurately given—that is, according to our best authorities; for these Dr. Nevins is indebted to the standard works on the subject, especially to the Encyclopædia of Dr. Pereira. Indeed, we do not find that any advance is shown on these works, or that the many recent investigations not hitherto embodied in them have been consulted: thus, under Opium, no mention is made of the alkaloid *Papaverine*, discovered by Merck, of Darmstadt, of which very much more is known than of other principles here introduced (such as *Thebaia* or *Paramorphia, Narceine* or *Meconine*), and which can also be readily procured in large quantities. Again, certain interesting researches have thrown much light on the composition of Calumbo-root, and of many other drugs; but to these we find no allusion in the work before us. We mention these omissions, because we think that in a new edition they might be advantageously introduced, without giving any undue importance to this division, and, in fact, without occupying more space than is at present taken up with the subject.

Under the head of the Operation and Uses of Medicines, our author's observations are full, and embrace most of the facts which have been really ascertained; the diseases in which the drugs have been successfully employed are clearly indicated; and the effects produced by too large medicinal or by poisonous doses are also detailed. We do not find much that is novel in this department, but it appears to be a careful and judicious compilation. With regard to several new remedies, especially Chloroform, considerable space has been devoted to their mode of action, but our space will not allow us to give any examples. The doses of remedies, the amount of important drugs in certain compounds, and the various officinal preparations, are correctly given.

Two Appendices and Addenda are attached to the work; and lastly a Chapter is devoted to the subject of the Classification of Remedies. In Appendix I are described certain substances which are not officinal preparations, and for which, therefore, no place could be found in the body of the work, together with other drugs not officinal. Appendix II contains a short account of the action of water upon lead, from which the author concludes that—

"Hard waters do not protect lead simply from the fact of their being hard; but the protection, when effected, is dependent not only upon the nature of the salt causing the hardness, but also upon the proportion present; for, whilst all experience proves that a small amount of a sulphate, at any rate of a sulphate of lime, does protect the lead, a large quantity of sulphate of magnesia acted considerably upon it. It appears, also, to be proved that chlorides act upon lead, whether with or without the presence of sulphates, but that their action is not so great as that of soluble carbonates, which produce a rapidly-injurious effect upon the metal. At the same time, these results do not practically affect the question of the safety of using lead for common water, so far as sulphates, at any rate, are concerned; inasmuch as the solutions here employed were, out of all proportion, stronger than any ordinary spring water, and the experience of years has proved that there are no bad consequences practically occasioned by the employment of this metal for water containing sulphates."

We have but little to remark on the section devoted to the Classification of Remedies. Under each class a very short explanation is given of the action of the medicines therein contained, followed by a list of the individual remedies, with their peculiarities pointed out and the diseases for which they are employed. It forms altogether a useful chapter of reference for the student.

In concluding our observations on Dr. Nevins's Translation of the London Pharmacopœia, we may state that we consider it very carefully executed, that it contains much more information than the title indicates, and that we can conscientiously recommend it as a book which cannot fail to be found very useful both to the student and practitioner of medicine, and which is well worthy a place in every medical library.

Dr. Hunter Lane, in this the second edition of his 'Compendium of Materia Medica and Pharmacy,' has preserved the plan of the original work, which was written, so states the preface, "as an attempt to supply some of the deficiencies of the Pharmacopœia Collegii Regalis Medicorum Londinensis, 1836, and to give a translation of that work, with the requisite chemical explanations;" and "it is hoped that the second edition, suited to the recent Pharmacopœia, will prove acceptable to the student, practitioner, and philosopher of medical science." The first edition of Dr. Lane's Compendium seems to have been favorably received, and was often referred to by the practitioner and dispenser for obtaining information as to the officinal preparations of drugs, their strength, mode of formation, doses, and so forth. It appears to have had the character of being accurate as to the information it imparted; and in such a work accuracy is of the greatest moment, as a want of this quality might lead to many and serious errors. We find in the book before us, first an Introduction of twenty pages, containing an explanation of the symbols employed; a table of the equivalents and specific gravities of the elementary bodies entering into the composition of drugs; a short sketch of the nature of certain classes of bodies, as acids, ethers, alkalies, earths, oxides, &c., and of the various Galenical preparations of the Pharmacopœia. We have very little to remark on this portion, except that the alteration of equivalents, evidently implied by the changes in the names of some metallic compounds in the Pharmacopœia, has not been made in the table above-mentioned; and this circumstance, as we shall presently see, causes no small confusion in the explanations of many processes.

The language employed in the introduction, and throughout the body of the work, is by no means remarkable for its perspicuity, and not unfrequently it is almost unintelligible; for example, in describing Tinctures, whilst remarking on the Edinburgh process of percolation, and its non-introduction by the London College, we find the following observation:

> "This process (percolation) has been strangely overlooked in the London Pharmacopœia. It is impossible to conjecture the reason, especially when are found such compounds as the Tincture *Quinæ Disulphatis*, &c., where directions are given for their preparation, implying a gross ignorance of the alphabetical knowledge of Pharmacy."

What relation the imperfection in the Tincture alluded to has to the process of percolation, we are at a loss to imagine; and if the College, as Dr. Lane not very courteously implies, is totally ignorant of Pharmacy, it would rather explain than otherwise the cause of the faultiness of the

Tincture of Quina as a preparation. Dr. Lane has not spared the College when an opportunity has offered itself; but we think that he should have noticed "the beam in his own eye" before indulging in such an expression as that contained in the passage quoted; for we shall soon have occasion to remark on imperfections in his Compendium much more grave, though less important to the profession, than any that are to be found in the Pharmacopœia. In the body of the work, or the Compendium, as it is called, the substances and preparations are alphabetically arranged; and generally under the following heads, viz., *Description and Botany*, &c., *Composition, Properties, Operation, Uses and Doses, Mode of Preparation* (if such), and an explanation of the process; occasionally, also, the substances *incompatible* with them. We must remark here that we have no means of knowing from the work whether or not a substance is officinal, or whether the process for making it is or is not contained in the Pharmacopœia; for, although an Appendix is afterwards devoted to the description of certain useful remedies employed abroad, yet substances not officinal in any British Pharmacopœia are not unfrequently found in the body of the work, whilst certain officinal substances are described in the Appendix. We are not aware of gold being officinal, yet its properties and uses are here described. Besides the Appendix just alluded to, there is one on Toxicology; a third on Weights and Measures; another, consisting of a table, showing the proportions in which some important drugs, as Opium, Arsenic, &c., enter into certain preparations; and, lastly, two others, containing Abbreviations, and Gaubius' Table of Doses.

Without dwelling much on the value of the information afforded by such a work as the one before us, we will give a sketch of the alterations in this second edition, and see how far Dr. Lane has preserved the character of his work in modifying it to the alterations in the new London Pharmacopœia. In a former part of this article we have had occasion to notice the omission of many processes by the London College, and the placing of substances in the Materia Medica formerly found among the Preparations. Our author has, however, retained in his *Compendium* most of these, and without modification. We think that they unnecessarily enlarge his work; and if processes not in the Pharmacopœia are to be given at all, they certainly should be such as have some pretensions to utility. Now, many of those in the Pharmacopœia of 1836 were undoubtedly bad, as the processes for procuring some of the alkaloids; yet they are to be found in this small book, occupying much space and to very little purpose. If anything was required to be stated on this head, we think that a simple explanation of the principle of their formation, capable of being easily stated in a few lines, would have been much more in keeping with the character of this Compendium. Again, although the second process for preparing dilute Hydrocyanic Acid is omitted in the Pharmacopœia, yet it is here retained. We presume that such processes have been allowed to remain, in order to preserve the bulk of the volume: we can see no other reason.

On our first inspection of the book, we were naturally led to look for our author's remarks on certain new officinal drugs, to see how they were treated in his new edition, and to find out whether the alterations, additions, or omissions of certain preparations had been duly attended to.

We must confess we were not a little surprised, on turning to three or four articles, to see errors abounding in almost every one. We might have been unfortunate in our choice, but we shall have no difficulty in showing that but little care can have been bestowed by the author in the compilation of this edition. Atropine was sought for, but in vain; no notice is taken of its introduction into the Materia Medica, nor of its sulphate, now officinal. Under Belladonna, we find the only officinal preparation indicated to be the extract; yet there are, besides this, the alkaloid and its sulphate, a tincture, an ointment, and a plaster. Under Pareira Brava, the infusion and extract are stated to be officinal; the former, however, is now omitted, and a decoction introduced. Under Ergota, no officinal preparation is indicated, though a tincture is contained in the Pharmacopœia. Many other omissions and errors might be enumerated, but the above are sufficient to justify our remarks on the want of care in correcting for the new edition.

One object of the work is stated to be to give the requisite explanation of the changes occurring in the various Pharmacopœia processes; also to give the composition of the various drugs and preparations. Let us see how far our author has attained this object, and how far the student is likely to be enlightened by his remarks. We will select some of the Antimonial preparations for the purpose of illustration, first taking the Pulvis Antimonii Compositus, or the old Pulvis Antimonialis. It must be remembered that the Sulphuret of Antimony, called in 1836 the Sesquisulphuret, is now very properly called by the London College the Tersulphuret, and the equivalent of Antimony must be regarded as twice that formerly taken, or 129 in place of 64·5. Dr. Lane employs the new name, but appears to forget that, if the Sesquisulphuret becomes a Tersulphuret, the names of the oxides must be also changed; and hence he speaks of a tersulphuret losing sulphur and absorbing oxygen, and becoming a binoxide, or of Antimonious Acid containing two in place of four equivalents of oxygen. This error, however, if occurring once only, might have been looked upon simply as an oversight; but on turning to other Antimonial Salts, as the Oxysulphuret and Tartar Emetic, we find evidence of singular confusion of ideas on the chemical composition of those bodies. The Oxysulphuret is said to be a compound or mixture of one equivalent of *Sesqui*oxide and five equivalents of *Ter*sulphuret of Antimony—an error similar to the one above alluded to; and in the explanation of the theory of the process, hydrosulphuret of the Oxide of Antimony is mentioned as being formed, and other changes are stated to ensue, which we should think no chemist would be rash enough to assert. Again, under Tartar Emetic, the same error is found in speaking of the Ditartrate of Antimony as entering into its composition. In explaining the changes which ensue in the formation of the Iodide of Potassium, and of the composition of the salt, an equal amount, or rather deficiency, of chemical knowledge is shown: first we are told that an Hydriodate of Iron is formed (a compound never proved to exist); then a Carbonate of Iron and Hydriodate of Potash; next, that the Hydriodate, which is stated to contain one equivalent of water, loses this amount during the evaporation of the solution, and that Iodine and Potassa combine together (Potassa is here erroneously put for Potassium), and yet that the crystallised salt, in some unaccountable manner, still contains an atom of water, or consists of one equivalent of potassium, one equivalent of iodine, and one equivalent of water. We find here in a few lines as many errors as could well occur in such a moderate space.

Some of the processes are elucidated (?) by diagrams: that given to explain the decomposition in the preparation of Sulphuret of Potassium will show the amount of elucidation they afford.—Here we find stated, that one equivalent of Carbonate of Potash (four equivalents is intended) contains four equivalents of Carbonic Acid, four equivalents of Oxygen, and four equivalents of Potassium, and these, together with four equivalents of Sulphur, constitute the arrangement of the elements of the compounds before decomposition; afterwards we find, as far as we can make out from the diagram, that three equivalents of Carbonic Acid and one equivalent of Oxygen form four equivalents of Carbonic Acid; that three equivalents of Oxygen and two equivalents of Potassium and one equivalent of Sulphur make together one equivalent of Sulphate of Potash; and with regard to the elements used in the formation of the Sulphuret of Potassium, we are at a loss to know how to enumerate them, one unfortunate equivalent of Sulphur and another of Potassium appearing to be drawn both in the direction of the sulphate and also of the sulphuret of the metal. Whether Dr. Lane is in fault for the appearance of this diagram, we cannot say; but certainly it by no means answers the proposed object of elucidating the process. When we look at the composition of the Sulphuret, we see also that errors exist. Sulphate of Potash is represented as (SO_3K); and a number is given as the equivalent of the resulting compound, made up of small numbers, in a way by no means easy to decipher:—of course, the putting of an equivalent number to a mixture of salts does not imply the possession of a very profound acquaintance with chemical philosophy, and the explanations now exhibited will not be likely to prove very acceptable (as in the preface it is hoped they will) to "the philosopher of medical science."

We have already employed more space than we had originally intended in speaking of this work, and cannot stop to notice many other errors, both of omission and commission, with which it abounds.

We cannot, however, pass by Appendix V, or that professing to give the proportions in which Opium, &c., enter into their respective pharmaceutical compounds, consisting of a table of about 23 lines; in this are, at least, six errors.

Tincture of Opium is said to contain 1 grain of Opium in each 19 minims:—1 grain of Opium is contained in between 13 and 14 minims.

The same error is found in Vinum Opii, and a similar one in Linimentum Opii.

Hydrargyrum cum Cretâ is said to contain 1 grain of Mercury in 4 grains of the preparation:—it really contains 3 grains in 8. In the Pilula Hydrargyri Chloridi Composita, 1 grain of Calomel is represented as existing in 4 grains of the Pill mass, in place of 6 grains. And 2 drachms and a half of Liquor Potassæ Arsenitis, in place of 2 drachms, are represented as equivalent to 1 grain of Arsenic.

No mention is made here of the strength of Liquor Arsenici Chloridi of the new Pharmacopœia.

Lastly, with regard to the doses of the two Arsenical solutions, we cannot see the reason why the Liquor Potassæ Arsenitis (containing 4 grains to the fl. ℥j), should be given in quantities varying from ♏iv to ♏xxi, whereas the dose of the Liquor Arsenici Chloridi (containing only $1\frac{1}{2}$ grs. to the fl. ℥j), should be confined within the limits ♏ij to ♏iv.

Some of the recent remedies not officinal are described in the Appendix, as Kousso, Matico, &c., but their description does not call for any observations.

In conclusion, we cannot help remarking, however much we regret being called upon to do so, that we have seldom found a book so faulty and incomplete; and certainly cannot recommend its use either to the practitioner, student, or dispenser, if any regard to accuracy is desired.

5. Since 1844, the date of the first edition of Dr. Neligan's work on Medicines, two other editions have been issued; this shows that the work has found favour in the eyes of his professional brethren, and to a certain extent is a guarantee of its merits.

The title of the work is 'Medicines, their Uses and Mode of Administration;' and in the Preface to the first edition, the author states that his object has been—

"To furnish a concise view, but as complete as possible, of the different substances, both simple and compound, which are deemed worthy of a place in the Materia Medica. While, therefore, sufficient attention has been bestowed on the different articles contained in the three British Pharmacopœias, an account is also given of those remedies of more recent introduction, which, though not officinal, are important agents in the treatment of disease."

Dr. Neligan has chosen a therapeutic arrangement of medicines; although he allows the difficulty of accurately classifying them with reference to their therapeutic properties, in the present state of our knowledge. The different chapters, and also the individual remedies, are, however, classed in alphabetical order.

Although we allow that such an arrangement presents some advantages, yet we think that, upon the whole, they are more than counterbalanced by the disadvantages. If we desire, for example, to know something of the physical or chemical characters of any drug, or the adulterations to which it is liable, some trouble and difficulty may arise in ascertaining under what chapter to find these properties; and again, in subdividing the therapeutic effects of a remedy into so many portions, as must necessarily occur in a work possessing such a classification, some important facts relative to its action are very apt to be omitted. We shall find no difficulty in proving the truth of this statement from the work before us; we think, however, that Dr. Neligan has been very judicious in the manner in which he has carried out his plan.

At the commencement of each chapter, a short account is given of the action of the medicines which are contained in that division; and the remarks are, for the most part, exceedingly good, containing a plain common sense view of their therapeutic action. No attempt is made to explain the mode in which medicines produce their peculiar effects, or to discuss the many speculative views which have been entertained. The author states that, although these remarks are somewhat enlarged in the new edition, yet they must be regarded as mere outlines, and simply introductory. We will present our readers with the author's observations on Cathartics, which may be taken as a specimen of the manner in which he treats this part of his subject:

"Cathartics (*purgatives*, evacuants,) may be defined, medicines which quicken or increase alvine evacuations. Cathartics vary much in the manner in which they produce their effects. Some act merely by exciting the muscular fibres of the intestines to increased peristaltic motion, and thus cause their contents to be more quickly and more completely evacuated. Some stimulate the mucous follicles and exhalants, so that a larger quantity of fluids than usual is excreted from the inner coat of the intestines, and thus the fæcal evacuations are rendered more liquid and more copious. In many, both these properties are united; and some extend their stimulus to the neighbouring viscera also, and hence produce an increased discharge of the supplementary intestinal secretions, as the bile and pancreatic juice. Cathartics differ also as to the part of the intestinal canal on which they act, the effects of some being confined to the small, and of others to the large intestines, while many of them appear to stimulate the entire tube. They differ, moreover, as to the degree in which they produce their effects, and hence have been generally divided into three classes:—*Laxatives*, which operate so mildly as merely to produce the evacuation of the intestinal contents, without causing increased secretion or stimulating any of the neighbouring viscera.—*Purgatives*, properly so called, which, besides remarkably increasing the peristaltic action of the intestines, occasion increased secretion of the fluids from the exhalant vessels, and from the neighbouring viscera, and also extend their stimulant effects to the system in general.—And *Drastic* or *Hydragogue* cathartics, which operate in the same manner as purgatives, but with much greater energy, and which, if given in an overdose, produce inflammation of the intestines, characterised by constant vomiting and purging, and intense pain. Although, for the sake of simplicity in our classification, we have arranged the remedies belonging to these three divisions under the one head, *Cathartics*, in prescribing them, due attention must be paid to the distinctions in their mode of operation, so as to fulfil the indications for which they may be administered. These distinctions will be more conveniently considered, when treating of the therapeutical effects of the individual remedies of this class. Cathartics may be also divided into two classes, depending on the manner in which their effects are produced; that is to say, whether their operation is caused by a direct local action on the mucous membrane of the digestive canal in the same manner as irritating or indigestible articles of food occasion diarrhœa, or indirectly by their being first taken into the circulation, as is known to occur with regard to rhubarb, and other medicines of the class, which purge if injected into the veins. But this division, it is apparent, can be of but little therapeutical value. The prescriber should remember, that the effect of cathartics may be much augmented, or their operation modified, by their judicious combination, or by the addition of medicines possessing other properties; as, for example, a stimulant or tonic. Indeed, I have been convinced by experience, that tonics are not ordered in combination with cathartics as frequently as they ought to be; they not only augment the effect of the cathartic, thus rendering a smaller dose of the latter necessary, but they give tone to the digestive canal, thereby removing a condition of the system on which habitual constipation is so frequently dependent." (p. 84.)

Our author's remarks on the action of individual remedies are also, in general, exceedingly judicious, though at times rather brief. Many of his statements have an increased value, from their being the results of his own experience, and not merely derived from the authority of others. Speaking of Cannabis Indica, which, as we have before noticed, has been introduced into the new edition of the Dublin Pharmacopœia, it is stated that—

"In its action on the system it is decidedly narcotic, producing at first the effects of a powerful stimulant, which, if the dose taken be sufficiently large, are soon followed by those of a direct sedative. The preparations of Indian hemp have chiefly been employed in the treatment of neuralgic and painful affections, in most of which they have proved very beneficial. Thus, they have been given in tetanus, hydrophobia, infantile convulsions, sciatica, chorea, neuralgic pains, and chronic rheumatism; they have also been used to subdue sleeplessness or disturbed rest, provided it does not arise from inflammation in the head. All who have tried the effects of this remedy in the British Isles, have come to the conclusion, that the Indian hemp must be given in much larger doses in this country than in the East; and on his return home, this was acknowledged by Dr. O'Shaughnessy himself. The trials made with it in the diseases above enumerated, would seem to show, that the Cannabis Indica may be often used with benefit, as a substitute for opium, in cases for which that drug is unsuited, from idiosyncracy or any other cause; and also that it does occasionally succeed in abating, sometimes in completely removing, pain, where this agent totally fails us. But the conclusion which an impartial observer must draw, from the numerous cases in which Indian hemp was used as a remedy, which have been made public since the first edition of this work was published, is that it is an exceedingly uncertain medicine, producing the most manifest narcotic symptoms in some individuals, and in others the very same preparation appearing to be perfectly inert: and my own experience of its use fully justifies this conclusion; yet this may, to a certain extent, depend on the bad preparations of it that were commonly sold—a defect which, now that it has become an officinal drug in the last Dublin Pharmacopœia, will not be so likely to occur." (p. 255.)

Again, Hydrocyanic Acid is stated to be—

"The most powerful poison with which we are acquainted [this is not quite correct]; death has been occasioned in man by a mixture containing scarcely one grain of the anhydrous acid, (Christison.) The usual symptoms produced by a poisonous dose are,—convulsions, difficult and spasmodic breathing, and insensibility, followed by death, in a few minutes; in some instances, however, life has been prolonged for half an hour or more; but if the quantity taken be very large, death occurs so rapidly that the only symptoms that can be observed are two or three deep, hurried inspirations, which have been, in some instances, it is stated, preceded by a loud shriek; this, however, is very doubtful. In medicinal doses, hydrocyanic acid acts as a direct sedative, producing, directly after it has been taken, a sensation of quiet and calmness throughout the whole system, diminishing the force and frequency of the pulse, lowering the sensibility of the nervous system, and allaying irritation when it exists; in addition to the above, which may be said to be its more immediate effects, hydrocyanic acid promotes the digestive powers, and, in many instances, acts gently on the bowels. As a remedial agent, this acid has been principally used to allay irritability, to diminish pain, and to lessen spasm. Thus, it has been used with much benefit in spasmodic and painful affections of the stomach and bowels, as in gastrodynia and enterodynia, in pyrosis, particularly when accompanied by much pain, in chronic vomiting, and in colica pictonum. It has also been found very serviceable in allaying irritable or spasmodic cough, in various pulmonary affections, as in simple hooping-cough unattended with inflammation, in pure spasmodic asthma, in the advanced stage of phthisis, and in the spasmodic cough of nervous and hysterical females. It has been successfully employed to allay vomiting and purging in severe cases of common cholera, and to check the colliquative diarrhœa and sweating of hectic. Lastly, it has been administered as a calmative and anodyne in neuralgia, tic douloureux, chronic rheumatism, cancerous affections, and nervous palpitations, but its success has been very equivocal. Externally, in the form of lotion, it will be found very serviceable in allaying the violent itching which attends many forms of skin disease. The vapour of prussic acid has been applied to the eye, in amaurosis, by Dr. Turnbull, but its efficacy is very doubtful; in one case, which I saw with Mr. Wilde, of this city, its employment for this purpose produced giddiness, temporary insensibility, and other symptoms of poisoning, followed by erysipelatous inflammation of the face and forehead." (p. 295.)

When we made the remark, that the statement that "prussic acid is the most powerful poison" was not quite correct, we meant, that other substances, in smaller doses, would produce death; but, if regarded with respect to its rapidity of action, our author's observation is probably true. Experiments on animals have shown us that Aconitine is much more powerful, but requires a long time, that is, half an hour or more, to produce its fatal effects; the same, perhaps, might be said of certain other active principles.

When speaking of Iodide of Arsenic, and its value in the treatment of chronic cutaneous diseases, as Lepra, Psoriasis, and Porrigo, Dr. Neligan makes the following remark:

"In the latter (porrigo), I have used it extensively, and with great success, even in very inveterate cases. Its use must be continued for some time after the disease is cured, in order to prevent a relapse. In some cases, in which the medicine has been taken daily for five or six weeks, the patients complained of headache and dryness of the mouth and fauces, which quickly disappeared on intermitting the use of the remedy for a few days." (p. 380.)

When discussing the action of another preparation of Arsenic (Donovan's Solution), also recently introduced by the Irish College, and the formulæ of which we have already given, the following observation is made:

"In my own practice, however, I have found it to fail in effecting a cure in many cases; and in others, I have seen it produce injurious constitutional effects, when the disease for which it was administered was invariably exaggerated. This I have been inclined to attribute to the presence of mercury in it, and I have, therefore, latterly substituted for it a compound in which the mercury is replaced by iodide of potassium." (p. 381.)

Dr. Neligan appears to be very partial to Valerianic Acid and its salts; he thinks that they are unquestionably more certain in their operation than the preparations of the herb, and considers that they will probably, ere long, displace the latter from the Materia Medica. Under the head of Valerianate of Zinc, we find these remarks:

"Valerianate of zinc is a tonic and antispasmodic of much power, and as such is peculiarly adapted for the treatment of neuralgic affections, which are so generally dependent on loss of tone in the system. It has been found especially useful in the treatment of facial neuralgia and vertigo; but I have seen it prove equally beneficial in most of the protean forms of hysterical neuralgia. In short, I look on it as a most valuable addition to the Materia Medica, and I fully agree with the observations of Devay, that the chemical combination proves much more beneficial than the Oil of Valerian and Oxide of Zinc prescribed together." (p. 43.)

We have remarked above, that on some points Dr. Neligan is very brief; and that, in the subdivision of the therapeutic action of certain medicines, some important points are altogether omitted. We cannot find, for example, in any part of the work, a notice of the symptoms produced by Arsenic, when given in rather large medicinal doses or in certain idiosyncrasies, or of the effects produced when unpleasant symptoms begin to manifest themselves; yet it is certainly very desirable that prescribers of this mineral should be made fully aware of these, in order that the use of the drug may not be persevered in under such circumstances. The symptoms we allude to are very characteristic, although apt to be overlooked; viz. the affection of the eyelids, puffiness of face, and gastric disturbance. We find, nevertheless, that some of the effects of poisoning by large doses are mentioned, together with the antidotes to be employed.

We cannot discover that any precautions as to the period of administration of this drug are alluded to in any part of the work; yet it is well known that patients can frequently tolerate the medicine when given soon after a meal, although they are quite unable to do so when it is taken on an empty stomach.

The same defects we find also when seeking to discover the effects produced by too long continuance in the administration of Lead, or when individuals are exposed to its toxic influence. No mention is made of the characteristic blue line on the gums, or of the lead rheumatism, paralysis, &c. Colic, indeed, is mentioned, but merely incidentally, as being produced by Carbonate of Lead more readily than by any other preparation of the mineral. These omissions, we think, are real defects in a work which is intended to be more "practically useful than theoretically scientific."

The accounts of the Physical and Chemical characters of drugs are generally very short; but at the same time embrace most of those points which directly bear upon the subject of Materia Medica.

The Preparations contained in the three British Pharmacopœias are all included in the work, so that it forms a complete dispensatory (of course, from the date of the work, the alterations in the new London Pharmacopœia are not included); we think, indeed, that the space allotted to this subject is disproportionally great, and that much might be advantageously omitted. For example, under the head of Opium, the space devoted to the Therapeutic action of the drug, and the diseases for which it is administered, does not exceed three pages; whereas more is devoted to the account of the various preparations of this Medicine, which are contained in the Pharmacopœias, and to condense them into this space, they are obliged to be so crowded together, as to be by no means readily referred to. Such formulæ are of but little service, unless they can be appealed to with confidence; and few accurate dispensers are willing to trust to works on Materia Medica for their accuracy, turning in preference to the Pharmacopœias themselves, or a good standard Dispensatory, professedly devoted to that department. We cannot speak much about the correctness of the formulæ in the work before us, not having specially gone over it for that purpose; but we can readily see that errors might arise in preparing medicine from it, thus:—

> "Tinctura Opii D.L.E. ('Opium (hard) L.) powdered (coarsely, D.), ℥iij; Proof Spirit, Oij; macerate for 14 days (strain, express, D.) and filter,' D.L.—&c. &c.;"

and after describing the proofs from the Edinburgh Pharmacopœia, it is stated that "the Tincture of Opium, Laudanum, of the three Pharmacopœias is about the same strength." Now, when we find formulæ condensed in this manner, we should naturally expect that when a weight of any substance was given, it would be the same for all the formulæ; but although three ounces of opium are ordered to the two pints of proof spirit, yet in the one Preparation (of the London College), it represents 1440 grains; in the other (of the Dublin College), 1312·5 grains, or a difference of 127·5 grains.

We have made these strictures upon the work before us, not from any desire to speak disparagingly of it, but to call Dr. Neligan's attention to what we think defects, in order that in future editions, which, doubtless, ere long will be required, he may consider whether they could not be altered with advantage. We have no hesitation in expressing our high opinion of the usefulness of the volume, and our conviction that it may be advantageously consulted by all who are engaged in practice. It possesses the virtue of containing a very large amount of useful information on most practical points, and is very free from the incumbrance of useless matter.

6. We have already lengthened this article beyond our original design, and this is one reason for not dwelling upon the Part of Dr. Pereira's elaborate Treatise now before us. There are also other and more cogent reasons for not entering into a discussion of its merits at the present time, for it forms only Part I of Volume II, and we have already noticed the first volume of the present Edition. We may, however, state, that this part is executed in a very superior manner, all new discoveries having been embodied, almost up to the day of publication; and a supplement has been added, containing the alterations in the new Dublin Pharmacopœia, as far as they concerned this Part. In the concluding Parts, Dr. Pereira purposes likewise to introduce the changes in the London Pharmacopœia, by which means he will greatly enhance its value. A steel engraving, containing the microscopic appearances of the different varieties of Starch-granules, has been added; together with many very excellent woodcuts, illustrating the character and structure of drugs. In this Part we are carried through the whole of the Cryptogams, Endogens, and part of the Exogens, viz. the Gymnosperms, Monochlamyds, and Corolliflorals. We shall, however, defer its further consideration, until after the appearance of the concluding Part, which is promised early.

7. We have only received Mr. Squire's edition of the Pharmacopœias since the preceding article was written; and must limit ourselves to a very brief notice of its plan and execution. It essentially consists, as its title imports, of a translation of the new London Pharmacopœia, in combination with those of Edinburgh and Dublin; the respective formulæ being tabulated in three parallel columns in each page. The order adopted is alphabetical; and nothing can more readily bring under comparison the correspondences and differences in the several formulæ than this method, which is aided by a judicious employment of different kinds of type. The different standard of weights employed by the Dublin Pharmacopœia is constantly indicated by the heading "Avordupois Weight," which stands at the top of the Dublin column throughout. And where a preparation essentially the same occurs under different names in the several Pharmacopœias, reference is made from one heading to another. The "Practical Remarks" are few and short; but they are useful as far as they go. We cannot say that we have collated the formulæ with the originals, so as to be able to speak of the accuracy of their transcription; and we have noticed several errata in names, which make us a little suspicious (thus *centaury* is three times spelled *centuary*); so that we must limit ourselves, in our recommendation of the work, to its general arrangement, which is extremely well adapted for the purpose it is designed to answer.

ART. IV.

Observations on the Diseases of the Rectum. By T. B. CURLING, F.R.S., Surgeon to, and Lecturer on Surgery at, the London Hospital, &c.—*London*, 1851. 8vo, pp. 123.

As Mr. Curling states in his Preface that he has not attempted to write a complete treatise on Diseases of the Rectum, but merely to give a concise account of the structural changes of this part, and of the symptoms and treatment of its diseases, with the object of offering such views of pathology as may lead to judicious practice, we have no hesitation in stating our opinion that his object has been fully attained, and that, regarded as a series of practical observations by one whose experience has been very great, the work will prove both acceptable and useful to practical men. The author has left us so little room for criticism, that we shall fulfil our duty to our readers by the simple extraction of a few of the paragraphs likely to be most generally interesting; premising that the book contains twelve chapters, the subjects being successively—Introductory Observations, with Remarks on the Instruments employed for examining and making applications to the Rectum; Irritable Ulcer; Spasm of the Sphincter; Hæmorrhoids; Prolapsus; Polypus; Fistula in Ano; Chronic Ulceration; Stricture; Cancer; Impaction of Fæces; Anal Tumours and Excrescences.

Mr. Curling's observations on the Speculum are worthy of attention:

"The examination of the parts diseased, as well as the performance of certain operations, may be considerably aided by the use of a speculum. They are made of various kinds, some of them ill adapted for the object in view. Thus, many of the dilators are of little use in consequence of the bulgings of the mucous coat of the bowel between the narrow blades of the instrument. There is an old-fashioned but serviceable instrument, consisting of a longitudinal section of a steel tube, with one extremity closed, which has long been employed at the London Hospital in examining these diseases, and which is well adapted for protecting the bowel, and finger of the surgeon, in operations for fistula. Mr. Hilton has recently contrived a plated speculum, with the end closed, and an aperture at the side into which a moveable piece slides. I have often used it, but have found the side-opening too narrow to afford a complete view of an ulcer or pile of any size; and in consequence of the aperture not being carried to the extremity, fæces are liable to lodge there and prove troublesome to remove in protracted examinations, as in searching for the inner orifice of a fistula. Ferguson, of Giltspur Street, has therefore made, by my direction, a plated speculum of a conical form, so as readily to penetrate the sphincter, with the side-opening of sufficient width, and carried to the blind extremity of the instrument: and instead, also, of a moveable piece, I have substituted an ebony plug, fitted with a plate which fits close into the aperture. The plug admits of being more readily removed and replaced than a slide." (pp. 2, 3.)

In the treatment of irritable ulcer, Mr. Curling naturally places his chief reliance on incision through the centre of the ulcer, with simultaneous division of the sphincter. He speaks highly of the use of an ointment containing chloroform, as an application to sensitive ulcers. His formula is as follows:

℞ Chloroformyl, ʒj–ij;
Zinci Oxydi, ʒss;
Olei Olivæ, ʒj;
Cerati Cetacei, ʒiv.
M. fiat Unguentum.

The following observations on the treatment of hæmorrhoids are a very good specimen of the species of practical observations which our readers will find scattered through this useful little book:

"Internal piles, when of such a size as to protrude at the anus, or when subject to inflammation, ulceration, and bleeding, so as to prove a constant source of annoyance and suffering, must be removed by operation. This may be done by excision, by cauterisation, or by ligature. Excision is the quickest and least painful of these proceedings; but there have been so many instances in which dangerous hæmorrhage has occurred after the removal of internal piles with the knife, that few good surgeons now advocate the operation, or venture to perform it. Several eminent operators who have tried excision have acknowledged that they have been obliged to abandon the practice, in consequence of the serious risks which some of their patients incurred from bleeding. Dupuytren, who was an advocate for excising piles, used frequently to have recourse to the actual cautery to arrest the hæmorrhage which ensued; and it is well known that Sir Astley Cooper had some fatal cases in consequence of bleeding after this operation.

"Mr. Colles, of Dublin, was in the habit of transfixing the base of the tumours with a hook before excising them, to prevent their being drawn up within the sphincter, which enabled him to command a view of the parts, in the event of any vessel requiring to be tied.* This mode of securing the parts affords some advantage to the operator; but it often happens in this operation, that, although the bleeding may be comparatively slight at the time the piles are cut off, a large quantity of blood escapes in the course of a few hours afterwards, and gradually accumulates in the rectum. Dieffenbach's plan is preferable to the preceding. He first passed a ligature through the base, and grasping the pile with the forceps, excised it between the forceps and the ligature, which was then tied. The pressure produced by bringing the edges together assists in preventing hæmorrhage. Small elongated piles can be removed in this way without risk.

"Internal piles admit of removal by cauterisation. Dr. Houston, of Dublin, in a paper published in 1843,† strongly recommended the use of nitric acid for the cure of the florid vascular pile; and I have since employed this escharotic in cases of the kind. It has the advantage of being a safe and mild remedy, and is certainly well adapted for destroying the bright fungous looking pile which is so often the source of hæmorrhage, and the cause of much local uneasiness. Means having been taken to bring the pile well into view, the patient should lean over a table, and his nates should be separated by the hands of an assistant. The surgeon may then take a glass pen, such as is now common in the shops, with rather a large aperture at the point, and, having dipped it in concentrated nitric acid, so as to load the bulb of the pen, may apply the escharotic to the entire surface of the hæmorrhoid, until its florid hue becomes quite changed to an ash colour. No speck of red should be allowed to remain. Care must be taken that none of the acid touches the skin at the margin of the anus. The moisture on the surface having been absorbed with lint, and the part smeared with sweet oil, the protrusion may be replaced within the sphincter. The pain consequent on the application is not severe, and the separation of the superficial slough and healing of the sore occasioned by the acid are attended with scarcely any uneasiness. If the pile be not large, this plan answers very well, but it is not sufficient for the removal of hæmorrhoidal flaps and tumours of any great size.

"For the cure of internal hæmorrhoids of any considerable size, the ligature is the safest and most effectual remedy. In order to apply the ligatures properly, it is necessary to promote the protrusion of the piles. For this purpose, a dose of castor oil should be given about six or eight hours before the time fixed for the operation; and a pint of warm water should be thrown into the rectum shortly before the surgeon's arrival. When the fluid is discharged, the piles will descend.

* Dublin Hospital Reports, vol. v.

† Dublin Journal of Medical Science, vol. xxiii.

The application of the ligature being a somewhat painful proceeding, chloroform may be given if the patient desire it. The operation should then be performed, the patient lying on the side with the thighs raised: otherwise the most convenient position is with the body leaning over a table, and the nates separated by an assistant. The growth to be tied should be seized with the volsellum, and drawn out. If the pile be an elongated one, a ligature may be tied tightly round its base. In other cases a curved needle set in a handle, with the eye near the point, and armed with a strong silk ligature, should be passed through the base of the pile from without inwards. The needle is then to be withdrawn, the ligature being left double. The loop being divided, the pile is to be strangulated by drawing the ligatures close round the base, and knotting them as tightly as possible on each side. The other piles are afterwards to be treated in the same way. When the hæmorrhoids are large in size, a notch made with scissors on each side at the part to be girt with the ligature, just before it is tightened, will facilitate the separation without any risk of bleeding. The ends of the ligatures having been cut short, the strangulated piles should be gently pushed up into the rectum. If the operation have been performed without chloroform, a strong opiate may be given at its conclusion. Any swelling and heat about the anus that may afterwards arise, must be relieved by poultices and fomentations. No aperient should be given for several days. The tighter the ligatures are tied, the sooner they ulcerate through and come away. They generally separate in about four or five days, during which period the patient should remain in bed or on the sofa. The detachment of the sloughs leaves, of course, at the lower part of the rectum a sore surface, which bleeds slightly when the bowels are relieved, and some attention will be required until this heals. The motions must be kept soft by mild aperient medicine—as the lenitive electuary, or castor oil. If the sore be slow in healing, it may be smeared night and morning with a liniment consisting of a drachm of the *liquor plumbi diacetatis* and an ounce of the confection of roses; or it may be brushed over with a weak solution of the nitrate of silver." (pp. 33-7.)

This and the following extract are rather lengthy, but we fancy that few of our readers will consider that the pages could be more usefully filled:

"In slight cases of prolapsus, accompanied with internal hæmorrhoids, in adults, the contraction that takes place after the removal of the piles by ligature, or in other ways, will often counteract the laxity of the parts, and afford sufficient support to prevent a return of the inversion. But if this should not be the case, the tendency to prolapsus may be effectually obviated by an operation which consists in the excision of portions of mucous membrane, and a little of the skin from the margin of the anus. The patient being placed on his back in the position usual in the operation for lithotomy, a fold of membrane, more or less broad according to the laxity of the part, is to be seized with a volsellum, or the curved entropium forceps, raised a little, and then excised with a curved pair of scissors. Two portions, one from each side of the rectum, will generally require removal, leaving two oval wounds in the longitudinal direction. It is desirable that the edges of the wound should be afterwards brought together with sutures, not only to secure the speedy healing of the wound, but as the compression occasioned thereby helps to arrest bleeding. Unless, however, chloroform be used, there is some difficulty in applying them, in consequence of the forcible contraction of the sphincter excited by the operation drawing in and concealing the wounded parts. The surgeon must be careful to tie any bleeding vessel that may be divided, for the operation is very liable to be followed by hæmorrhage, which may go on into the bowel without his being aware of it. An examination with the speculum should be made before the patient is left. Cases in which this operation is called for are certainly not common. In persons who have suffered from prolapsus in childhood, it sometimes happens that the parts do not recover their tone at puberty, and that the complaint continues to prove troublesome afterwards. Such a case is very fit for excision. In 1835 I assisted my colleague, Mr. Luke, in performing this operation upon a lad in the London Hospital. He was nineteen years of age, and had been troubled with prolapsus ever since he was three years old. The bowel always descended several inches when he went to stool, and was a source of great annoyance to him. Two oval portions of mucous membrane were excised from the verge of the anus in the way above described. The sphincter immediately afterwards contracted strongly, and completely buried the wounded surfaces. There was no reason at the time of the operation to expect any bleeding; but on visiting the lad in the evening, for Mr. Luke, I was surprised to find him in a state of prostration, with a cold, clammy skin, and shivering. It appeared that on two or three occasions he had discharged a considerable quantity of blood, which had collected within the rectum. Having given him some brandy, I introduced a thick plug of lint, previously oiled, which was effected with some difficulty, owing to a strong spasm of the sphincter. There was no recurrence of hæmorrhage, and the two wounds healed up in the course of a month. The operation was quite successful in preventing further prolapsus. If another case of hæmorrhage from vessels at the lower part of the rectum occurred to me, I should insert a good-sized piece of sponge, which, expanding as it became moist, would more effectually plug the part." (pp. 50-2.)

In applying a ligature to rectal polypi, Mr. Curling very properly cautions us not to tie it so tight as to divide the soft neck. He quotes cases in which dangerous hæmorrhage resulted from this mistake.

The opinions of the author as to the utility of the ligature in the treatment of fistula in ano will be read with interest:

"When the opening in the rectum is more than an inch and a half above the external sphincter, the division cannot be made without risk of hæmorrhage, which the surgeon may find great difficulty in arresting—indeed, death from bleeding has happened after the division of a rectal fistula high up. These cases are best treated by a ligature, which, if properly applied and very gradually tightened, answers very well, and is less tedious and painful than is commonly supposed. The application of the ligature to fistula, though often practised formerly, is now seldom resorted to, the knife being found a less painful and tedious mode of curing the disease. Some years ago, my colleague at the London Hospital, Mr. Luke, devised an ingenious screw tourniquet for gradually increasing the tension of the ligature, by which an improvement is effected in this mode of treatment. A strong cord of dentist's silk having been carried through the fistula by the introduction of an eyed probe with a moveable extremity, and withdrawn at the anus by means of a spring catch passed into the rectum upon the fore-finger of the operator, is to be attached to the screw apparatus, and secured with moderate tightness, but not so as to cause pain. The tension of the ligature is afterwards to be very gradually increased by turning the screw as it gets loose, until the cord cuts its way out. While this process of ulceration is proceeding, the gap behind becomes filled up by granulations; so that, in a day or two after the removal of the ligature, the fistula is found to be cured. Though the treatment by ligature in this way is safe and nearly free from pain, and admits of the patient moving about, the application of it gives more trouble than division of the parts by the knife; and, owing to the necessity for turning the screw at intervals, more attention is required afterwards. On these grounds, incision is preferable in ordinary cases, and, I may add, is so regarded by Mr. Luke; but, in cases of fistula opening so high up in the rectum that the knife cannot be used without danger of hæmorrhage, I should certainly employ the ligature. Such cases, I know, are not common in practice, but they do occasionally occur. I witnessed the treatment by ligature of two of Mr. Luke's cases, in one of which the internal opening of the fistula was two inches above the anus, and the other as high as the point of the finger could reach; and a patient with a fistula of a similar character to the last was under my care in the hospital last year, but the state of the man's general health prevented my adopting any mode of curing the local disease." (pp. 71-3.)

The following practical hint is worth notice:

"Mr. Copland has the credit of having first practised the division of the external sphincter for the cure of fistulous communications between the rectum and vagina. The proceeding is applicable to another class of cases, to which I may here briefly allude. The extremity of the septum between the vagina and rectum occasionally becomes lacerated in labour, the patient being afterwards unable to retain her fæces. The cure of this distressing infirmity may be effected by paring the edges of the gap, and, after division of the external sphincter on each side, bringing them together with sutures, which should be tied in the vagina. An opiate afterwards will keep the bowels at rest for two or three days, and the sutures may be taken out on the third day. The operation does not always succeed, but the double division of the sphincter much lessens the chances of failure." (pp. 75-6.)

The morbid anatomy of chronic ulceration of the rectum is thus described:

"My inquiries into the morbid anatomy of the rectum have led me to remark the frequency of ulceration of its mucous lining, not only in cases of dysentery, and as a consequence of the ordinary diseases of the part, such as stricture and cancer, but as a separate affection. In several specimens which I have examined, ulceration was diffused over a considerable extent of surface. I have observed the whole of the lower part of the rectum stripped of its mucous membrane for a distance of two or three inches. This extensive disease is sometimes, indeed generally, attended with thickening and consolidation of the subjacent tissues, without diminution in the caliber of the bowel. The muscular coat is in some instances hypertrophied. In one case, the mucous coat for a short distance within the sphincter was so riddled with holes as to form, as it is described in the post-mortem book, 'a perfect cribriform tissue,' the submucous tissues being at the same time much thickened. I have seen the mucous membrane ulcerated in patches, the sound portions being in some places detached from the muscular fibres beneath, so as to form bridges more or less broad, or merely some narrow bands or bridles. There were frequently abscesses and fistulous passages in the thickened tissues around the diseased rectum. In two instances ulceration had produced a perforated opening communicating with the peritoneum, death having been caused by the escape of some feculent matter into the abdomen, and inflammation of the serous membrane. In other cases the peritoneum was involved in the consolidation, and inflamed without being perforated, the omentum in one case being adherent to the anterior part of the rectum." (pp. 77-8.)

The chapter on Stricture of the Rectum is worthy of a careful perusal. The author refers to cases in which laceration of intestine was caused by the use of the rectum bougie by patients themselves. In tight strictures, with considerable induration of the submucous tissue, Mr. Curling facilitates dilatation by previous division of the thickened parts. He uses a straight probe-pointed bistoury, and directs two or three notches towards the sacrum through different parts of the contracted ring.

We shall conclude our series of extracts by some observations on the means of relieving impassable strictures of the rectum by artificial openings for the passage of the fæces:

"In obstructions of the rectum, an artificial opening for the passage of the fæces may be made into the colon, in the left groin, by the operation commonly called Littré's; or in the left lumbar region, by the operation known as Callisen's, modified by Amussat. A careful consideration of the advantages and disadvantages of these two operations leads me to give the preference to the former. I do not ground this conclusion upon the statistics furnished by the tables of Amussat and Vidal, because I do not attach much value to them. The cases of Littré's operation are not only limited in number, but, in several of them, the colon was not opened in the left groin, the division of the peritoneum being the only circumstance in common. Nor do the tables afford information of the period of constipation, or of the extent to which the viscera were disturbed in the operation. Callisen's operation is not only difficult of execution, but the wound is necessarily of large size, especially in stout people. But, it is not so much for these reasons that I am indisposed to adopt it, as in consequence of the operation leaving the patient exposed, afterwards, to risks and annoyances, which are, in a great measure, avoided when the colon is opened in the groin. Thus, I find in the published account of several of these cases, that the artificial anus in the loin had a strong disposition to contract, so as to interfere with the free passage of the fæces, and that repeated dilatation was necessary to secure the patency of the opening, the least neglect exposing the patient to considerable danger. It is also extremely difficult to adjust any apparatus, in order to occlude the aperture, and prevent the discomfort arising from the continual escape of flatus and fæces; and, as the orifice is without the observation of the patient, he becomes very dependent on the assistance of others. These serious inconveniences, if experienced at all, are so, in a much less degree, when the anus is in the groin. The patient can cleanse and attend to the part himself. The aperture being nearer the surface, does not show the same disposition to contract, and it admits of being closed by a well-adjusted truss, when no need exists for relieving the bowels. And I think that these advantages, so important to the comfort of the patient, are by no means counterbalanced by any increased risk from opening the peritoneum. The operation is easily performed, and, as no exploratory attempt is required to relieve the obstruction, a very small opening in the peritoneum is sufficient for the object in view. Even Callisen's operation is not free from all risk of peritonitis from the disturbance of parts; and the magnitude of the incision probably renders the danger to life from its performance quite as great as that resulting from the operation in the left iliac region, carefully and cautiously performed.

"The abdomen may be opened in the left iliac region by a perpendicular incision, about three inches in extent, commencing two inches above Poupart's ligament, and an inch external to the course of the epigastric artery. The fibres of the abdominal muscles being cut across, will help to keep the wound open. The peritoneum being divided, the distended colon will immediately protrude at the wound. A curved needle, armed with a silk ligature, being passed through the coats of the intestine, above and below, to prevent its receding when emptied of its contents, the bowel may be opened by a longitudinal incision, about an inch in length, in the space between the retaining ligatures.

"In December last, Mr. Luke performed this operation on a man, aged sixty, in consequence of an obstruction produced by a stricture at the point of termination of the sigmoid flexure. The patient recovered, and is able to follow his occupation, his comfort being greatly promoted by the application of a truss over the orifice. In this case, the intestine was retained by a ligature, passed through one of the appendices epiploicæ." (pp. 106-8.)

We are not disposed to consider this book as the best of Mr. Curling's works, nor as a remarkable addition to surgical literature: but regarding it, as the author evidently intends it to be regarded, as a series of practical observations on a subject of considerable importance, all we could say of it would be in its favour.

ART. V.

1. *Researches on Magnetism, Electricity, Heat, Light, Crystallisation, and Chemical Attraction, in their Relations to the Vital Force.* By KARL, BARON VON REICHENBACH, PH.D. Translated and Edited, at the express desire of the Author, with a Preface, Notes, and Appendix, by WILLIAM GREGORY, M.D. F.R.S.E., Professor of Chemistry in the University of Edinburgh, &c. Parts I and II, including the Second Edition of the First Part, corrected and improved.—*London,* 1850. 8vo, pp. 463.
2. *Letters to a Candid Inquirer on Animal Magnetism.* By WILLIAM GREGORY, M.D. F.R.S.E., Professor of Chemistry in the University of Edinburgh.—*London,* 1851. 8vo, pp. 528.
3. *On the Truths contained in Popular Superstitions; with an Account of Mesmerism.* By HERBERT MAYO, M.D., formerly Senior Surgeon of Middlesex Hospital, Professor of Anatomy and Physiology in King's College, Professor of Comparative Anatomy in the Royal College of Surgeons, London, F.R.S. F.G.S., &c. Second Edition.—*London,* 1851. 8vo, pp. 217.
4. *What is Mesmerism? An Attempt to explain its Phenomena on the admitted Principles of Physiological and Psychical Science.* By ALEXANDER WOOD, M.D., Fellow and Member of Council of the Royal College of Physicians (of Edinburgh), &c.—*Edinburgh,* 1851. 8vo, pp. 32.
5. *The Mesmeric Mania of* 1851; *with a Physiological Explanation of the Phenomena produced. A Lecture.* By JOHN HUGHES BENNETT, M.D. F.R.S.E, &c. &c.—*Edinburgh,* 1851. 8vo, pp. 21.
6. *Electro-Biological Phenomena Physiologically and Psychologically considered.* By JAMES BRAID, M.R.C.S. Edin. ('Monthly Journal of Medical Science,' June, 1851.)

Mr. MAYO makes a shrewd remark on Mesmer, which has an application to more modern philosophers of his class:—"As the eyes of some animals are fitted to see best in the dark, so the mental vision of some original minds prefers exercising itself on obscure and occult subjects. *Whoever indulges this turn will certainly pass for a charlatan: most likely he will prove one.*" The first three authors on our list are, as the reader will observe, men of some eminence—scientific and professional; they all manifest a preference for the "occult;" and whether they are to be considered real or suspected charlatans, will be decided differently according to the bias of their incredulous readers. The common object of their works is to prove that astrology, magic, witchcraft, sorcery, divination in various ways, as necromancy, hydromancy, phantasmomancy, &c., and the "black arts" in general, have a foundation, more or less substantial, both in philosophy and fact. The only point in which the ancient or mediæval "black arts" differ from the modern, is in the circumstance that no "black" or Satanic agency is believed to be in operation in the latter, but only a force universally prevalent in creation, termed "Od," or more euphoniously "Odyle." By means of this force, converse may be held with departed spirits—events past, present, or to come, rendered visible to the illuminated or clairvoyant; the soul enabled to range freely through matter and through space; to scan with equal facility the moon or Sir John Franklin's condition at the pole; the thoughts of the by-stander, or the minute anatomical structure of his bodily frame. To all which we can truly say with Cicero, "Magnifica quædam res et salutaris, *si modo est ulla:* quæque proxima ad deorum vim natura mortali possunt accedere."* We do not find, however, that the phenomenal histories detailed by these learned writers are the less wonderful or the more probable, because shown to result from this newly-discovered "Od" force. We are sure the imaginative readers of both Mr. Mayo and Dr. Gregory will not the less thrill with mysterious terrors (like the former, when listening many years ago to tales of seers, ghosts, and vampyres), will raise their eyes with uneasy apprehension to mirrors, and will feel their skin creep through the sensible odylic afflatus of an invisible presence. The cold, calm light of science thus apparently thrown upon these phenomena, is, indeed, but a lurid phosphorescence, rendering them more dismal. We need only instance the following story told by Dr. Gregory: A lady Pythonissa, a patient of Mr. Atkinson's, is described as watching (mentally and clairvoyantly) the daily doings in the distant sick-chamber of a near relative. She "*saw* the medical men, described the treatment, and pursued the case from day to day, involuntarily and against her own wish, as it distressed her severely, till the distant patient died." This dreadful state of mind appears to have been induced by "long and laborious magnetic treatment," to which she had been subjected by Mr. Atkinson, for the cure of "a most distressing complaint." If no other result than that mentioned had followed, it may well be doubted whether the cure was not more "distressing" than the disease. But these were only a portion of the phenomena that she saw thus mentally and spiritually: the spectacle of the death of her relative did not close the revelations of earthly doings:

"After this, she was still involuntarily drawn to the scene of death, saw the corpse, described its appearance, and all the proceedings connected with the interment. Even after that, she felt compelled to visit the corpse in its grave, and *described with horror the change which took place in it.* It was not for a long time that she was enabled to get rid of these painful visions. But everything that could be ascertained and verified was found exact." (p. 470.)

Dr. Gregory also shows that highly-lucid clairvoyants "can call spirits from the vasty deep."

"They hold long conversations with spirits, to whom they often give names, and who, in many cases, according to their account, are the spirits of departed friends or relations. The remarks and answers of these visionary beings are reported by the ecstatics. * * Some can summon, either of themselves, or with the aid of their attendant spirit, the spirit or vision of any dead relation or friend, and even of persons, also dead, whom neither they nor the magnetiser have ever seen, whom, perhaps, no one present has seen; and *the minute description* given in all these cases, of the persons seen or summoned, is *afterwards found to be correct.*" (p. 225.)

The earliest necromantic Pythonissa mentioned in history, was consulted by a monarch about to do battle for his kingdom and his life. He wished to question the spirit of his departed friend and adviser, whose body had been committed to his last home amidst the lamentations of the

* De Divinatione, lib. i, § 2.

whole people; and resolved that the odylic force should be put in operation to bring his friend again to him. "Dreams, and Urim, and the prophets," had all been deaf to his earnest inquiries; he therefore commanded his servants to seek out "a woman that hath a familiar spirit." The sacred narrative of her proceedings is as graphic and conclusive as it is simple. After stating his wishes to her, "Then, said the woman, whom shall I bring up unto thee? And he said, Bring me up Samuel. And when the woman saw Samuel, she cried with a loud voice; and the woman spake to Saul, saying, Why hast thou deceived me? for thou art Saul. And the king said unto her, Be not afraid, for what sawest thou? And the woman said unto Saul, I saw Gods [*Elohim*, the priest-ruler] ascending out of the earth. And he said unto her, What form is he of? And she said, An old man cometh up, and he is covered with a mantle." 1 *Samuel*, chap. 28, v. 11-14.

With accounts *in all essential respects analogous to this*, the works of Dr. Gregory and Mr. Mayo abound; accounts stated with the appearance of philosophic calmness and accuracy, and skilfully adapted to establish the proposition we have mentioned. If Saul asked as to his future destiny from the clairvoyante of Endor, in Judea, the proceedings of Sir John Franklin, at the Pole, are, upon inquiry of her, accurately detailed by the clairvoyante at Bolton, in Lancashire. The Witch of Endor summoned the spirit of Samuel; the military wizard of Edinburgh (magnetised by Major Badeley) saw Mary Queen of Scots by "sympathetic retrovision," and the murder of Rizzio, in all its details, even to the dagger and the gashed throat. The uses of this new science are as numerous (necessarily) as those of the "black arts." Dr. Gregory states these with admirably-consistent logic. After enumerating the *therapeutic* use of animal magnetism, and noticing its value in psychological investigations, inasmuch as some clairvoyants "can see and describe consistently the actual physical changes in the brain" which accompany all mental operations, he mentions those of the branches termed sympathy and clairvoyance:

> "They may enable us to obtain information about absent friends or relations; nay, they are actually often used for that purpose. They may be used to discover missing or stolen goods and documents, and for this purpose also they are daily employed. In Part II will be found some instances of such applications of lucidity. I have already said that I think it far from improbable that this power may be so used as to throw light on obscure historical points, and to discover documentary evidence in regard to them. Moreover, I have already described the principle, which certainly has nothing impossible in it, of the use of animal sympathy in the projected snail telegraph, or, as it is called by the inventors, the Pasilalinic Telegraph; and I have also mentioned the application of lucidity to the inspection of the living frame, healthy or diseased, for anatomical and physiological, as well as medical purposes." (p. 271.)

These uses are, indeed, most important, and may be attained by means of any suitably "lucid" person. They will all be understood, probably, by our readers, except the "Snail Telegraph." This is founded on the *very* remarkable discovery, that snails "after having once been in communication or contact, continue ever after to sympathise, no matter at what distance they may be." Every letter, therefore, of this telegraph has a corps of snails attached to it at each terminus, in sympathetic support. A telegraph between "Paris and America," based on this discovery, has already been worked. "Words spelled in Paris by M. Benoit, and also by M. Allix [the narrator] himself, were instantly read in America, and as instantly replied to by words spelled there, and read in Paris." What advantages the "Pasilalinic telegraph," with its numerous corps of snails, will have over a duly qualified and trained clairvoyant, fully possessed with a spirit of divination—the *pneuma Puthonos*—is not stated; but trifling incongruities of this kind are not usually noticed by our authors. Mr. Mayo hardly goes so far as Dr. Gregory in his practical applications, but he supplies an omission or two worthy notice. Thus, he gives the rationale of the divining-rod, and describes an instrument of his own invention termed the divining-ring, or *Odometer*, a measurer and detector of the "Od" force. *If* its alleged uses be proved to be real, it will be hardly less valuable than the compass. Mr. Mayo observes, with regard to his researches as to its uses, "but I am not presuming in asserting that the present inquiry has immediate practical applications such as seldom fall to the lot of so young an investigation. The odometer will prove a useful test of the presence and qualities of electric, chemical, and magnetic actions; it will serve to determine the electro-chemical qualities of bodies; and in large or small crystalline masses—in the diamond, for instance—will serve to show the axes and distinguish the opposite poles. In reference to biology, it will probably furnish the long-wanted criterion between death and apparent death; for I observe that, with an egg long kept, but still alive, though no longer likely to be very palatable, the odometer freely moves in the way described in the fourth section; but it treats the freshest egg, when boiled, as if it were a lump of zinc." (p. 216.)

Our readers, we really think, will curiously expect a more detailed explanation of all this. Amongst the antiquated practices that have been revived of late years, the practice of the "black arts" might reasonably have formed an exception. In a humorous story in a late number of his 'Household Words,' in which Charles Dickens graphically describes the astonishment created by the parish of Tittlebatington, by the appearance of the Reverend Arthur de Notre Dame, and the performance of his mediæval innovations, the "two medical men" of the parish are represented as being horrified on hearing that the reverend gentleman had brought a homœopathic case of medicines with him, and "had actually prescribed three globules of Bryonia of the third dilution to an old woman with the lumbago;" the said old woman increasing their alarm by talking of homœopathy, and recommending "globulars" to every one she met. Will Charles Dickens permit us to observe, in the name of "the two medical men" of Tittlebatington, that "globulars" are trifles light as air compared with "magneto-magic crystals, and therapeutic animal magnetism?" Hitherto the revival of mediæval forms has been limited to theology and church matters; but the revival of *Pagan* practices has seized on medicine and the medical art. Practitioners have felt, when they heard or read of this mania amongst their neighbours the clergy, as they have felt when reading of the plague in Egypt or the cholera in India; now it is amongst them, with this difference, that *the whole of the Christian æra* is passed over!

The entire course of events, indeed, has of late been characterised by the marvellous. We prayed in our national liturgy for protection from "plague, pestilence, and famine," but with little thought that the prayers were needed;—tradition and history obscurely spoke of certain frightful ills of this kind that happened at some far distant time in the mediæval dark ages, but now past for ever. Yet we are at this moment counting up our nationally lost and dead from the three, and reckon them by millions! In like manner we have prayed for protection "from battle, murder, and sudden death," and with similar feelings; yet the din of fierce battles has hardly ceased to reverberate from the adjoining continent, and we still hear from time to time of the "murder and sudden death." The ancient Egyptian philosophers had a notion which, inasmuch as it was coeval with a belief in the science and art of magic and its analogues, and their professed practisers, the "*Isiacos conjectores*," we can here very fittingly mention, as completing our picture. They had a notion that the whole world, and all that therein is, is subjected to regularly recurring periods of destruction and renovation. At the end of certain cycles, kosmic and terrestrial events begin again in the same order. The same men are doomed to be born again, and perform the same actions as before; the same arts are to be invented, and the same cities built and destroyed. Virgil elegantly introduces this doctrine into his fourth Eclogue, and applies it alike to the time then present and the time future—not forgetting a hint at its clairvoyante origin. Of the present he observes:

> "Ultima Cumæi venit jam carminis ætas:
> Magnus ab integro sæclorum nascitur ordo.
> Jam redit et virgo, redeunt Saturnia regna:
> Jam nova progenies cœlo demittitur alto."

Of the future he is very clear:

> "Alter erit tunc Tiphys, et altera quæ vehat Argo
> Dilectos heroas; erunt etiam altera bella:
> Atque iterum ad Trojam magnus mittetur Achilles."

Here, then, in the completion of the cycle, the alarmed "medical man" may find a reasonable source of comfort. The inevitable fate which has brought round to us the wicked or foolish doctrines of a past age, will now; as then, bear with it the inevitable antidote to their folly and knavery. We shall have another Cicero, another Bacon, another Newton, another Sydenham, another Luther, another Hampden, another Elizabeth. Again the age will stamp out the reviving embers of despotism, superstition, and false philosophy—

> "Atque iterum ad Trojam magnus mittetur Achilles."

We have already observed, that the works before us are professedly scientific and philosophical, and that they profess to demonstrate, by experiment and observation, the existence, the laws of action, and the results, of a new force. Now, as Mr. Mayo and Dr. Gregory are but disciples of Von Reichenbach, it seems to us that, in the first place, we ought to analyse critically the researches of the latter, that we may be able to estimate the validity of the basis on which these new doctrines rest. Having done this, we shall be in a position to investigate the nature of the phenomena, as seen from the side of human and comparative physiology; and so, in the third place, determine the practical and philosophical value of the results. If we do not mistake this matter, we shall be able to show—that, although many of the phenomena are real, the "Od" force is imaginary, and the scientific basis altogether unsound;

—that, although the *alleged* uses of animal magnetism are for the most part visionary, and at best hypothetical, it presents to the medical practitioner a new means of investigating the functions of the brain and nervous system, and of elucidating their physiology, pathology, and therapeutics; —and that, therefore, the phenomena and alleged phenomena are most deserving the notice of the profession.

It is, indeed, with reference to these secondary and practical results alone, that we bring these works and the investigations they detail before the profession; and we cannot omit this opportunity of congratulating Messrs. Braid, Bennett, and Wood upon their efforts to place Mesmerism and mesmeric doings in their true position, in a way calculated to reach and enlighten the popular mind. As regards the public at large, the statements we have to notice cannot go forth without rousing an intense spirit of curiosity; as regards the practitioner, they cannot be passed over with a sneer at the credulity of the writers or an imputation on their veracity. The public will demand and *will have an explanation* of statements made on what must be considered to be competent authority, whatever the professional man may say; and, if there be any uses or advantages to be derived from the investigations and inquiries, will require that those uses and advantages be rendered professionally available. It is true that, amidst the numerous duties of the practitioner, there is little time left him for investigation and inquiry into new views; and he cannot reasonably be expected to waste his precious time in the examination of every wind of doctrine, however absurd and however clamorous for notice its author be. These principles apply to all branches of science bearing on the art of healing; but in the present instance they have a special relation to the so-called science of Mesmerism. Dr. Gregory (we need hardly say), as the descendant of a very eminent physician, and as the occupant of an important chair in a University renowned for the culture of mental philosophy and medicine, will command the attention of the public to a very great extent; and has certainly a claim upon the critical notice of the profession, which is justly denied to men of minor position and pretensions.—Now, as we are about again to pass over the dangerous ground of "Mesmerism" and mesmeric doings, we may be excused reverting to and again stating the principles we formerly announced, as those by which the medical profession should, in our judgment, be regulated in their relations with this form of empiricism. In our former article on the relations of quackery to true medicine, we maintained that the honest, pains-taking inquirer had a right to be *heard*, however novel or however startling his views. We maintained that the medical profession, of all others, ought to show a spirit of philosophical inquiry and of unsectarian, unbigoted habits of thought. We maintained that these were necessary to the attainment of the great objects of professional life, and to the well-being of the profession itself. On the other hand, we maintained that an appeal to the judgment of the lay public in matters of professional dispute was injurious to that well-being, and obstructive to the attainment of the great objects for which Divine Providence had called the profession into existence. Hence, public exhibitions of mesmeric phenomena, with a view to gain or the attainment of notoriety and professional employment; the insertion of advertisements and essays in journals intended for popular circulation to the praise of

Mesmerism as a therapeutic agent, and the dispraise of those methods which are founded on the experience of many generations of physicians and surgeons; the public sanction of the practice of medicine, mesmerically, by persons not duly educated in the knowledge of disease and its treatment, and the defence of such persons when their ignorance, or folly, or knavery has rendered their patients their victims, and has been reproved and exposed by the duly authorised and qualified practitioner,—are all to be discountenanced in every possible way, and those who are deliberately guilty of them placed without the pale of professional communion. Our ethical views have been controverted by some of those persons to whom they were especially applied, but we cannot see in the defensive arguments advanced any justification of the line of conduct pursued: the *tu quoque* is a weak answer to the reclamations of the profession, for, even if justified by facts, it can never alter the foundations of correct professional morals. As to the extreme party *in* the profession who will not sanction any inquiry, however guarded, into the statement and views of excommunicated or erring persons, we can only state here that we have little to add to what we have already adduced, and nothing to retract.

The professional position of Dr. Mayo is stated on the title-page of his book.—The following extract will show that the position and reputation of Dr. Gregory are equally in favour of Baron Von Reichenbach, of whose work he is the translator and sponsor; for he thus speaks of that gentleman, and of his qualifications for conducting an inquiry like that necessary to demonstrate the existence of a new, universal, and most potent force in nature:

"The qualifications of the author for such an inquiry are of the very highest kind. He possesses a thorough scientific education, combined with extensive knowledge. His life has been devoted to science, and to its application to the practical purposes of mankind. He is known as a distinguished improver of the iron manufacture in his own native country, Austria. He is a thorough practical chemist; and, by his well-known researches on Tar, has acquired a very high position. But in Geology, Physics, and Mineralogy he has been equally active; in particular, he is the highest living authority on the subject of meteorites or aerolites, of which remarkable bodies he possesses a magnificent collection: of his knowledge on this subject good use is made in this work.

"But these are the least of his qualifications. He has a turn of mind, observing, minute, accurate, patient, and persevering in a rare degree. All his previous researches bear testimony to this, and, at the same time, prove that he possesses great ingenuity and skill in devising and performing experiments; great sagacity in reflection on the results; and, more important than all, extreme caution in adopting conclusions, reserve in propounding theories, and conscientiousness in reporting his observations. He has been found fault with for too great minuteness of detail; but this fault, if in such matters it be a fault, arises from his intense love of truth and accuracy—a quality which, when applied to such researches as the present, becomes invaluable, and cannot easily be pushed to excess.

"It therefore appears that Berzelius, who well knew the value of the author's labour, was right in saying that the investigation could not be in better hands. Having myself been familiar with the author's writings, and in frequent correspondence with himself, for twenty years, I have here ventured to add my humble testimony to that of the great Swedish philosopher." (*Editor's Preface*, p. xvi.)

Although Dr. Gregory is *Medicinæ* Doctor, his friend is *Philosophiæ* Doctor; but Dr. Gregory is only nominally *Medicinæ* Doctor, his whole time being, we believe, devoted to chemistry and physics, to the exclusion of medicine and its dependent sciences. The medical reader will not, therefore, be surprised to observe, that in the most flattering and impressive enumeration of Baron Von Reichenbach's qualifications for this investigation, no mention is made of his *theoretical or practical knowledge of physiology*, or of that branch of it which has been designated *neurology*. To what extent Reichenbach or his learned translator are conscious of the importance of such knowledge to a right investigation of the phenomena discussed in this work, does not appear; but, from Dr. Gregory's total silence on this point in his able laudation, from certain other remarks which he makes, and from the general tenor of the work itself, we can only infer, and infer most decidedly, that neither of these gentlemen possess that most essential qualification, or are even conscious that they ought to possess it. These remarks are not applicable to Dr. Mayo, inasmuch as he *was* well known at one time for the ability and zeal with which he handled neurological questions. Whether the atmosphere of the Rhine-land has mystified his vigorous intellect, or whether long-continued disease has exalted his imagination at the expense of his logical powers, we cannot say; but the unreserved trust with which he receives Von Reichenbach's results as grand truths, and adopts his "facts" as the facts of Mesmerism passed through "the filter of science," lead us (necessarily, as we shall show,) to suspect that something of the sort has happened, and that he has utterly mistaken show for substance, and an investigation of dreamy visions for the rigid realities of scientific research.

In making these preliminary remarks, we do not wish in any degree to deprecate the truthfulness, sincerity, and accuracy of any one of the three authors. In particular, we are willing to take the statements contained in Baron Von Reichenbach's work as statements of facts, and allow them, in respect of their *truthfulness*, the same degree of weight that we would allow to any statements whatever. We do not in any way impugn the *moral* qualities of the narrator, nor of the persons upon whom he experimented and whose observations he chronicles; for to do this would deprive the lesson they teach us of one half its value. The whole is a marvellous history of mutual and unintentional deception; and in this respect it is that, in proportion as the record is truthful and the phenomena accurately described, in the same proportion this book of Baron Von Reichenbach's may be esteemed as an important contribution to the *physiology of the nervous system* and *the philosophy of mind*. But if we were to look on the work as an ordinary scientific production, we do not think we ought to accept the account of the phenomena and the inferences derived from them as *faultless*. We should think it right to draw different *inferences* from the phenomena; or even to suppose, occasionally, that the phenomena themselves are not stated so clearly or so well as they might be; nay, we should claim the privilege of lovers of truth, and dissent altogether (as we shall do) from the conclusions drawn by these earnest and enthusiastic inquirers, without impugning or wishing to impugn the veracity of any individual whatever.

We are induced to make these remarks here, because we have read this volume with an opinion touching the author from which hardly any one will dissent; namely, that there is great merit due to Baron Von Reichenbach for his indomitable perseverance and disinterested labour. If he has not, in our judgment, discovered a new force, he has at least merited success; if, in treading the treacherous bogs of neurological inquiry by means of hysterical women, and "sensitives" of each sex, of all ages, and of every station in life, he has been led away by the Will o' the wisps, so apt to betray the unsuspicious and inexperienced inquirer in this branch of knowledge, into difficulties and bewilderment, he has only succumbed to the fate which has overtaken men of equal eminence; while he has the merit, at least, of not having converted his researches into cash, or, being *self*-deceived, of not having wilfully deceived others. This is, indeed, no slight merit, when we reflect how fatally to their own honour, to the public good, and to the reputation of our common profession, some of the medical body have thus erred. With these views, and on the principles and for the reasons stated, we shall proceed to analyse the phenomena alleged to depend upon the new force, and show, at the same time, to Dr. Gregory and to his very enthusiastic and pains-taking friend, the Baron Von Reichenbach, in what way they have abandoned the true method of inductive inquiry, such as it is in our judgment; and how, by so doing, they have been led into errors which cannot fail to lessen their reputation as philosophers. To adopt Dr. Gregory's doctrine, "we must carefully distinguish between the facts themselves and the various, often absurd, explanations and theories which unscientific observers seem to think it necessary to devise and insist on giving." We also fully accept the rules of criticism laid down by Von Reichenbach, and cordially too, for he assures us that, as he strongly feels how defective his labours may be, he will thankfully receive every notice of any deficiency expressed with proper feeling, and experimentally improve his work accordingly. We are inclined to think that we shall convincingly point out many defects, to the overthrow of his grand deduction; and we shall, we here frankly state, abide as much as we can (for we also have our defects) by the rules of criticism subjoined:

"He who assumes the right publicly to sit in judgment and to pronounce sentence on a scientific work is, before all things, bound in duty to inform himself thoroughly of its contents; and he is further bound to support his sentence, as all public judges do, by the reasons on which he thinks himself justified in pronouncing it. This duty is the more indispensably incumbent on him, because his judgment is one-sided, and requires the control of public opinion; moreover, the author, whose work is judged, is justly entitled to have the means of defending it." (*Preface to Second Edition.*)

With such sound principles, we hardly expected to find Baron Von Reichenbach, in speaking of the medical practitioners and physiologists who are unbelievers, say—"The majority of the former (medical men) reject all study of the phenomena, because they *cannot* comprehend the connection of cause and effect in them; the majority of the latter do so, because they *will not* comprehend this." Undoubtedly *all* new views, whether true or false, must at first be doubted: it is the natural course of events, and is dependent upon fundamental properties of the human mind. *To what extent* they may be doubted, and for *how long*, will depend upon various circumstances; but we think Baron Von Reichenbach hardly does the medical profession and physiologists justice while thus doubting, when he ascribes *incapacity* to the majority of the one, and *dishonesty* to that of the other. He must really tolerate that scepticism which can only excite the inquiry he courts, and the exposure of the defects which he acknowledges to exist, without a sweeping condemnation like this. And if, after all, he succeeds in establishing his doctrines, he may well bear in mind, that the doubts which naturally arise as to their correctness may after all be in great measure attributable to defects in his own method. Physiologists and the medical profession would hail the *satisfactory* demonstration of the new force with delight. To us, *some* of the odylic phenomena, considered as really *objective*, are not at all incredible, and are, in this respect, quite unlike those of clairvoyance. We know of no reason why magnets should not appear luminous in the dark, except that we and the greater number of men and women cannot see their luminosity; but this may be from a defect in us. Still, on the other hand, the "sensitives" who *do* see it, *may* see it *subjectively* from a defect in them; and as the non-seers are to the seers in the acknowleged proportion of two in three, we really think the probability that the phenomena are subjective rather than objective is in about the same proportion. Now, if, after applying the current doctrines of cerebral phrenology and mental philosophy to the elucidation of these phenomena, the majority of medical practitioners and physiologists can also *explain* them as *subjective* phenomena, we do not think Baron Von Reichenbach or his friend, Dr. Gregory, is justified in imputing incapacity and dishonesty; for we regret to observe that the translator, more than once, endorses the unfavorable opinions of the author. These gentlemen must be a little more tolerant and a little less dogmatical. No men of independent habits of thought can be driven into belief; their reason must be appealed to, and their objections calmly met.

In seven treatises, Baron Von Reichenbach successively demonstrates the existence, phenomena, and relations of "Odyle," and at the conclusion of them gives a summary of his experiments and observations in the form of "propositions, physical and physiological." We subjoin a condensed abstract of the leading views contained in these, using the *words* of the author:

"The time-honoured observation, that the magnet has a sensible action on the human organism, is neither a lie, nor an imposture, nor a superstition, as many philosophers now-a-days erroneously suppose and declare it to be, but a well-founded fact, a physico-physiological law of nature, which loudly calls on our attention. It is a tolerably easy thing, and everywhere practicable, to convince ourselves of the accuracy of this statement; for everywhere people may be found whose sleep is more or less disturbed by the moon, or who suffer from nervous disorders. Almost all of these perceive very distinctly the peculiar action of the magnet, when a pass is made with it from the head downwards. Even more numerous are the healthy and active persons who feel the magnet very vividly; many others feel it less distinctly; many hardly perceive it; and, finally, the majority do not perceive it at all. All those who perceive this effect, and who seem to amount to a fourth or even a third of the people in this part of Europe, are here included under the general term 'Sensitives.'

"The perceptions of this action group themselves about the senses of touch and of sight,—of touch, in the form of sensations of apparent coolness and warmth; of sight, in the form of luminous emanations, visible after remaining long in the dark, and flowing from the poles and sides of magnets.

"The power of exerting this action not only belongs to steel magnets as produced by art, or to the loadstone, but nature presents it in an infinite variety of cases. We have first the earth itself, the magnetism of which acts more or less strongly on sensitives. Next the moon, which acts by virtue of the same force on the earth, and, of course, on sensitives. We have, further, all crystals, natural and artificial, which act in the line of their axes; also heat, friction, electricity, light, the solar and stellar rays; chemical action especially; organic vital activity, both of plants and animals, but especially that of man; and, finally, the whole material universe.

"The cause of these phenomena is a peculiar force, existing in nature and embracing the universe, distinct from all known forces, and here called Odyle." (pp. 209-10.)

The *means* whereby these and numerous minor propositions (to be noticed hereafter) were determined, were the kind of individuals referred to, who, on being shut up in a dark room, stated what they *saw* to Baron Von Reichenbach, or who, when subjected, under very various circumstances, to the influence of Odyle, stated to him what they *felt*. The philosopher himself *never either saw or felt* the phenomena during the whole course of his inquiries. To him the whole "Odylic" creation is a sealed book, and "we presume" remains so. This is a very curious circumstance; for, as the faculty of "sensitiveness" depends upon a peculiar disposition of the whole nervous system, it might reasonably be expected, either that Baron Von Reichenbach himself would have at least *temporarily* enjoyed this peculiar property, or, any how, would have endeavoured to *induce* it in himself, artificially. With regard to the sensorial phenomena, on which the whole series of propositions hangs, the author observes:

"The light diffused by odylically-excited bodies is exceedingly feeble, and is, probably on this account, not visible to every eye. Those who are only moderately sensitive must remain a long time, perhaps two hours, in absolute darkness before their eyes are sufficiently prepared to enable them to perceive this light. During the whole of this time the eye must not be reached by the smallest trace of any other light. But the power of perceiving the odylic light cannot depend alone on a peculiar acuteness of vision, because all those who are capable of seeing it are, without exception, possessed also of that peculiar sensitiveness which enables them to recognise odylic impressions by the sense of feeling, and to distinguish between the odylic sensations of warmth and coolness, as well as between the pleasurable and offensive feelings they experience; and these sensations are constant. Now, since these different powers of perception are, in certain persons, namely, in the sensitive, always present together, we must regard them as associated; and they seemed to depend on a peculiar disposition of the whole nervous system, the nature of which is unknown, and not on any peculiar state of individual organs of the senses." (pp. 214-15.)

Baron von Reichenbach, so far as we can gather, *never instituted any inquiries into the nature of this "peculiar condition of the nervous system."* This is, again, a very singular circumstance; and, viewing his researches as a systematic experimental inquiry, (which they profess, in truth, to be,) an unphilosophical omission. It is the first duty of the experimenter to be acquainted with the construction of his instruments, and to be assured of their accuracy. What would Baron von Reichenbach or Dr. Gregory say of the physiologist who would attempt to investigate the laws of heat, and demonstrate new laws, without a knowledge of the construction and use of the thermometer? or to fathom space, without a knowledge of the telescope and of the laws of light? Yet, what the thermometer and telescope are to the chemist and astronomer, Mlles. Reichel, Maix, and the other "sensitives," are in the hands of the inquirer into the laws and properties of odyle—*mere instruments of research*. Here, then, is a fundamental omission, fatal, we fear, to the Baron's high character as an inductive and experimental philosopher of the most rigid school. We have endeavoured to gather his views on this point, or, at least, to get at the facts touching the nature of this sensitive faculty; and all we find is, that he lays much stress on the greater number of persons who were his instruments of research being in "perfect health." He gives a list of thirty-five "sensitives" of this class, and remarks—

"All these perfectly healthy persons, thirty-five in number, were entirely ignorant of their very remarkable and interesting powers of perception, and were, without exception, much surprised to discover, under my direction, in themselves such powers, of which they previously had no suspicion. The way in which I obtain indications of the existence of such persons, which I then follow up until I lay hold of them, is now simply this: I enquire among my acquaintances whether they know of any one who frequently suffers from periodical headaches, particularly migraine; or who now and then complains of oppression of stomach; or who frequently, without any known cause, has disturbed or restless sleep, or who speaks often during sleep; rises up in bed, or even gets out of bed, during the night; or who is, in general, disagreeably affected by the moon's light; or who easily becomes faint or sick in churches or theatres; or who is very sensitive to strong odours and to unpleasant sounds, such as shaving and sawing. I then seek out all such persons, if otherwise healthy, make a pass with a finger over the inner surface of their hands, and I hardly ever fail to find them sensitive. If they now come to my dark chamber, and remain there for one or two hours, they soon begin to be astonished at themselves, and at the perception of a multitude of luminous phenomena, of which they had not previously the remotest conception. The number of persons who possess this degree of sensibility is, indeed, almost incredible; and I am certainly within the mark when I say that at least one third of people in general are more or less sensitive." (p. 263.)

The practitioner will recognise in these data the phenomena of disease, and will not, we fear, suppress a smile at the frank simplicity of the statement. We cannot, however, omit this opportunity of remarking, that a declaration like this is very creditable to the Baron's scientific truthfulness, however much it may militate against the estimate he places upon his researches. At the same time, it is our duty further to observe, that his want of medical and physiological knowledge is the cause of this defect; if he had been qualified to judge of the construction and mode of action of his instruments of research, he would never have fallen into the error of speaking and acting as if persons situate like those he describes were "perfectly healthy." The simple and notorious fact is, that if such persons be placed in a *dark room* for two hours, they may be made, by a practised operator, to see, not merely "a multitude of luminous phenomena," but to hear varied sounds, to smell odours, to feel tangibly intangible things, and to see anything that can enter into the imagination of man, quite independently of "odyle," or of magnets, crystals, the moon, or any things in the material universe whatever, from which that imagined force may be supposed to radiate. We say this is a notorious fact; and we suspect that persons much more healthy than the Baron's "perfectly healthy sensitives," would see visions, and "a multitude of luminous phenomena" in darkness, if it had so happened to them in infancy that they had been well plied with ghost-stories, in the usual way, or had unusually vivid imaginations.

We must not omit a word to Dr. Gregory, who acts as sponsor to the

German experimentalist, and who seems to be even less impressed than the latter with the necessity of a philosopher knowing something of the nature and construction of his instruments of research. When Baron von Reichenbach very justly congratulates himself on having the observations of his diseased "sensitives" confirmed by those of the "perfectly healthy," Dr. Gregory demurs, in a foot note, to the apparent concession, and observes:

"*One* case, observed as our author, a practised observer in many departments of science, would observe it, may yield results so clear that nothing material can be added by subsequent cases to the force of the evidence. The facts so admirably brought out in the First Part [by diseased girls] would not the less be facts, if only one sensitive person existed in Europe, although their practical applications, in that case, might be less important. And with regard to observations made on diseased subjects, and depending on their statements, in whole or in part, everything depends on the sagacity of the experimenter, and his experience in scientific researches—qualities which, as Part I shows, Baron Von Reichenbach possesses in a very high and rare degree. A large proportion of the most undoubted facts in medicine depend, as every physician knows, entirely on the statements of the patients." (p. 222.)

Every physician of experience knows that there is nothing more difficult in the whole range of scientific inquiry than the observation of disease for the purposes of science; even the merest tyro knows, *practically*, that something more is requisite than sagacity and experience. Successive ages have only confirmed more and more the wisdom of the Hippocratic aphorism—"Life is short, and the art long; the occasion fleeting, experience fallacious, and judgment difficult." Yet Dr. Gregory would lay the foundations of a new science, transcending in its supposed relations any hitherto discovered, on the "facts" stated by *one* diseased person, and that person a hysterical girl! Or, in other words, he would, from the testimony of *one* such an individual, knowing her to be predisposed to perceive *subjective* phenomena, *infer* that all she thought or said she saw, *was* actually seen by her, and an *objective* reality. We may be able better to illustrate Dr. Gregory's idea of the inductive method by an illustration. The Ptolemaic system of astronomy was founded on the false and hasty notion that the *apparent* motions of the heavenly bodies are their *real* motions. Now, if that system was false and hastily built up, which was founded on a phenomenon perceptible by *every* human being, what shall we say of the system which Dr. Gregory would found on phenomena apparent to no more than *one*, and that one *such* an one?

We can assure Dr. Gregory that we make these remarks with deep regret. We would much rather believe in the new force, and the consequent glorious addition to science, than demonstrate his incapacity to handle this matter philosophically and inductively, or show the worthlessness of his praise of Reichenbach. But we have no alternative, except faithlessness to what we think truth and the true method of arriving at it. Dr. Gregory, then, must forgive our comments.

We will say no more as to the *instruments* of Reichenbach's researches generally, because any individual cases of weakness or folly, or even of deception (if that could be shown to have been practised), would not invalidate the *general* results at which he has arrived, touching, at least, the *magnetic light* and its modifications. We shall therefore proceed to examine the *methods* by which he used his instruments and arrived at his inductions. The *instruments* chiefly employed were five girls, namely—1. Nowotny, aged 25, who had suffered for eight years from increasing headaches, and had then become affected with cataleptic fits, accompanied by spasms, both tonic and clonic. In her had supervened intense acuteness of the senses, so that she could not bear either sun-light or candle-light. "*She saw during the darkness of night her room* as if in twilight, and clearly distinguished the colour of all objects in it, such as clothes." 2. Sturmann, aged 19, "suffering from pulmonary tubercles, and long affected with the lower stages of somnambulism, with fits of tonic spasm and catalepsy." 3. Maix, aged 25, having "paralysis of the inferior extremities, occasionally accompanied by convulsions." 4. Reichel, aged 29. This girl, the daughter of a servant in the Imperial Palace of Luxemberg, was Reichenbach's principal subject, and daily assisted him in his researches, being domiciled with him at his private residence. "When seven years old (it is stated at page 10) she had fallen out of a window two stories high, and had ever since been subject to nervous attacks, *sometimes passing into insanity,* at other times into sleep-walking and speaking in her sleep. Her illness intermitted, often for long periods." At page 89 we find it incidentally stated that, "when a child, her mother had often been obliged to raise her in her arms, that she might convince herself that there was no fire proceeding from nails and hooks in the walls, as she often spoke of such appearances with exclamations of wonder." When, after a long series of experiments, Reichenbach commences a course on the odylic properties of matter in general, and expresses his astonishment that a human being (as shown by experiment on this girl) "distinctly perceived a metallic plate, gold paper, or tinfoil without seeing them, at the distance of 100 feet or more," we find it stated, (page 150,) that "Mlle. Reichel only smiled, as she has all her life been accustomed to it." When, again, the same young woman (§ 206, p. 160) saw coloured lights emanating from various substances, as the metals, sulphur, selenium, iodine, &c., they were regarded by her "as nothing else than a lower degree of the, to her, familiar appearances which she had known from her childhood, and of which, in compliance with the warning given by her mother, now dead, she had hitherto said nothing, from the fear of being regarded by other persons as one possessed of supernatural and forbidden powers." This sensitive made a drawing of the appearances of the magnetic light (which accompanies the volume), "but she regretted that she could not attain to an exact image of the phenomena as presented by nature." 5. Atzmannsdorfer, aged 26, who "suffers from headaches and spasms, with sleep-walking, but looks well, and walks like a healthy person through the streets."—Such are the five means or instruments of research by which Reichenbach has carried out by far the more important and more striking of his researches. We have now to consider the *method* he followed.

It will have been observed that almost everything in nature is odylic. Hence, in conducting experiments with magnets, crystals, elementary substances, and determining their behaviour to odyle and to the human organism, it was of the utmost importance to avoid having the results of experiments modified by conflicting agencies. How difficult this would be (if not *impossible*) is apparent from the following proposition:

"Odyle has, like heat, the property of existing in two different states—that in which it is sluggish, and is slowly communicated to, and slowly passes through, bodies; and that in which it is radiated to a distance. In this latter form it is instantly felt by healthy sensitives, *without any sensible lapse of time, at the distance of the length of a whole suite of rooms*, from magnets, crystals, *the human body, and the hands.** All bodies and processes which diffuse odyle over other bodies by slow conduction, radiate it at the same time in all directions, but with varying force; as is seen in friction, electricity, heat, chemical action, and bodies in general. *The rays of odyle penetrate through clothes, bed-clothes, boards, walls,* yet obviously with less facility than magnetism, and with a certain degree of slowness. The conduction and *charging of odyle from one body to another*, by mere proximity, *without contact*, as from the poles of magnets and crystals, *from the hands*, or from amorphous bodies high in the series, such as sulphur, &c., &c., seem all to depend on the radiation of odyle; and this explanation also applies to the so-called magnetising of sensitive persons." (p. 214.)

It is obvious that, in conducting the experiments detailed by Baron Von Reichenbach, this penetrating diffusive property of odyle, and especially in connection more particularly with the human body and hands, must have rendered even the mere presence of Reichenbach, or of any other human being, a highly disturbing cause. Yet we do not find that this circumstance is noted more than once or twice; usually the experiments are made as if the human hand or person was altogether negative and inactive. When describing continual and absolutely necessary manipulations with magnets, crystals, elementary substances, &c., (the hands being brought, of course, into use,) we find no reference to the odylic properties of the person or hand. The first experiment by which the odylic action of the human hand is shown, is thus detailed:

"And now our investigation has brought us to the portal of what is called Animal Magnetism. This *noli me tangere* we shall now be able to seize. When I made a few passes down with a magnet the person of Mlle. Sturmann from head to foot [*sic in orig.*], she became insensible, and was attacked by spasms, generally rigid. When I performed the same passages *with my large rock crystal*, the result was the same. *But I could also produce the very same effect by using, instead of the magnet or crystal, my hands alone. The peculiar force* (we shall call it *crystalline*) *found both in magnets and in crystals, must therefore reside also in my hands.*"† (p. 78.)

We do not stay to criticise this example of erroneous Baconian induction, but will give the reader the means of knowing the phenomena alleged to be produced by crystals. It is requisite to premise, however, how Baron Von Reichenbach was led to this part of his discovery. When experimenting with magnets on the female sensitives, he found that—

"When a glass of water was placed between the poles of a horse-shoe magnet, that is, in the course of the magnetic current, in the manner described in all the works on Animal Magnetism, and thus magnetised, as it is called, not only could every sensitive patient instantly distinguish it from ordinary water, but the glass of water, when placed in the hands of the cataleptic patients, immediately after being magnetised, attracted the hand like a magnet, and even solicited it to follow, as described in my treatise on the peculiar force of crystals. *Something, therefore, must have passed from the magnet into the water, and remained in it; something that is not proper magnetism, which we have no chemical means of arresting or detaining, and the presence of which we cannot by means of any of our ordinary senses recognise.*" (p. 75.)

* The italics in this extract are our own.

† The italics are in the original.

We will not stay here, again, to criticise the induction, except that it might well have been written, "*some change, therefore, took place in the nervous system of the patient,*" &c., &c., but add another extract:

"The distinguished botanist, Professor Endlicher, paid a visit to Mlle. Nowotny, and in his presence her physician made the following curious experiment:—Professor Endlicher requested him to allow himself to be magnetised by stroking with the magnet, and then to act on the patient. To his surprise he found, what had never previously been the case, that now he was able with his hand to attract that of the patient, to attach it to his own, and to cause it to follow in every direction, exactly as the glass of water had done. This power he retained for nearly a quarter of an hour, after which it had gradually disappeared. *The same unknown something* which had remained in the iron bar after contact with the magnet, and which had also passed into the glass of water, *must therefore have entered into the whole person of the physician.* The same cause had produced in his fingers the same power of causing certain sensations." (p. 75.)

Here we must again call the reader's attention to the inductive method adopted by the Baron Von Reichenbach, and to (as we think) the altogether incorrect inference deduced from the phenomena; but our principal object is to request Baron Von Reichenbach's attention to two "facts,"—first, that the whole person of the physician was (according to his view) *already* embued with odyle, and diffusing it to a considerable distance, his fingers in especial could *already* produce the effects of the magnet itself; and, secondly, that the manipulator's hands were inherently, and without any relation to the magnet, themselves strongly magnetic. Consequently this "*unknown something*" would pass from the operator's hands to anything he stroked or touched, just as readily as if it had been magnetised: hence, *whatever the operator touched would become magnetic.* This was, in fact, demonstrated by direct experiment:

"I now wished to try whether bodies could be charged with the force from the hand. I began with Mlle. Sturmann. I laid the German silver rod near her, and allowed it to lie for a quarter of an hour. I then begged her to take it into her hand, and thus to become accustomed to the sensation it might cause. After doing so, she laid it down; and I then took it in my hand for some seconds, and laid it down. When she now took hold of it, she felt it warm, and *so strongly charged, that the well-known sensation, caused under similar circumstances by crystals, rose through the hand as far as the elbow.* This was, of course, repeated, with many variations, for the sake of control. Her physician, Dr. Lippich, made a similar experiment. At my request, in another room, he took into his hand for a short time one of two precisely similar porcelain saucers, not touching the other. They were now presented to the patient, who, with the greatest facility and accuracy, distinguished that which had been held in the hand from the other. After about ten minutes the effect was dissipated, and both saucers felt alike. The experiment with the rod was soon after repeated with Mlle. Maix, in the same way as above. It yielded the same results; the rod was charged by my fingers, and the charge, which Mlle. Sturmann had felt for five minutes, was perceived by the more sensitive Mlle. Maix to last, gradually diminishing, for twenty minutes. In both patients the sensation was the same—one of warmth, rising into the arm, and coinciding exactly with that caused, under similar circumstances, by the rock crystal. I observed the same phenomena, some months later, in Mlles. Reichel and Atzmannsdorfer. The most surprising result is that obtained with a glass of water. If it be taken in one hand, and grasped below by the fingers, and if this be continued for about ten minutes, it then possesses, for sensitive patients, the smell, the taste, and all the well-marked and curious properties of what is called magnetised water. Those who have never examined the matter experimentally may exclaim irrationally against this. I was formerly myself one of this number; but all those who have tested the fact by experiment, and witnessed the effects, as I have done, can only speak of it with astonishment. The water, thus charged, which is exactly similar to that treated by magnets or crystals, has, therefore, received from the fingers an abundant charge of the peculiar force residing in them, and retains it for a considerable time. *I could, after a time, produce similar effects in all possible substances, by holding them for some time in my hand.** The patients, who had tried them all before I touched them, now perceived in all of them the same change, as if they had been stroked with the poles of magnets or crystals; and this whether they knew of my having touched the objects or had been kept in ignorance of my doing so. It follows plainly, from all this, *that bodies may be charged with the force residing in the hand exactly as with the crystalline force.*" (pp. 82-3.)

Now, here we have most explicit statements. Baron Von Reichenbach charges a rod of German silver with odyle, after simply holding it in his hand for some seconds—certainly for less than a minute—and so strongly that the well-known sensation caused by crystals is felt, even so far as the elbow, when the patient takes the rod into her hand; and "*all possible substances*" are equally capable of being charged. Let us now examine the report of the observations made on crystals and other substances, and see if this source of odyle and this property of the experimenter's hand were taken into consideration; for it is clearly manifest that whatever substances he took into his hand, even for a few seconds, would become highly charged with it, however destitute of this new force *per se*. Now, Baron Von Reichenbach thus states the method he pursued when trying substances (various) with Mlle. Nowotny:

"I placed the different substances in the hand of the patient, while she was in the insensible cataleptic state, and observed the effects. I then repeated the experiments when she was in her usual conscious state, and free from cataleptic affection."

Again, in describing particular experiments, we find the same free use of the hand, regardless of its intense odylic force: *e. g.*

"The patient felt, when I drew the point of the crystal slowly down from the wrist over the palm of the hand and along the fingers, an agreeable, gentle, cool *aura*, which she said I led along the hand." (p. 36.)

"I now tried making the passes from the head over the face. The results were exactly similar, and the sensations particularly distinct along the temples." (p. 37.)

"In order to enable every one to repeat these experiments, I would state expressly that a large detached natural crystal, with a natural termination, is necessary. . . . . . The crystal should be drawn over the inner surface of the hand, from the wrist over the palm and down to the point of the middle finger, as near as possible, but without contact, and at such a rate of motion that one pass occupies about five seconds. The crystal is held vertically over the hand. Among my family and friends, I have found more than one half to be sensitive. I never told them my object, but asked for the hand, drew the crystal several times over it, and then asked whether they felt anything, and what? The usual answer was, a cool or warm aura." (p. 40.)

Now, as a "pass" occupies at least five seconds, and "several" are to be made, we cannot take the whole process to occupy much less than a minute. But we have already seen that Baron Von Reichenbach charged a German silver rod in the same time by simply holding it in his hand. What reason is there, then, for inferring it was not the odyle of the hand,

* The italics are ours.

rather than of the crystals, which caused the sensations? Or, since (according to a previous extract) passes made by the hand simply, produced *exactly* the same effect as the crystals, why conclude that the crystals, or a force in *them*, are the agent? We quote another example of this kind, to prevent any misapprehension:

"When in the case of persons sufficiently sensitive to perceive distinctly the passes made with a large crystal along the inner surface of the hand, I drew along the left hands of the patients the points of the fingers of my right hand, turned laterally, so that one finger followed the other, and all passed over the same line, which was drawn from the wrist down to beyond the point of the middle finger, there was not one among them who did not perceive the effect, exactly as from the point of a crystal." (p. 78.)

With such a hand—with a hand so singularly endowed—Baron Von Reichenbach manipulates with crystals and various substances according to the method he lays down; and not only finds many active (as might have been expected), but, what is very astonishing, *a great number absolutely inert!* that is to say, held in the hand and passed after the method described, they produced no spasmodic action and no sensations, although the hand itself could transmit odyle to all "possible substances" and excite violent spasms. These inert substances are principally amorphous bodies, of which a list of forty is given. In making this experiment, it appears to us to have been quite forgotten that the odylic force of the hand could be conducted by, communicated to, and concentrated in bodies. *E. g.*, in the case of Mlle. Reichel, the Baron Von Reichenbach states:

"I have often caused her, when she was quite unconscious, to rise from her chair and to follow my hand for a considerable distance. Even when I presented to her, in that state, bodies incapable of polar induction, such as a lump of chalk, I could raise her hand with it, and, if she were standing cataleptic on the floor, lead her forward some paces. Here it was the force from my fingers which acted, being conducted by the chalk, at the point of which it was concentrated (according to the laws developed in § 81); in which case the chalk represented the whole of my fingers, and that so completely, in their force and action, as to attract and cause to follow it the hand of the patient when I moved backwards, as my fingers have done without the chalk." (p. 88.)

We apprehend the *reader* is by this time fully satisfied as to the inconclusive character of these experiments; but to leave no possible ground of complaint on the part of Baron Von Reichenbach and Dr. Gregory (*the authors*), as to the fulness and fairness of our analysis, we quote § 81 referred to above. It will be seen that it establishes the direct conductibility of "odyle" as a *general law*, to which *amorphous bodies are no exceptions; consequently they are conductors.* Yet, after handling the numerous substances in the way already described, Baron Von Reichenbach finds more than forty amorphous bodies *totally inert*—that is to say, they do not conduct, concentrate, or contain "charges of odyle" *in any degree whatever*. The following is § 81:

"I now compared the two forces with reference to their conductibility. I caused Mlle. Sturmann to take hold of one end of a rod of German silver with her right hand, taking care previously to avoid touching it myself. I allowed her some time to become accustomed to the sensation caused by the rod, taken alone. I now placed on the other end the points of the fingers of my right hand, which were rather moist. She instantly perceived a warm sensation where the rod touched her hand, and this passed upwards as far as the elbow. I now added the fingers of my left hand: the sensation became much stronger, and reached to the shoulder. I removed my fingers: the sensation rapidly diminished, without, however, instantly disappearing. I next attached and removed my fingers alternately: the sensations kept pace with the changes, increasing and diminishing regularly. On another occasion I requested Dr. Lippich to do the same: his fingers produced exactly the same effects. I tried the same experiments on Mlle. Maix. I caused her to take hold of one end of the same rod, and, after a short interval, I first applied five, then ten, fingers to the other end. The warm sensation was instantly perceived, and it rose and fell as I applied and removed the fingers. With the whole ten, it was so strong as to pass through *the whole arm and into the head.*" (p. 79.)

The patient's confessor, physician, and nurse tried the powers of their fingers. The fingers of the confessor were as strong as Baron Von Reichenbach's; those of the other two more feeble. Similar experiments were tried on the sister of the patient, and other individuals, with the same results. May we not fairly infer, then, that no dependence is to be placed on the experiments made by Baron Von Reichenbach, as to the tactile sensations said to be excited by odyle, and as to the attractive force exercised by crystals, magnets, the human hand, &c., on the persons experimented on?

Baron Von Reichenbach entertained the idea that the *solar rays* might be a source of odyle, and he therefore determined to investigate the matter. The reader will not be surprised to learn that the experiment showed his idea to be perfectly correct:

"I took advantage of the first clear sky to try some experiments in this direction with Mlle. Maix. *I placed in her hand** the end of a copper wire, and allowed her to become accustomed to it. *I then thrust a long portion* of the opposite end before the window, into the sun's rays. The patient *instantly* perceived the usual effects of the crystalline force, in no degree of energy, but quite distinct." (p. 92.)

Yet, according to the laws of odylic concentration and conduction, she *ought* to have felt it, *independently of the sunshine,* with considerable energy, at least "as far as the elbow!" Being felt so feebly, Baron Von Reichenbach tried to collect a larger quantity by exposing a larger surface to the rays:

"I now attached to the wire a copper plate, having a surface of 64 square inches; and, after allowing the patient to become accustomed to the sensation caused by the end of the wire, placed the copper plate in the rays of the sun. This had hardly been done, when I was greeted with an unexpected cry of delight from the sick bed. As soon as the rays had fallen on the plate, a strong influence of the crystalline force was felt in the hand, in the form of the usual sensation of warmth, which rose rapidly through the arm to the head; but along with this well-known and *not unexpected feeling* there was perceived a peculiar coolness, and this so powerful and predominating, and with such a sensation of restoration and refreshment in all her limbs, that the patient expressed herself as being highly refreshed and rejoiced by it." (p. 93.)

This well illustrates the ordinary method Baron Von Reichenbach pursues in his experiments. He acknowledges that the ordinary sensation *was not,* but *assumes* the new *was* "unexpected." How little acquainted he must be with mental phenomena, and especially with those dependent on *suggestion* (to be noticed shortly), is obvious enough from this remark. The present point is, however, the

* The italics in this quotation are ours.

total disregard of the odylic force of his hand in almost all his experiments. Then we find such expressions as the following: "I drew the wire back into the shade, the coolness disappeared; I placed it in the sunshine, the coolness returned," &c. Again, when experimenting on the effects of heat, he observes, "*I placed* in the patient's hand one end of a stout iron wire, which *I held myself* near the other end, and allowed her to become accustomed to it. *I* then *applied* the flame of a candle to the further end," &c. So, also, in trying the effects of *Friction*, we find the same disregard of the odylic force so powerfully present in his hands:

"I first laid a copper plate on a deal floor, connected it with the hand of Mlle. Maix by a long copper wire, and then *gently rubbed the plate* with a board of wood. She immediately perceived rapidly-increasing warmth. . . . . . . The sensation increased and diminished regularly as *I rubbed* more or less." (p. 108.)

"I connected with the hand of Mlle. Reichel, by a brass wire, a copper plate which lay on the floor of oak, waxed, as is usual on the Continent. *I placed* on the copper a piece of wood, and *rubbed* the former with the latter. She immediately felt the sensation," &c. (Ibid.)

But, by way of showing the care he took to avoid all possible sources of fallacy, and to provide against any disturbing or complicating agencies, Baron Von Reichenbach remarks, (referring to experiments made to show the odylic light, as well as the tactile sensations:)

"In all these experiments the objects were never isolated, but either lay on the floor or *were held in my hands or those of an assistant*, so that the electricity excited by friction had always a ready means of escape. The heat caused in the objects by the friction could not possibly, when the friction was arrested, disappear so rapidly, as the flames sank to the eye of the patient. The electricity of contact which could be excited in these cases, in which, generally, similar substances were rubbed together, must have been so trifling that it might have been neglected. Indeed, in the instance in which zinc and copper were rubbed together, and where contact electricity must have been excited, there was *hardly a trace of light produced.* (!) Galvanism, therefore, can as little have been concerned in these phenomena as friction electricity. For the same reasons, I consider the possible thermo-electricity as being here not powerful enough to account for the phenomena in the amount and degree observed." (p. 110.)

Remarks of this kind cause, at a first glance, the impression, that the observer is very careful and pains-taking; but the brief analysis we have given of Baron Von Reichenbach's experiments and deductions (and it might have been prolonged indefinitely in the same direction), shows that such precautions are of trifling moment, compared with those which *should* have been taken against possible fallacies, derived from the behaviour of the supposed *new* force, and do not meet any of the really important objections against Baron Von Reichenbach's method.

Experiments of exactly similar weight, and deductions equally inconclusive from them, enable Baron Von Reichenbach to assert that artificial light, chemical action, electricity, all material bodies, whether gaseous, liquid, or solid, and even the vast masses of matter whirling through space, whether as grand systems, suns, or planets, all possess, diffuse, and communicate "odyle." The facility with which the experimenter arrives at the astrological conclusion will astonish the astronomer. The windows of his house, the Baron asserts, command an uninterrupted view towards east and south to the distance of 190 to 140 miles (?), and when he placed Mlle. Reichel before it "she perceived an indubitable effect, such as I had suspected." The same lady, taken to the neighbouring hills at eight o'clock on a calm evening in the middle of October, "perceived coolness from some quarters of the heavens and warmth from others." A frequent repetition of the experiment at different hours of the night enables the experimenter to state, that the variations in the results were obviously the consequence of the position of the sun and of the influence of terrestrial magnetism.

"But these half terrestrial, half solar phenomena, must not be confounded with the stellar, with which, in our sensations, they are complicated. When Mlle. Reichel was out in a clear night, she always pointed out the milky way as decidedly cool, as also the Pleiades, the Great Bear, and others; and, in general, the starry expanse was felt cool, and only individual stars caused a sensation of warmth. These were invariably stars of the first magnitude, and, when I examined them with the dyalite, I found them to be Saturn with his rings; Jupiter with his four satellites; Venus; in short, always a planet. It appeared, therefore, that stars shining with borrowed light appeared to the patient warm, and all others, shining with their own light, appeared cool." (p. 161.)

It never, apparently, occurred to Baron Von Reichenbach, that the starry expanse might well feel cool to any one on an elevated tract of ground, at midnight or at four o'clock in the morning, in the middle or end of the month of October.

Baron Von Reichenbach's experiments on the odylic states of the human body will, perhaps, be more interesting to the medical reader, than those professedly of a more physical character. We subjoin a few illustrations:

"When I raised my hands towards Mlle. Reichel, she felt, even at a distance, my left hand diffusing warmth, my right coolness, on her, like a distant magnet. Mlle. Atzmanndorfer felt the same thing more strongly. When I approached Mlle. Reichel, so that my right side was next to her, she felt me, as soon as I entered the room, cool; but if my left side was next her, she felt warm. Not only the hands, but the whole sides of human beings are respectively positive and negative. I made her try me from head to foot, by holding her hand near me. She found, next to the hands, the head most powerful in its action on her, positive on the left, negative on the right side. The toes were also powerful. In regard to front and back, the forehead was altogether cooler, the hindhead especially warm till towards the neck. In the arms and hands, she, as well as Mlles. Maix and Nowotny, observed the following arrangement:—the points of the fingers were the strongest," &c. (p, 192.)

Again:

"One point of very remarkable power is the mouth, with the tongue. It is very cool, and therefore negative. The sensitive feel everything most distinctly with the lips, especially the odylic influence of the bodies thus examined; and conversely, the mouth of healthy persons is a point by means of which all objects may be charged with odyle still more strongly than by the hands. When I took in my mouth one end of a glass tube, a wire, a silver spoon, a bar of wood, &c., and caused the patients to feel the other end, they all found them very strongly charged with odyle. When I held a glass of water to my mouth as if to drink, and gave it soon after to a sensitive patient, she took it for magnetised water. (p. 192.)

The intelligent reader will remember that we have already shown that a glass of water or a porcelain vessel may be charged by the hand alone. Consequently, he will remark, that in the last-mentioned experiment, the property acquired by the water while held by the operator to his mouth, as if to drink, may have been derived from the hand. Baron Von Reichenbach does not appear to have entertained the slightest doubt on this point; he therefore adds—by way of comment—an explanation of the true nature and significance of a "kiss." We subjoin this paragraph, with Dr. Gregory's explanatory note—adding that the italics in the extract are not ours, but the author's:

"Here we obtain a not altogether uninteresting explanation of *the true nature and significance of a kiss.* The lips are one of the foci of odyle; and the flames, which our poets describe as belonging to them, do in fact play there.* The next of these treatises will clearly prove this.

"It may be asked, how can this be consistent with the fact that the mouth is negative? But, in fact, the two statements harmonise very well; for the kiss gives nothing, but rather seeks, strives after an equilibrium which it does not attain. It is not a negative; but physically, as well as psychically, its state is one of negativity." (p. 193.)

Our regard for the gravity of the critical chair, and our determination to respect the critical canons laid down by Dr. Gregory and his friend the Baron, will not allow us to indulge in any mirthful digressions, however pleasant it may be to enliven dull philosophy with "quips and quirks and wreathed smiles." And if, perchance, the reader be moved to smile at the facts and philosophical deductions contained in the extracts just quoted, we must beg of him to indulge his smile, and then give us again his serious attention. Mirth would be unseasonable. The translator, Dr. Gregory, is grave and serious; the original author is grave and serious; we are *endeavouring,* to the best of our ability, to be grave; consequently, the reader must sympathise with us in our feelings, while we examine the facts and inferences contained therein. First as to the facts. We have already noted the cardinal objection, that the hand which held the magnetised water or other substance supposed to be charged with odyle from the lips, as "one of the foci of odyle," might have communicated the odyle. We may further add, that this method of investigation (like all the others) is quite opposed to Baron Von Reichenbach's own precautionary rule when examining the question of conductibility *without contact.* "I learned," he remarks, "from these observations, that I ought not, in these delicate experiments, to use my own hands, on account of their magnetic force, if I wished to avoid complications in the results. I also obtained hints towards the explanation of many anomalies in my earlier experiments, where the sensitives did not agree in the sensations of heat or cold. *It might often have been my hands which had by their own force altered the natural state of the substances,*"—an objection we have ourselves repeatedly urged. Next, it has been already proved, that odyle—the force which emanates from all bodies—"is conducted and its effects conveyed through bodies of all kinds, even through living persons; and that this occurs by merely approaching the body, without contact, to the further end of the conducting mass;" so that there might

* The translator adds as a foot-note: "Of course the author here speaks of flames visible only to the sensitive. The reader will remember the flames from the points of the fingers, described in § 92. These, and similar phenomena, such as the one alluded to in the text, will be fully discussed in Part III. But, in the mean time, we may remind the reader of the very frequent observations made by persons in the mesmeric state, who are always sensitive, and by others, also, in some cases, of flames from the points of the fingers.—W. G."

have been odylisation by conduction from surrounding bodies *without* material contact, just as there might be *with* contact. The facts, therefore, on which Baron Von Reichenbach and Dr. Gregory found their "explanation *of the true nature and significance of a kiss*" are altogether invalid.

But, granting the *facts* to be faultless, the deductions are faulty. How can it be said that a "kiss gives nothing?" or what proof is there that it "rather strives after an equilibrium which it does not attain?" Perhaps the attempt to obtain "an equilibrium" (whatever that may be, and we candidly confess that we have not mastered that particular point) may have been made by the experimenters when placed out of the plane of the magnetic meridian; or perhaps the moon's rays were directed towards the "foci," or there may have been stellar influences at work,—or *fire*, which "invariably acts on sensitives as a negative body," and two negatives are equal to a positive. We are ready (with most men) to grant the assumption that a kiss "is not a negative;" but we question very much whether the Baron will be allowed to assume, unquestioned, and without a doubt as to his philosophical capabilities, that "physically, as well as psychically, its state is one of negativity."

There is a plate of a most statistical and imposing appearance, which professes to show the odylic force of different parts of the body at each hour of the day. Baron Von Reichenbach suspected he "might obtain highly interesting hints, at least, if not explanations, concerning sleep, digestion, hunger, mental affections, and many other questions bordering on the psychical."

"With this view I caused Mlle. Reichel to make hourly observations on me; and I represented the results graphically in curves, of which the abscissæ represented the times, and the ordinates the relative strengths or intensities of odyle. I further extended the observations to my daughter Hermine, and to Mlle. Reichel herself. She had periods, belonging to her illness, of three weeks, during which she never slept; and I made use of these periods to continue the observations without interruption during the night. They were made as follows:—Every hour the patient took hold of my right hand, examined its relative power, and estimated this, so that I could mark on the table, prepared for the purpose, the point as nearly as possible corresponding to the power observed by her." (p. 194.)

From observations thus made, and with certain precautions, the plate, exhibiting the odylic intensity of "man's right hand" and "man's left hand," is constructed. Some difficulty arose as to the nocturnal hours, but it was not insuperable:

"I succeeded in persuading Mlle. Reichel, by explaining to her the scientific value of such an investigation, and the merit she would have in making it, to come, as she could not sleep, every hour, during several nights, to my bedside, while I slept, to examine the state of my hand, and to note the result. There was no other way of managing it; for, in order to obtain unmixed results, it was necessary that I should sleep exactly as I was accustomed to do, and that in my own bed." (p. 197.)

The Baron had also his head examined for twenty-four successive hours in the same way; the lady found it "cool" on the right side, like the right hand; "warm" on the left; and he infers, *more suo*, that the brain appears "to be less concerned with the ordinary business of nutrition than the hands which have to provide it." He had also his occiput and sinciput examined in like manner; the lady found "the hindhead very warm, and not only in human beings, but also in animals." Another important organ of the Baron's was also subjected to continued horary observations, nocturnal as well as diurnal; and in specially mentioning the nocturnal, we mean to impute nothing unchaste or immoral to either the Baron or his midnight visitor. We are quite sure this would be perfectly unjustifiable; but we must really be permitted to smile at the peculiarity of the relations into which he was led with his young and (we hope and believe) fair friend, the nun, by their mutual enthusiasm for odylic science. A man of fifty-six and a philosopher, wrapt in repose in bed, with a young nun's hand placed every hour on the "pit of his stomach," sagely measuring the intensity of its "odylic" warmth, is a vision which strikes our imagination somewhat ludicrously, and excites a risible, yet philosophical, curiosity for details. Did the Baron Von Reichenbach wear his night-cap? If so, was it a mob or a tassel cap? Has he a beard? Is it long or short? What is the lady's hand like? Were its taper fingers decked with rings? and if so, of what *metal*, and set with what sort of *crystals?* Was the exploration made with the aid of "artificial light?" and if so, was the philosophical lady's candle made of stearine, mutton, or wax? Did she wear slippers? What was the fashion of her dress—nocturnal or diurnal? If nocturnal, was the garment calico or linen—starched in any part, and of a lovely white, or pink, or of an Indian shawl pattern? If diurnal, did she wear stays with "busks?"—if busks, are they of steel, wood, or whalebone? Was the Baron the *lonely* occupant of his chamber in his Schloss Reisenberg? or has he a spouse? Did the moon shine into the room? How were "stellar influences" excluded? These and a multitude of other philosophical inquiries arise in the mind when the vision of our (we must say) faithful and frank narrator and experimenter is presented to the mind, as subjecting the "pit of his stomach" to Mlle. Reichel's scientific nocturnal palpation.

We must here close our illustrations of the researches which were pursued in investigating this important subject, and of the inductive method followed by our philosopher. We have purposely left one or two points unnoticed in the former; as, for example, the condition in which the subjects were placed with reference to the operations of their consciousness; these, however, we shall notice when reverting to the new phenomena of Mesmerism. We may, however, be permitted in this place to inquire into the mental constitution of Baron Von Reichenbach himself, such as is delineated in this volume. As an illustration, we mention the following:—One of his subjects, named F. Weidlich, had been in the English navy, and had acquired, it appears, "no very good name" amongst the Viennese physicians. Baron Von Reichenbach frankly states that he did not know whether the man had acquired it justly or not, for "I do not inquire into such matters." This line of conduct must, we think, be considered as not in accordance with a sound method of experimental research, if our doctrine be correct, that the first duty of the inquirer is to make himself acquainted with the construction and working of his instruments. The Baron, however, not only did not inquire into such matters, but was thoroughly satisfied as to his own infallibility in detecting fraud and falsehood—a state of mind, we need hardly say to the experienced pathologist and physician, altogether incompatible with a satisfactory investigation into a class of phenomena, in the examination of which the most studiously careful observers have been deceived. That we may do the Baron Von Reichenbach justice on this important point, we quote his description of his own powers in this respect:

> "It is absolutely impossible for a sensitive, in consequence of the mass of observations I have made, and the experience I have acquired in the matter, to address to me even a single untrue sentence, the untruth of which shall not be instantly detected; either because it is already controlled beforehand, since, with every new subject, I go through all the investigation from the beginning, even those parts of it on which no doubt any longer rests; or because, if I hear to-day a new statement from one sensitive, I repeat the experiment to-morrow with others. Besides, such a sensitive, even were he *magister* in physics, cannot perceive or even guess the meaning and object of the cross-examination to which I unceasingly subject him, purposely disconnecting the questions." (p. 271.)

This credulous confidence in his own sagacity is quite in accordance with the general tone of Baron Von Reichenbach's histories: errors or failures seem rarely or never to have occurred at all, or to have been easily rectified. We have seen, however, how the most ordinary precautions against error were neglected, in prosecuting what may be termed the *physical* portion of his researches; and we may thence conclude how far, with a decided conviction of his incapacity to be mistaken or deceived, he took the necessary precautions in conducting the *psychological* portion.

A characteristic of Baron von Reichenbach's mind, as displayed in this work (for we have none other means of judging), more fatal to his reputation as a scientific inquirer, is his continual tendency to adopt theories and foregone conclusions in "explanation" of remarkable phenomena; and for the confirmation of which his experiments are usually made, as in the "explanation" of a kiss. We will give some additional illustrations of this peculiarity. Reflecting on the influence of the magnet on highly sensitive people, it occurred to him that, as the *aurora borealis* appeared to be nothing else than an electric phenomenon caused by the magnetism of the earth, such persons might possibly be able to see a luminous appearance in connection with the magnet. If it should be found to be so, it seemed to him "likely to supply the key to the explanation of the northern lights." The "explanation" is accordingly supplied. The light is seen as "a luminous vapour emanating from each pole of the magnet, surrounded by a sort of shining rays. The rays, however, were not tranquil, but shortened and lengthened themselves continually, producing a shooting and sparkling of uncommon beauty, as the *patient assured me*." We underline the last words, to remind the reader of the assertions which the electro-biological patients make of the existence of anything suggested to them. Again, as light proceeded from the poles of magnets, "it was obviously natural to imagine the possibility of the same phenomenon at the poles of crystals; indeed, I was compelled to regard its occurrence, *à priori*, as even probable. I made, therefore, an experiment," &c.—with the *expected* result. "Since we may imagine crystallisation to be the link uniting dead to living matter, I thought I might entertain some *hope* of giving, from experimental research, to animal magnetism a point of union with other departments of natural science, and perhaps of supplying it with that firm scientific foundation, to which it had hitherto striven in vain." As a matter of course, in this also he is successful, and the "hope" becomes a reality. Again, with regard to the influence of terrestrial magnetism, he observes, "if the magnet or a crystal exerted so marked an action on sensitive persons, the power of terrestrial magnetism, which gives direction to the magnet, could not certainly fail to influence the animal nerve. . . . . . With this view I therefore tested," &c. The result is, that he finds the most comfortable horizontal position for "sensitives" to be, with the head directed towards the north, or in the plane of the magnetic meridian, and the most unpleasant towards the west. After making a series of observations to determine this point on the "sensitives," we find, further, that—

"All these patients now recollected, how painful it had always been to them to remain for any length of time in church. All Roman Catholic churches are built from west to east, so that the members of the congregation find themselves, when opposite the altar, in the position from west to east; consequently in that position which is to sensitive persons, of all others, the most intolerable. In fact, they often fainted in that position in church, and had to be carried out." (p. 71.)

Assuming that terrestrial magnetism exerts no influence on *crystals*, the Baron von Reichenbach seeks for the analogue—he experiments on *himself*, and observes, also, the proceedings of silk-worms, and other animals, with respect to the influence of position in reference to the poles; and finds, accordingly, that he and they (not being "sensitives" really) are equally indifferent with crystals towards terrestrial magnetism, and so concludes, "the force residing in the hand coincides, therefore, perfectly with the crystalline force." He anticipates that dualism is a property of odyle. "The polar opposition in the magnet; the dualism in every crystalline form; the symmetrical and sexual opposition in all living organisms, naturally led me to suspect that something analogous might here be found to prevail;" experiments are made to this end, and odyle is accordingly demonstrated to exist in a polar form in crystals, the human body, hand, &c. Researches on the magnetic influence of the solar rays directs his attention to the doctrine of sol-lunar influence: he anticipates certain results as probable; they are sought for; they occur accordingly. He finds that the rays of the moon have an influence which, when conducted along a wire, renders it so magnetic as to attract the whole arm of Mlle. Maix to it, and concludes, "it can hardly be doubted that this is the irresistible attraction which acts so violently on lunatics!" A general law is established, and sol-lunar influence is placed on a scientific foundation. He was led from the calorific rays to anticipate and investigate the influences of radiant and ordinary heat; it was accordingly found to be a sixth source of the new force. Friction and light were in like manner found to be a seventh and eighth. Chemical action is, however, the most fertile in "explanations."

"For about seventy years an apparatus has been occasionally used in medicine, called the *magnetic baquet*. I can hardly venture to describe it, because it will prove an abomination to every one accustomed to scientific investigation into natural truths. A tub of wood is filled with a hotchpotch of the most absurd and senseless kind, which is stirred about with magnetised water. A rod of iron is now introduced, and from it woollen threads are carried to various patients, who are thus to receive a health-giving current of vital magnetism from the *baquet*. Now this extraordinary mixture consists of iron slugs, broken glass, steel filings, roots, iron ores, grains of corn, sulphur, saw-dust, glass plates, wool, old iron, aromatic herbs, mercury, all magnetised and mystically stratified." (p. 113.)

Although Baron von Reichenbach speaks rather contemptuously of this "hotchpotch," he fully believes in the evolution of "a health-giving current" from it, "which must depend upon some unexplored cause."

"If I asked myself, what could take place in the *baquet*, I could only see my way to one agent, namely, *chemical action*. Space had been blindly given for a confused play of affinities, and combinations and decompositions must go on slowly in the *baquet*. In the preceding treatises I had discovered eight different sources of one and the same power. Now here, according to the statement of physicians, this same (!) force flowed from a mixture of substances, many of which act on and decompose each other. Did chemical action in itself, then, set in motion the same imponderable agent? Was chemical action another source of the power residing in crystals, in living man, in the sun, in heat, &c.?" (p. 114.)

Baron von Reichenbach makes a simple experiment, and soon finds that *his* Ideal is indeed the Real. Mlle. Maix takes hold of a wire inserted into a glass containing a soda powder in the act of effervescence, and odyle was found to be evolved during the process so strongly, "that it produced flushing of the face." This was sufficient, and henceforth numerous experiments are equally conclusive; the evolution of odyle during chemical action "explains" beautifully the "magnetic *baquet;*" it shows also the probable source of "odyle" in the human body to be *digestion*,—"since digestion is nothing else than a process of chemical change;" and *respiration*—because the same doctrines apply to it as well as to digestion. "We now see whence it is ever renewed, and whence it, ever burning, blazes out of us." Chemical action explains also the appearance of ghosts in grave-yards, and over the place of sepulture of an interred body; a remarkable ghost-story of this kind the Baron mentions and explains; but to make the matter *experimentally* certain, he took Mlle. Reichel to a cemetery on two very dark nights, and observes, "the result justified my *expectation* in the fullest degree. She saw very soon a light, and perceived on one of the grave-mounds, along its whole extent, a delicate, fiery, as it were, a breathing flame." Other experiments followed, equally conclusive; and the Baron has the satisfaction to observe, "thus I have, I trust, succeeded in tearing down one of the densest veils of darkened ignorance and human error." With similar directness he explains the revels of the Walpurgisnacht on the summits of the Blocksberg. The phenomena witnessed are simply odylic lights given off from the magnetic rocks.

Other difficult matters are "explained" and "made easy" by the Baron's researches. Astrology is not altogether so baseless a science as men have supposed. The evolution of "odyle" from "the whole material universe, even beyond our earth," (fully demonstrated by his researches on the sensations of Mlle. Reichel), "*shows that we stand in a connection of mutual influence, hitherto unsuspected, with the universe;* so that, in fact, the stars are not altogether devoid of action on our sublunary, perhaps even on our practical world, and on the mental processes in some heads." The nature of sleep and of consciousness is also quite cleared up by this discovery. Remarking on the difference of his own odylic state during the hours of day and night, and sleeping and waking respectively, the Baron von Reichenbach observes:

"I am here also privileged to find a confirmation of a law already discovered. The sun, a mighty source of odyle, sends it to us along with light and heat, and saturates everything on which it shines with this force, till the maximum is reached in the evening. *Instantly, as soon as the sun sinks below the horizon, the odylic tension on the human frame diminishes;* and with the commencement of this fall there occur, in persons leading a natural life, weariness, dullness, and sleep. *As soon as the great source of odyle is closed, the fountain of conscious, waking life dries up.* Not alone by heat and light does the sun call living beings into existence; that luminary employs yet another influence, namely, odyle, which, like heat, penetrates all bodies," &c. (pp. 197-8.)

The same general fact renders "the explanation" of the aurora borealis perfectly simple, and reconciles all anomalies. The earth being a magnet, the terrestrial poles give off magnetic lights, but it also receives odyle from the sun and moon:

"It is, therefore, not alone the odyle inherent in the earth, considered as a magnet, which appears in a concentrated form at its poles. . . . . When it 'reaches' the earth's surface, it not only flows towards those parts, where, at the moment, the odylic charge is weaker, but it immediately becomes polarised." (p. 449.)

We could multiply instances of this ambition to explain everything remarkable or mysterious in science or nature, and of this tendency to infallibly confirm preconceived hypotheses by experiment. The above will, however, we hope, satisfy the author and translator, as they are sufficient for our purpose, which is to give the reader such *facts*, as will enable him to form a correct idea of Baron von Reichenbach's qualifications to conduct inquiries of the kind under discussion. We need hardly say that our conclusion, on this point, differs very much from that of his translator. *We find von Reichenbach highly hypothetical; very credulous—credulous as to the extent of his own powers—credulous in receiving subjective for objective phenomena;—negligent to the highest degree of those precautions which ought to have been adopted in making the experiments, so that they should have scientific accuracy; and incompetent from his manifest ignorance of the construction and working of his principal instruments of research—the human brain, and the senses of touch and vision—to make the experiments at all.* We do not mean to deny him the praise of indefatigable industry, or to say that he has not honestly investigated, and to the best of his judgment; we do not say that he has not reported as accurately as he was able, the *statements* made to him by the persons upon whom he experimented; but we deny altogether that those statements are *facts*, as the word is used by philosophical inquirers. It is a *fact*, that they *said* they saw certain phenomena,—it is only an *inference* drawn from various circumstances, that the objects which they said they saw really impinged on the organ of vision, and were not *illusions* of that sense. We have a decided opinion, that they were, in truth, the latter; the grounds of which we shall briefly state. We cannot, however, in justice to Baron von Reichenbach, and to his sponsor, Dr. Gregory, omit to quote the opinions of the latter, agreeing with him in leaving it to the reader to judge whether his or ours are well-founded:

"As to the question, whether the phenomena studied in this work be *facts* or not, I may safely leave this to the decision of the careful reader of the work; and I shall only express my conviction, that if these facts be not facts, then do no facts exist in any department of science. As to the mode of investigation adopted by the Author, it is the inductive method, the same to which we owe all the progress of modern science; and in which the conclusions are deduced from carefully observed phenomena, varied by experiments performed with due attention to accuracy. . . . . *I feel constrained to say, that in the course of a life devoted to science, I have met with no researches in which the true and universally approved rules of investigation have been more perfectly adhered to and followed out than in those before us:* which, were it necessary, might serve as a model to all experimental inquirers." (Editor's Preface, p. 15.)

We have italicised a passage in this quotation, that the reader may more distinctly perceive the great divergence of opinion between Dr. Gregory and ourselves, as to Baron von Reichenbach's experimental inquiries and discoveries. We shall next have to examine the qualifications of Dr. Gregory himself, for the new line of investigation he (a chemist) has taken up. It is obvious, that much responsibility devolves upon Dr. Gregory for presenting this work of Von Reichenbach's to the British public with the authority of his name; and it is an interesting question to determine, how he has been led to form the opinions he has enunciated so decidedly, respecting the value of Von Reichenbach's researches.

II. Dr. Gregory's 'Letters on Animal Magnetism,' are very similar to Mr. Mayo's work, both in plan and object; for the little volume of the latter consists also of a series of letters addressed to "Dear Archy," the principal object of which is to establish the phenomena of Animal Magnetism on a scientific basis, as phenomena of the "Od" force. There is this difference, however, in the position of the two authors, that, at the time the former published his work, certain gentlemen from the United States had already demonstrated certain phenomena, which they termed "electro-biological," whereas Mr. Mayo seems to have been altogether unacquainted with them. This difference is important, for it cannot be doubted that "electro-biology" is not only amply sufficient to show that all the phenomena described by Baron von Reichenbach as objective phenomena are really subjective, but that *all* the phenomena of animal magnetism belong to the same category.

It cannot have escaped the reader's notice, that certain persons from the United States have been "exhibiting" in public as experimenters on the cerebrum and nervous system of such individuals, as were weak enough to permit themselves to be made the subjects of experiment. One or other of these have made their appearance in London, Edinburgh, and the principal towns of the United Kingdom, with very varied success—pecuniary and otherwise. Without mentioning the names of all these, it is sufficient for our purpose to say, that a Dr. Darling and a Mr. Lewis exhibited both in private and public in the modern Athens, to the great delight of its citizens, who dearly love some "new thing." We shall not here particularise the method used by these persons to modify the action of the nervous system; we need only observe that, by acting on the *attention* and the *will* of certain individuals predisposed to disease of the nervous system, they induce a temporary morbid condition of the hemispherical ganglia, not unlike that of insanity, or modified somnambulism. The following quotation will inform the reader as to the results of the process:

"CASE 6.—Mr. W., an officer, met Mr. [Dr.] Darling at my house. Col. Gore Browne had ascertained nine weeks before, that Mr. W. was susceptible, but had made no further experiments, and Dr. Darling had never before spoken to him. He was found, in about two minutes, quite susceptible or impressible. His muscular motions were controlled in every possible way. He was rendered unable to raise his hands, or to let them fall; he was made unable to move one, while he could move the other; unable to sit down, or to rise up; or to take hold of, or to let go an object. One arm was deprived of sensation, or both arms, or the whole frame. He was made to feel a knife burning-hot, and the chair on which he sat equally so. When he started up, he was made to feel the floor so hot that he was compelled to hop about, and wished to pull off his boots, which burnt him. He was made to feel the room intolerably warm, and actually perspired with the heat; after which he was made to feel it so cold, that in a minute or two he buttoned his coat, and walked about rubbing his hands. In about five minutes his hand was really chilled, as I found, like that of a person exposed to frost. He was made to forget his own name, as well as that of Col. Gore Browne, who was present, and to imagine Col. Browne a total stranger. He was compelled, for a time, to give a false answer to every question asked; and then was forced to give true answers to every question, in spite of any effort he might make to do otherwise. He was told he was on duty, at drill; and began to give the word of command, as if in the Barrack-yard. He was compelled to sing and whistle in spite of himself; to laugh immoderately, and then to feel sad, and even to weep, all in spite of his own will. He was told that a stick was a gun, and with it he shot and bagged a grouse, which he was made to see before him. He was told the pianoforte was a horse; and after feeling and closely examining it, he specified its points and defects, and appraised its value. He tasted water precisely as was suggested to him, as lemonade, tea, or wormwood. He was told that Dr. Darling's hand was a mirror, and in it he saw himself with a black face, as Dr. Darling told him to do. He was made to look at his watch, and then convinced that it pointed to a different hour from the true one. He was then made to believe the watch to be a daguerreotype of Col. Browne, and, again, of a lady. Dr. Darling's empty hand became a snuff-box, from which he took a pinch, which made him sneeze violently, and this passed into a most severe cough, as if he had inhaled snuff, which sensation was not removed for about half-an-hour. He was made to go to sleep in one minute, and in his sleep to be deaf to the loudest sounds. He was made to see, in Dr. Darling's empty hand, a bank-note for £10, to read its number, to fold it up, and put it in his pocket; and when afterwards asked, he declared he had done so, and was surprised not to find it there. He was rendered quite unable to jump over a handkerchief laid on the floor; and was compelled, according to Dr. Darling's command, and in spite of every effort, either to come down upon it, or on one or other side of it, or straddling across it. In every one of these experiments, Mr. W. was quite aware that the suggested idea was false, but found it impossible to resist the impression. About fifty persons were present, including Sir David Brewster and other men of science. . . . . . The suggested idea was always instantly dissipated by the words 'all's right;' and Mr. W.'s countenance then expressed confusion and shame at what he had just done or said." (pp. 353—355.)

Dr. Gregory adds, that in these and other experiments of Dr. Darling, "the impressible state was not produced by his magnetic influence, but by the subject gazing at a small coin placed in his left hand." But to prevent mistake, we subjoin Dr. Gregory's explanation:

"Now when we inquire into the cause or the explanation of these facts, the first point to be borne in mind is, that the subjects, in order to be successfully operated on, must not only be susceptible, but must be brought into a certain state. This, in Dr. Darling's process, is done chiefly by themselves, by steadily gazing at the coin, which, according to Dr. Darling, has not, as some imagine, a direct electric or galvanic action, but simply assists in enabling the subject to concentrate his thoughts, and thus to bring himself into a state of abstraction, favorable to the further operation. Mr. Lewis produces the same state, by gazing, for five minutes only, with extreme earnestness and concentration, at the subject, while the latter gazes either at him, or at an object in the same direction.

"The other conditions are the same as those of Dr. Darling. He adds certain gestures and passes, all of which are most deeply imbued with that energetic concentration of will, which I have never seen so strongly," &c. (pp. 194-5.)

The condition of the cerebrum thus induced is "favorable to the further operations," namely, the *suggestion of thoughts, ideas, sensations, and volitions by the operator*. It is, indeed, on this principle that Dr. Gregory explains the influence of the latter:

"It must be at once obvious to every person acquainted with physiology, that the peculiar phenomena now under consideration, and which occur in the conscious ordinary waking state, *depend on the principle of suggestion*." (p. 195.)

"The subject having been brought into the state above mentioned, is found to be under the control of the operator. He is accessible to, and so deeply influenced by, *any suggestion made by the latter*, that he finds it impossible to resist or counteract it." (p. 198.)

Again, when discussing the phenomena of clairvoyance, Dr. Gregory recognises the influence of suggestion. The "thought-reader," or reader of the thoughts of others, odylically—

"May receive erroneous impressions from suggested ideas so powerful in his sympathy with other minds, that an idea, directly suggested or indirectly introduced, as, for example, by *a leading question*, may often produce on his mind an impression as vivid as that caused by the thoughts or memory of his questioner, and all three may become mixed together. For this reason all suggestions and leading questions should be carefully avoided, and the sleeper encouraged to tell his own story. . . . It often happens that in early experiments, the operator is so excited by the novelty and interest of the facts, that he does not calmly examine, and *involuntarily suggests*, by silent sympathy, *his own ideas* to his subject," &c. (pp. 127—9.)

So, also, phreno-mesmeric phenomena are complicated by this suggestive influence of the operator:

"I therefore admit, nay *I maintain*, that there are *many* cases, in which *suggestion*, or the will of the operator, or sympathy with him, *will suffice to explain* the occurrence of the facts." (p. 235.)

Dr. Gregory passes from a consideration of this *suggestionable* condition (if we may be allowed to coin a word for the moment) to the rationale of the methods by which it is produced; and after reviewing them, observes:

"It appears, then, from what has been stated, that electro-biology, electro-psychology, and hypnotism, are essentially the same with animal magnetism, although there is probably some difference in the precise characters of the states produced," &c. (p. 206.)

The reader will doubtless anticipate (as we did when perusing Dr. Gregory's work), that these just views of the phenomena of animal magnetism would be applied by him to a critical examination of Baron von Reichenbach's experiments, since it is upon the Baron's doctrines that the *new* scientific *explanation* of the phenomena is wholly founded. The reader will at least anticipate, that the probable *suggestions* of Von Reichenbach, and the process by which the "sensitive" was rendered able to perceive odylic phenomena under his manipulations, were duly examined and allowed for by Dr. Gregory—and will doubtless be incredulous when we inform him, that no such analysis or examination is made. Yet this is the simple fact. We have already seen how Baron von Reichenbach *expected*, from hypothetical considerations, that "sensitives" would be cognisant of certain phenomena, or not cognisant; and with what remarkable regularity the expected result followed. We have also seen how distinctly Dr. Gregory points out this special source of fallacy in magnetic experiments (unknown to Von Reichenbach, or if known, not mentioned amongst those against which he thought it necessary to guard); and yet *Dr. Gregory never once* (that we can discover) *so much as hints at the possibility of the influence of suggestion in Von Reichenbach's researches.* But what is perhaps still more singular, he never even analyses the process by which these sensitives were enabled to see odylic light, the artificial aurora borealis, and the other spectral illusions (for such we believe they were), described by Von Reichenbach's "sensitives." Yet a mere glance at a description of the method given by Dr. Gregory himself, is sufficient to show the unbiassed reader, that Von Reichenbach's process is in all essential respects identical with that by which Dr. Darling brings persons into "a favorable condition for further operations." The following are some of the "precautions" which (Dr. Gregory states) must be attended to in experimenting odylically:

"In order to have the odylic light seen and described to us, we must strictly attend to the following conditions; and, if we neglect any of them, we must not hope to succeed: 1st. We must have a truly sensitive subject; one, for example, who in the darkness of night has observed light from objects or persons. It is not enough that the subject be nervous, or hysterical, or subject to spasmodic attacks, although these are things usually favorable to sensitiveness. He should feel the magnet strongly; but after all, we must try him with the light, before we can pronounce him sensitive to it. 2d. The darkness must be absolute. In any ordinary room, and during the day, this condition is not attainable, but with ease it may be secured at night. 3d. The subject should remain an hour, or an hour-and-a-half, or even two hours, uninterruptedly, in this total darkness, that the eye may acquire its full sensibility, and the pupil be enlarged to the utmost, before any trial be made. The time varies in different cases. 4th. Not a ray, nor even the faintest glimmer of day-light, must be admitted, after the subject enters the dark chamber. All arrangements must be previously made, and no one must come in or go out during the whole time; for the light admitted by opening the door, &c., is sufficient, even if feeble, to dazzle the subject's vision, so as to render him blind to odylic light for half an hour or longer. 5th. The magnet should be powerful . . . . . . 6th. No one should hold the magnet in his hand, or on his knee, or touch it at all, while the subject looks at it. . . . . 7th. No one should sit or stand near or close to the subject; for if they do, their influence destroys the sensitiveness more or less. . . . . . . 8th. The subject, to see the flame, must be at a certain distance from the magnet . . . . . Now this distance is different in every subject. . . . . . 9th. The subject should be placed, sitting, with the body in the plane of the magnetic meridian, and the back towards the north. . . . . . There is not one of these conditions, the neglect of which may not cause failure in an ordinary sensitive in the conscious state." (p. 329—31.)

This entire process is evidently admirably calculated to develop a *suggestionable* state as to visual illusions. The process for exciting the tactile illusions is not specifically described by Dr. Gregory; but the subjoined observations from Von Reichenbach's work shows that it is absolutely identical with the methods used in electro-biology, auto-magnetism, and mesmerism. Von Reichenbach observes, in reference to the sensations caused by passes made with magnets, crystals, &c.:

"The sensitiveness varies in the same person according to the time and state of health. The effect is often not perceived in the first pass, but becomes distinct on the second, third, or fourth. It occasionally happens that a person perceives the first stroke distinctly; it is not perceived on the second and third; and again comes on with the fourth or fifth. The stroke must not be made too fast; for the full effect requires a certain time for its production. It also sometimes happens that *the sensation precedes the crystal,* and is perceived at the point of the finger while the crystal is yet over the palm. In other cases it only becomes distinct when the crystal is leaving the point of the finger. In some places, persons have been blindfolded, and, *when tried, have given uncertain answers.* This cannot surprise us, after what has been said. Very sensitive persons, even blindfold, will always give consistent answers; those who are less sensible will be uncertain, and possibly inconsistent—the more so, as blindfolding in itself produces an unnatural and disquieting state, *in which the attention is divided and scattered,* and the tranquillity necessary for such delicate observations is generally absent. If, also, many persons be present, if they speak much and ask all manner of questions, move backwards and forwards, it is natural that we should obtain more or less incoherent answers. ...... Nothing whatever must be allowed to come into contact with the patient during the experiment, because the delicacy of feeling is thus disturbed and the attention distracted, whereas, for these experiments, we require the whole power of attention of the patient." (pp. 40-1.)

These conditions are obviously such as are requisite to produce the "electro-biological" condition, "favorable to further operations" by *suggestion.* But, to render this matter the more certain, we add Dr. Gregory's account of the conditions which Dr. Darling requires to be fulfilled, and we then can ask Dr. Gregory, with ample reason for the inquiry, *why* HE *has not pointed out the striking similarity between "electro-biological" and "odylic" methods and phenomena?* Dr. Gregory thus states the conditions requisite to insure "electro-biological" success:

"The process followed by Dr. Darling, which, he informs me, he has never made a secret, is to cause a certain number of persons *willing to try,* to gaze for ten or fifteen minutes steadily at a small coin, or double convex mass of zinc with a small centre of copper, placed in the palm of the left hand. The other conditions are, *perfect stillness, entire concentration of the mind on the object, and a perfectly passive will or state of mind.*" (p. 190.)

We could trace this analogy between Von Reichenbach's method and the other methods adopted to affect the cerebrum *suggestionably,* even to the most minute details; but we forbear, having, we are satisfied, stated enough to show how fatal an omission Dr. Gregory has made in neglecting this comparison. Yet a greater fault remains to be noticed. Mr. Braid had already, in 1846, demonstrated, in his essay entitled 'The Power of the Mind over the Body,' that the phenomena of the "Od" force, as described by Von Reichenbach, *could be produced to any extent by suggestion alone.* Now, Dr. Gregory not only has omitted to notice the analogies referred to, and neglected to analyse and investigate those phenomena by the light which the experiments of Dr. Darling and others have thrown upon them, but has made no reference whatever to these conclusive experiments of Mr. Braid. In the 'Edinburgh Monthly Medical Journal' for June, 1851, Mr. Braid has published a very interesting paper on this subject, recalling attention to his researches, and restating the results of his experiments. We subjoin one or two illustrations; and first as to the "Od" power of magnets:

"With nearly all the patients I have tried, many of whom had never been hypnotised or mesmerised, when drawing the magnet or other objects slowly from the wrist to the points of the fingers, various effects were realised, such as a change of temperature, tingling, creeping, pricking, spasmodic twitching of muscles, catalepsy of the fingers or arms, or both; and reversing the motion was generally followed by a change of symptoms, from the altered current of ideas there suggested. Moreover, if any idea of what might be expected existed in the mind previously, or was suggested orally during the process, it was generally very speedily realised. The above patients being now requested to look aside, or a screen having been interposed, so as to prevent their seeing what was being done, and they were requested to describe their sensations during the repetition of the processes, similar phenomena were stated to be realised, even when there was nothing whatever done, beyond watching them and noting their responses."

Then as regards the odylic light and the aurora borealis:

"A lady upwards of fifty-six years of age, in youth a somnambulist, but now in perfect health, and wide awake, having been taken into a dark closet, and desired to look at the poles of the powerful horse-shoe magnet of nine elements and describe what she saw, declared, after looking a considerable time, that she saw nothing. However, after I told her to look attentively, and she would see fire come out of it, she speedily saw sparks, and presently it seemed to her to burst forth, as she had witnessed an artificial representation of the volcano of Mount Vesuvius, at some public gardens. Without her knowledge, I closed down the lid of the trunk which contained the magnet, but still the same appearances were described as visible. By putting leading questions, and asking her to describe what she saw from *another* part of the closet (where there was nothing but bare walls), she went on describing various shades of most brilliant corruscations and flame, according to the leading questions I had put for the purpose of changing the fundamental ideas. On repeating the experiments, similar results were repeatedly realised by this patient. On taking this lady into the same closet after the magnet had been removed to another part of the house, she still perceived the same visible appearances of light and flame, when there was nothing but the bare walls to produce them; and two weeks after the magnet was removed, when she went into the closet by herself, the *mere association of ideas was sufficient* to cause her to realise a visible representation of the same light and flame."

Mr. Braid details in his Essay just mentioned, a number of experiments like those made on persons in the waking state, to the complete overthrow of all the more important experiments and observations of Von Reichenbach, and the extinction of the whole odylic philosophy based thereon; yet Dr. Gregory takes no notice whatever of them, so that he has neither experimented crucially himself, nor noticed the crucial experiments of others.

One sentence must be devoted to Mr. Mayo's imaginary odometer. A gold ring with a plain stone (but a fragment of anything of any shape will do) is suspended to a piece of silk or cotton thread, or over a table or any object, the end being wound round the forefinger two or three times. The thread must be sufficiently long to allow the ring, or whatever it is, to reach about half an inch from the table. When various substances are placed under it, it describes sundry oscillations, longitudinal and transverse, and divers gyrations under various circumstances, all of which differences are supposed to depend on various manifestations of the "od" force. We need not describe Mr. Mayo's researches with his odometer; the results are exactly analogous to those of Von Reichenbach, and we may add, that we have repeated these experiments, and find that all the phenomena are suggested.

It now only remains for us to draw the general conclusion, that the phenomena described by Von Reichenbach and Dr. Gregory *afford no proof whatever of the existence of the new force*, and that, consequently, all the theories and hypotheses founded upon the assumed demonstration of its existence are altogether baseless. The phenomena themselves have long been recognised, more or less, by those physiologists and pathologists who have specially directed their researches to the nervous system; and although curious and interesting, they have never been thought inexplicable, provided *all the conditions* under which they occur be stated. Mr. Braid, Professor Simpson, Dr. Bennett, Dr. Wood, and others who have carefully investigated the matter, all agree in this. Mr. Braid very justly observes:

"I have never yet seen any phenomena during either the hypnotic or mesmeric sleep, or during the state for manifestly vigilant phenomena, which were not in accordance with generally admitted physiological and psychological principles. The senses and mental powers may be torpified or quickened to an extraordinary degree; but I have never seen anything to warrant a belief, that individuals could thereby become gifted with the power of reading through opaque bodies,......and other transcendental phenomena, called by the mesmerists the *higher* phenomena. The power of a strongly fixed attention, vivid imagination, and self-confidence, however, enables them to perform some extraordinary feats of phonic imitation, and writing and drawing by touch, without the use of their eyes; discovering parties who own certain articles worn by them, through the quickened sense of smell; overhearing conversation in a distant apartment, which they could not do in the waking condition; of recalling to mind things long forgotten when awake; and also of deducing conclusions, manifesting uncommon shrewdness, from premises suggested to them, or arising in their minds spontaneously from recollection of past events, to which they have directed their concentrated attention."

Dr. Gregory, indeed, allows this, as it respects the greater proportion of the phenomena, (as we have already seen,) but, at the same time, he maintains, that in those of clairvoyance there are a number of *residual phenomena* to the explanation and classification of which modern neurology is unequal. Now this conclusion we not only altogether deny, but we assert that it is reached by the same unsatisfactory method by which Dr. Gregory has attained to other conclusions, palpably erroneous. While acknowledging the power of *suggestion*, and the spontaneous occurrence of those morbid states which can be artificially excited by the processes described, he omits to found any rational theory on them (as a physiologist would), or to draw any sound conclusions from the admission, like a man of common sense; but prefers to yield to his love of the marvellous, and enlist his facts in favour of a "universal sympathy," or the "od force." Hence his vain struggles after a sound method of inquiry—his continual divergence from his own rules and data. The following (amongst many similar instances) will illustrate his method:

"I have been informed, on good authority, that round or oval masses of glass are made in England, and sold at a high price, to the ignorant, for the purpose of divination. The persons who sell them perform a certain process, which they say is necessary to their virtue. *It is probably a process of magnetisation*, as water is magnetised. The purchaser is then directed to gaze into the crystal, concentrating her thoughts (for it is generally females who resort to them) on the person she wishes to see. She then sees her lover, or any other person in whom she is interested. Now, I believe, that by the gazing and concentration of the thoughts, *aided by the odylic influence of the glass*, she may be rendered more or less *lucid*, and thus see or dream of the absent person. So that the dealers in these crystals are not mere impostors, but, as I suppose, trade in a natural truth, imperfectly known to themselves." (p. 315.)

The reader will observe in this extract a double current of thought. By the one we are induced to conclude that Dr. Gregory considers the phenomena to be "electro-biological;" the condition ordinarily induced by "gazing" at a bit of zinc and copper, being induced by gazing at the crystal, and the vision of the lover or other person in whom the girl is interested being excited by suggestion; by the other we are led to infer, that the crystal has been "magnetised," that it has acquired an "odylic influence," and that in virtue of that influence the person gazing becomes "lucid." Now *lucid* is a mesmeric term, which, if it means anything definite in Dr. Gregory's work, means this,—that the person can *see* past or coming events as being acted, however distant in time past or time future, and know events acted at the moment, however distant in space; can see the inner mechanism of the human body, independently of the eye or organ of vision, and determine the nature and treatment of its diseases; can hear independently of the organ of hearing; can hold communication with the spiritual world; and, in short, is the "very form and pressure" of the divinity that stirs within us. We say, this is what Dr. Gregory means by the term "lucid;" consequently, in this extract he affirms that a woman may be thus transformed in her nature by looking into a "magnetised" mass of glass! And this is no solitary instance of his method of reasoning; it continually recurs.

As the phenomena of clairvoyance or "lucidity" constitute, in reality, the residual phenomena referred to, we will notice them briefly. They are exceeded in the marvellous by one thing only, and that is, that philosophers and men of education believe in their reality. A *medical* illustration will, perhaps, interest our readers more than any other; but we premise one which sounds as Germanic as any *diablerie* we ever read.

"Clairvoyants can also see, not only dead persons, but those of former ages, and the events in which they are concerned. I have heard of some very striking instances of this, in reference to historical personages, ... in which all that could be verified was found to be correct. One clairvoyant, for example, traced the history of a ring for about 300 years, and was found to be accurate for seventy or eighty years back, or more. The shelled [dead] lady seen by E., as mentioned in the preceding paragraph, was in the costume, and the room had the furniture, of 280 to 300 years ago. She saw various events connected with this shelled lady; and when asked what she had died of, started back in surprise, and, with a very significant gesture, said that she died of having her head cut off." (p. 155.)

Now for the medical illustration, which, in addition to its "lucid" affirmations, contains an interesting statement respecting a novel mode of practice "by medical gentlemen of character and standing:"

"Another power exhibited by the clairvoyant is that of seeing the structure and interior of his own frame. The most eloquent descriptions ever given of the wonders of the human body never produced half the effect on the mind which is caused by the simple but graphic words of the clairvoyant, who is, perhaps, altogether ignorant of anatomy, and yet sees, in all their beauty and marvellous perfection, the muscles, vessels, bones, nerves, glands, brain, lungs, and other viscera, and describes the minutest ramifications of nerves and vessels with an accuracy surpassing that of the most skilful anatomist. ...... I cannot doubt that, when intelligent medical men shall be themselves rendered clairvoyant, some useful information will be derived from the exercise of this power.

"It is easy to understand, that when the sleeper sees his frame in this perfect way, he can detect disorder and disorganisation in it. This, indeed, he very readily does, and his diagnosis is often confirmed by that of the physician who attends him when he is suffering from illness. The clairvoyant, in some cases, possesses the same power in reference to the bodies of those *en rapport* with him. . . . . . He can often exercise it at a distance, with the help of the hair or of the hand-writing. I have seen it done both ways, and repeatedly with very great minuteness and accuracy. . . . . It is much to be regretted, that some persons, not at all qualified for the task, use genuine or possibly spurious clairvoyants, who are made to examine and prescribe, for the object, exclusively, of pecuniary profit to their employers. This ought to be discouraged; but, on the other hand, where a well qualified medical man, of good character, is fortunate enough to meet with a good clairvoyant, *he does rightly in availing himself of the power, to assist his diagnosis. I rejoice to know that this is done*, in more than one instance, *by medical gentlemen of character and standing*." (pp. 155-7.)

We could not think it credible, did not we ourselves know the fact, that the gentleman who has written and published this, is professor of chemistry in one of the first medical schools in the world. The ethical questions involved in this matter we at present pass over. Our present business is to call the reader's attention to the total abandonment of all caution as to the origin and nature of these alleged powers, and the utter neglect of those experiments which have demonstrated (according to the writer himself) the irresistible power of *suggestion*. But, as if to render his conduct in this respect more striking, Dr. Gregory adds the following remark, affording the most complete proof of the true origin of the alleged clairvoyance:

"I have already, in treating of sympathy, alluded to this, and stated my opinion, that the treatment recommended by the clairvoyant is almost always a reflection of that which *he has himself experienced*, or *learned from some medical man*. It has generally, in each prescribing clairvoyant, one unchanging character. It is homœopathic, or hydropathic, or mesmeric, &c. &c. But some clairvoyants do appear to have an instinctive power of selecting unknown remedies, although *I have had no opportunities of seeing this done*." (p. 157.)

As Dr. Gregory does not seem at all aware of the sources of error in the communications of clairvoyants, we will subjoin a quotation and illustration from Dr. Mayo, who thus describes them:

"1. When indulging their lively fancy, they are liable to have a sort of waking dream, during which they describe imaginary scenes with the precision and minuteness of reality, and represent them as actually passing at some place they name. 2. They are liable to recall past impressions, and to deliver bits of old conclusions for intuitions. 3. They are liable to adopt the thoughts of others who may be near them, especially those of their mesmeriser, and to deliver them as trance-revelations. 4. In one instance which came to my knowledge, a young lady, previously unacquainted with mathematics or astronomy, would, when entranced, and sitting with her mother and sister, write fluently off pages of an astronomical treatise, calculations, diagrams, and all. She averred and believed, in her entranced state, (for when awake, it was all a mystery to her,) that this performance was the product of an intuition. *Her manuscript was afterwards found to run word for word with an article in the 'Encyclopædia Britannica.'* That book, however, stood in the library, in a remote part of the house. She certainly had it not with her when she used to scribble its contents; nor did she remember ever having looked into it, awake or asleep. . . . . It is difficult to say how the preceding sources of error are to be effectually guarded against. Possibly, by rigid training from the first, the patient might be brought to distinguish false promptings from genuine intuitions. But even the latter vary in lucidity and certainty. This admission was made to a friend of mine by M. Alexis, the celebrated Parisian clairvoyant." (p. 162.)

These are hard words from a believer, and ought to go far to convince Dr. Gregory, that his proposed new order of entranced prophetesses and sorcerers will never take root amongst us. We regret this conclusion; for delightful, indeed, would it be thus to attain knowledge otherwise unattainable. With such powers as Dr. Gregory attributes to the clairvoyant, man might become little less than omniscient; or, at least, far transcend the ancients in the art of divination. It is clear that *they* had no anatomical clairvoyants, or Plato, Hippocrates, and Aristotle, would have been transcendental anatomists;—no geological "lucids," or the mysteries of the eternal past would have been familiar things in their literature. They would not only have seen the builders of the pyramids of Egypt by means of "lucid" eyes, but by the same means have been eye-witnesses of the gambols of mighty saurians in the ocean of a long-departed æra, of huge behemoths trampling primæval forests, of winged dragons darting hither and hither through the æther. Or the *future* would have been revealed them, and the *epea ptereonta* of the electric telegraph, and all the wonders of modern art which characterise the year 1851 of the Christian æra, might have been equally as visible as the antediluvian pterodactyls. Yet the professors of the art constituted an integral portion of the social polity of ancient nations, and the practical science was interwoven with the events of their every-day life. With the established hierarchy of diviners and augurs, there also coexisted a multitude of private and unauthorised practitioners, who "made much gain by soothsaying," and were no better than quacks and impostors. We gather that Dr. Gregory would make the modern practice of divination a great improvement, in this respect, on the ancient method, for he would have it limited to the "duly qualified." In deference to the medical profession and its just claims, he specially objects to persons "not at all qualified for the task," causing clairvoyants to examine and prescribe professionally for the object, exclusively, of pecuniary gain, and would sanction the use of such persons only "by well qualified men of good character,"—a singularly transparent piece of *ad captandum* prudery, but in unison with the whole tenor of his "Letters." Surely, Dr. Gregory does not really think, that so feeble a concession to supposed prejudices and vested interests, will disarm the opposition and evade the sound common sense of the medical profession? Or that his mild protests will avail against the superstition and knavery, that will inevitably result from the sanction which his name and position in the University of Edinburgh afford to the practice of a pseudo-scientific method of divination? All history, whether sacred or profane, proves, that nothing but folly and knavery can result from these Pagan practices, the revival of which, on a scientific basis, Dr. Gregory so boldly advocates. Hysterical and vision-seeing men and women are as weak and worthless now as ever, and their psychomancy as vain a thing:

> "Non enim sunt ii aut scientiâ aut arte divini,
> Sed superstitiosi vates, impudentesque harioli,
> Aut inertes, aut insani, aut quibus egestas imperat;
> Qui sibi semitam non sapiunt, alteri monstrant viam:
> Quibus divitias pollicentur, ab iis drachmum ipsi petunt."*

III. The slightest consideration of this matter will excite the deep conviction, that a duty has devolved on the medical profession which has

* Ennius apud Cic. de Divinatione, lib. i, § 56.

hitherto been foreign to it. The medical practitioner must now be so fully acquainted with the physiology and pathology of the nervous system, as not only to be able to treat its diseases, but to demonstrate the origin of the popular delusions which those diseases (when artificially excited) will (with the general circulation of the works before us) inevitably cause, even in the minds of otherwise highly educated persons. He must be able to demonstrate scientific and pseudo-scientific fallacies, and to assign a good and sufficient reason for a large number of phenomena, that to the non-professional person appear nothing less than supernatural; he must be equally able to say when the practices by which those phenomena are dangerous to the healthy action of the cerebrum, when beneficial to the cerebrum if functionally or structurally diseased, when applicable to the alleviation of pain. The three essays last on our list have each their respective value for these ends, and will be useful to the practitioner; we here propose to add something to the views their writers have advanced, and to criticise others.

Mr. Braid has given the most useful index to the causation of these phenomena by his special inquiry into the physiology and pathology of *attention*. In looking at the methods by which that morbid condition of the cerebrum is produced, which Mr. Braid designates *hypnotism* (a term we propose to use generally in place of the words mesmerism, animal magnetism, &c.), it is obvious that they all, without exception, are calculated to *excite the attention*. This point Mr. Braid has seized; and has expanded and elucidated views respecting the physiology of attention, and its influence on the cerebrum and system generally, which have been already advanced by several writers, but especially by Dr. Holland. The examination of the matter is, however, only cursory and superficial. Dr. Alexander Wood notes another point in the methods by which hypnotic phenomena are excited, namely, that the *volition* or will of the individual is particularly brought into action, omitting, however, to consider the concurrent influence of attention. Dr. Bennett considers the influence of both the attention and the will, but very generally. Dr. Mayo, we ought to add, now and then gets a glimpse of the true state of things, although, unfortunately, he quickly runs off after some Will-o'-the-wisp of an "Od force" or mesmeric influence. He can see the nature of the relations which attention bears to ordinary sleep; but when we hope he will carry out the analogy so as to illustrate trance-sleep, he stops. The following passage regarding sleep is interesting:

> "The attention alone slumbers; or, through some slight organic change, it is unlinked from the other faculties, and they are put out of gear. This is the basis of sleep. The faculties are all in their places; but the attention is off duty; itself asleep, or indolently keeping watch of time alone." (p. 79.)

We shall endeavour to elucidate this part of the question by a more minute analysis of the hypnotic phenomena; and, in the first place, it will be necessary thereto, to examine more carefully the normal phenomena of attention. In considering this matter, it is necessary to observe in the outset, that the act of attention may, like movement, be either voluntary or involuntary; and hence we find, not only a considerable difference in the origin of the phenomena, but an interesting analogy to follow out. This view of the two-fold nature of attention was pointed out some years ago by

Dr. Laycock. He observes, after mentioning instances of *volitional* attention:

"But an act of the attention may be, and often is, involuntary or instinctive, when it is to be classed with the conservative acts, or rather with the excito-motory phenomena. Point at a nervous female, and she will complain of a sensation as if cold or warm air were blowing on the part pointed at. The sensation probably depends upon changes in the central terminations of the sensitive nerves."*

Of course, what is applicable to a part, is applicable to the entire portion of the system, as the attention may be directed to every portion. Hence Dr. Laycock applies this principle to the explanation of various phenomena termed sympathetic, in cases in which the attention has been involuntarily and even *unconsciously* turned to various organs or systems of organs. He quotes the example of a female, in illustration, who had not menstruated for eight years, at the age of 48, but who then experienced uterine pains, and a sanguineous vaginal discharge, while attending upon her daughter during a tedious labour. On the third day the mammæ were swollen and painful to the touch, and a milky fluid escaped from them, when all the symptoms disappeared. Dr. Laycock mentions other instances, for which we refer to his work,—adding, that "the effects of mesmerism, of the 'evil eye,' of sorceries and incantations, are of this class." Numerous illustrations of this kind of attention continually occur in practice. Under this head may be classed all the morbid phenomena induced by the observation and study of diseased states, as the syncope some fall into at the sight of blood or wounds; the symptoms manifested by medical students and by persons who read works on popular medicine; the occurrence of spurious cases of hydrophobia, cholera, &c. In the relation of a case of hydrophobia, Dr. Maclean observes: "for several days after attending this man I had a most distressing oppression about the præcordia, which caused frequent sighing similar to that with which he was affected."† At a meeting of the Westminster Medical Society, held Dec. 21st, 1833, Mr. R. Quain detailed the case of a gentleman who had constantly witnessed the sufferings of a friend afflicted with stricture of the œsophagus. So great was the impression on his mind, that after some time he experienced great difficulty of deglutition, and died of spasmodic dysphagia.‡ Professor Dubois applies this doctrine of the *distant* influence of attention, *according to a certain and definite chain of ideas,* to an explanation of the phenomena of hypochondriasis. He divides the phenomena into three stages:— In the first stage the mind only is affected; the patient is harassed by imaginary diseases, and he concentrates his attention upon one or other of the viscera; hereby corresponding morbid changes in the innervation of those viscera are excited, when the second stage of the disease is developed; and in the third, these merely functional disorders terminate in *structural* disease.§

We might multiply illustrations *ad infinitum,* of the influence, both morbific and curative, of automatic, instinctive, or involuntary attention on distant organs; at present we need only refer to those cases in which mesmeric "passes," and other modes of exciting it, have been influential

* A Treatise on the Nervous Diseases of Women, p. 111.
† Remarks on Hydrophobia, &c. By R. Hamilton, M.D. Vol. ii, p. 316.
‡ We find this recorded in Renshaw's 'London Medical and Surgical Journal,' vol. iv, p. 701.
§ Histoire Philosophique de l'Hypochondrie et de l'Hysterie—Ouvrage Couronne, &c.

in modifying the healthy or morbid action of various distant structures, over which the will could exercise no *direct* influence. It is evidently in total ignorance of these physiological facts, that Dr. Gregory asserts the acceleration and retardation of the pulse, the fixation and insensibility of the pupil, &c., "to be entirely beyond our control," adding, "the magnetised person is quite unable, however strongly his imagination may be excited, to produce these *facts* in his own person, without some process of what is called magnetisation." The "fact" is, that Dr. Gregory (like many others) confounds automatic, unconscious, and instinctive *attention*, with *imagination*—a fundamental error in mental philosophy. It was from a knowledge of the *curative* influence of unconscious *attention* on distant parts (as when excited formerly by the royal touch, by pretended charms, &c.), that we admitted (on a former occasion) the possibility of a lymph-deposit being absorbed from an opaque cornea by the daily direction of the attention to the part for a prolonged period, by means of mesmeric passes.

In the preceding remarks we have considered together two forms of involuntary attention, namely, the purely *instinctive* or *automatic*, in which the attention of the individual may be directed to a part of the body, of the existence of which he has no anatomical or physiological knowledge, and *the sensational*, in which there is some such knowledge in a greater or less degree, but, nevertheless, the knowledge is indefinite and the attention immediately dependent upon external impression. There are two other more volitional forms to be considered, which run closely into each other, namely, the *emotional* and the *intellectual*.

When an individual has exercised an automatic or merely instinctive act of attention, its influence has been exercised through the grey or organic nerves; but when the will is directly involved in the act, the cerebral system is the sphere of its operation, and the cerebral nerves are its special agents. Strictly speaking, an act of *intellectual* attention is an act of the will, and seems to differ from a volition to move, only in this, that the will is directed to the material organ of perception instead of to that of motion. Metaphysical and physiological considerations combine to establish this analogy in rather a singular manner. The late Dr. Wollaston, forty years ago, noticed that an act of the motorial will was *intermittent* in its nature, and not continuous; he founded this idea upon the fact, that a willed muscular action is accompanied by a vibratory sound like that of carriages passing rapidly over a pavement at a distance, the vibrations being at the rate of about twenty or thirty in a second. Wollaston listened to them by applying the ball of the thumb to the ear, and, at the same time, pressing the end against the head. They may be readily heard by placing the side of the face against a moderately-firm pillow, and then closing the jaws firmly, so that the masseter is forcibly contracted against the pillow. It will be found that the rapidity of the vibratory movement is directly proportionate to the vigour with which the masseter is voluntarily contracted. Now Dugald Stewart remarks, that in a *strictly voluntary* act of attention,—that is to say, an *intellectual* act,—as when we are employed in studying an object not interesting or not *exciting* the attention, "it is not an exclusive and steady act of attention that we give the object, but we are losing sight of it, and recurring to it every instant; and the painful efforts of which we are conscious are not (as we are apt to suppose them to be) efforts of uncommon attention, but unsuccessful attempts to keep the mind steady to its object, and *to exclude the extraneous ideas* which are from time to time soliciting its notice." *

This striking analogy between a forced act of attention and a motor volition, has an interesting bearing on various hynoptic phenomena involving the muscular system, as the cataleptic; and renders it necessary to *discriminate between the percipient and the motor will,* just as we distinguish between sensation and motion, and between the sensory and motor portions of the material organ. Carrying out this analogy we should necessarily infer, that the percipient will acts on the sensorial portion of the encephalon, just as the motor will acts on the motor portion; and it becomes necessary to inquire what is the nature of the changes thus induced?

An *intellectual* act of attention may be directed either towards external objects or towards the thoughts; hence two forms, *internal* and *external.* An external act of attention, whether instinctive and merely *excited,* or whether it be volitional, is effected by means of a special influence on a special apparatus,—namely, the organ of sense. Thus, if we wish to attend to sounds, or *to listen,* we combine various muscles into action for that purpose, so that the undulations shall fall in the most fitting manner upon the auditory nerve. So, also, if we direct our attention to visual objects;—not only are the *motores oculi* combined into suitable action, and the optical apparatus itself properly adjusted to the distance of the object, but the muscles of the head and neck are brought into the combination. This process is, in fact, the usual process of acts of attention, involving any of the senses; so that an act of *motor* will accompanies every act of *external* attention, and the *motor* portion of the nervous system is called into special action. It is of importance to notice, that during this process, the action of the motor will is suspended with reference to the other portions of the motor system, and none but *excited* (or excito-motory) acts—namely, acts independent of consciousness—go on. This is admirably illustrated in the act of listening or gazing attentively, when all the *purely* voluntary motions are suspended. This being without doubt the fact, the question arises, whether the *sensory* portion be not equally called into special action, and if so, what is the nature of the change? The answer to these questions is obvious. It is clear that the same process which renders the mechanical apparatus more fit to receive the impressions on the organs of sense, would, *à fortiori,* render the percipient apparatus more fit to transmit and present them to the mind. As to the nature of the change, we know, from a multitude of facts, that the sensibility to impressions may be increased in the cerebral nerves by diseases of the nerves themselves, or of those portions of the central axis which they transverse; and, in particular, by changes in the distribution of blood through the tissues. From various considerations, with which we need not cumber our pages, as they will occur to any one with an ordinary knowledge of the physiology of the nervous system, it is probable that some change in the capillary circulation, or in the microscopic tissue of the nerves, or of that part of the sensorium with which they are in special relation, is the cause of the exalted sensibility manifested by them in external acts of attention. Be this as it may, there *is* a change, and there *is* exalted sensibility during acts of the kind.

But there is more than this; for in acts of *external* attention, the only

* Philosophy of the Human Mind, chap. vi, § 1.

organ of sense open to receive impressions, and the only trains of ideas capable of modification (so that the impressions and the resulting modifications shall *become objects of consciousness*) are the organs of sense and the train of ideas to which the act of attention is directed. The same general law is in operation in regulating the phenomena of volitions, or *motor* acts of attention. If there be an idea already predominant and *irremoveable*, it is in vain that the individual exercises his volition, according to another idea, until the existing idea be dissipated. Hence it follows, that this condition of the motor portion of the hemispheres should be remembered and well understood in investigating certain hypnotic or mesmeric phenomena; for it is only by such considerations that we can understand how it is that "biologised" persons find themselves unable to rise from their chair, or perform other muscular acts, when the idea of their inability is fixed in their minds by the assertion of the operator,—however violently they may attempt the acts,—until the brain is restored to its free condition by the communication of the contrary idea, by means of the blow on the back and the emphatic cry of the operator—"all right." In like manner, the fixed trains of ideas produced in the biologised subject require certain impressions, and certain impressions only are effectual, for their modification. Phenomena of this kind were mentioned by Professor Simpson of Edinburgh, at a meeting of the Edinburgh Medical and Chirurgical Society, when Dr. Alexander Wood's paper on this subject was discussed. In a published report of that discussion, we find the following:

> "Dr. Simpson mentioned a case witnessed by Dr. Arbuckle, Dr. Wood, and Dr. Taylor, in which a biologised patient slept, as commanded, for thirty-five hours, with two short intervals of permitted awakening. In that and other cases of deep biological sleep, the voice of *no person except the biologiser* seemed to have any effect in producing re-awakening. They were deaf for the time to other sounds. Bells may be rung in their ears, strong noises of all kinds made, tickling, shaking, rubbing the cornea, &c., practised, but they sleep on, apparently listening alone to the voice that sent them asleep, to summon them again to the wakening state. Many doctors will awaken at the sound of their own door-bell, while louder sounds fail in the same effect." *

During an act of pure *internal* attention, the mind occupies itself *exclusively* with its own thoughts, and therefore the changes in the hemispherical ganglia which constitute the *material* basis of its ideas, are the exclusive objects of its consciousness. Such being the fact, it follows (in accordance with the physiological laws of the attention), that the *whole of the senses are shut*, and the *motor volition suspended*. Under these circumstances the insensibility and immobility will be partly proportionate to the intensity of the act of attention, and partly to the susceptibility of the nervous system to pass completely into the appropriate state. Internal acts of attention differ considerably in their nature. When it is simply *excited* or *automatic*, as in *reverie*, the mind takes cognisance only of the ideas flowing spontaneously through it; when the mind concentrates its attention on its own thoughts by an act of will, the state is termed *abstraction*. In both abstraction and reverie the *internal* act of attention may be combined with an *external* act; in this case, those impressions on the senses and the ideas they excite, will alone be received or perceived *at the moment*, which belong to and harmonise with the ideas to which the

* The Monthly Journal of Medical Science, May, 1847, p. 486.

mind is directed. But, although not perceived at the moment, other external impressions, and the sequent ideas, may become objects of consciousness *after* the internal act of attention has ceased.

These considerations render it very clear, that in all acts of attention there is a very complicated condition of the nervous system,—both motor and sensitive,—and that this condition must be well understood, for the right comprehension of the origin and nature of the so-called mesmeric and biological phenomena. One or two additional illustrations of these views may, therefore, be useful; and we would select the condition of the brain, induced by an act of the faculty of imagination, as the best of these.

When a person would represent or recall to his mind, through his imagination, absent persons or scenery, his first effort is an *act of memory*, whereby the previous visual impressions or ideas are re-excited. Immediately after (or concurrently), there is an act of *internal attention* by which the mind has more vividly placed before it those phenomena which the act of memory recalls. During this act of internal attention, the *external* senses are shut, so that the mind is in the same condition (as regards its consciousness) as it is in dreaming, in which the visual and other ideas passing through the mind have all the characteristics of reality. The state of the mind in an act of the imagination, is clearly described by Melville, a writer in the 'Scot's Magazine,' quoted by Dugald Stewart. Referring to the imagination of absent scenery, he observes, "It is evident that while the imagination lasts, be it ever so short, I think myself present in the imagined scene as truly as when I *dream* I am there, or even as when I see and feel I am there. It is true we cannot so well apply the word belief in this case, because the perception is not clear or steady, being ever disturbed and dissipated by the *superior strength* of the *intruding* sensation." * The applicability of this singularly interesting analysis to clairvoyant phenomena is obvious, for we need only suppose that the intrusion of disturbing sensations is prevented for a continuous period (as it is in clairvoyance), or the ordinary condition produced in an act of this kind, changed into the extraordinary condition produced by hypnotism.

Somnambulism is capable of similar illustration. When the mind is directed to a continuous series of ideas, and the attention concentrated thereon, the bodily and mental condition is closely analogous to that occurring in a vigorous act of imagination; but there is this difference,—that, if the engrossing subject of the thoughts require external attention and voluntary motion, these will be exercised. Nevertheless, the thoughts and volitions will be limited to the sensations and actions in relation with the ideas with which the mind is occupied, and all others excluded. This is precisely what occurs in somnambulism, and (with this difference, that the ideas are *suggested*,) in the so-called electro-biological phenomena. Many interesting anecdotes, illustrative of this theory of *abstraction* (as it is termed), may be found in bibliographical notices of eminent philosophers and deep thinkers. It is stated of Beethoven, that, in the ordinary concerns of life, he was in a condition approaching somnambulism, so engrossed was he with his musical ideas. At Vienna, on one occasion, he went into a tavern, called for the *carte*, replaced it on the table, took a pencil from his pocket, and began to write music on the back of it. Soon after, a garçon brought him soup: he replied that he had dined;

* Philosophy of the Human Mind. Second Edition, 1802, note *o*, p. 569.

and before any objection could be made, paid a sum of money and went away.

With regard to *emotional* attention, we need only briefly observe, that in its various forms of external and internal attention, it has been constantly the source of remarkable psychological and physiological phenomena. Sir Walter Scott, observing on the following passage in 'Rokeby,'—

> "Far townward sounds the distant tread,
> And Oswald, starting from his bed
> Hath caught it, though no human ear,
> Unsharpen'd by revenge and fear,
> Could e'er distinguish horse's clank," &c.,—

says: "I have had occasion to remark, in real life, the effect of keen and fervent anxiety in giving acuteness to the organs of sense. My gifted friend, Miss Joanna Baillie, whose dramatic works display such intimate acquaintance with the operations of human passion, has not omitted this remarkable circumstance;"—and here he quotes from one of that authoress's tragedies. This effect of external emotional attention,—namely, to render the senses more acute, but especially to certain kinds of impressions,—is, in fact, so much a matter of popular remark, that illustrations need not be multiplied. The effect of internal emotional attention, on the other hand, in rendering all or certain of the senses *impressionless*, is also well known; one illustration will suffice, and Sir W. Scott again supplies it from 'Marmion,' exemplifying also the cataleptic condition:

> "She sat upon the galley's prow,
> And seem'd to mark the waves below;
> Nay, seem'd, *so fix'd her look and eye,*
> To count them as they glided by:
> She saw them not,—'twas seeming all,—
> Far other scenes her thoughts recall."

The violent emotions, as rage, terror, display the *paralysing* effect of emotional attention the most vividly. Thus, the *fascination* which the reptile exercises over the timid bird ("pure animal magnetism," according to Dr. Gregory,) is, in fact, the result of the intense concentration of the emotional attention, which the *gaze* of the reptile excites. The bird is "biologised;" its mental condition is analogous to insanity, and is the "suggestible" condition of the human being; in this state it falls to the ground, or is irresistibly impelled onwards, to become the prey of its enemy; in the same way that an individual, looking from a dangerous, or what *appears* to be a dangerous, height, is impelled to throw himself headlong.

The passions vary much in their action on the hemispheres in this way. An intense amatory feeling will literally *entrance* the subject of it for a moment; and it is curious to remark how popular phraseology illustrates the etiology of these morbid states.

Intense concentration of the emotional or intellectual attention is the most usual cause of *trance*. Mesmeric trance is only induced, except in highly susceptible subjects, by a long course of treatment. The attention must be repeatedly acted-upon (hundreds of times), and for a long period, before the "higher phenomena" result. The same remarks apply to the various modifications of the sense of personal identity and of self-consciousness which accompany different forms of trance. The phenomena of all these are readily comprehensible on the principles already stated. With regard to trance, it is obvious that not only is sensation abolished in the more intense forms, but even movement also. The cerebro-spinal axis no longer normally receives impressions, except to a limited extent, and therefore has no reaction on the muscular system; and in trance-death, whether as seen in the hysterical woman, in the apparently dead Faquir, or in vampyrism (of which Dr. Mayo gives some very interesting details), the sympathetic system is involved in the same functional disorder as the cerebro-spinal, and is equally thrown "out of gear," so that the animal functions become absolutely dormant.

The doctrines of reflex action and the laws of consciousness combined, illustrate the nature of trance and trance-consciousness. Man cannot manifest the phenomena of consciousness, unless, at least, some external impressions reach, as such, the sensorium.

"I cannot call myself *I* (says Victor Cousin), I cannot perceive myself to be the being whom I call *I*, without confounding it with any other being foreign from itself, unless I distinguish myself from all the rest; and to distinguish myself from anything, is to suppose that the thing, from which I distinguish myself, exists; man, then, does not discover his own existence, without discovering, at the same time, the existence of some other thing, which surrounds and therefore limits him."*

The direct conclusion, then, which we draw from all these facts and considerations, is this,—that the immediate cause of a very large proportion of hypnotic, mesmeric, and "biological" phenomena, is that condition of the organ of mind which is induced by an ordinary act of attention, but produced artificially by the excitation of an act of attention, according to the various methods described; or, in other words, that the brain and nerves of special sense are placed *permanently* in that condition, in which they are *for a moment* in an ordinary act of attention,—we mean *permanently* in a comparative sense, that is, for *several* moments, or, at most, for a few hours.

Having thus traced generally the causation of the hypnotic condition, we have to consider the course of the phenomena. It is curious that all experimenters agree in one proposition, namely, that a predisposition to disordered action of the nervous system must exist, if we would *speedily*, at least, induce the artificial state, and throw the organs of attention "out of gear." Reichenbach's "*perfectly healthy* sensitives" were persons, as we have seen, who, in some way or other, evidenced this predisposition. Having ascertained the existence of certain signs, he establishes his diagnosis by directing the external attention to the tactile sensorium;—"I make a pass with a finger over the inner surface of their hands, and I hardly ever fail to find them sensitive." So, also, Dr. Gregory states, that Major Buckley first ascertains if he can excite certain well-known hyperæsthetic phenomena in the skin of his subjects. Major Buckley "first ascertains whether his subjects are susceptible, by making, with his hands, passes above and below their hands, from the wrist downwards. If certain sensations, such as tingling, numbness, &c., are strongly felt,

* An Introduction to the History of Philosophy. Translated by H. G. Limberg. United States, 1842, p. 149.

he knows that he will be able to produce the magnetic sleep." Again, when Dr. Gregory is describing the best method of magnetising, he recommends that the points of the operator's fingers be drawn "without contact, but very near, over the hands of several persons, downwards from the wrist . . . . . and repeat this several times." He adds: "Some will feel a slight warmth, others a slight coolness, others a pricking, some a tingling, others a numbness." All these phenomena are recognised at once, by the pathologist, as the well-known symptoms of disease of the tactile nerves, or of the cerebro-spinal centres, and known technically under the terms *ardor, algor, formicatio, pruritus,* &c. That they are produced by very slight changes in the capillary circulation, or in the nervous tissue in the trunks of the nerves, is equally well known; that they may be produced, and even reflex acts excited by them, by strongly concentrating the attention, volitionally, on a part, may be proved by a simple experiment. To exemplify this, we will quote Dr. Elliotson, the "facile princeps" of English mesmerists. "We can bring ourselves to sneeze," he observes, "by attending closely to the sensation, and by increasing it. I have often amused myself by looking more or less at the sun, or thinking more or less of tickling, felt at the moment in the nostrils, and so increasing or lessening the inclination to sneeze."*

Each nerve having special endowments, and each portion of the encephalon its appropriate powers, the phenomena will vary according to the nerve upon which attention is concentrated, or the class of ideas which are suggested or attempted to be realised. Thus, when the optic nerve is diseased, various subjective luminous or chromatic phenomena are seen, as flashes, gleams, sparkles, irides, the complementary or primary colours, &c. So, also, in the state artificially excited by the influence of attention. After Reichenbach has ascertained that he can excite tactile hyperæsthetic sensations in his "sensitives," he takes them into his dark chamber, where they remain for one or two hours, "and soon begin to be astonished at the perception of a multitude of luminous phenomena." Luminousness is not an uncommon appearance in spectral illusions. Luther saw angels, when approaching him, "as a ball of light." Joan of Arc saw the Archangel, Michael, surrounded with light, and the saints with a halo of light. So Alexis, of mesmeric notoriety, observes: "Plus j'éprouve de l'attraction aux objets que je veux voir, ou qui me touche—plus il y a de lumière." Very frequently, clairvoyants see their spectral illusions as if in or through a mist of light, or a grayish white mist is alone visible at first. When the imagination is put into action by long-continued gazing, and visual objects are *expected*, spectral illusions are readily developed. Gazing into crystals, water, &c., is an efficient method of exciting this condition of the optical sensorium. Dr. Gregory made experiments on two boys; "and the general result was, that when they gazed long and steadily, they generally saw figures of some sort, sometimes of a father, mother, or brother, but sometimes of persons quite unknown to them, without such persons being asked for. . . . . . But when, as often happened, *their attention wandered,*" Dr. Gregory naively adds, "they saw *nothing.*" These facts, adduced by Dr. Gregory as *proofs* of the existence and production of clairvoyance, are very ordinary phenomena, and per-

* Human Physiology, part ii, p. 497.

fectly explicable by the known laws of cerebral action. We find, in fact, the regular ascending development of the morbid phenomena. First, gazing into the *bright* crystal induces the spectral illusion of a *dark* cloud; then, as the morbid change advances and progresses from the sensorial ganglia to the hemispherical, the black cloud is no longer an object of perception, but spectral light or figures appear; and finally, the process being intensified or frequently repeated, the senses are wholly shut, the spectral illusions are combined, and a dream is enacted before "the mind's eye" of the astonished "subject," with all the vividness and positiveness of reality. If, at the same time, there be auditory illusions, he may hold a conversation with his imaginary companions; and, if his train of excited imaginations and ideas be in connection with a certain individual, he may hear only the voice of that individual, or see only his figure, and be blind and deaf to all others. All these circumstances, which are strictly logical deductions from the well-known laws of thought, of attention, of suggestion, and of association, actually occur.

The group of diseases induced by hypnotism (for diseases they are), is constituted of a well-marked class of cerebral affections, long known to pathologists. The novelty regarding the majority is, that they can be artificially produced; and regarding several, that they can be excited temporarily and evanescently by operating on the brain by means of the attention. This is a real and solid addition to the physiology and pathology of the nervous system. The diseases may be thus catalogued: — sleep-coma, trance, ecstasis, catalepsy, somnambulism, spectral illusions, delirium, insanity in various forms, hypochondriasis, perverted sensations, anæsthesia of the nerves of special sense, and exalted sensibility both of the nerves and the cerebral ganglia, coincidently or separately.

If we investigate the pathology of these artificial diseases, we can have no difficulty in placing their seat in the sensorial and hemispherical ganglia; as to the nature of the changes which those ganglia undergo, we can speak much less confidently. The typical changes are two-fold, namely, those which characterise, and which, in fact, constitute sleep; and those which accompany acts of attention, in its instinctive forms as well as in its emotional and intellectual manifestations, namely, abstraction, reverie, imagination, &c. One great fact proper to all, is, that the action of the will and of consciousness is suspended; and the encephalic ganglia partially or wholly placed in the condition of the "true spinal" or reflex system. Dr. Alexander Wood refers to this principle in his explanation of the phenomena, and quotes a case adduced by Dr. Laycock in proof of his doctrine of cerebral reflex action,* in opposition to the current doctrines of Marshall Hall's school, which limit reflex acts to the "true spinal system," to the total exclusion of the hemispherical ganglia. Dr. Alexander Wood notices (to adopt) the docrines propounded by Dr. Carpenter as to the functions of the ganglionic centres connected with the nerves of sensation and lying along the base of the skull, in man as far forwards as the olfactive ganglia, and as to their being an important portion of the automatic apparatus; and he seems to extend his views further upwards to the hemispherical ganglia, when he points out the fact, that *excited* movements result from *ideas only*. "Let a ridiculous idea pass through the mind,"

* Lancet, 1845, vol. ii, p. 364.

he observes, "instantly we smile, or even sometimes laugh outright, despite the opposition of the will." And this appears to be the view he takes when he places the seat of volition in the *corpora striata* with ample communications with the spinal cord,—considering those and the "adjacent parts as the medium by which the ideas originating in the mind find expression in the muscles. We, perhaps, may be excused quoting Dr. Wood's "general conclusions in regard to the anatomical relations of this inquiry:"

"1st. That the great centre of muscular motion is the spinal cord, and that its functions are unimpaired during the mesmeric trance. 2d. That the great centre of volition in the brain is found to be in the corpora striata, and that their functions are impaired during the mesmeric trance. 3d. That the centre of sensation lies in the optic thalami and olivary columns, and that their functions are more or less impaired in the mesmeric trance. 4th. That the close anatomical connection which subsists between the corpora striata and optic thalami, explains the frequency with which lesions of the one affect the other, and also explains their joint affection in the mesmeric trance. 5th. That the true optic ganglia are very closely connected with the thalami optici, and hence the latter may be affected through them, and in their turn affect the functions of the corpora striata. 6th. That the central convolutions are the great centres of intellectual action. That their functions are not impaired, but that, owing to a temporary suspension of some of those sources of information which they ordinarily depend on, they may suggest erroneous ideas. 7th. That the upper and posterior part of the mesocephale is the seat of emotion, and that it can either act on the muscles by volitional impulse, or directly, as the case may be. 8th. *That the central hemispheres can excite the cord to motion, independently of the other centres;* hence *ideas* in the mind may *produce motion, independently of volition.*" (pp. 27-8.)

We should not do literary justice if we did not draw the reader's attention to a little book published in 1845 by Mr. George Combe,* in which he makes this matter very clear, simply by developing the doctrines of spinal reflex action in accordance with Dr. Laycock's views, and applying them, *mutatis mutandis,* to the sensorial ganglia at the base of the brain, and to the cerebral lobes. He observes:

"The nerves of hearing, seeing, taste, and smell, commonly called the nerves of *special* sensation, are closely analogous in their functions, connections, and laws of action, to those of common sensation. They receive peculiar impressions from external objects, adapted to the constitution of each nerve, and they transmit these impressions inwards to certain cerebral ganglia composed of grey matter [at the base of the brain]. .... The ganglia of special sensation which receive the impressions of the nerves of sense, transmit them to the organs of the mental faculties situated in the cerebellum and in the convolutions of the brain, and the impressions produce different phenomena *corresponding to the particular portion of the brain which the impression excites.* For example, a very loud and discordant sound suddenly striking on the auditory nerve, may, when communicated to the cerebral organ of Cautiousness, excite the emotion of terror.............The excitement of Cautiousness may communicate an impulse to the motor column of the spinal cord, and produce flight..................Or the same impression acting on combativeness, may produce the instinctive attitudes of resistance and self-defence," &c.

As in the system of spinal reflex actions there are the class termed *centric,* so also there are *cerebral* reflex acts of *centric* origin. These are best seen in cases of *impulsive insanity,* in which a person is hurried by

* The Relations between the Structure and Functions of the Brain.—Elements of Phrenology. Sixth Edition. Edinburgh, 1845.

an internal impulse, which he can neither account for nor control, to do various insane acts. Now a condition exactly analogous is excited temporarily in persons affected electro-biologically; the difference is only in the mode in which the impulsive idea is produced. In the one it is the result of functional disease; in the other, it is suggested to a mind in which the brain is in the condition we have already considered. We might subjoin the numerous statements from Dr. Gregory's publication. Mr. B., for example, had his movements controlled by Dr. Darling in many ways; "he reasoned on every experiment as it was made, and told us, that, in spite of perfect consciousness, he found it *impossible, by any efforts,* to resist the suggestions of the operator." Perhaps the excitement of the so-called phrenological organs, or (what in many cases it more probable was) the *suggestion* of certain *classes of pathemic ideas,* supplies the most strikingly illustrative examples of this doctrine. In numerous instances the muscular expressions of various emotions and passions were excited by the apparent process of simply touching the head. Thus Caution being excited, the muscular acts constituted "the most distressing, nay, often appalling, pantomime of fear or misery." If Self-Esteem be the organ, the subject "throws back his head, struts with immense dignity, and declares himself superior to the rest of mankind." These cerebral actions occurring naturally, Mr. George Combe classes amongst the instinctive movements, which again are centric-reflex acts. He observes most appositely to the subject,—"*Instinct* is not a special function; it is the name given to a *certain mode of action,* common to a variety of parts of the encephalon. An *instinctive* action is one which takes place from excitement of a particular organ of the brain, *without volition,* and *without a reasoning* process;"—just the conditions under which these electro-biological phenomena are produced.

These facts and considerations lead us again to the general deduction we arrived at by another course of investigation, namely, that the brain of a hypnotised, or mesmerised, or electro-biologised person, is placed in the condition (as regards its *laws of action*) of the "true spinal system;" or, in other words, *performs its functions like that portion of the central axis which ministers to the automatic and instinctive actions of organisms.* This deduction may be applied analytically to the explanation and elucidation of certain mysterious phenomena, with very interesting results. To take the facts of clairvoyant "*prevision.*" It is well known to those who have turned their attention to the subject, that the relations of instinct and of instinctive acts to the nervous centres, are really quite as inscrutable and difficult to comprehend as the relations of mind, and of mental acts, to the material organ. The "prevision" displayed by clairvoyants and "lucids" is of a very helpless kind, compared with the instinctive prevision of numerous animals, particularly in reference to duration of time. This is seen especially in the articulata, whose provident care for themselves or their offspring, by the measurement of duration, would be considered absolutely miraculous, were it not of the most ordinary occurrence, and its practice a *fundamental law* of their nature. So, again, their intuitive *scientific* knowledge is far beyond any intuition displayed by the most able clairvoyant on record; whether it be applied to the arts of manufacture, construction, &c., or to government and political economy. Dr. Mayo occasionally manifests glimpses of a return to his former vigorous habits of thought;

and nowhere is this more strikingly the case than when he compares the instincts of animals "with some of the feats of lucid cataleptics." Again, the instinct of *imitation* is one which is occasionally remarkably excited, and gives rise to singular phenomena. Its development is very remarkable in certain birds, as the "mocking-bird," the dotterell, and parrots, and their mammalian analogues, the monkeys. It is also very highly developed in certain races of men of a low type. The exploring expedition of the United States found the natives of Terra del Fuego great mimics, both in gesture and sound. "They would repeat any word of our language with great correctness of pronunciation. . . . . . Their mimicry became annoying, and precluded our getting at any of their words or ideas. It not only extended to words and sounds, but actions also, and was at times truly ridiculous."* Let us now turn to the pages of Dr. Gregory for the mesmeric counterpart:

"It [the control of the magnetiser] further appears in the power of causing the sleeper instantaneously to imitate, with the most perfect and admirable mimicry, every gesture of the operator, and every tone of his voice. If the magnetiser speaks German or Italian, languages perhaps quite unknown to the subject, and with the greatest rapidity, the sleeper will speak after him so exactly, that it is often impossible, when his ear is acute in catching the minute shades of sound, to perceive the slightest difference. If the magnetiser laughs, he instantly laughs; if the former make any gesture, however ridiculous, the latter imitates it exactly, and all this with closed eyes, &c." (p. 96.)

Perhaps no instinct is so frequently in operation as that of imitation: it not only induces *manifest* imitative acts, as yawning, squinting, &c., but, like attention, to which it is closely allied, it acts automatically, and altogether independently of the consciousness of the individual, upon the whole system; moulding anew or modifying his thoughts, manners, facial expression, and even his organic life. Ample illustrations of this proposition might be adduced from Natural History and Ethnology, if our shortening limits permitted. Occasionally, the precise condition of the instinct, described by Dr. Gregory as being artificially excited, occurs spontaneously. In Southey's charming literary melange, 'The Doctor,' we have a case quoted of a person living at Strathbogie in 1676, who "imitated unawares all the gestures and motions of those with whom he converses."

"This person named Donald Munro, being a little, old, and very plain man, of a slender body, has been subject to this infirmity, as he tells us, from infancy. He is very loath to have it observed, and therefore casts down his eyes when he walks in the streets, and turns them aside when he is in company. We had made several trials before he perceived our design, and afterwards had much ado to make him stay. We caressed him as much as we could, and had then an opportunity to observe that he imitated not only the scratching of the head, but also the wringing of the hands, wiping of the nose, stretching forth of the arms, &c.; and we needed not strain compliments to persuade him to be covered, for he still put on and off as he saw us do, and all this with so much exactness, and yet with such a natural and unaffected air, that we could not so much as suspect that he did it in design. When we held both his hands, and caused another to make such motions, he pressed to get free; but when we would have known more particularly how he found himself affected, that he could only give us this simple answer, that it vexed his heart and brain."†

* Narrative of the United States Exploring Expedition, vol. i, p. 125.
† The Doctor, vol. iv, chap. 128.

Numerous illustrations of a similar nature might be related; but perhaps it will be more useful to point out the important fact, that all these imitative phenomena are in reality due to the *principle of suggestion*. It is a case in which "facts speak louder than words,"—in which the ideas are suggested through the *eye* as well as the *ear*. That this mode of suggestion is very commonly adopted in the excitation of "animal magnetic" phenomena is certain; although the operator is perhaps as unaware as his subject of the circumstance; and it is equally certain that it constitutes the real source of those phenomena, apparently so inexplicable, that their explanation seems to require some recondite "odylic," magnetic, or other hypothesis.

An important circumstance connected with the manifestation of these phenomena of hypnotism, mesmerism, and the like, and to be remembered carefully in the analysis of them, is the singularly acute receptivity displayed by the brain or portions of it, in regard to suggestive impressions, and the greatly increased adaptability of the motor organs to the ideas suggested. It is a matter of the most common observation, that thousands of ideas—ideas belonging to the true "Ideal,"—pass through the mind, the expression or execution of which, by the motor apparatus, is found to be either impossible or imperfect. In *idea*, a person will do every thing well—dance gracefully, speak eloquently, pronounce a strange language correctly; compose fluently and abound in fine ideas; but so soon as he attempts to realise his ideas or execute the conception—to dance or discourse, or converse in a foreign tongue, or to write out his ideas—he finds his execution falls far short of the conception; and it is only by training and practice that the motive apparatus responds to the intellectual will. But in various forms of lucidity or clairvoyance, the execution is equal to the conception; and hence, whatever ideas or classes of ideas pass through the sensorial portion, the effect is equal on the motorial, and the muscular action constitutes an exact reflex of the mental.

This is more particularly observable in those actions, which may be termed cerebral-instinctive actions. Dr. Gregory referring to experiments of this kind states:

> "I have observed, and it has been recorded by others, that the gesture and voice, the manner and expression, in short, the whole physiognomical and natural language, is extremely perfect. The attitudes of pride, humility, anger, fear, kindness, pugnacity, devotion, or meditation, and all others, are, with peculiarities in each case, depending on the idiosyncrasy of the individual, beautiful studies for the artist. The most accomplished actor or mimic, a Garrick or a Matthews, falls short of the wonderful truth and nature of these attitudes and gestures, as I have seen shown in numerous cases, and most frequently in persons of limited intellectual cultivation, who, in their waking state, showed no peculiar talent for pantomime." (p. 98.)

Such a condition evidently results from the same influence whereby the acuteness of the perceptive organ is exalted in attention. What its true nature may be is doubtful, but it is a well-known phenomenon in various forms of functional cerebral disorder, as in poisoning by opium, haschish, &c.; in hysterical delirium, somnambulism, &c. In ordinary dreaming the motor portion of the system is incapable of action, but the sensorial portion is affected analogously; and, occasionally, the ideas excited even in dreams have found expression, on awaking, as in the musical piece termed "Rousseau's Dream," and Coleridge's "Kubbla Khan."

The ideas arising from *acts of memory* exercised during the "magnetic" state, have the same characteristics as the ideas suggested; hence the apparently inexplicable phenomena, like those in the case mentioned by Dr. Mayo, (ante p. 414,) in which the young lady wrote off whole pages of an astronomical treatise, word for word, while in trance; hence the reproduction of anatomical views and plates in clairvoyance, as "lucid" phenomena; hence, also, another source of "prevision," in the resuscitation of past trains of thought, with their deductions and conclusions. It is a fact well known to those who have closely observed the phenomena of thought, that many complete trains of ideas pass through the brain, just as many impressions impinge on the senses, which either never become objects of consciousness, or become such at a subsequent period. In like manner, the various steps of a conclusive inference or deduction never become objects of consciousness, but only the inference or conclusion itself. In ordinary life this faculty, when the conclusions are correct, is termed soundness of perception, and is one of the most valuable faculties the medical practitioner can possess, as it gives the character of intuitions to his diagnosis. The ganglia of instinctive and automatic life, seem to be endowed with this property to the exclusion of *conscious* mind, if the phrase may be permitted, for the soul-less animal comes to conclusions touching things that concern its welfare, by what seems to be a process of *mechanical* intuition; that is to say, there is no *knowledge* of the phenomena of the material universe; no consciousness of external impressions, no perception of internal changes, no mind. It is into a state somewhat similar (as we have seen) that the hemispherical ganglia are reduced, during the hypnotising or mesmerising process; and being in that state, either new series of ideas are developed and manifest themselves as veritable conceptions, or else, previously existent series of ideas, of which the individual had never had the consciousness, are re-excited, and have all the characters of new ideas. Under such circumstances the individual says truly, that he had no previous knowledge in his healthy condition of the things or circumstances of which he displays an intuitive knowledge during the morbid state; but in fact, so far as the material organ is concerned, the ideas or the materials for the ideas were already there, ready for combination; the mind alone never had any cognisance of them, or only a brief and imperfect cognisance, during their transit.

Illustrations of the true relations of the so-called electro-biological and magnetic phenomena might be multiplied to any extent; for they depend upon known laws and principles of physiology and mental philosophy. The preceding are, we think, amply sufficient for our purpose. Our readers will now, therefore, be in a position to judge for themselves, as to the scientific merits of Baron Von Reichenbach and his English sponsor, Professor Gregory; as to the nature of the phenomena by which the existence of a new and universal force in nature is demonstrated, and from which its laws of action are deduced; as to the real basis of "animal magnetism," and as to the value of the principal uses to which Professor Gregory declares them applicable; namely, divination, the discovery of historic facts, the investigation of the histology of vital organism, and especially of the working of the brain in thought by direct clairvoyant inspection, and the discovery and treatment of diseased states. As to ourselves, we are of opinion that those writers have not considered the "odylic," "mesmeric," or "electro-biological" phenomena, which have been the object of their diligent investigation in the true spirit of the inductive philosophy; that they have wandered widely from the right track; and that by far the largest proportion of their deductions, and all their fundamental principles, are utterly wrong. We have not come to these conclusions without a careful, and we trust a dispassionate, inquiry into the whole matter. We have read the works carefully; we have experimented ourselves; we have largely collated recorded facts; and to no other conclusions could we come.

Nevertheless these books, and the phenomena they record, are equally interesting; the books, as pregnant warnings against an erroneous method of scientific inquiry; the phenomena, as being destined to lay the foundation for a complete revolution in metaphysics and mental philosophy. We have briefly touched upon some of their relations; we trust those relations will become the subject of careful inquiry. It will, doubtless, require some exercise of moral courage to touch a subject so defiled by empiricism; but the merit of the investigator will be only the greater, and the success the sweeter. We therefore suggest to our philosophic readers to investigate—not mesmerism,—but the phenomena which it has appropriated, and so transfer them to the domain of true science; undeterred on the one hand, by the blind zeal which condemns without discrimination, or on the other, by the blind credulity which asserts and defends with fanaticism.

ART. VI.

Die Geburthilfliche Praxis erläutert durch Ergebnisse der II Gebärklinik zu Wien. Von Dr. F. H. ARNETH.

Obstetrical Practice, illustrated by the Results obtained at the Second Midwifery Clinic of Vienna. By Dr. ARNETH, Assistant-Physician to the Clinic.—*Vienna*, 1851. 8vo, pp. 250.

WITH one of the largest and best appointed Lying-in Institutions in Europe, Austria has contributed little or nothing to the advancement of the science or practice of Obstetrics, although the due culture of so vast a field ought to have enabled her leading practitioners to have raised an authoritative voice in the discussion and determination of various questions yet at issue. M. Arneth seems resolved, so far as he is concerned, to remove the stigma of indolence and incapacity; and some idea of the wealth of the *materiel* left unemployed may be obtained from the fact, that his work furnishes a statistical account of 6527 labours, though it only details the experiences of one section of the institution during a period of two years.

Dr. Arneth compares the results he has thus obtained with those published by various English, German, and French writers; but he is no admirer of indiscriminate statistical statements. He points out the preferability of such documents as only embrace short periods of time; since when very long ones are included, many discrepant circumstances may be brought into improper comparison, and this especially when the institution has passed under the management of different practitioners, who may entertain widely different views of the necessity for operative interference, &c. There can be no doubt, we think, of the correctness of this remark; for while the accumulation of large undigested masses of figures has an imposing appearance, it may lead to the most fallacious conclusions, and be of infinitely less utility, than smaller numbers well analysed, so that the justly comparable circumstances are brought into their true position. Where, however, large numbers of cases treated by the same practitioner are faithfully and minutely recorded, as in the collections of Collins, Hardy and M'Clintock, and Dr. Arneth, they constitute the most valuable description of teaching, the diligence of the observer presenting us with the facts in their natural relations, and the number of these neutralising the effects of individual peculiarities.

During the sixty-seven years of its existence, (1784—1849,) there have been delivered within the walls of the Vienna Lying-in Hospital, 204,243 women; 7878 mothers, and 11,092 children (of 189,185 born alive), dying within the same period. The establishment is divided into a *paying* and a *gratuitous* section. Into the former, for very moderate payment, women of all ranks are admitted, every provision being made for the secrecy of such admission, and even, if they desire it, for the concealment of their persons during their residence. Ladies, even of high rank, avail themselves of so convenient a retirement. Into the gratuitous section, admission is made temptingly easy. Any woman who declares herself pregnant with an illegitimate child, is admitted, as a matter of course, a considerable time prior to labour; and if she chooses, she is, after its conclusion, transported with her child to the Foundling department, or she may leave her child there without charge. If, however, she is a married woman, some little difficulty is made as to her admission, until she has procured the necessary certificates of extreme poverty. So popular is this portion of the establishment, that of 7772 women delivered in 1849, 7394 resorted to it. In this the woman may, if she choose, also conceal her name and residence. With such an institution as this, it is not surprising to find Knolz reporting from official sources, that in 1843 for 9140 legitimate births, there were 8427 illegitimate, and that almost a third of the entire number of the births occurred within the walls of the hospital. We think all must agree with Mr. Wilde,* in regarding this ready reception and comfortable maintenance of unmarried women, as possessing a very demoralising tendency. The only compensation that we are aware of, is the smaller number of infanticides which obtains in Vienna, as compared with most cities; and this by some may be deemed sufficient. We think otherwise. A large proportion of illegitimate births indicates the prevalence of a state of society that is fertile in the generation of many crimes, much more beyond legislative interference than is infanticide; and the frequent severance of a tie so instinctive and so forcible as that of maternal love, which the existence of a large population of foundlings implies, is accompanied by the deterioration of other virtues and qualities, whose integrity can alone secure a sound frame-work of society. Moreover, the consignment of large numbers of young infants to Foundling Hospitals, is, owing to the enormous mortality of such establishments, but infanticide in another form. We have frequently heard it stated as an anomaly, that London, amidst its numberless charities, should possess no receptacle for the admission of unmarried women in this their bitter

* Literary, Scientific, and Medical Institutions of Austria. 1843.

hour of trial; but we feel convinced that, taking an enlarged view of the subject, the introduction of this class of establishments is not desirable. Every woman, married or unmarried, and unprovided with means, has a legal claim upon the parish in which she resides.

In return for the advantages they receive, the women admitted into the gratuitous division are rendered serviceable for the instruction of pupils: two Clinics being thus formed,—the list for the instruction of medical students under Professor Klein, and the second, for the instruction of midwives under Professor Bartsch. To the latter Dr. Arneth was attached in the capacity of Assistant for the space of three years. From the period of the establishment of the second Clinic in 1833 to 1849, 39,141 women were delivered of 39,572 childen. Of the mothers, one in 24¾ died. For every 95 female births, there were 100 male ones. The child was born dead or still-born, once in every 25 births, (1 in 24 males, 1 in 26 females,) 1 in 25 of the children born living died within the first nine days, (1 in 21 males, 1 in 29 females.) The number of children's deaths was, however, greater than this actually; as, when they were deprived of their mother's breast, from any cause, they were at once removed to the Foundling department, beyond the sphere of the author's observation. A twin birth occurred in every 87, and a triplet in every 13,040. This department was under Dr. Arneth's practical management, from October 1847 to October 1849, during which time 269 midwives were educated, and 6527 women delivered. Besides this, anything remarkable occurring in the other departments during this period, as well as during another year that he continued at the institution, came under his cognizance: so that, although the period was short, he was made acquainted with all the interesting facts of more than 14,000 cases. His statistical statement, however, only embraces the 6527 labours which occurred during his two years of office in the second Clinic.

Dr. Arneth prefaces his statistical details with an account of the building and its management. The most scrupulous care is paid, he says, to its ventilation and cleanliness; and he declares that if the fact, narrated by Mr. Wilde, of a patient being placed in a bed in which a puerperal woman had just died, be correct, it was exceptional, and in direct violation of the most stringent regulations made for the purpose of securing due change and lavation of all the bedclothes after they have been used, and for which purpose laundry operations are carried on upon an immense scale at the establishment. The apartment in which the women are confined, is only used for that purpose; and three hours after delivery they are obliged to get up and walk (unless exhausted from faintness or other cause, when they are carried,) about 60 paces to the room in which clean, fresh beds have been prepared for them. From this practice, which *à priori* one would consider a very questionable one, Dr. Arneth says, he has never known any ill-consequence result. Soon after reaching her new bed, the mother puts her child to the breast, it being a law of the establishment that she shall suckle her child; and to such law the author attributes the absence of those ravages from aphthæ, which devastate so many other establishments, while among the 6527 women inflammation of the breast only occurred in three instances: on the seventh day the woman is allowed to leave her bed for some days, and on the ninth she is transferred with her child to the Foundling department, where she assists in suckling another child as well as her own, until hired as a wet nurse, which she soon may be, as the Viennese ladies are not in the habit of encumbering themselves with this maternal duty.

We will now furnish a brief abstract of Dr. Arneth's statements, merely premising that they have conveyed to us the general impression of having emanated from a careful, candid, and discriminating observer, perhaps rather too much imbued with the expectant views now dominant at Vienna.

1. *Natural Labour.*—The rules laid down for conducting this differ little from those prevailing here. The *position* on the left side is that usually adopted, although, if the woman prefer lying upon her back, she is permitted to do so. Weak pains may be sometimes increased in power by change of position. The position on the back with the thighs drawn up is desirable when the woman's co-operative exertions are called for; but when these are premature and too violent, the lateral position is best. As the almost invariable rule, the *membranes* are preserved *unruptured* in regular labour, as long as possible; and Dr. Arneth believes that the cases in which rupture, on account of inordinate thickness, is necessary, are of very rare occurrence. Such cases are known to exist, when, with sufficient pains, and circumstances otherwise normal, the membranes, which usually then contain but little water, do not enter within the os uteri, and the dilatation of this makes no progress. On discharging the *liq. amnii,* the progress of the labour is usually then much accelerated. If no hæmorrhage or other important circumstance occur, three hours are allowed to elapse before the hand is introduced to remove the placenta, when friction on the abdomen has proved insufficient to produce its detachment. No binder is applied after delivery.

Of the 6608 children, 6363 were born with the *occiput* forwards, *i. e.* 96 per cent., a proportion almost exactly corresponding to that stated by other observers, as Boivin (95), Lachapelle (93), Collins, Hardy and M'Clintock, (96).

Presentations of the Face.—In this case, the most especial care is taken to leave the membranes as long as possible unruptured, for the protection of the child's face; for although born alive and vigorous, it sometimes dies in a few days, in consequence of the injuries it has sustained preventing its sucking and swallowing. And Dr. Arneth, on several occasions, insists upon the desirableness of stating in Midwifery Returns, not only how many children are born lifeless, but how many die within the first few days after birth. On the other hand, he enters a child as born alive, when most practitioners would denominate it still-born,—viz. if the heart pulsates after birth, though the child still die within a few minutes later. With the same intention of avoiding injury to the face, the forceps are abstained from in all save especially difficult cases. Dr. Arneth is no advocate for attempting rectification of this position, when complete, either by the hand or by instruments; and in proof of the success attendant upon non-interference, he refers to the Dublin and Göttingen returns. (N. Zeit. f. Geb., 1849.) At the Göttingen Lying-in Institution, the number of children who were born alive was found to be in the inverse order of the frequency with which the forceps were resorted to by the different directors.

Among the 6608 births, 40 cases of face-presentation occurred (1 in 165). Of the males, 22 were born living (4 dying within nine days), and

3 dead. Of the females, 13 were born living (3 dying soon after birth), and 2 dead. All these deliveries took place without any artificial aid. This presentation was met with by Boër once in 116, by Clarke in private practice once in 545, by Collins once in 504, by M'Clintock and Hardy once in 478, by Boivin once in 277, by Lachapelle once in 216, by Oldham once in 220, by Busch once in 112, and by Hoffman at Würzburg once in 132. The lesser proportion met with at Dublin arises from the vertex-presentation with the face to the pubes not being considered as a face-presentation, as it seems to have been by some of the other observers. Adding together the entire number of cases occurring in Dr. Arneth's practice and in the practices of the various authors he quotes, we find that in 144,694 births, there were 732 face-presentations, *i. e.*, about 1 in 197½.

Breech-Presentations.—Among Dr. Arneth's 6608 cases, there occurred 113 breech-presentations (1 in 58), in 34 of which there were twins, and in 28 premature births; so that there were only 55 single births of full-timed children, or 1 in 120. Of the 113 children, 44 males were born living (7 dying during the first few days after birth) and 9 dead; and 50 females born living (11 dying within nine days) and 10 dead, *i. e.*, 19 children being born dead, or near 1 for every 5 born living. Boër met with 1 breech-presentation in 54 births, Collins 1 in 52, M'Clintock and Hardy 1 in 50, Simpson 1 in 53, Lachapelle 1 in 44½, and Boivin 1 in 55. Taking the entire number of figures furnished by the author, we find that of 159,593 births, 3000 were breech-presentations, *i. e.*, 1 in 53.

Feet-Presentations.—The number of these in the 6608 births amounted to 59, *i. e.*, 1 in 112, of which, however, 17 were premature and 16 twin-births, giving but 29 single full-timed children, *i. e.*, 1 in 227. Of the 59 children, 29 males were born living (6 dying within nine days), 9 dead, and 19 females living (of whom 4 died) to 2 dead—giving $5\frac{4}{11}$ children born living for 1 born dead. Boër found 1 footling in 119 births, Klein 1 in 193, Bartsch 1 in 152, Collins 1 in 90, M'Clintock and Hardy 1 in 109, Oldham 1 in 194, Lachapelle 1 in 66 from 1803—11, and 1 in 71 from 1812—20, and Boivin 1 in 82. A sum total of 168,596 births furnished 1623 foot-presentations, or 1 in 104.

Cross-Presentations.—In the 6608 births, turning, on account of cross-presentation, was required 32 times, *i. e.*, 1 in 206½. The proportion of such presentations met with by Klein was 1 in 266, by Bartsch 1 in 147, by Clarke 1 in 347, by Collins 1 in 416, by M'Clintock and Hardy 1 in 257, by Lachapelle 1 in 230 for 1803—11, 1 in 188 for 1812—20, and by Boivin 1 in 256. In 214,592 births there were 964 cross-presentations, *i. e*, 1 in 222; these numbers, including 48,557 births reported by Ramsbotham, furnishing 1 cross-birth in 326.

Turning.—Besides turning in 32 cases on account of cross-presentations, this operation had to be resorted to 6 times for placenta prævia, twice for descent of both arms with the head, three times on account of narrow pelvis, and once for descent of a foot with the head—in all 44 cases, or 1 in 150 births. Of these 44 children, 14 were born dead, 6 died within nine days, and 24 were alive and well at the end of that period. Of 43 mothers, three died. In giving directions for the performance of turning, the author emphatically cautions against dilating the os uteri, either by the hand or instruments; and advises, in the case of any rigidity of the part, that the woman should early resort to the bed, and by quietude endeavour to prevent the premature rupture of that most useful dilating agent, the bag of the membranes. If the rigidity continues considerable, warm fomentations and tepid baths must be resorted to. Dr. Arneth prefers the left hand to turn with, it being smaller and more flexible than the right; and is content with bringing one foot down. He usually delivers the patient on her back; but if she desire to be delivered on her side, or if the feet lie towards the symphysis pubis, which very rarely is the case, he has her laid upon her side. The opportunity of *turning by the head* rarely occurs, and should only be attempted when the head lies nearer the pelvic aperture, and is more moveable than the feet; and when the mother or child give no signs of suffering from the delay, the os uteri being well dilated, the membranes unruptured, and the pains powerful. Those who are aware how frequently even the most severe pains suddenly disappear after the introduction of the hand or instruments, will pause before they resort to cephalic turning in any but the most favorable cases. *Turning by external manipulation*, or pressing the head and feet in opposite directions, so that the child's body is forced from its cross-position into the lengthwise axis of the uterus, is easily accomplished at the commencement of labour; yet no sooner do the really strong pains set in, than the child almost invariably resumes its old position, plainly showing how frequently the cross-position is due to the peculiar configuration of the uterus and pelvis. When, again, labour has seriously commenced, a sufficiently long interval between the pains to allow of the manipulation is rarely to be obtained, especially as any compression of the abdomen almost always excites the uterus to action. The influence of such action upon the child is far too powerful and too immediate, to allow a mere superficial pressure to effect so complete a change in its position. From this statement it will be seen, that Dr. Arneth has little or no confidence in external turning. Other German practitioners, however, report far more favorably of its feasibility;* and we think the subject is deserving of more attention than it has met with in France and in this country.

Forceps Operations.—In the 6608 births, the forceps were resorted to 45 times, or once in 146 births. Boivin employed them once in 214, Lachapelle during 1803—11, once in 168, and during 1812—20, once in 288. Collins employed the forceps and lever (which last M. Arneth makes no mention of employing) once in 574 cases, and M'Clintock and Hardy resorted to the forceps once in 279, and to the lever once in 378 births,—viz., to either forceps or lever in 42 out of 6702 births, or once in 159½. Boër used the forceps once in 251 births, Klein once in 48, Bartsch once in 72. The two latter practitioners are the directors of the Vienna Lying-in establishments; and it will be observed, that Dr. Arneth's own forceps operations are proportionally far less than theirs, viz., 1 in 146. Adding together the cases recorded by all the above-named observers, we find, that in 168,179 births, the forceps were employed 1289 times, or once in 130 births.

In 28 of the author's 45 cases, the forceps were applied simply for deficient pains, in 5 for the same cause united with slight pelvic narrowness, 3 times for moderately contracted pelvis, 5 times for eclampsia, once for

* See Brit. and For. Med.-Chir. Rev., Vol. IV, p. 296; N. Zeitsch. für Geburtsk., vol. xxv, p. 187; and Froriep Notizen, vol. x, Nos. 16, 17, 18.

eclampsia and hæmorrhage, once for a prolapse of the funis and hand, once for the prolapse of the arm with the head, and once for the delivery of the head in a breech-presentation. Of the children, 31 were born living (7 dying within a few days), and 14 dead.

In *deficient pains*, when the necessity of having recourse to the forceps is feared, endeavours are made to excite uterine action by the application of vapour to the parts. The patient is seated comfortably in an arm-chair having an aperture in the seat, under which a vessel containing hot water is placed, her feet being supported by a stool placed within the vessel. The whole is surrounded by a cloth, so as to prevent the escape of the vapour. She remains so seated until the pains augment in severity, or until she feels fatigued; and if, on her return to bed, the pains again diminish, the vapour-bath is not unfrequently repeated. Although this bath undoubtedly induces a softened and yielding condition of the parts, Dr. Arneth is disposed to attribute its quickening power upon the pains as much to the assumption of the erect posture, as to the influence of the vapour. When the os uteri is very unyielding, cloths dipped in warm water are kept applied to the external parts, by the hour together, if necessary; and if this has no effect, the patient is placed in a tepid hip-bath, which often proves of the highest utility. If the dilatation of the os has been thus obtained, and yet the pains do not follow, the vapour-bath just alluded to is employed. Dilatation of the tense os uteri by the hand or by instruments is never practised. The *secale cornutum*, as a means of exciting pains, is very rarely employed; and it seems to us that Dr. Arneth by no means appreciates the utility of this valuable drug in the various emergencies under which it is employed here. This may arise from his giving only 5-grain doses every five minutes, until half a drachm has been taken. If he gave the half drachm altogether, and repeated this dose in half-an-hour, if requisite, we believe his opinion as to the power of the remedy would undergo some change. It is by no means our wish, however, to urge the more frequent employment of the ergot among ourselves; as we feel convinced that it has been, and is, from ignorance and impatience, far too frequently resorted to, both in England and France; and that many examples of infantile asphyxia are due to its injudicious administration. While, therefore, we maintain its great power for good or evil, accordingly as its exhibition is prudently resolved upon or not, we are glad to put prominently forward some of the above-named substitutes in use among the more cautious and more leisurely practitioners of Vienna.

Very different is the result of the application of the forceps for mere deficiency of pain, and for even only a moderate degree of *contraction of the pelvis*. Of the 8 mothers in whom the latter existed, only 2 could be dismissed at the end of the ninth day, like the other women; 1 was attacked by cholera, 1 suffered from urinary fistula, 2 suffered from puerperal fever, but recovered, and 2 died. On the other hand, of the 28 women in whom the forceps was only resorted to on account of defective pain, 23 left the establishment at the end of the ninth day, 3 fell ill, but recovered, and 2 died.

Dr. Arneth never resorts to the "long-forceps," the instrument he employs measuring 15 inches. He especially cautions against the movement of the instrument towards the sides of the pelvis during traction, for although this may sometimes hasten the progress of delivery, it does so at the expense of the soft parts of the mother. He cannot sufficiently express his astonishment at finding, that all the writers whom he has consulted, direct a pendulum or rotatory traction to be employed; and insists that this should be made direct, simultaneously with the pain, when possible, and with uninterrupted and increasing force, so long as the operator's strength allows. In this way much more is done than by occasional violent pulls, and the instrument is held more securely in the operator's power. The author has met with no instance of *separation or loosening of the pelvic symphyses*, stated by Scanzoni as a frequent consequence of the employment of the forceps in a narrow pelvis. One interesting case of this kind following a forceps operation, he relates; but in this the forceps were resorted to (under the same circumstances that most practitioners in this country would give ergot) for a deficiency of pain, notwithstanding complete dilatation. They were applied with great ease, and four or five tractions completed delivery. Soon after delivery, the patient complained of pain in the left inguinal region, and afterwards along the whole course of the ischiatic nerve. There was no tenderness of the abdomen, except over the symphysis pubis; but excessive pain was complained of on moving the hip-joint, and deep-seated fluctuation was perceptible external to and above the ischiatic tuberosity. The last week prior to her death, she complained of excessive suffering near the left acromion. Delivered on the 26th of October, she died on the 6th of November. Much yellow purulent exudation was found between the bladder and symphysis, and the periosteum of the horizontal ramus of the pubis was loosened. The fibro-cartilage of the symphysis was in part destroyed, in part infiltrated with the above exudation. The symphysis was so separated that nearly an inch intervened between the ends of the bones, which were also roughened; within the left shoulder and hip-joints, greenish-yellow pus was found. The uterus was lined with a darkish exudation, a few drops of pus being observed where the placenta had been attached. Several small darkish-coloured, fissure-like wounds of the cervix uteri, and one in the vagina, were observed.

Perforation.—In the 6608 births, Dr. Arneth had to resort to perforation but 4 times for very narrow pelvis, *i. e.*, once in 1652, two of the women dying. Boivin perforated once in 1282, Lachapelle (1803—11) once in 1118, and (1812—20) once in 1853, Collins once in 139 (once in 210 for tedious labour), M'Clintock and Hardy once in 106 (once in 129 for tedious labour), Boër once in 587, Klein once in 668, Bartsch once in 1475. The total numbers give for 168,179 births, 336 perforations, or one in 500 births. Of these, the Paris female practitioners have reported 68,412 with 42 perforations (1 in 1628), the practitioners of the Vienna Hospital 76,411 with 111 perforations (1 in 688), and the Dublin practitioners 23,356 with 183 perforations (1 in 127½). Dr. Arneth advocates the preferability of perforation to the *Cæsarean section* far more strongly than is customary with continental practitioners; and he informs us that for the 193,271 births that have occurred at the Vienna Hospital (1789—1849), the section has been only resorted to five times, and always with a fatal result.

Summary of Operations.—M. Arneth furnishes us with tabular views of the operative procedures of the different practitioners above referred to, which are far too long for reproduction here. We think, however, it will prove of interest to our readers, if we prepare from them a synoptical view of the comparative frequency with which these different operations are resorted to, and which we have already stated in detail. Mesdames Boivin and Lachapelle in this represent the Paris practice; Boër, Klein, Bartsch, and Arneth, that of the Vienna Hospital; and Collins, M'Clintock, and Hardy, that of the Dublin Hospital—the cases in which the lever was employed by these last being here included under the head of "Forceps Operations." We regret that we cannot present also a comparative view of the mortality of the mothers and children; but an account of this is only given in some of the tables, and is consequently useless for purposes of comparison:

	Paris.	Vienna.	Dublin.
Total Births	68,412	76,411	23,356
Total Operations	875 (1 in 78)	1526 (1 in 50)	321 (1 in 72½)
Turning	547 (1 in 125)	451 (1 in 169½)	70 (1 in 333)
Forceps	266 (1 in 257)	955 (1 in 80)	68 (1 in 343½)
Perforations	42 (1 in 1628)	111 (1 in 688)	183 (1 in 127½).

Prolapsus of the Funis.—This accident occurred 33 times in the 6608 births (1 in 200). There were 22 children born living (of whom 3 died in nine days), 7 were already dead at the commencement of the labour, and 4 died during its progress. Of the coinciding presentations, 19 were occipital, 2 face, 5 feet, 4 breech, and 3 cross. In 10 of the cases no interference took place, either because the child was dead, or (as in 3 of these) the labour was so rapid as not to endanger it. In 11 cases *reposition* was employed, and 10 children were born alive, the forceps being required in the other case on account of the delay of the labour. Turning was employed in 6 cases, the child being born living in 3, being already dead in 2, and dying during the operation in 1. In 5 cases of foot or breech presentation, the labour was completed by extraction, with the effect of saving the child's life. Comparing the author's proportion of prolapsed-funis cases, 1 in 205, with that of other observers, we find that Collins met with it once in 171 births, Hardy and M'Clintock once in 181, Lachapelle once in 411, Boivin once in 521, Bartsch once in 276, and Klein once in 100.

Dr. Arneth protests against the too hasty adoption of turning in the treatment of these cases, thereby endangering the mother, and scarcely placing the child's life in a much more safe position. If the occurrence is discovered prior to the discharge of the waters and the dilatation of the os uteri, he confines the woman to bed in order to delay the rupture of the membranes as long as possible. If the cord occupies a central position, he has her buttocks kept raised; and if it inclines too much to one side of the pelvis, he inclines her to the opposite one. When the waters are discharged, and the os uteri still undilated, he believes there is little to be done, although attempts should be made to support the funis beyond the reach of pressure, unsuccessful as these then generally are. If the os uteri is dilated, and the breech or feet present, extraction should be at once performed; but turning, he thinks, is only admissible after frequent failures at reposition. In all cases, when the head is the presenting part, continuing moveable at the entrance of the pelvis, and the waters are discharged, the author takes as large a quantity of the cord as possible in the palm of the hand, passes it up beside the head, and lays it on the neck of the child.

To the objection that has been made, that this procedure only increases the pressure that is to be dreaded, Dr. Arneth replies, that he insists upon the introduction of the entire hand, which effectually shields the funis from pressure; and that, moreover, a strongly pulsating funis will bear a very considerable degree of temporary compression, the ill consequences we so often meet with probably resulting from the long continuance of a moderate degree of pressure over a large extent of the cord. This mode of reposition he regards as of very easy accomplishment, having only met with one case in which he could not succeed. This occurred in a forehead presentation of a very large child, the head having entered the pelvis, and the funis being tightly drawn across it. The introduction of the hand is often followed by a great acceleration of the labour, a highly desirable occurrence in these cases. Of the 43 times in which reposition has thus been performed at the Vienna Hospital, the children have been born living in 38 instances, and in 3 of the other cases, at the time the attempt was made, the pulsations could scarcely be felt. In another case, the only one which proved fatal to the mother, the delay of labour necessitated the employment of the forceps.

Dr. Arneth quotes with approbation a caution delivered by the experienced Böer, viz., that life is not extinguished in the child at the very moment the pulsation ceases to be felt; and, moreover, if there is much obstacle to the reposition, the sensation in the fingers may become so diminished, that the pulsation of the cord is no longer perceptible. In fact, reposition of the funis, or extraction of the child, as the case may be, should always be practised as long as we have not indubitable signs of the death of the latter, which is indicated by a prolonged descent of a non-pulsating, cold and flabby funis. Reference is made to 4 cases, in which a live child was born by extraction, or after the reposition of a pulseless funis. Dr. Arneth regards the mere replacement of the funis by the fingers,—the Danaidic labour, as Boër characterised it,—as worse than useless, being positively mischievous by the loss of precious time it entails.

Placenta prævia.—In 6527 labours, the author met with 9 cases of *placenta prævia*, or 1 in 725. Of these cases, 5 of the women were dismissed on the ninth day, 2 became ill, but soon recovered, 1 suffered from syphilis, and 1 died two days after delivery. Of the children, 4 were born living, 2 dying soon after. In 2 cases, the labour was left to nature; in 1, the waters were discharged; and in 6, turning was employed, one of these proving fatal. In 7 of the 9 cases, the labour was premature. In 3 cases, the presentation was cross; in 1, breech; and in 4, occipital. Bartsch observed one case of *placenta prævia* in 398 labours; Klein, one in 760; Collins, one in 1492; M'Clintock and Hardy, one in 829; and Hoffman, one in 472. From the consideration of his own and others' cases, Dr. Arneth concludes:—1. That the proportion of labours with *placenta prævia* to normal labours is very different in different institutions. 2. That the mortality of the mother varies. 3. That the abnormal position of the placenta frequently indicates the advent of labour before the expiration of the natural period of pregnancy. 4. The *placenta prævia* is more frequently found in pluriparæ than in primiparæ. 5. Preternatural presentations are of much more frequent occurrence when the placenta is thus attached.

Extraction of the Placenta.—When internal or external hæmorrhage does not call for interference, the author leaves the separation of the placenta entirely to the uterine contractions; but, if it is not thrown off in three hours, and slight traction does not suffice, the entire hand is passed into the uterus, and its separation accomplished, if, as not unfrequently happens, the introduction of the hand does not excite the uterus to expel it. Numerous observations prove that a longer delay only renders the operation, which yet will have to be undertaken, more difficult, in consequence of the womb contracting closer around the placenta. In 6527 labours, such introduction of the hand was required in 31 cases, *i. e.* 1 in 210; 14 times on account of hæmorrhage, 11 for more or less adhesion of the placenta, and 6 for a combination of hæmorrhage with such adhesion. From these two last categories, 5 mothers died. From a consideration of his own and the Dublin cases, the author concludes:—1. The operation is required once in about 225 labours. 2. It is a very dangerous one, at least when the placenta is adherent. Above one half of such patients became ill, and near a third died; while only one seventh became ill when the placenta was extracted on account of hæmorrhage only. The detention of the placenta from spasmodic action is very dangerous; for, of Collins's cases, a third died; while all the others, which underwent turning for other causes, recovered. 3. Of Collins's 10 cases of adherent placenta, 6 occurred in pluriparæ, and 4 in primiparæ. Of the author's 7 cases, 4 belonged to the former, and 3 to the latter. Of Hardy and M'Clintock's 11 cases, 10 were pluriparæ. 4. Detention from spasmodic action occurred once in 6634, in Hardy and M'Clintock's cases; once in 1631, in the author's; once in 864, in Collins's cases; and once in 682, in Hoffman's.

Hæmorrhage.—In respect to this, the author only furnishes his experience for one year, during which, in 3452 labours, 84 cases of hæmorrhage were observed, or 1 in 41. Of these 84 cases, 3 were examples of placenta prævia; and 74 occurred soon after the birth of the child, of which number 10 died, as well as 10 children; 13 children of the whole 88 being born dead. Collins met with a case of hæmorrhage in every 125 labours; M'Clintock and Hardy, 1 in 71; and Hoffman, excluding cases of placenta prævia, 1 in 40.

In treating hæmorrhage after the birth of the child, Dr. Arneth recommends, if the insertion of the funis into the placenta cannot be felt, to pass the hand at once into the uterus, to remove it. If bleeding continue after the placenta has been detached, and it is certain that it does not depend upon the detention of a portion of this organ or a coagulum, the energy of the uterus should be excited by powerful friction with the mere hand; or, where this does not alone suffice, with sulphuric ether dropped on to it. So powerful is this means, that the author was obliged to resort to additional measures only five times in two years. When such became necessary, thick cloths, soaked in cold water, were laid upon the belly, and frequently reapplied, powerful uterine action being usually induced in from five to ten minutes. In only two cases did the bleeding return again, after being thus checked, and then was suppressed by injecting cold water into the uterine cavity. Since the establishment of the second clinic, in 1833, this plan of treatment has been adopted; and, in 39,121 deliveries, in 2 cases only was death the direct result of hæmorrhage. In explanation of the great utility of this external treatment, the author observes, that it is the contraction of the fundus uteri, and not of the lower segment, that is chiefly effective in checking hæmorrhage. The ergot is not employed by the author. The following are his general conclusions:—1. The frequency of the occurrence of hæmorrhage is very different in different institutions. Although it is much oftener met with in some years than in others, this will not alone explain the difference, which is more probably due to the indefiniteness of the expression, hæmorrhage, by which the exact limit between it and a normal loss of blood is not indicated. 2. Hæmorrhages occur at some epochs in great number, and at other periods are seldom met with. From the observations which have been made, it follows that the hot months are not those in which this increase is observed. 3. In lying-in institutions, hæmorrhage is by far the most frequently met with between the expulsion of the child and the removal of the placenta. In private practice, in which premature labour is more frequently met with, the proportion is less. 4. It is by no means decided as yet, whether primiparæ or pluriparæ are most liable to hæmorrhage. 5. Twin births dispose to hæmorrhage. 6. Hæmorrhage appears to occur about as frequently in premature labour as in full-timed.

Rupture of the Uterus.—Three instances of rupture of the uterus during labour are narrated by the author, though not occurring within the two years. The experience both of the lying-in hospital and of the dissecting-room at Vienna, for a long period of time, proves that spontaneous rupture of the uterus occurs with different frequency in different years, although the cause of this is quite unknown. On examination after death, a remarkably increased development of the upper portion of the uterus, with no corresponding increase of, or even with a diminution of, the cervical region, is sometimes observed; and Dr. Arneth suggests it as highly probable, that the rupture of the organ may be due to the tissue of the cervix being unable to withstand, during its dilatation, the action of the hypertrophied fundus and body of the organ,—a hypertrophy that may be, in part, congenital, or entirely developed during pregnancy. That, for the production of a fatal rupture, a prolonged labour is not essential, is proved by Collins's observations, who found it occurring after one, four, five, six, &c., hours of labour. Its occurrence, too, in cases wherein there is no contraction of the pelvis, and when there is consequently no mechanical injury inflicted on the uterus, and when no excessive bearing-down efforts are made, favours the above view.

In the cases which Dr. Arneth has had the opportunity of seeing examined, the rupture has always extended from the vagina, through the neck and a part of the body of the uterus, it being always placed on one side, and taking an oblique direction, sometimes penetrating the muscular layer, and leaving the peritoneum uninjured. In one case, in which the labour had been quite normal, with the exception of the large quantity of liquor amnii, the peritoneum was exclusively ruptured to a considerable extent; while the "parenchyma and muscular layer" remained uninjured. In another case, only the lining membrane of the organ was found ruptured.

Eclampsia.—Among 6527 labours, 13 cases of eclampsia were observed, or 1 in 502. Collins observed 1 in 547; M'Clintock and Hardy, 1 in 510; Bartsch, 1 in 398; Boivin, 1 in 1071; and Lachapelle, 1 in 434. Of the 13 cases, 8 are indicated as primiparæ, but in 2 the circumstance is not stated. In 12 of them, the head presented. In 2 cases, the convulsions came on with the earliest pains; in 5, during the dilatation of the os; in 3, when the head was already deep in the pelvis; and in 1, two days after labour. In 2, no statement is furnished. In 9 cases, attacks recurred after the labour was over. In 6 of the cases, labour was completed by the forceps, the others being completed by the pains. Of the mothers, 4 died; 1 was seized with mania; and the others were, sooner or later, dismissed well. Of the 14 children, 3 were born dead, and 3 others died within a few days; 10 of the children were male, 4 female. Ramsbotham's opinion, that eclampsia especially occurs in hot weather, is not borne out by these cases, the great majority of which occurred in autumn and winter. The following points may be taken as proved:—1. Eclampsia especially attacks young primiparæ. 2. The proportion of cases, in a given number of births, varies. 3. About a fourth of those attacked die. 4. The majority of the children belong to the male sex. 5. In fatal cases, no changes in the brain, which can be regarded as the cause of death, are found. Points which yet require to be cleared up are:—1. The frequency of the continuance of the attacks after the completion of labour. 2. The mortality of the children. And 3. The influence of the weather.

In treating these cases, cold applications to the head, tartar emetic, and, if the pulse is hard and full, bleeding are employed. The labour, whenever possible, is left to the pains, the membranes being ruptured if they are thick or do not project into the os. Turning is very unwillingly resorted to; but if the head is low enough down, the forceps is applied.

Puerperal State.—The author's observations upon this are brief and fragmentary, the women being removed from his supervision after the ninth day. However, of 6527 who were delivered, he returns 127 as dead. Some of the causes influencing the mortality at different periods are alluded to, though the immediate causes of death are only imperfectly given. From October 1847 to October 1848, 25 of 3216, or 1 in 128, died; the numbers being pretty equally distributed. But from the 1st of November to the end of June 1849, 95 women, or 1 in 25, died. This increased mortality became manifest in November, and augmented so much in December, that 19 women (or 1 in 15) died, and it only ceased at the end of June. It was in October, 1848, that the commotions first commenced in Vienna, and exerted an especial effect upon the pregnant women of the hospital, whose male friends and relatives became engaged in the contest which followed. "The firing of cannon, which, during the last days of October, continued almost uninterruptedly, was sad music for the ears of the lying-in women." Moreover, one of the first consequences of the surrounding the capital and the daily skirmishes, was the cutting off the supply of water from the hospital, where an immense quantity is consumed daily; and the prevention of the removal of the refuse of the establishment.

The influence of the cholera upon the prevalence of puerperal diseases, was very observable. During the month of May, and early in June, 11 women died of puerperal diseases. On the 10th of June the first case of cholera was observed; and during the whole of the period of its prevalence (to the 11th of August), no fatal result attended the few puerperal diseases that presented themselves; yet no sooner had it disappeared, than these recovered their former virulence. 24 patients were attacked with cholera, 15 of whom were removed to another department, 5 recovered, and 4 died.

Twins.—In 6527 labours there were 81 twin-births, *i. e.*, 1 in every 80. Of these 61 were at full time, 20 premature. There were 80 boys born living (14 dying in a few days) and 5 dead, 74 girls born living (19 died) and 3 dead. In 73 cases both children were born living, and in 33 both with occipital presentation. In no case were both children born dead. In 51 cases both children were of the same sex. The proportion in 129,172 births occurring at the Dublin Hospital, was 1 in 62; during Collins's mastership 1 in 68; and as reported by M'Clintock and Hardy 1 in 69. Lachapelle reports 1 in 78, Boivin 1 in 132, Klein 1 in 107, Bartsch 1 in 104, Spengler (Mechlenburgh, Schwerin) 1 in 65, and Hoffman (Würzburg) 1 in 62. In the Second Clinic of Vienna, in 39,121 labours, the proportion has been 1 in 87. The following are Dr. Arneth's conclusions:

"1. That twin-births are met with seldomer at Vienna than at Mecklenburgh, Würzburgh, and Dublin; but they do not observe the same proportion to single births in different years. 2. About a fourth part of all twin-births are premature. 3. Hæmorrhage occurs more frequently after twins than after single births. While this occurred only once in 42 of the births in general, it was met with once in 16 in twin-births. 4 By far most frequently, both children are born with the occipital presentation. Next follows the case in which the first or second child presents with the occiput, the other with the breech. The other varieties of presentation are more rare; but beyond all others that is most uncommon, in which both children are placed crosswise. 5. In Dublin, as well as in Vienna, three-fifths of the twins occurring at the same birth, are of the same sex. 6. Most of the children at Vienna (154 out of 162) are born living; and among our cases, in more than three-fourths of the whole, both children were so, and continued living until the ninth day. In only 8 cases were a dead and living child born; and in no case (in 9 of Collins's it occurred) were both children born dead. 7. Eclampsia occurs much more frequently in twin-births, for while in our cases and those of Collins's it was met with only once in more than 500 labours, it occurred to us once in 81 twin-births, and to Collins three times in 240. 8. While in our cases we met with 1 death in 51 labours, we had 3 deaths in 81 twin-labours. Collins lost one woman in 100 labours, and 7 in 240 twin-labours; and M'Clintock and Hardy lost 1 in 102 labours, and 1 in 47 twin-labours. 9. In the few cases in which this was noted, we found 19 pluriparæ to 1 primiparæ, while in the entire number of labours, 15 primiparæ occurred for 19 pluriparæ. Collins found 72 primiparæ for 168 pluriparæ, while there are three and a half times as many pluriparæ as primiparæ admitted into the Dublin Hospital. M'Clintock and Hardy met with 32 primiparæ out of 95 twin cases, being the ordinary proportion." (p. 221.)

Premature Labour.—In 3452 labours, which occurred during one year, 243 women failed to reach their full time, that is, 1 in 14. Among the 255 children which they bore, there were 19 who offered a foot-presentation, 21 breech, 2 face, and 2 cross-presentations. In 12 instances there were twin-births. There were born 89 living and 31 dead males, 100 living and 34 dead females. Three mothers died, and 8 became ill, but recovered. Prof. Klein has met with 1 premature birth in 47, Bartsch 1 in 20, and Hoffman 1 in 12. The following conclusions are drawn:

"1. A premature birth is usually met with in Vienna and Würzburgh for every 12 to 14 labours, though Bartsch found 1 only in every 20. 2. In by far the greater number of cases, the premature labour occurs in the latter months, and frequently in proportion as the natural term of gestation is more nearly approached. 3. In the above-named places about a third part of the children was born dead. 4. Prematurely-born children more frequently present in abnormal positions. In our cases, and in those of Würzburg, 1 in about 5 of the presentations is preternatural, whereas only about 1 such in 32 is observed at the completed term of pregnancy. While in the Vienna Hospital 1 child in 58 offers the breech presentation, 1 in 100 the foot, 1 in 163 face, and 1 in 199 cross-presentations; the premature births presented by the foot 1 in 13, by the breech 1 in 12, by the face and in cross-presentation 1 in 127. 4. These abnormal presentations occur the more certainly in proportion to the less advanced period of the pregnancy. Thus, while they were present in 1 in 9 of eighth-month premature labours, they occurred in the proportion of 1 in 4 of seventh-month, and in that of 1 in 3 of sixth-month labours. 5. A very frequent cause of premature labour is syphilis of the mother, which also endangers the life of the child in a very high degree. In the course of two years we saw 99 mothers suffering under different forms of syphilis (*i. e.*, almost 1 in 66 of the entire number). The seventh part of the 99 had premature labour, and in 1 of 9 of such mothers the infants were born dead. At Würzburg 1 in 95 of the mothers was the subject of syphilis, and almost a third of these women brought forth dead children. 6. At Würzburg most of the premature labours occurred in June, July, August, and December; the fewest in March, April, and January. Our cases observed the following order in their frequency:—October, March, September, February, November, July, August, April, January, June, December, and May." (p. 232.)

For the purpose of *inducing premature labour,* Dr. Arneth prefers to all other means the *warm uterine douche* recommended by Kiwisch, as doing least violence to the maternal organs, and giving the child the best chance of life. Still, as different women vary much in their excitability by this means, it may, in cases where the avoidance of delay is of importance, prove too slow in its operation. An objection to this means is, that the heated tube cannot be brought immediately in contact with the os uteri, so that the stream is often in part expended on the vagina, especially as the paralysing effect of the warm water upon the operator's hand prevents him from continuing to direct the stream with any certainty. Three cases are narrated. In the first, the douche was employed at a temperature of from 104° to 113° twice a day for half an hour, from Dec. 22 to Jan. 10, but with so little effect that the ergot had to be resorted to. In the second case, the douche was employed twice a day at a temperature varying from 95° to 116°, from Sept. 26 to Oct. 5. By the second douche, the os was sufficiently expanded to admit of two fingers. After the fifth or sixth douche, the child's hand could be felt. After the eighth, cephalic version was effected by external manipulation; but next day the foot seemed to present, which proved to be the case on the 6th of October (after 19 douches had been used), when labour set in vigorously. The child was born alive. The third case occurred in the same woman, who became subsequently pregnant again. On this occasion two douches sufficed to induce actual labour pains. There was a prolapsus of the funis, and notwithstanding prompt turning, the child was lost.

Still-born Children.—During the two years, of the 6608 children, 6384 were born living, and 224 dead (118 males, 106 females); but Dr. Arneth does not distinguish exactly between those who died in utero, and those who were merely still-born; and, moreover, he considers a child born alive whose heart pulsates, however soon afterwards it may die. The result of numerous experiments enables him to confirm, as regards still-born children, the position laid down by Bouchut for adults, that death is actual when the heart has not pulsated for 5 or 6 seconds. No still-born child was brought to life under these circumstances. Long after the heart's action cannot be felt by the hand, it is audible to the ear; but as long as the slightest sound is to be heard, attempts at recovery should be persevered in. The advantage ensuing from artificial respiration is regarded by Dr. Arneth as being chiefly due to its stimulating the mucous membrane of the air-passages into increased activity, and thus inducing a copious separation of secreted matters. When endeavours at breathing have commenced on the part of the child, its continuance becomes mischievous; but the fear of injuring the air-cells by the propulsion of air even by the operator's mouth, has been proved to be, by numerous experiments made by M. Semelweis and the author, quite groundless. The child is to be gently rubbed with dry towels and placed in a warm bath. The vapour of water exerts an injurious effect upon it, but the application of cold water to the head is beneficial. Of the 6384 children born living, the author returns as dying, within nine days, 244 (129 boys and 115 girls); but the numbers were in reality considerably larger, as several children were, soon after birth, removed from his section of the establishment.

In conclusion, we may again express our satisfaction with Dr. Arneth's little work, which, from the exactitude of most of its details, will always be in future referred to, whenever statistical results have to be brought to bear upon questions of obstetrical practice.

ART. VII.

1. *Remarks on Insanity, its Nature and Treatment.* By HENRY MONRO, M.B. Oxon., Fellow of the Royal College of Physicians.—*London*, 1851. 8vo, pp. 150.
2. *On the Comparative Liability of Males and Females to Insanity, and their Comparative Curability and Mortality when Insane.* By EDWARD JARVIS, M.D., of Dorchester, Massachusetts. (Published at the New York State Lunatic Asylum.)—*Utica*, 1850.

DR. MONRO recognises three distinct sources of mental "obliquity;" firstly, "spiritual agency of an evil nature"—the devil—we presume;—secondly, "one in which the body acts, as it were, a secondary part, and where it is made instrumental to such evil spiritual agency,—as where our animal functions and propensites are perverted, and the baser parts of our being made to rule over the more honorable,"—this is "the flesh," in theological phrase;—and thirdly, "one which is wholly and primarily of bodily origin, into which the question of morals does not enter,—as where the brain, the mental instrument, acts imperfectly, disturbing manifestations, and obtruding (as all bad instruments do) its own instrumentality and imperfections." Dr. Monro is of opinion that "insanity is essentially dependent on this last source of mental obliquity."

Now this is simply to state that insanity is a disease of the brain, and that the insane are neither sinners nor possessed; but who with the least knowledge of neurology or cerebral pathology, doubts these propositions? Surely the time is past for formal proofs and arguments in their favour. Those who ignore modern science and study exclusively a dogmatic theology, may and do still entertain other views; but we believe the attempt to convince their judgment or enlighten their understanding to be utterly hopeless, unless they be placed for three months in personal contact with the insane.

The object of Dr. Monro's Essay is not, however, to demonstrate that insanity is a cerebral disease, but rather that it is due to "loss of nervous tone;" and that all its phenomena may be ascribed "to the two well-known consequences of loss of nervous tone (acting coincidently), namely, excess of nervous energy or irritable accumulation, and paralysis or loss of nervous energy." Dr. Monro was led to this proposition from a consideration of the phenomena resulting from loss of nervous tone in the motor system of nerves; and, consequently, to consider "how far the mental excesses and deficiencies of insanity could be accounted for by the same rules that account for spasm and paralysis of motion,"—stating his reasons thus:—"for, however much the spiritual being—mind, may differ essentially from such functions performed or controlled by nervous agency—as motion, nutrition, &c., yet as each uses a common instrument, namely, nervous matter, and as this mechanism is of the same nature, subject to the same infirmities, and intimately connected in its various parts both by sympathies and continuity, we must believe that, *so far as the various phenomena presented through nervous instrumentality are really dependent on this similar mechanism*, similar results are to be anticipated."

So fair an opening of the argument necessarily excites pleasing anticipations in the reader's mind. The phenomena of spasm and paralysis have, of late years, been so well elucidated by physiological and pathological researches, and the close analogies existing between the intra-spinal and intra-cranial portions of the cerebro-spinal centre, have been so well established, that a careful analytical comparison of the phenomena of insanity considered as originating in a morbid condition of the hemispherical ganglia, with the phenomena of paralytic and spasmodic affections dependent upon a similar morbid state of the true spinal ganglia, could not fail to clear up much of the obscurity hanging about mental pathology, and even lead to new views in theory and practice. This comparison we hoped to find in Dr. Monro's volume, but we regret to say that there is no trace of it. The author prefers to expound old doctrines and well-known facts, in language which gives them a deceptive air of newness. We must be content, then, to take his book as he has pleased to present it to us; and examine the theory and practice it sets forth.

The following is a syllabus of the theory:

"The theory laid down in these remarks is—that insanity is a disease of loss of nervous tone; that this loss of nervous tone is caused by a premature and abnormal exhaustibility of the vital powers of the sensorium; that this infirmity is essentially a local one, though torpor of the general, physical, and vital powers assists it; and that its origin is to be esteemed constitutional, congenital, and frequently hereditary. As a part of, and arising from this theory, the *coincident excesses* and deficiencies of mental phenomena manifested by the insane are attributed to the *coincident* existence (in different parts of the sensorium) of those two stages of loss of nervous tone,—*irritable excess of action* and *paralysis*. This view of the case renders it unnecessary to believe that the violent excesses of the insane must arise from a too sthenic condition of the system,—a doctrine which experience so much contradicts, but which the want of experience so often propagates." (p. xiii.)

The critical reader will detect an imperfect mode of expression in the above extract; the grammatical construction is faulty, and the words used do not express accurately the meaning of the writer. These faults run through the whole of Dr. Monro's Essay, and prove him to be a young and inexperienced author. When he states, for example, that insanity is a disease of loss of nervous tone, he doubtless means to say, that it is a disease which depends upon, or consists in a loss, &c. So, also, when he states that this loss of nervous tone is caused by a premature and abnormal *exhaustibility* of the vital powers of the sensorium, we apprehend that he really intends to say it is caused by, or identical with, an *exhaustion* of the vital powers, &c. It is better, however, that Dr. Monro shall define his own views; we, therefore, subjoin another extract:

"The theory of the pathology of insanity which I wish to put forward in this treatise, is as follows:—1. That it is an affection consequent on depressed vitality, which depression of vitality is wont to manifest itself with peculiar and specific force in the cerebral masses, owing to a congenital, and frequently hereditary, tendency in the brain thus to succumb when oppressed by an exciting cause. 2. That when the cerebral masses are suffering from this condition of depressed vitality, they lose that static equilibrium of the nervous energies which we call tone (and which is peculiarly indicative of healthy vigour), and they exhibit in their functions the two different degrees of deficient nervous action (coincidently), namely, irritable excess of action, and partial paralysis; that, in consequence, the brain becomes an imperfect instrument for the manifestation of mind............... 3. That these two degrees of deficient nervous energy do not fall alike upon all the seats of the mental operations, but that the seats of the more elementary faculties (such as the conception of ideas, &c.) maintain generally only the first condition, namely, that of irritable excess, which is exhibited either by excessive rapidity of succession of ideas, or undue impression of single ideas; while the seats of the less elementary but higher faculties, such as reason, will, &c., generally succumb to this second degree, namely, partial suspension of action." (pp. 12, 13.)

The critical reader will again note obscurity and indefiniteness of expression. The "loss of tone" is simply a "loss of static equilibrium of the nervous energies;" and this (as we find subsequently, p. 65) is a "loss of nervous perfection;" while elsewhere (p. 64) the phrase "loss of nervous power" seems to be used synonymously with loss of "tone," of "static equilibrium," and of "nervous perfection." But yet, again, at page 22, the author proposes "to show the probability, that insanity is simply a disease of nervous depression consequent on loss of vitality."—*Loss* of vitality is death, and would cause extinction of cerebral function, and not diminish functional activity. Now, although a careful perusal of Dr. Monro's sentences enables us to fathom his general meaning, we must say that he is occasionally incomprehensible. However, we will not be hypercritical; and we think we can say, in a few words, what Dr. Monro's theory really is. In certain individuals the cerebral tissue (the organ of mind) is not so perfectly organised as to be able to resist disturbing agents under all circumstances. This condition constitutes the *predisposing* cause of insanity, and is congenital and hereditary in the greater number of cases. When an individual so constituted is exposed to agencies which depress the vital powers of the cerebral tissue, its mode of action is changed, and irritability or increased and irregular activity of function is excited, or its normal activity is simply diminished. The agencies thus operating are termed the *exciting* causes; while the *proximate* cause of insanity is the depression in the vital powers of the cerebral tissue thus excited. In proof of his views, Dr. Monro investigates certain states of the cerebrum analogous to insanity, as sleep, and its various modifications, as trance—somnambulism—dreaming—the mental and bodily condition induced by violent passions—the condition of the brain in infancy and old age—the disturbance of cerebral functions of toxic origin—and the delirium occasioned by mechanical pressure. He attempts to show that the brain and nervous system is in a state of depressed vital activity under all these agencies; and seeing that the resulting mental condition has points of analogy with insanity, Dr. Monro concludes that that affection is dependent upon a similar cerebral change:

"To sum up, then, the *second inference* which I would draw from the investigation of the various mental conditions given above, I would say, that I believe the state of nervous depression in the insane does not essentially depend on poisons *ab externo*, on pressure, want of development, &c., but rather to be consequent upon a constitutional and congenital want of vital power in the brain, which manifests itself by such a state of exhaustibility as is unlike the ordinary exhaustibility of healthy nature, is of a more permanent character, and occurs prematurely when compared with the vital condition of the general system. That external agents act as exciting causes to this state of loss of vigour, but that both the predisposing and proximate cause is in the organ itself. And (anticipating somewhat what I have to say on the relation which I believe deterioration of blood to hold in the pathology of the insane), I would say, in the terms of microscopic anatomy, that I believe the fault to exist in the nerve-model rather than in the matter assimilated." (p. 75.)

We do not know whether the reader will understand Dr. Monro's theory the better either for our comment or quotation. We may state, however, that his pathological anatomy is nowise different from that to be found in books. His plan of treatment is expectant and hygienic. A disciple of Pinel and Esquirol, and a follower of Conolly and all the abler psychiatric physicians of the day, he recommends muscular exercise in the open air, good diet, tepid bathing, and the cold douche, and moral management. Whatever may be thought of Dr. Monro's theories, no fault can be found with his *methodus medendi* in the class of cases (the asthenic) which he describes. Still he has not supplied us with the great desideratum in psychiatrical literature, for he has not ventured from the beaten track. With much that is true there is much that is speculative, but nothing that is new.

We think Dr. Monro might do better than he has done in the work before us; and we trust that as his mind matures and his judgment ripens—that as his reading becomes more extensive and his observations more numerous, he *will* do better. We would suggest, however, more observation of the exciting causes, and less speculation on the proximate causes. A useful line of research might be marked out in that direction; morbid conditions of the heart, for example, might be considered as exciting causes in all their relations. Foville, Prichard, Thurnam, and others, have pointed out the close relations generally existing between insanity and cardiac disease. We want something, however, more specific as to the particular form of mental derangement associated therewith. Or, following out the useful analogy which Dr. Monro announced only to abandon, our author might trace the relations of mental disorder in its various forms to disease of other viscera. The stomach, liver, spleen, kidneys, ovaria, uterus, intestinal mucous membrane, and skin, have each their special relations to the hemispherical ganglia, and when diseased in a person already predisposed to insanity, are apt to develope the latter affection. Here is a vast field of research, which, if duly cultivated, would return a hundred-fold to the philosophical inquirer.

Although we have not been able to award much commendation to our author, either for his philosophy or his style, we readily acknowledge that we have found some interesting and useful statements in the volume. We ought to remark, however, that the work is addressed to the general as well as to the professional reader,—an unwise step on the part of a young author, if he seeks reputation amongst his brethren, or reputation of any sort. It is always better to be decided, and not attempt to serve two masters.

It will be observed with interest, that the pamphlet of Dr. Jarvis has issued from the press of a Lunatic Asylum. It is refreshing to turn from Dr. Monro's theoretical expositions, to the contemplation of a practical fact like this. The pamphlet itself is a business-like *multum in parvo*, full of statistical details, which indicate diligent labour, at least, and if not to be altogether relied on as accurate exponents of general facts, are, nevertheless, valuable as approximations to truth.

Dr. Jarvis observes, that unfortunately all our data, which should show the number of lunatics among all the people, or the proportion of lunatics in the two sexes, are of a secondary nature. Even the returns of our Commissioners in Lunacy are inexact, as the patients in private families or at their own homes are not reported. The Belgian Commissioners have supplied the only reliable document of this kind; in 1835 there were in Belgium 2744 male lunatics and 2361 females, being an excess of the former over the latter of about 16 per cent. In the absence of materials for a better, Dr. Jarvis has followed the plan which Esquirol adopted to determine the relative occurrence of insanity in the two sexes, who collected the records of many hospitals, and ascertained that there were and had been confined in these, during various periods, but equal for both sexes, 38,701 females and 37,825 males, or in about the proportion of 38 to 37; and this he inferred was about the ratio in which insanity affected the two sexes. Dr. Monro has adopted this plan with regard to Bethlehem Hospital, and finds that with equal accommodation for the male as for the female sex, with an equal ease of admission, and with equal means of cure, the proportion of females to males is as 3 to 2; and hence he infers, that that is the ratio of frequency of occurrence in the two sexes. Dr. Monro's own figures do not give this result; for it appears that during the twenty-nine years ending December, 1848, 3979 females were admitted, and 2657 males, the proportion of which numbers is very nearly as 8 to 5.

Dr. Jarvis collected the reports of the following hospitals and asylums for the insane; in England and Wales 159, Scotland 8, Ireland 12, Belgium 37, France 11, Germany 2, United States 21, Canada 1, making a total of 250. The reports of these institutions are for various periods, but equal for both sexes. He has found that the 21 establishments of the United States received 13,473 males and 11,100 females, or in the proportion of 1·21 to 1; in England and Wales of 70,582 of both sexes, the proportion of males to females was as 1·02 to 1; while in the Scottish it was as 1·16, in the Irish as 1·08, in the French as 1·14, in the German as 1·60. In Belgium only were the male patients admitted less numerous than the females, the latter being to the males in the proportion of 1·07 to 1. The grand totals of the 250 establishments are 64,786 males and

60,242 females, or in the proportion of 1·07 to 1. The results of Dr. Thurnam's tables are not widely different from these. Thus, he shows that the records of admission into 32 asylums, British, American, and continental (excluding St. Luke's and all cases admitted into Bethlehem), indicate the proportion to be nearly 1·14 to 1 of male and female patients; but including the exceptional cases, only 1·006 to 1.

It is obvious that the relative proportions of the two sexes attacked by insanity, as indicated by returns and statistical details like those above quoted, will depend upon certain other proportions. Firstly, we have to determine the relative proportions of the sexes absolutely constituting the entire population to which the returns refer. Thus the proportion of females to males admitted into the Friends' Retreat, near York, is as 1·18 to 1; and hence it might be erroneously inferred, that in that proportion the women members of that religious body were more liable to insanity than the men; but, in fact, the proportion of females of all ages to males in the entire society, is as 1·20 to 1; or, to put the case more correctly and take males and females of the age at which insanity usually occurs, (namely, above the age of 15,) the proportion of females to males is as 1·30, or 1·35 to 1; so that, so far from women Friends being more liable to insanity than men, in the proportion of 1·18 to 1, the ratio is reversed. Secondly, the numbers sent to asylums out of a given insane population, will modify the returns. Thus in Belgium the sexual proportion of lunatics in the general population, was of males to females as 1·16 to 1, whereas the proportion in hospitals was as 0·94 to 1, or, in other words, 60 per cent. of the insane women in Belgium were in hospital, but only 48 per cent. of the insane men. Dr. Thurnam shows statistically that condition in life is an important element of this difference. Thus of 67,876 patients admitted into British asylums from their opening to January, 1844, the males were to the females as 1·13 to 1; but on analysing this table we find, that in the pauper class the proportion is as 1·03 to 1, while in the private it is as 1·31 to 1.

This curious statistical result has an interesting bearing on the Etiology of insanity, for it is in the pauper class of a population that we should expect to find the fewest of those moral and spiritual agencies which act banefully on the organ of mind. Dr. Thurnam also shows, that in The York Retreat there was a progressive accumulation of females, so that at the end of 45 years they exceeded the males by 30 per cent. This circumstance is dependent, doubtless, on the greater viability of the former; for if in each year an equal number of the sexes were admitted, as the males would die more rapidly, every year would show a continually increasing preponderance of the females. Dr. Thurnam also thinks that there is a greater difficulty in removing males to asylums than females, since the latter are sooner rendered more entirely dependent than the former.

It is curious that all statistical inquiries show a difference in different countries in the proportion of the sexes afflicted with insanity. Dr. Jarvis's tables demonstrate that the males predominate in the asylums of America, England, Scotland, Ireland, and France; the females in the asylums of Belgium; whilst among the population of Norway and Paris, and the paupers of England and Wales, the females predominate. Esquirol stated, twenty years ago, that in Scotland the sexes were affected with insanity in equal proportions, in England the insane males predominated. The north of France presented a large proportion of females, the south of males. In Naples the female lunatics predominated over the male in the proportion of two to one; in Milan the proportion was the reverse.

Dr. Jarvis tabulates the causes of insanity, with a view to determine the reasons of this difference in the two sexes, and arrives at results which might be expected *à priori*. His tables show by figures what he states in words:

"In as far as men, from their habits, their position, and their exposures, are more frequently intemperate; in so far as they have more of the sexual passion, and less delicacy of sensibility, and, therefore, more given to masturbation and sensuality; in as far as they are more involved in business, and more interested in property, in politics, in schemes of aggrandisement, and in pursuit of knowledge, and are, therefore, more frequently bankrupt, or disappointed, or over-wrought with labour and anxiety; in as far as they are employed with machinery, and with powder, or more frequently travel and go over dangerous places, or are involved in strifes and bodily quarrels, and, therefore, meet with more accidents, falls, blows on the head, &c., than women...............there are more male than female lunatics.

"But, in as far as females have more sensibility, and stronger affections, and more active sympathies, and, therefore, suffer more intensely from grief, and loss and sickness of friends, and more from a cause almost peculiar to themselves in the want of domestic sympathy, and in the ill-treatment of intemperate or unkind husbands, or children, or other kindred; in as far as females are more sedentary, and are, therefore, more frequently dyspeptic, or suffer secondary irritation from the sympathy with the reproductive system, and have, therefore, more ill-health; and inasmuch as they are more timid, and are, therefore, more exposed to fright,...............females are more liable to insanity than males." (p. 23.)

Dr. Jarvis has, however, extended his statistical inquiries into the comparative liability of the two sexes to fatal diseases of the nervous system, and he finds that the greater fatality is amongst the males. Taking the Reports of the Registrar General for five years and a half, of the States of New York and Massachusetts, and of the Cities of New York and Philadelphia, he finds that comparing the deaths from diseases of the brain and nervous system in the two sexes with the total deaths from all causes, the numbers were 16·15 per cent. males, and only 13·85 per cent. females. Dr. Jarvis gives an elaborate table showing the proportion of deaths in the two sexes from diseases of the nervous system, arranged under twelve heads; under the head of "palsy" only, is there a predominance of female deaths. It is interesting to notice that this difference in the greater mortality of males is not limited to adult life, as in insanity, but extends to all ages. Thus, under the age of ten years, the deaths of males from "convulsions" predominated over those of females by 22 per cent., and from "hydrocephalus" by 24 per cent.

Dr. Jarvis also establishes statistically the greater mortality among male than among female lunatics, as well as their less curability; the deaths in 57,980 males admitted into various asylums amounted to 21 per cent.; amongst 54,163 females to 15 per cent. Dr. Thurnam's statistical researches show more decidedly the greater mortality of male lunatics, the excess varying from 96 per cent. at St. Luke's, to 9 per cent. at Schleswig. At the Senavra, Milan (from the peculiar circumstance of insanity being connected with pellagra), the mortality of the females exceeded that of the males by 13 per cent. Dr. Monro gives some statistical statements which are evidently erroneous; he makes the mortality amongst the male patients in Bethlehem "more than 5 per cent.," of females "about $4\frac{1}{3}$ per cent."

Now, according to Mr. Farr's statistics of Bethlehem "curables," for the thirteen years from 1827—39, the mean annual mortality of the male patients was 14·07 per cent., of females 8·23 per cent. We are surprised to see that Dr. Monro writes as if in total ignorance of Dr. Thurnam's very valuable "Statistics on Insanity;" for he not only never refers to the work, but commits errors in his statistical arithmetic, which a careful perusal of that work would have prevented him from making.

We observe that Dr. Jarvis has some ideas on the question of hereditariness, which appear either very peculiar or very indefinite. In America, the Tennessee Hospital alone reports this as a sole cause, and gives only 5 males and 5 females amongst 306, as instances of its operation. Dr. Jarvis observes, "Certainly but a small proportion of those who are born of insane parentage or ancestry—and therefore inherit the tainted constitution—become insane." This is so contrary to all our facts and observations, and, indeed, so contrary to his own, that we cannot but pronounce it erroneous. We need only quote himself for proof:

"The most remarkable family which I have been able to investigate, has had insanity in some of its members for four generations. I have learned the history of 65 members of this family. Of these, 15 are or have been insane, 1 idiotic, 2 epileptic, 3 had delirium tremens, 3 died of brain fever, 1 is subject to depression of spirits and unable to attend to business a part of the time, 1 is subject to frequent and violent headaches, 1 has nervous trembling, amounting almost to constant chorea, and 1 has low spirits. All the rest, including children, so far as I can learn, are sound. But all these cases are so distinctly referable to some new and exciting cause, that the family deny that there is any hereditary taint in their blood." (p. 20.)

Dr. Jarvis seems to indorse this opinion; yet what are the facts? Insanity, he justly observes, rarely attacks persons under 20 years of age; now from every million of persons aged above 20 years, in 15 States of the Union, 9214 were received into the Lunatic Hospitals, or at the rate of about ·01 per cent.; whereas we have in this family supposed to be without hereditary taint, actual insanity occurring at the rate of 21 per cent., and if we group all the cognate diseases, at the rate of 33 per cent., and this including the children! If there be any truth in statistics, by far the larger proportion of the adults of this family must have suffered from insanity or some cognate affection of the cerebrum, and must, therefore, have suffered from it in an infinitely greater ratio (as about 80·00 to 0·01) than the general population. Ten per cent. of the adult members of a family would be a large proportion to suffer from *actual* insanity, and would assuredly indicate a hereditary predisposition to *cerebral* disease. As this point in the etiology of mental disorder seems to be not at all (or, at least, imperfectly,) understood in the United States, we commend it to the special attention of Dr. Jarvis.

Art. VIII.

The Transactions of the American Medical Association. Vol. III.—*Philadelphia,* 1850. 8vo, pp. 500.

This volume is a chronicle of the proceedings of the Third Meeting of the American Medical Association, held at Cincinnati, under the presidency of Dr. Warren, sen., in May, 1850. A high and laudable tone seems to have prevailed throughout all the proceedings, and we do not find in the present Reports any of those inconsistencies we felt it requisite to comment upon in their predecessors. On the present occasion our task will simply consist in bringing under the notice of our readers some of the most prominent points contained in the Reports of the respective committees, which were charged with the management of the details of the business of the Association upon the occasion of its second meeting.

Medical Education.—Little progress, we are sorry to find, has been made in improving the glaring defects which we adverted to in our notice of the former volumes; and which are thus tersely summed up in the present report.

"The opinions relate chiefly to conclusions, thought to be deducible from the facts. So far as they have assumed a definite form, they assert, first, in general terms, that the system of medical education, in this country, is defective. Second, that it is so, because the number of schools is too great, their situation (in many cases) bad, their instructors too few, their students and graduates too many, the time devoted to instruction too short, the quantity too limited, the quality too superficial, the bestowal of honours too profuse, and the mode of bestowal too unrestricted; all this involving, inevitably, a depreciation of the profession, and an impairment of the dignity, honour, and usefulness of the healing art." (p. 146.)

What can be more distressing to high-minded members of the profession, than finding the directors and teachers of the various schools, in place of vying with each other in the extent of the field of study they offer to the pupil, and appealing to all the better feelings that should actuate him, outbidding each other with promises of the shortness of their courses, the exemption of certain objects of study of vital importance, the easiness of their examinations, and the cheapness of their wares? And yet such a state of things is far more easily deplored than amended; for the interests, sordid as they are, to be opposed, are numerous and powerful; and little effect seems as yet to have attended the appeals of the Association. "Neither of the parties most interested seems willing to assume the responsibility. The student dislikes to have his self-complacency offended: the practitioner fears to hazard the good will of the student; and the school, it is feared, is often too anxious, lest the portal of some active rival should be found of easier access than its own." We much doubt whether either of the above "parties" are those who are most interested in the change; for we are at a loss to understand how the unfortunate public is to be efficiently supplied by such means as those deprecated. Difficult as any legislative interference in such matters is in this country, it is absolutely unavailing in one composed of numerous independent states, as America; and whatever good is to be accomplished will be only effected by influencing public opinion in a more indirect manner. As the first step towards this, it is consoling to find so important a body as the Association basing every professional aspiration upon this first great preliminary requirement—improved medical education; a position, in its degree, as applicable to this country as to the United States.

Medical Literature.—While believing that American Medical Literature exhibits some hopeful signs, the Association is by no means disposed to deny its great inferiority in scientific research, and in scholarship both general and professional; its aim being chiefly at present the application of the doctrines or precepts derived from Europe. Beside others recommending the bestowal of prizes on the authors of the best American medical works, the following were some of the resolutions adopted:

"Resolved; that, in the opinion of this Association, the only legitimate means within our reach, for the encouragement and maintenance of a National Medical Literature, is to increase the standard of preliminary and professional education required of those who would enter the medical profession; to promote the circulation among the members of the profession of the medical journals of the day; to encourage the establishment of district medical libraries; and to induce every practitioner to cultivate with care the fields of observation and research which are within his reach.

"Whereas, the interests and the dignity of the medical profession of the United States, as well as a true spirit of patriotism, and a love of independence, demand that we should use all proper and honorable means for the establishment of a National Medical Literature; and whereas, we have hitherto paid too blind and indiscriminate a deference and devotion to European authorities, and not sufficiently patronised and protected our own; therefore, Resolved,—1. That this Association earnestly and respectfully recommend to the medical profession generally, and to the various medical schools in particular, the employment of native works as text-books for their pupils, instead of the productions of foreign writers. 2. That the editing of English works by American physicians has a tendency to repress native literary and scientific authorship, and ought, therefore, to be discouraged by all who have at heart the objects contemplated in the preamble. 3. That this Association will always hail with satisfaction the reprint, in their original and unmutilated form, of any meritorious work that may emanate from the British press." (p. 45.

Perhaps some of the expressions in the above resolutions too much imply that a sound American medical literature exists, to which the productions of foreign writers have been injudiciously preferred. Such preference has, however, been a matter hitherto of necessity, in the absence of sterling works of native growth. The Association has referred in other formal resolutions to the only means, conjoined with improved medical education, that can confer such a boon upon their country, viz. the speedy passing of an International Copyright Act. A memorial to Congress strongly advocating this measure has been agreed upon; and if the attempt now being made to destroy the validity of American copyrights in this country should prove successful, the obvious interests of the American writers must urge the matter further forward. Indeed, the Reporters state that a very general feeling in favour of the measure exists, both among them and the respectable publishers; and as such has long prevailed here, it is difficult to see why an adjustment should be much further delayed.

In the Reports of the Committee on Medical Science, Practical Medicine, Surgery, and Hygiene, there are several notices of American contributions on these subjects, both of interest and originality; and some of these we may briefly advert to.

Report on Medical Science and Practice.—The first article under this head refers to some investigations of Professor Wyman on the *Arrangement of Cancelli in Bone.* The cancelli, he states, of bones which assist in supporting the weight of the body, are arranged either in the direction of that weight, or so as to support and brace the cancelli so placed; so that they may be regarded as acting in nearly all these bones as a series of "studs" and "braces." These fibres of some of the bones in the human subject have a definite direction in relation to the erect posture. In the *lumbar vertebræ* some are disposed vertically for the purpose of directly sustaining the weight of the body, and others brace these at right angles. The *cervix femoris* is supplied with two sets of cancelli:

"One set rests or abuts on the convex surface of the thick shell which forms the under wall of the neck, and from this they diverge towards the upper portion of the head, neck, trochanter major, and portion of the shaft just below this last; those which extend into the head are much the longest. The fibres of the second series are arranged in parallel curves, the extremes of which are attached on the one hand to the wall of the bone at the base of the great trochanter, and on the other to that portion of the preceding class of fibres which supports the upper surface of the head, as well as to the shell of bone between it and the trochanter. Both of these series are braced by other fibres, which are arranged at right angles to their direction. The cancelli of the great trochanter have no determinate form. The internal fibres thus act as braces, assisting the outer walls to support the weight of the body; the curved fibres resisting by their tenacity, the straight or radiating ones by their rigidity. The long straight fibres transmit weight directly to the under side of the neck, and are themselves supported by the curved fibres, and these in turn by the radiating fibres." (p. 55.)

In the lower part of the femur and the tibia, the cancelli are adapted to support vertical pressure. On the astragalus the pressure is vertical; but as the bone rests itself upon the os calcis below and the scaphoid in front, there exist two series of cancelli, directing pressure on the surfaces of support. In the os calcis, too, there are two intersecting sets of fibres, the one radiating, the other in concentric curves.

These arrangements are not found in quadrupeds, and only traces of them exist in apes, while in man himself they are chiefly confined to the bones engaged in locomotion. In these a certain direction of fibres coincides with a certain direction or certain directions of the transmission of pressure.

Capacity of the Crania in different Races of Man.—Dr. Morton, author of the great work on *Crania Americana* (whose death we regret to have seen recently announced), has given the results of the internal measurements of 623 human crania. His process for admeasurement was to fill each cranium with leaden shot, and determine its absolute capacity, or the bulk of the brain, in cubic inches; the following are some of the facts elicited:

"1. The Teutonic or German race, embracing the Anglo-Saxons, Anglo-Irish, Anglo-Americans, &c., possesses the largest brain of any people. 2. The nations having the smallest heads are the ancient Peruvians and the Australians. 3. The barbarous tribes of America possess a much larger brain than the demi-civilised Peruvians or Mexicans. 4. The ancient Egyptians, whose civilisation antedates that of all other people, have the least-sized brain of any Caucasian nation, except the Hindoos, for the very few Semitic heads will hardly permit them to be admitted into the comparison. 5. The Negro brain is 9 cubic inches less than the Teutonic, and

3 cubic inches larger than the ancient Egyptian. 6. The largest brain in the series is that of a Dutch gentleman, and gives 114 cubic inches; the smallest head is an old Peruvian, of 58 cubic inches; and the difference between these two extremes is no less than 56 cubic inches. 7. The brain of the Australian and Hottentot falls far below that of the Negro, and measures precisely the same as the ancient Peruvian. 8. This extended series of measurements fully confirms the fact stated in the 'Crania Americana,' that the various modes of distorting the cranium occasion no diminution of its internal capacity, and consequently do not affect the size of the brain." (p. 57.)

Embryonic conditions persistent in the African Cranium.—Dr. John Neill affirms, that the African cranium is characterised by a division of the articulating surface of the occipital condyle into two faces by a transverse ridge or groove; the two faces being sufficiently rounded off in some cases to give a figure of 8, instead of a single oval. This appears to be the persistent indication of the fissure which originally separates the basi-occipital bone from the ex-occipitals. It is far, however, even on Dr. Neill's own showing, from being a constant character of the African race; for it is more frequently absent than present, and occasionally shows itself in other races. We have looked through at least twenty African crania in a large collection, without once meeting with it, the only skull which unequivocally presented it being that of a young Tasmanian.—Dr. Neill's second character is also the persistence of a fœtal peculiarity; namely, the want of the sharp edge at the lower boundary of the posterior nares, which is found in the higher races. This we have found to be more constantly present in the African skulls; but it is found also, at least as strongly, in Australian; and an approximation to it exists wherever there is a tendency to the prognathous conformation.

Hair of different Races of Men.—Dr. Browne has attempted to base a specific distinction of races on the microscopic characters of their hair; but as he is obviously unaware of the variations which may present themselves in this particular, within the limits of any one race, the foundation of his diagnosis is completely fallacious.

Revaccination.—The utility of this practice is now pretty generally admitted in Europe; and in the report under examination we find the following propositions advanced by the late Dr. Fisher, of Boston, who is described in it as having long given his attention to the subject of variola and kindred affections, and as being the author of the best description of them extant:

"1. That one single and perfect vaccination does not, for all time, in all cases, deprive the system of its susceptibility of variolous disease. 2. That one or more revaccinations *do*; and that, consequently, a physician should recommend revaccination, when questioned as to its necessity. 3. A portion of vaccinated persons are protected from smallpox through life by one vaccination. 4. An indefinite number are protected only for a certain period of time. 5. The length of time they are thus protected is undetermined. 6. Some individuals require to be vaccinated a number of times during life. 7. The system is protected from variolous contagion, when it is no longer susceptible of vaccine influence, as tested by revaccination. 8. The cowpox virus does not seem to be more efficacious than the human vaccine virus in its prophylactic virtues, and the influence of the vaccine virus does not seem to be diminished by the number of its removes from the cow, or passages through the human system. 9. The appearances of vaccine cicatrices furnish no indication that the system may or may not be again influenced by repeated vaccinations. 10. A plurality of vesicles have no more effect in rendering the system less obnoxious to the influence of revaccination than a single vesicle has. 11. The lapse of time from the period of primary vaccination to that of revaccination has some, though but little, effect in preparing the system to be further influenced by the vaccine virus. 12. The age of puberty tends in a great degree to destroy the effect of primary vaccination. 13. The virus contained in vesicles resulting from revaccination has the same anti-vaccine and anti-variolous power as that which is the product of vesicles produced by the primary vaccination." (p. 74.)

Dr. Flint on Serous Effusion in the Cavity of the Arachnoid.—Dr. Flint believes that this affection has been of late too little regarded, attention being chiefly fixed upon effusion within the ventricles. It may be easily overlooked in the mode in which autopsies are usually made. By taking precautions, the fluid will be found at the base of the skull, and within the spinal canal. When morbid, its quantity may vary from 5½ oz. to 6 oz. or more, and it may be translucent, opaque, or bloody. During life it settles towards the spine, pressing on the medulla oblongata, and producing early derangement of respiration and deglutition. Dr. Flint has met with twelve such cases, where death evidently began by failure of respiration, the heart continuing to act after breathing had ceased. Eleven were examined, and in these serous effusion was found within the arachnoid. The following is a condensed statement of the propositions which Dr. Flint believes sufficiently sustained to deserve investigation:

"Effusion in the arachnoid cavity at the base of the brain, is liable to occur as an incident in the course of various affections, being immediately produced by cerebral congestion, and without premonitory symptoms from which we can predict it. It is not an unfrequent cause of sudden deaths, such as are ascribed to cerebral congestion, or effusion within the ventricles. It produces death by apnœa, from pressure on the medulla oblongata. Among the means of distinguishing the affection are these: 'Somnolency, or coma, without paralysis, not preceded by the symptoms of meningitis; disordered respiration; deglutition, lost or impaired; intellect not disordered, if the patient is susceptible of being roused; forces carrying on the circulation not affected in proportion to the disturbance of respiration and deglutition; and the *sudden* development of grave cerebral trouble, and rapid tendency to a fatal result by apnœa.'

"The gravity of the symptoms, the danger, and the suddenness of death will depend on the rapidity of effusion, as well as on the amount of fluid. Insolation or sun-stroke, when fatal, probably induces this affection. The presence of fluid on the arachnoid cavity, producing grave symptoms by compressing the medulla oblongata, is almost necessarily fatal; and it is doubtful if much can be expected from treatment." (p. 78.)

Spontaneous Hydrophobia.—Dr. Condie reports a case of this curious occurrence, another example of which we quoted in our 3d Volume, p. 265. A man, æt. 35, of temperate habits, who could recollect no severe illness, except a short convulsive attack several years ago, was seized on the morning of Aug. 28 with stiffness along the left side of the neck, and a sense of numbness in the arm of that side. This was followed by pain extending from the occiput along the left side of the neck and body, thirst, a sense of suffocation, and convulsions on attempting to drink, and, next day, by the fully developed symptoms of hydrophobia. He died on the morning of the 30th. No autopsy. He declared he had never been bitten, nor had he received any wound or contusion for eighteen years. A minute examination could detect no cicatrix.

Causes of the Mortality of Children in relation to Sex.—It is well known, that the number of male children at birth considerably exceeds that of female children, but that the preponderance is soon lost by reason of their greater mortality, so that the numbers of the two sexes living at the age of 10 are very nearly equal, while at the 15th year the proportions which existed at birth have become reversed. The excess of mortality is not explicable by the greater exposure of males to hazardous sports and employments, for the greatest excess is observed in the earliest months of infancy, when this does not exist, and at a later period girls are more exposed to burns and scalds than boys.

If the particular diseases which prove fatal to children be examined, these will be found to be, in *boys*, inflammatory affections, and especially those of the brain and appendages, and their consequences. On the other hand, the principal diseases of which *girls* die in the greatest proportion, are pertussis, smallpox, scarlatina, measles, thrush, and consumption. So that the diseases most fatal to *male* children, are of the *sthenic* class, and are those usually characterised, in their incipient stages, by excessive inflammatory action, prone to attack subjects in whom the energies of life are most highly developed; while those which carry off most *female* children are allied to the *asthenic* class, being most liable to prove fatal where the forces of organic life are comparatively feeble, and having their seats mostly in the mucous and cutaneous tissues. After quoting some of the English mortality returns, in corroboration, the reporters continue:

"These statistical results, while they correct the common error of ascribing the excessive mortality of male children to their greater exposure to casualties, weather, &c., show that the main cause is to be sought in the sexual differences and organisation, each sex being endowed with peculiar physical characteristics, which tend, even at this early age, to develope certain diseases and determine their results. These facts, besides being curious, suggest hints as to the medical treatment of children. The prominent fact, that boys are more prone to succumb to diseases of high inflammatory action, must surely demand more particular attention to discrimination in the treatment of their diseases, and the adoption of more prompt and energetic measures for reducing these exalted actions, which tend to disorganisation and death. On the other hand, with patients of the female sex, even in infancy, increased caution should be observed, to guard against the effects of debility, and provide timely support to the more feeble resources of the female system." (p. 93.)

Circumstances influencing the Number of Births and the Proportion of the Sexes at Birth.—The following document was communicated to the Committee by Dr. Gouverneur Emerson, already well known to our readers, in relation to this description of investigation; and, as it is both brief and interesting, we will give it *verbatim:*

"1. The Seasons. The following general results, relative to this point, were obtained from estimates, based upon 65,542 births in Philadelphia. The greatest number of conceptions occurred during the winter and spring months, the maximum being 17,645 in the spring months. The smallest number occurred in the summer and autumn months, the minimum being 15,200 in the summer quarter. The greatest excess of male conceptions is shown in the winter season, when, the total being 17,184, the males were 9007, and the females 8177. The excess of male conceptions for the other three quarters or seasons varies but little from the minimum excess, which occurs in the spring.

"2. The plenty or deficiency of food, purity or impurity of air, overworking, and whatever tends to exalt or to impair the vital energies of the people. In many parts of Europe, where the general population is overworked and underfed, the excess of male births is very small; being, throughout France and Prussia, under 6 per cent., and in England about 5 per cent. In Philadelphia, where the general condition of the population is very favorable, the male births exceed the female about 7 per cent. In the rural districts of the United States, and especially in the newest settlements, the preponderance of boys at birth is believed to be not less than 10 per cent. An opposite result is found, when fatal epidemics alarm and depress the public mind. Thus, children born in Philadelphia, whose conception occurred during the prevalence of cholera in 1832, show a preponderance of females. The same results are shown in the births, at Paris, which took place nine months after the epidemic prevailed there in 1832. The births at a somewhat longer period after the visitation of an epidemic, exhibit an increase in the amount of males, because the parents are endowed with vital energies above the average, as is shown by their exemption or recovery from a disease." (p. 94.)

Ozone.—As the physicians at Chicago supposed a connection to exist between the manifestation of ozone and the presence of cholera, considerable attention has been paid to this substance of late. Professor Peter made daily observations at Lexington from June 30th to August 12th, being nearly the complete period during which cholera prevailed there. He found that there was no constant relation between the amount of ozone and the number of deaths. The indications of its presence did not cease with the cessation of cholera, and the principle which discolours the iodine test seems to be rarely absent from the atmosphere. The analyses which Professor Ellet made of the air of New York during the prevalence of cholera, furnished wholly negative results in regard to the presence of foreign matters. As to ozone, he says—"I was forced to the conclusion, not only that no such peculiar principle or condition existed in the atmosphere at the time, but that the experiments of those European chemists who have announced the production, by artificial means, of such a new form of matter, or such a modified or 'allotropic' condition of any of those forms previously known to us, are unsatisfactory." Professor Horsford has observed that ozone, subjected to a heat of 130° F., loses its properties.

The Cholera of 1849.—There is an excellent Report on the cholera visitation of 1849, which, without professing to give any detailed history of this, adverts to several important points. One of these is the fallacy of employing comparative hospital statistics in order to judge of comparative methods of treatment, without being assured that the cases related to the same stages of the affection or the same class of patients. The committee, too, justly animadverts upon the common error of supposing some of the thousand remedies curative, because they check some urgent symptom:

"It has been erroneously supposed that much has been effected when the vomiting and purging are checked; but if the medicines by which that result is obtained do not also abate exhalation into the *primæ viæ*, the patient is placed only in the greater danger. The Boston reporters tell us that in *no case* did wood-naphtha fail, after the second or third dose, to relieve the vomiting *perfectly*. 'Even in cases which ultimately proved fatal, we were perfectly satisfied of the power of the drug to check vomiting speedily.'" (p. 117.)

Considerable pains is taken to trace the advent of the disease into the United States; and the reporters (Drs. Mitchell, La Roche, and West) express their conviction that it is a *portable* but not a *contagious* affection.

The first portion of this conclusion is sought to be supported by the history of its first introduction into New York, of which an account was given in the Appendix to Dr. Parkes's Report. (See Vol. IV, p. 274.)

For the purpose of negativing the supposition of the cholera having been propagated by *contagion*, the Committee present the result of an investigation into its progress in Philadelphia, analogous to the important and laborious one which Dr. Parkes undertook when the disease broke out in London. The dates and places of occurrence of the first twenty-three cases are given, authenticated by the testimony of the respective medical attendants. This inquiry has shown the utter want of intercommunication of the persons successively attacked; and, indeed, the separation of their residences from each other by distances varying from a quarter of a mile to five miles.

Treatment of Aphonia by Inhalation of Stimulant Vapour.—Dr. Pancoast draws attention to the successful treatment of a form of aphonia which sometimes succeeds to an ordinary cold, without leaving any perceptible organic lesion in the pulmonary apparatus. The voice is reduced to a mere faint, hoarse whisper, distinguishable at only a few feet; and a continuance of attempts to talk induces great fatigue, as if from an obstruction in the larynx. There is little or no difficulty of breathing, and the patients can undergo a considerable amount of bodily exertion.

Dr. Pancoast, believing this condition to be due to a partial paralysis of the muscles regulating the motions of the *chordæ vocales*, determined upon the employment of stimuli. In his first case, the aphonia, supervening on a cold in a healthy girl, had resisted all treatment for six months. He caused her to inhale chlorine gas for some minutes two or three times a day, according to the amount of irritation it induced, and in three days her voice was as strong as ever. Recently he has treated an eminent medical practitioner, who was prevented by an aphonia of seven months' duration from pursuing his profession, and who had in vain, among other means, repeatedly applied strong nitrate of silver lotion to the glottis. In from a week to ten days his voice was quite restored.

REPORT ON SURGERY.—*On the Employment of Anæsthetic Agents in Surgery.*—It is a matter of interest to examine the reputation these bodies retain in the land of their birth, after three years and a half's experience in their employment; and it is gratifying to find, amidst considerable diversity of opinion as to which particular agent is the preferable one, that complete unanimity prevails in regarding them as of inestimable value, and as unproductive of danger, when used with proper precaution. Apparatus of any kind does not seem to have been resorted to, a hollow sponge or a napkin being the usual means employed. Upon the whole, *chloroform* does not meet with so many supporters as its rivals, sulphuric ether and chloric ether, composed of chloroform one part, and alcohol nine parts. Dr. Hayward, of Boston, Professor Horner, of Philadelphia, and the Drs. Warren still employ *sulphuric ether*. The two last-mentioned practitioners, as well as others of note, have resorted to chloric ether of late, as more agreeable to the patient, from its not inducing headache, or leaving so disagreeable a smell behind as common ether. It gives rise more easily to nausea, however, than the latter does; and to this Dr. Warren attributes the more rapid disappearance of any unpleasant feelings it causes. While Dr. Warren discards chloroform entirely, other eminent persons speak warmly in its favour. Dr. Atlee has used it most extensively; but since the publication of accounts of death from its use, he has combined one part of it with two (by measure) of ether, and with the most satisfactory results. Dr. Gross prefers chloroform to all other means; and has administered it under every variety of age and circumstance, sometimes keeping up the effect for half an hour. He never met with but one case wherein he felt alarmed in consequence of the prolonged insensibility of the patient, who was remarkably susceptible of its influence. Professor Howard, of Ohio, regards chloroform as beyond all comparison the best anæsthetic. Professor Eve states that he has used ten pounds of it without having affected more than two patients in a greater degree than he wished; and several others speak in warm terms of it. At the Bellevue Hospital, New York, a mixture of one part by weight of chloroform and two of sulphuric ether has been long in use.

Question of operating for Cancer.—The removal of scirrhous breast seems to be regarded with as much disfavour by many able surgeons of the United States, as it is by the most experienced among ourselves. One of great eminence, Professor Dudley, of Lexington, says he has never removed the breast for this disease without finding it recur. He has been enabled to keep carcinomatous action in check for years by severe abstinence, but he has never seen it cured. Professor Eve, of Georgia, never operated in a case in which the diagnosis was unequivocal, without the disease returning. Dr. Rodgers, of New York, a surgeon in large practice, states, that when he commenced practice, he operated on almost every case, having been taught that, although the disease might return, its doing so was an exceptional occurrence. The result, however, was, that not two survived the operation in good health for two years, the greater number dying within twelve months, and many within six. Dr. Parkman, of the Massachusetts Hospital, observes, that while several distinguished surgeons decline operating on the ground of the certain return of the disease, or even the shortening of life by hastening its appearance elsewhere, others operate, although well aware that the cure is only exceptional; but maintaining that, as the disease rarely returns severely in the cicatrix, the patient is saved the loathsomeness arising from a foul external ulcer. The danger of the operation itself is not taken into account, and the discovery of anæsthetic agents renders it painless. In order, however, to determine in some measure the amount of immediate danger attendant upon the operation itself, Dr. Parkman has examined the records of the hospital from the period of its establishment, twenty-eight years since; and finds that there have been 88 operations for the removal of the breast, furnishing 6 deaths. In 4 of these there is reason to believe that such disease was already present in the internal organs, as would, by more careful means of examination, exclude surgical interference at the present day; one of the others died of pleurisy, and the other of erysipelas. The operation therefore, *per se*, is in nowise a dangerous one. In 1847, the year after the discovery of the anæsthetic power of ether, there were three times as many operations performed as in any preceding year.

The chairman of the Committee, Dr. Mussey, states, that of those cases of which he has been able to learn the sequel, he knows but of two in which the disease did not return in some part of the system within four years, and in most of them within one year. These two cases, from the small advance of the disease, were very favorable ones for the operation. One of the patients is still alive, eight years after, and the other was quite well ten years after. Sir Philip Crampton and Dr. Physick each mentioned a case to him. Sir Philip's was one of ulcerated breast, with indurated axillary glands, the patient being lost sight of after seven years, until which time she continued well. In Dr. Physick's case, likewise an example of malignant ulceration, the patient lived for nine years, when the disease reappeared in the cicatrix. Professor Gross observes, that all his earlier operations, save one case, having turned out unfavorably, he has of late years repeatedly declined interference. Considerable experience has convinced him, that patients will, on the average, live as long, or even longer, where no operation is performed; and that the disease, by strict attention to diet and the general health, may often be kept in abeyance for several years.

Dr. J. C. Warren communicated to the Committee several cases, in which he regarded the operation as having been completely successful. They all resided at Boston, and their histories were well known. Four of them were examples of scirrhous disease of the breast, and the patients lived for periods varying from twenty to thirty years afterwards. The other four were examples of cephalomatous or fungoid tumour occurring in the antrum, the parotid region, the groin, and the dura mater. None of these latter patients exceeded thirty years of age, and they are all, at the expiration of some years, doing well. It is to be observed, that the microscope had not, at the period of the occurrence of these cases, been brought to bear upon the diagnosis of morbid growths. A portion of the fungoid tumour of the antrum was, however, examined by Dr. Barnett, and declared to be encephaloid carcinoma. It is to be regretted that Dr. Warren has not furnished us with a statement of the proportion which these eight cases bore to the number of operations with which his immense practice must have furnished him.

The question can certainly not be as yet regarded as decided, and it should be carefully investigated; for it is evident that, unless the operation positively *hastens* the development of disease elsewhere, its removal from the breast, where it induces such distressing suffering, even with the certainty of its eventual reappearance, now that it can be accomplished without pain, would be a boon for the patient. We are almost disposed to believe, that some of our leading surgeons, discouraged by the unfavorable results of the somewhat too indiscriminate procedures of the early portion of their career, may be now going into the other extreme. May this practical protest against a too overweening confidence in surgical procedures, have its effect in checking the recklessness with which some of their younger brethren of this metropolis have of late undertaken operations of every description!

On the Employment of Bandages in Traumatic Aneurisms, Gunshot Wounds, and Fractures.—The Committee make some long extracts from some papers furnished to the 'Transylvania Journal' by Professor Dudley, an eminent surgeon, the extent of whose practice may be judged of by the fact of his having performed lithotomy 200 times. He states, that the most complete success attends his practice, in *brachial aneurism*, of applying a roller to the entire limb, with thick compresses over the aneurism and along the course of the artery to the axilla. An aneurism of the *anterior tibial* similarly treated is referred to. Cases of severe *gunshot wounds*, with great laceration of soft parts, or comminuted fracture, are cited as examples from among "hundreds, wherein the triumph of the bandage has been complete in preventing inflammation, sloughing, suppuration, &c., while in place of these the recuperative effort consisting in adhesive union has been complete." The bandages and compresses are so applied, immediately after the injury, as to maintain the parts in close contact without injurious constriction, inefficient and partial compression only maintaining hæmorrhagic excitement. The bandages are wetted for the first few days with spirit lotion. Splints and machinery are dispensed with, and the cure takes place rapidly. One end thus attained is the exclusion of the air; and the means employed bears some analogy to Chassaignac's plan of dressing wounds by "occlusion." (Brit. and For. Med.-Chir. Rev., Vol. V, p. 262.) For the treatment of *fractures* Dr. Dudley resorts to the same means; first thoroughly extending any muscles that may be in a state of preternatural contraction, and regulating the force with which the roller is applied, by the amount of cellular tissue, or of interstitial deposit. The muscles are rendered quite passive, and the pain incidental to isolated pressure is here unfelt. As the swelling subsides, the bandage has to be renewed:

"Being long convinced of the error propagated in the profession regarding the peculiarity of gunshot wounds, my solicitude in all cases which have been placed under my charge, has been, by the aid of properly adjusted pressure, to preserve the surfaces of the interior lacerated fibres pressed closely together; to close the cutaneous openings, and confer thereby all the advantages upon this wound, which are known to attend one from subcutaneous section; to prevent all tumefaction, inflammation, and its consequences by the skilful distribution of pressure above, below, as well as upon the seat of the injury; to apply the dressings with that very moderate force only which is necessary to secure these purposes, and which at the same time imposes unconditional quiet on all the muscles involved in, or associated with, the wounded part......... A bandage wet with spirit, and then well stretched in the rolling, if applied to a broken limb, in an extended state of the muscles, presents an effectual barrier to all muscular contraction, either voluntary or spasmodic, although the force used in its application may be only barely sufficient to render the pressure equable at all points, and to secure a smooth dressing." (pp. 349, 53.)

Among the cases referred to by this "hero of the bandage," as he is termed by the Committee, are examples of fractured patella (osseous union being obtained in three weeks!); fracture of the cervix femoris; and the painful disease of the terminal phalanx of the fingers, known as "bonefelon."

Reduction of old Dislocations of the Hip.—Dr. Crosby reports a case, in which he reduced one on the sixty-eighth day, chloroform being employed, and the traction maintained for four hours. Professor Atlee reduced one of *four months'* standing. After rendering the patient insensible, the reduction was accomplished in *one minute*. In both cases, Jarvis's adjuster was employed.

Chloroform in Traumatic Tetanus.—Two cases are related in which this was successful. One occurred in a lad, æt. 15, who had been wounded by a stick thrust into the perineum; and during the suppuration of the healing wound, tetanus occurred on the seventeenth day. Purgatives and morphia having failed to give relief, chloroform was resorted to, and inhaled whenever a paroxysm came on, with the effect of procuring a recovery in a case which, before its employment, seemed hopeless. The other case occurred in a man, æt. 29, who received a wound from an adze below the internal malleolus. Six weeks after the injury, violent spasms came on, and continued very severe for a week, during which there was opisthotonos as well as rigidity of the muscles of the abdomen and jaws. Morphia and alcohol were freely given, and sinapisms applied to the spine with only palliative results. Chloroform was next inhaled, and a mixture of it and alcohol applied to the wound. Relief was obtained in two or three days; and by repeating its inhalation whenever a paroxysm returned, a cure was accomplished in a fortnight.

Treatment of Nævus.—Dr. Driesbach applies *creosote* twice a day until an eschar forms, resuming the application after this has fallen off. In this way he says a cure may be always procured. If the creosote proves too irritating, he dilutes it with oil or water. Prof. Brainard has cured several cases by the mere application of *collodion*, applying a fresh portion when the crust falls off. He has seen nævi of considerable size cured by this plan; and he believes that even very large ones may, by perseverance, be rendered amenable to it.

Treatment of Nasal Polypus.—Dr. M'Ruer passes a piece of cat-gut from the nostrils to the mouth, to which is fastened a piece of soft and dry sponge, corresponding in size, when firmly compressed, to the narrowest parts of the nasal passage. This is then gently drawn forwards by the posterior fauces through the nose; and in the author's experience of ten cases, has brought away soft or gelatinous morbid growths very effectually.

Modification of the Operation of Laryngotomy.—Dr. Mussey relates the case of a boy, æt. 15, brought to him labouring under dyspnœa from the introduction of a burr of *xanthemum strumarium,* half-an-inch long, into the larnyx. The cricoid cartilage and the crico-thyroid membrane were divided; but attempts to introduce a small forceps brought on convulsive cough. A small silver director, bent at an obtuse angle, was passed upwards between the vocal chords, and drawn forwards, so that its grooved sides should rest firmly against the posterior surface of the anterior parts of the thyroid cartilage, which was then slit up almost to its upper edge—the patient being exhorted to avoid coughing or breathing the while. The finger was next passed in and the burr felt, and dug from its bed in the ventricle of Morgagni, and pushed up, through the chink of the glottis, upon the root of the tongue, and discharged by spitting.

Professor Pancoast on the Displacement of Hard Cataracts by Lateral Traction.—Dr. Pancoast has practised the following method in about 20 cases of hard cataract, without any permanent failure, little or no pain and inflammation usually resulting. After thoroughly dilating the iris for several days with belladonna, he passes his needle (a modification of that of Scarpa, reduced to the smallest possible size, and having an almost rectangular curve) into the sclerotica, at a distance nearly equal to the diameter of the lens behind the margin of the cornea. It is carried forward in the hyaloid humour in the usual manner, so as to break into the aqueous chamber between the edge of the lens and the ciliary body, without touching the iris or ciliary processes. After the division of the capsule, its hooked part is sunk into the centre of the lens, and this body is drawn horizontally backwards through the track of the needle to the place of puncture in the sclerotica. The handle is then to be depressed and the needle detached from the eye. The lens is thus prevented from lying in contact with the more delicate structures of the eye, and has no tendency to resume its old position. In the same way have several secondary capsular cataracts been displaced successfully. Occasionally where the lens has been large, this horizontal displacement has not been accomplished, and couching in the usual manner had to be performed; but in the cataracts of old persons the operation will be found easy and satisfactory.

Dressing of Burns and Scalds with White Paint.—Professor Gross proposes an addition to the thousand applications for burns, in the shape of *white paint,* which he has employed during nine years with great success, both in superficial and deep-seated burns and scalds. It should be applied by a pencil or small mop (evacuating the vesicles and drying the surface), and the whole covered with carded cotton, or old linen, and supported by a roller. In mild cases, one application, allowed to remain on four or five days, suffices. No ill effects have ever resulted from its free employment. In many cases it gives instant relief, like a charm.

Professor Brainard on Iodine Injections in Ascites and Spina Bifida.—Some cases are referred to in the Reports, in which iodine injections were employed in *ascites* without exciting inflammatory action. From his experience on this subject, Professor Brainard concludes:

"1. Medicinal substances may be injected into dropsical cavities without danger of inflammation, provided care be taken to have them sufficiently diluted in the fluid of the cavity. It requires further experiments to determine the classes of substances which may be thus used with most safety and benefit. 2. But slight inflammation is necessary to produce absorption of the fluid and transformation of the effusing surface. It is probable that any considerable change in the quantity of the fluid produces this result without inflammation. 3. Injections of iodine and hyd. potass., possess the power of rapidly and safely curing spina bifida. If it were not for other diseases and states of organs associated with it, and which are fatal, the defect under this mode of treatment might be considered a trifling one." (p. 371.)

Professor Pancoast on the Sub-cutaneous Division of recently Strangulated Hernia.—In cases of hernia, where the seat of stricture can be detected at the external ring, and where the surgeon is so well satisfied with the state of the contents of the sac, as to be willing to restore them by the taxis if that were possible, Dr. Pancoast considers that the sub-cutaneous division of the upper column of the external ring may be most advantageously substituted for the ordinary operation in which all the tunics are laid open. The operation was devised by M. Guerin; but as far as Dr. Pancoast knows, his own three cases are the only ones in which it has been practically carried out. In these its success was complete. The operation is applicable only to a small proportion of the cases which call for surgical interference, the seat of stricture being so commonly found at the neck of the sac; but in these cases it will be found of great value.

Professor Atlee on the Removal of Intra-Uterine Fibrous Tumours.—Professor Atlee's operations for the removal of intra-uterine fibrous tumours, are characterised in the report as among the most striking and extraordinary achievements of modern surgery. At present he only offers a synopsis of the four cases he has met with, intending to publish the details hereafter. "They refer to intra-uterine tumours, removed by the knife per *vias naturales,* although the *os tincæ* was closed upon them. This operation originated with me; at least, I am not aware that it has been performed by others." We must wait for the fuller account of these cases; but in the mean time we may state they occurred in persons respectively aged 45, 49, 30, and 36. The cervix uteri required incision for its due dilatation, the tumours were removed at several operations or sittings, and weighed from 4 to 10 lbs. each. All the women recovered except one, who died from a pneumonia acquired during convalescence.

Substitute for the Gaiter in the Maintenance of Extension in Fracture of the Lower Extremities.—Dr. Crosby recommends the following plan, which the Committee reports as acting admirably:

"Place two wide adhesive straps, one on either side of the leg, from the knee to the malleolar processes, and let the strips, without adhesive plaster, extend a foot below them; pass another strip around the leg above the calf, and one above the malleolar processes; then pass a roller from the toes to above the knee; then your loose strips below the limb may be fastened to the foot-board, and you may employ any amount of extension required without giving pain, except when the strips press hard on the malleoli, and this may be relieved by small pads placed under them, below the processes, so as to lift them off from these processes. The strips may be wide enough to envelope the entire leg above and about the calf." (p. 383.)

In the Appendix, Professor Evans of Chicago describes a modification of the forceps, which he terms an "Obstetrical Extractor," but of which we can convey no idea to our readers without the accompanying engravings; and even with their aid, have not been able ourselves to appreciate the advantages of the instruments.

We also find a short Paper by Dr. N. S. Davis, on the question,—*Has the Cerebellum any Special Connection with the Sexual Propensity or Function of Generation?*—Dr. Davis seems to have been fully satisfied by the evidence of Comparative Anatomy, and by the results of Leuret's observations on the size of the cerebellum in castrated animals, that the organ as a whole cannot be regarded as the instrument of the generative propensity; and he has applied himself to the inquiry, whether, as suggested by some physiologists, the central portion, with the vermiform processes, may be considered as specially connected with the sexual instinct, whilst the lateral lobes are concerned in the regulation of motion. To this end he has made a series of measurements of these parts in bulls and oxen, which, so far as they go, tend to disprove even this lingering remnant of the phrenological hypothesis, by showing that they are at least as large in the latter as in the former, if not actually larger. He does not mention, however, the number of cases from which his averages are drawn; so that we have no means of judging of their value. It does not appear to us that any measurements of this kind are likely to decide the question. There is certainly strong evidence, derived both from *à priori* considerations, and from pathological phenomena, that a centre of sexual sensations exists either in the medulla oblongata, where it is passing through the pons varolii, or in the pons itself, or in the central lobe of the cerebellum. What is the precise locality of this centre, we would not venture to affirm; but we would refer Dr. Davis and our readers to some remarks we made on this subject on a former occasion, (vol. V, p. 477.)

The present Volume also contains some interesting reports upon Hygiene, to which we may advert at another opportunity, in connection with other works we have received on that subject from the same source.

ART. IX.

Practical Remarks on the Treatment of Aneurism by Compression; with Plates of the Instruments hitherto employed in Dublin, and the recent Improvements by Elastic Pressure. By JOLLIFFE TUFNELL, M.R.I.A., Surgeon to the City of Dublin Hospital, &c. &c.—*Dublin*, 1851. 8vo, pp. 154.

MR. TUFNELL had under his care a man who had been three times the subject of Aneurism, twice in the popliteal and once in the femoral arteries. On each occasion compression seemed to effect the cure; and Mr. Tufnell in the work before us has given full details of this interesting case, appending a number of practical remarks on the mode of conducting the treatment of aneurism by compression, with a comparison between this method and the ligature, and answering the various objections which have been urged against the employment of the former.

We need not follow this case in detail further than to state that the first popliteal aneurism appeared in November 1847. On the seventh day after applying the compress to the femoral artery, all pulsation in the tumour had ceased, and the sac was solid on the eighth day. On the ninth, pressure was removed; and the patient was walking about perfectly well on the thirteenth day from the commencement of pressure. In November, 1848, a popliteal aneurism formed in the opposite leg. Pressure was applied, with some interruptions, from November 27th to January 11th, with the same good result as before. The collateral circulation in each limb was maintained by two large anastomosing vessels, one running over the centre of the tumour, the other on the inside of the knee. The man continued working as a sawyer until September, 1850, when an aneurismal tumour of the size of a Florence oil-flask, had formed in the thigh. On the 10th of September, pressure was applied over the pubis, and a ring tourniquet, with elastic pressure, on the middle third of the thigh; the hip apparatus of Dr. Carte on the pubis being occasionally changed for a seven pound weight laid upon the artery in the groin, the adaptation of the instruments being left to the patient himself. The consolidation of the aneurism was complete on the fourteenth day. Diminished pressure was kept up a few days longer as a measure of precaution, and then the man returned again to his work.

We need not follow Mr. Tufnell in his history of compression in the treatment of aneurism, since its introduction by Mr. Todd in 1820, and its revival by Dr. Hutton in 1842. No one is now disposed to deny the great service which Crampton, O'Ferrall, Bellingham, Cusack, and other Dublin surgeons, have rendered to practical surgery by their advocacy of this practice, and the successive improvements they have made in the instruments and in the mode of employing them. Mr. Tufnell disclaims for his Dublin brethren any wish to discard the ligature, but still wishes pressure to be preferred as the general rule in ordinary cases.—He says:

"You may ask me, then, what are the cases in which I recommend pressure, and what those, where I would resort to the knife? I will tell you.

"I *consider compression applicable* to every ordinary circumscribed aneurism in an extremity, where there is sufficient room for the application of the compressing medium at two different points above the tumour, premising, of course, that pressure on the trunk of the vessel completely controls pulsation in the sac, thus proving that no high bifurcation exists.

"*I do not advise* it in cases which are rapidly extending in size, or where they continue to do so after compression has been tried. These aneurisms have no distinct sac; and to afford any chance of saving the limb, the blood through the main channel *must be cut off, and at once*, by securing the vessel.

"*I do not advise or sanction it* in cases where the disease has been allowed to run on unchecked, where the limb has become œdematous and swollen, and the surface of the aneurism a dusky, yellowish red. In such a case, the vein is most probably engaged, and, if it be a popliteal aneurism, the knee-joint inflamed. Here, I believe, amputation is the only resource.

"Understand me, then: compression I advocate only in cases where the sac is entire, and where sufficient room exists for applying the pressure on two points of the artery above. At the same time, cases have so frequently occurred where the application of a single instrument has been sufficient for a speedy cure, (such, for instance, as one that I saw under the care of Dr. Hutton, where popliteal aneurism of a considerable size, was, in seven hours and a half, by means of a single instrument, constructed on Dr. Carte's plan, rendered completely solid,) that, although, for prudence-sake, *and as a general principle*, I advocate the employment of two points of pressure, yet I by no means hesitate to employ a single instrument, and give the patient every chance, prepared at the same time to use the ligature, if any necessity arise." (pp. 45-7.)

In preparatory and constitutional treatment, Mr. Tufnell follows Dr. Bellingham. In selecting the instrument to be employed, he discards all but a conical weight from six to ten pounds, padded, laid upon the artery at the groin, and retained there by the patient's hand; with the elastic apparatus of Dr. Carte. In this apparatus, Indian rubber takes the place of, or rather relieves, the pressure of the unyielding screw; an important improvement, which, however, may be easily accomplished in various ways by those who do not possess Dr. Carte's instrument.

The author advocates such an amount of pressure as stops pulsation in the aneurism to the *touch*, in which case the ear will still detect the flow of blood into the sac. He urges us to employ the minimum amount of pressure with which *complete* command over the circulation can be obtained, with the view of obtaining a more rapid cure than when a wave of blood is permitted to pass through the sac; although in irritable persons less pressure may be attempted, as aneurism is cured by a mere diminution of the current of blood through the tumour. Cases are also quoted, proving that when it has been necessary to suspend compression, a curative action still goes on. The temporary interruption to the current of blood appears to line the inside of the sac with a fibrinous deposit, which increases in thickness and completes the cure.

It is an important question to determine how far the employment of compression interferes with the subsequent application of the ligature, should it be required from the intolerance of pressure or increase of the aneurism. Of course a careless practitioner might so injure the artery, that it would be dangerous to apply a ligature upon the part he had compressed; but Mr. Tufnell quotes from cases where an opportunity has been afforded of examining the limb after death, proving that no injury whatever was inflicted upon either artery or vein, at the spots where compression was applied. He quotes from other cases, in which compression was given up and the ligature employed, the previous use of compression in no way affecting the operation in its results.

That compression is an *effectual* cure for aneurism, is proved by the fact that, during eight years, the ligature has only been used three times in Dublin, either in hospital or private practice, in two of these cases the aneurism being traumatic. During this period, compression has been employed in 39 cases. In 30 of them, a perfect and complete cure was the result. In 1, compression was discontinued, and the tumour did not increase in size. In 2, the ligature was used successfully. In 3, amputation was necessary, the patient surviving in each instance. Three patients died; 1 from erysipelas, and 2 from disease of the heart. It is quite clear, from the details of the cases in which amputation became necessary, that the ligature would not have lessened the necessity for removal of the limb; but, on the other hand, would have almost certainly induced gangrene, and thus lessened the probability of saving the patient's life. The death from erysipelas occurred during a prevalence of this disease in the hospital, galvano-puncture having been employed, and the patient himself having unduly increased the amount of pressure.

The average duration of treatment in these cases, was twenty-five days. The average of the eight most favorable cases was only twenty-eight hours. In one case, seven hours and a half only were required for total solidification of the contents of the sac. There can be no doubt, that, from the admission of a patient to the time he leaves the hospital, after the employment of the ligature, a longer average stay than twenty-five days takes place, and that a very speedy cure cannot be hoped for.

Statistical returns of the success of the ligature give the following results:

"In Dr. Crisp's work* are detailed the particulars of 188 cases, where the vessel was secured for popliteal or femoral aneurism. Of these—

Died from the effects of the operation	35
Recovered after suffering subsequent amputation . .	11
Recovered after sloughing of the sac	2
Recovered after mortification of the toes . . .	1
Recovered after sloughing of the integuments . . .	1
	50

So that more than the fourth of these cases either terminated fatally, or were maimed for the rest of life.

"Mr. Phillips's experience and researches are the next to be considered. They are thus quoted by Mr. Storks:†—'Mr. Phillips collected 171 cases of aneurism affecting the lower arteries, which were submitted to the Hunterian operation. Of these cases, 57 (or exactly 1 in 3) were unsuccessful, in which all the patients except 2 died, not of the disease, but of the operation. Amongst the successful cases, secondary hæmorrhage took place 15 times. 59 of these cases required ligature of the femoral artery, 39 of which were unsuccessful; thus giving a mortality of 2 in 3 in the artery most frequently subjected to the operation.'

"Mr. Norris‡ gives a fuller report, his table embracing 177 instances (155 of popliteal, and 22 of of femoral aneurism) where the operation was performed. He

* See Crisp on 'Diseases of the Blood-vessels,' page 235 *et seq.* 9.

† Lancet, May, 1846.

‡ American Journal of Medical Science, October, 1849.

gives the surgeon's name, the sex and age, situation of disease, its duration, periods when each operation was performed, when the ligature came away, (if fatal,) the date and cause of death, with reference as to where the particulars of each case are recorded. There is, then, no getting behind this collection, no stating, in general terms, that statistics are wrong, and cannot be relied on. If truth is spoken in the first published details, it is re-echoed in Mr. Norris's table.

"He gives, I say, 177 cases, of which—

Died from the effects of the operation	38
Recovered after subsequent amputation	6
Recovered after suppuration of the sac	10
Recovered after gangrene of the foot	2
	56

So that nearly 1 out of every 3 cases operated upon, either terminated fatally, or were, to a certain extent, maimed for the remainder of their lives." (pp. 148-50.)

Doubtless these returns contrast very unfavorably with the results of the treatment by compression, and will assist in convincing the profession, that compression must become the *general*, the ligature the *exceptional*, practice in the treatment of aneurism. This great improvement in the principles and practice of surgery we owe exclusively to our Irish brethren, who cannot be too highly commended for the ardour and ability with which they have laboured in the cause of science and humanity.—We need hardly add an opinion, that Mr. Tufnell's book is a good and useful one.

ART. X.

1. *On the Threatenings of Apoplexy and Paralysis, &c.; being the Croonian Lectures, delivered at the Royal College of Physicians, in March*, 1851. By MARSHALL HALL, M.D. F.R.S. &c.—*London*, 1851. 8vo, pp. 90.

2. *Lectures on Clinical Medicine.* By JOHN HUGHES BENNETT, M.D. F.R.S., Professor of the Institutes of Medicine and of Clinical Medicine in the University of Edinburgh. Lectures I to IV.—*Edinburgh*, 1850-51.

WE cannot flatter ourselves that the study of the morbid conditions of the Nervous System has advanced in anything like the ratio of the cognate subjects of its anatomy and physiology. Some advances there have undoubtedly been; but the difference between the relative progress of the physiology and pathology of the nervous centres is strikingly shown by the fact, that the descriptions of the diseases of the brain published a quarter of a century ago might almost serve as the text-books of to-day, while the publications of the same period on its anatomy and physiology are completely obsolete. Our knowledge of brain-diseases contrasts very unfavorably with that of pulmonary, cardiac, or renal affections; and errors both of diagnosis and of treatment are familiar to all who have had any experience in this most difficult branch of pathology. The causes of this comparative ignorance are obvious enough, and arise from the difficulty of referring symptoms to their physical causes, of ascertaining the physical and dynamic conditions of the brain during life, and of discovering structural changes after death. In this department of pathology, also, there has been an unusual proneness to speculation, and some writers even appear to have thought that the very obscurity of the subject licensed the play of their imaginations. Many statements have also been made on the authority of an insufficient number of observations, and have been too generally received. As we write, the examples occur to us of the erroneous presumptions of the necessary connection between rigidity of the limbs and softening, and between loss of the power of articulation and softening of the anterior lobes. Even at the present time, many believe that the doctrine of Bouillaud on this latter subject is correct, although there are almost as many cases on record of softening of the anterior lobes without loss of articulation as with it. We have no hesitation, indeed, in asserting that the proper duty of the cerebral pathologist is at present to collect facts with the utmost caution; and to admit no statements into the region of admitted truths, until they have been confirmed by numerous observers.

Among the recent physiological discoveries which have had the greatest influence on practical medicine, are those which have rendered illustrious, in the profession, the name of Marshall Hall. We have ever endeavoured to do justice to the genius and perseverance which have elucidated so vast an array of previously incomprehensible phenomena. There is no man whose teaching on this subject we should be more disposed to listen to with respect, and with the assurance that we should not listen in vain. It was, therefore, with no lack of disposition to receive Dr. Marshall Hall's new views, that we perused the little work embodying the Croonian Lectures for the present year; and it is with no little reluctance that we feel compelled to dissent, not from the truth of the facts contained in it, but from the immediate and universal application which Dr. Hall would give them. As we hope to explain in the sequel, Dr. Marshall Hall's opinions on the subjects discussed in the lectures appear to us of that class, which require more minute investigation than the most zealous single investigation can give them. They have been produced; they must now be proved:—they have been placed in the porch of the temple; they must pass through the ordeal before they can be admitted into the sanctuary.

Shortly and simply expressed, Dr. Hall's observations are as follows:—An essential distinction is to be drawn between cases of apoplexy and epilepsy which depend on organic lesion,* and those which arise from non-organic causes. In the latter case, no trace of disease can be found after death; and in this class are therefore included Abercrombie's "nervous apoplexy," many cases of epilepsy, and some of paralysis. In these non-organic cases, the original cause of the paroxysm is irritation, either mental or of gastric origin, which is transmitted by reflex action, or, as Dr. Hall now terms it, "diastaltically," to the muscles of the neck. By the contraction of these muscles, which is termed "trachelismus,"† the veins are compressed (a state termed "phlebismus"), and the return of blood from the head is of course impeded. The congestion thus resulting is the immediate cause of the fit, and the character of the fit is presumed to depend on the various conditions and sites of the congestion.

* Both centric and eccentric, we presume.

† From τραχηλον, the neck.

Thus, if the congestion is from compression of the jugular veins, and is, therefore, cerebral and uniform, apoplexy results; if it be greater on one hemisphere than on the other, hemiplegic paralysis occurs; if the compression be of the vertebral veins, the medulla oblongata is especially affected, and epilepsy is produced; if the venous circulation of the medulla oblongata is not merely impeded, but is irregular, pallor, sickness, and at last "spinal syncope" may occur, which last state must be carefully distinguished from syncope from exhaustion. On the congestion thus resulting, organic changes may supervene, particularly softening.

As the cause of the congestion is simply spasm of the cervical muscles ("trachelismus"), the disease is cured for the time by relaxing the spasm; in the case of gastric irritation, this can be done by an emetic. The common expression, "determination of blood to the head," is declared to be erroneous, and all such cases are held to be, in reality, instances of "impeded venous return."

Let us now take up Dr. Marshall Hall's proofs of those statements. We require him to show—

1. That such contraction of the cervical muscles can press upon the great veins.
2. That such contraction is present in the paroxysmal cases he refers to.
3. That the resulting congestion can produce the effects ascribed to it, viz., apoplexy when one set of veins are implicated, epilepsy when another.

There is another inquiry which ought logically to be fundamental to all the rest, viz., as to whether, in these paroxysmal cases, organic changes are really absent, or have been merely overlooked.

1. The following is the main argument brought forward to support the first proposition:

"For the following most valuable fact I am indebted to J. Russell Reynolds, Esq., of University College, a gentleman of great talent and promise.

"A girl, nineteen years of age, was admitted into University College Hospital for aphonia, and amongst other things in the treatment she was ordered to have galvanism applied to the larynx daily, by the electro-magnetic machine. While using this machine, I observed the effect upon the muscles of the neck, and remarked that when the wheel was turned slowly, and the superficial muscles were alternately contracted and relaxed, *the colour of the face was heightened*, and of a florid hue, and no unpleasant feelings (further than those arising from the shocks) were experienced; but when the wheel was turned rapidly, with a less powerful current, and the muscles were maintained, during the rapidly intermitting action, in a state of almost permanent contraction, the face became of a deeper colour, the lips and angles of the mouth livid, the eyes suffused, and some feelings of confusion of thought, headache, and dimness of sight, alternating with flashing of light, were induced. The latter effects remained after the cessation of the current for a few minutes, and then disappeared." (pp. 25-6.)

These facts present, says Dr. Hall, "the *demonstration* of the nature both of trachelismus and of phlebismus, and of their *effects*." (p. 26.)

The same gentleman (Mr. Reynolds) subsequently details another case bearing on this point:

"I have been watching with great interest, during the last five days, a case of epilepsy in University College Hospital. The patient, a stout woman aged twenty-six, was brought in early on the morning of June 1st, in a fit. She had several attacks before I saw her, which was about half-past ten, a.m. She was then lying very restlessly, her face a little flushed, and some convulsive twitches playing around the mouth. I placed my finger on the omo-hyoid muscle, which I could at times see distinctly in the posterior triangle of the neck. It contracted and relaxed several times under my finger; then some of the surrounding muscles were strongly contracted, and a general but not severe convulsion followed. There was *total loss of consciousness*, but *not* any great *turgesence of the external veins*. Two days after this I was again watching her. She had had several severe attacks in the night, and there were now the same convulsive twitches of the muscles of the lower part of the face. I placed my finger in the direction of the omo-hyoid muscle, but could not distinguish it. As I was doing this, the *platysma myoides* contracted violently; its fasciculi stood out in full relief; it was exceedingly rigid; the veins of the neck became much distended; the face deeply livid; the surrounding muscles of the neck were *then* strongly contracted, the thorax was drawn towards the head, and the general convulsion which followed was one of the most violent I have ever seen." (pp. 38-9.)

A case is also cited from Mr. Bryant, of Bathurst Street, of epilepsy, followed, as it sometimes is, by transient hemiplegia:

"Having occasion to apply a mustard plaster to the nape of the neck, I was struck by observing the peculiar manner in which the skin of the neck was drawn, as it were, into a band. *I could distinctly observe this band arresting the flow of blood through the external jugular vein, which, with the veins of the face of the left side, was turgid*, and in one part dilated into a *varix*. To ascertain whether this band was influencing the circulation, I raised it up, and immediately the veins emptied themselves, and the patient was able, for the first time, to reply to a question from me." (p. 40.)

Mr. Bryant goes on to say that in a subsequent attack he "found the same band of skin raised by the contraction of the platysma; and it was now that he was able at once to arrest the phenomena of convulsive action by raising the band. The paroxysm ceased almost immediately." (p. 41.)

These are all the facts we have been able to find, which are brought forward in proof that contraction of the muscles can press on the great veins; and it is evident at once that, admitting all they imply without reservation, they are not sufficient to prove the doctrine of trachelismus. The utmost they show is, that the external jugular may be compressed; they prove nothing as to the internal jugular and the vertebral veins. Now, the great point is to show that the internal jugular, lying deeply in its sheath with the artery, and the vertebral vein, protected by bone and by strong fascia, can be compressed. We confess to a strong doubt whether such compression be possible; but, whether it is or not, it is evident that it has not been proved by Dr. Hall's facts, unless the slight symptoms of confusion of thought, &c., mentioned in Mr. Reynolds's first case, can be held to be sufficient evidence. The external jugulars are very easily affected in various ways. In many persons, their alterations in slightly accelerated breathing can be observed at once in the recumbent position; muscular contraction can also doubtless affect them to a certain point; but, in estimating the effect of this, it is absolutely essential to eliminate from the experiment the simultaneous influence of the respiration.

But it appears to us that we are not required to enter fully into the subject, until the advocates for this doctrine of trachelismus and phlebismus have brought forward more arguments in support of their case.

2. We do not find a much greater amount of evidence which can be

brought to bear on the second proposition, viz., that such contraction, admitting its existence, is present in the paroxysmal cases of apoplexy and epilepsy. Mr. Reynolds's second case, and Mr. Bryant's case, afford the only arguments which are derived from actual observation of such cases; and in both it might be questioned how far they support the inferences drawn from them. As an additional argument, Dr. Hall states, that the trachelismus "must be inferred from the turgescence of the head and face." (p. 15.) And thus having previously called upon muscular contraction to cause turgescence, he cites turgescence as a proof of muscular contraction.

In such a case as this, nothing can supply the place of actual observation. The question is not whether the cervical veins are compressed by muscles, but whether this occurs at the outset of paroxysmal apoplexy and epilepsy. Dr. Marshall Hall cannot in any way prove his position, except by bringing forward a number of cases, in which, as the very first step of the diseases in question, contraction of the cervical muscles occurs. Admitting that in the two cases given above this was the first step (which, however, we might question), two cases are insufficient to prove anything.

From positive observation we have reason to think, that trachelismus and phlebismus are not the first symptoms of epilepsy;* and that when lividity of the face *is* the first symptom, it may be dependent on other causes than those included under these two terms; but we are not called upon to cite objections to the views brought forward, but simply to examine into the validity of the affirmative evidence which supports them.

3. The evidence for the third proposition is not more perfect than for the former two. That a certain amount of obstruction to the return of venous blood from the head, such as is caused by hanging, by tying veins, or by other kinds of mechanical pressure, may produce apoplexy, is admitted on all hands. Yet Dr. Marshall Hall's arguments are directed to prove this, which nobody doubts, and not to the main point, which is whether the congestion (which for the sake of the argument, we allow, is produced by trachelismus) consequent on the muscular contraction, can suffice to produce epilepsy and apoplexy.

We have not been able to find the shadow of an argument for the idea that epilepsy results from compression of the vertebral vein, and, indeed, Dr. Marshall Hall, although in one place (p. 64) he speaks with much confidence on this point, and in another (p. 16) refers to it as simply a case for inquiry. As to the other part of the question, it must be entirely a matter of observation, whether an arrested flow of blood produced in this way, can cause apoplexy. Dr. Hall's arguments on this point may be thrown into a syllogism, and will then stand thus:

1. Mechanical obstruction to venous return, carried to a certain point, causes cerebral congestion and apoplexy.
2. Trachelismus produces mechanical obstruction to venous return;—
3. Therefore trachelismus produces apoplexy.

The middle term employed is ambiguous, as the mechanical obstruction of the major and minor premises are not identical. Dr. Hall assumes, indeed, that the obstruction produced by trachelismus equals that produced

* As to paroxysmal apoplexy, we know nothing of its early stages, and Dr. Hall advances no case in point.

by mechanical obstruction from ligature or from tumour; but we cannot find any proof of this.

In these observations we have guarded ourselves against expressing any decided opinion upon this doctrine of "trachelismus." That it cannot be admitted on the ground brought forward by Dr. Hall, is evident; but it does not follow that it may not be really true. We certainly have strong doubts upon this point; but as the case for the prosecution has broken down, it is not necessary for us to call our witnesses into court. When Dr. Hall has pursued the subject further, we shall be prepared again to consider it; and we need not assure Dr. Hall, that while it has given us great regret to be obliged to differ from him, on this occasion, and to speak in doubtful terms of what he considers a great step in advance, it will afford us much pleasure to move out of our neutrality and to join his camp, when his facts have been brought together in greater number and in more regular order.

We have before alluded to a very interesting subject, which we regret that Dr. Hall has not discussed at greater length, viz., the absence of structural lesions in some cases of apoplexy and paralysis, and in many of epilepsy. That cases not unfrequently occur in which the naked eye detects no organic lesion, is familiar to every one, but the belief has been gradually gaining ground, that these cases are not so numerous as they were once supposed to be; and that even in those which appear to the most careful investigation to present no structural change, improved means of examination may hereafter detect them. It has been surmised, with great probability, that many of the cases formerly called "simple or nervous apoplexy," were in reality cases of fatal syncope from various causes, particularly from fatty heart, attended or not with rupture. Admitting this, however, and admitting that cases of softening and real disintegration of brain-substance are often overlooked, there yet remain a certain number of cases in which experience seems to warrant the assertion, that no disease of texture can be found at all commensurate with the fatality of the symptoms. What, then, is the condition of the brain in these cases? Is it absolutely healthy? This is very unlikely. Is there, or has there been, congestion, the traces of which have disappeared? Is there some molecular alteration of particles,—a change in chemical constitution which, at present, we are not competent to discover? The time, perhaps, has not yet come for answering satisfactorily these questions.

In his Fourth Lecture Dr. Bennett has a very interesting chapter on the Pathology of Cerebral and Spinal Softenings, which bears on this subject. He distinguishes only two kinds of softening,—inflammatory and non-inflammatory, the last being dependent on cadaveric or mechanical causes, and therefore not pathological in the proper sense of the term. When softening is inflammatory, the microscopic element always contain numerous granules and granular corpuscles, mixed with broken-up normal structures. In non-inflammatory softening, on the other hand, there are no granular corpuscles, but the nerve-tubes have simply lost their natural firmness, are easily separable, and are broken up into fragments.

Dr. Bennett rejects as hypothetical the opinion that there is a softening of the brain, *sui generis*, due to diminution of nutrition, to gangrene, or obstruction of arteries. In all the cases he has collected of inflammatory softening, well-marked symptoms existed; in the cases of non-inflammatory softening there were no symptoms. Dr. Bennett also states, that inflammatory softening often exists when it can only be detected by finding with the microscope granular corpuscles mixed up with the brain-substance; and he cites an interesting case, in which a very able physician believed that a corpus striatum was softened, when, microscopically, it was found normal; and that the pons varolii was healthy, when in reality it had undergone inflammatory softening, as proved by finding granular corpuscles among its tissues.

On this subject the following remarks are made:

"All practical men agree in considering it a matter of extreme difficulty to reconcile, with any certainty, the morbid appearances found in the brain with the symptoms previously observed. The future microscopic examination of the softening may serve to prevent much of the error that has hitherto been committed. For instance, softening of the fornix, septum lucidum, and central parts of the brain, may exist in two cases. To the naked eye they may be in every respect identical; and yet the microscope enables us to determine that the one contains granular corpuscles, while in the other not one of these bodies is to be found. It becomes evident, then, that previous to this distinction having been made, two distinct lesions were confounded together; and that a different train of symptoms should, under such circumstances, be occasioned, is only to be expected. Again, it has frequently excited surprise, that notwithstanding the existence of well-marked symptoms of softening, nothing was to be discovered after death. Now I have demonstrated in several instances, that although to the naked sight no morbid lesion was apparent, still portions of brain might contain the same granular corpuscles as are to be seen in more apparent lesions, and that by considering such parts diseased, all the symptoms might be explained according to the pathological laws I have previously explained. By excluding these sources of error, therefore, and by being able at once to distinguish the lesion dependent on inflammation from others which simulate it, we shall be enabled to obtain more exact data for future investigations. From the observations recorded, however, the two following propositions may, I think, be established.

"1st. *That pathologists have hitherto confounded softening dependent on inflammation with softening occasioned by post-mortem changes or mechanical violence.* 2d. *That notwithstanding the most anxious search, and the existence during life of the most decided symptoms of softening, inflammation, though really present, has escaped observation.*" (pp. 181-2.)

If we adopt these views, we do not see how we can avoid coming to the conclusion, that no brain can be pronounced healthy from examination with the naked eye alone; and that all the alleged cases of so-called nervous and simple apoplexy, *may have been* caused by undetected inflammatory softening. It does not follow that there is no such disease as apoplexy without structural lesion, only that, according to Dr. Bennett, it must be demonstrated by renewed observations. It cannot be doubted, however, that, as before remarked, many brains and spinal cords have been examined by good microscopists, and yet nothing has been detected.

Dr. Bennett discusses, at some length, the question first raised by Monro secundus, and subsequently examined by Kellie, as to whether the total amount of fluid within the cranium is liable to variation. That the *relative* amounts of different fluids, as of blood and cerebro-spinal fluid, can and do undergo continual changes, must, of course, be admitted. Dr. Bennett's arguments in favour of Kellie's views are well put forth;

but as they are founded chiefly on the late Dr. John Reid's review of Dr. Burrows's book, and as this review is included in his 'Physiological, Anatomical, and Pathological Researches,' we need not further allude to them. We need only remark, that Dr. Reid's able review should convince any one that the doctrine of Kellie is in the main correct. If the quantity of blood in the cranium be increased, the cerebro-spinal fluid must be diminished; and if the cerebro-spinal fluid be unaltered in quantity, congestion of one portion of the brain must be attended by comparative bloodlessness of another. The only other possible variations in the circulation within the cranium, are altered rapidity of transit, and changed relative proportions of arterial and venous blood. One important practical point resulting from this doctrine is, that in every case of partial congestion of the brain, another disease is present, viz., anæmia of another portion, and the symptoms of this latter condition are mixed up with those of the more formidable lesion; some of the difficulties in the diagnosis of cerebral diseases are, perhaps, owing to this combination.

Dr. Bennett details several very instructive cases of cerebral and spinal disease. The last case is very interesting. A woman, æt. 35, had partial amaurosis, spectral illusions, and perversions of hearing, smell, and touch. The sense of taste is not mentioned, and therefore was, we presume, unaltered. The perversion of smell was very marked, strong snuff having the odour of tea. The most interesting point about the case was, however, the facility with which the patient was influenced by suggestive ideas, like the person under the influence of the state of mind produced by "electro-biology." A piece of cold or hot metal felt hot or cold to her, according as the impression was given that it was hot or cold. There was no evidence of any organic disease; and pressure over the fourth and fifth dorsal vertebræ caused acute pain, as usual in such semi-hysterical cases. In this instance all the symptoms were "functional," that is to say, no organic disease would have been detected had the patient died.

It appears impossible to give a scientific formula (if we may use the expression) for such cases; but we have no doubt that Dr. Bennett is right in supposing that the dynamic, or, possibly, physical changes, giving rise to the perversion of sense in this case, were similar to those produced by electro-biology, although we do not know exactly what these are. In the present instance this mental condition was connected with uterine disease, profuse menstruation, leucorrhœa, and tumefied os uteri. Dr. Bennett does not allude to any such cause; but we have seen similar cases, less strongly marked, however, apparently produced by masturbation.

Dr. Bennett discusses, at some length, the curious subject of Electro-Biology, which has recently agitated the philosophers of the modern Athens, and which has now been appropriated by the mesmerists, although it should be kept quite distinct from that chaos of nonsense with which certain modern works on Mesmeric and Od Forces overwhelm us. Dr. Bennett calls it a "certain condition of the cerebral functions, in which individuals of sound mind are liable to be temporarily influenced by predominant ideas." This chapter has, however, been published separately; and we have elsewhere alluded to it in the present number.

The remaining Lectures which have been published are on Simple, Cancerous, and Tubercular Exudations; on Cutaneous Diseases; on

Auscultation and Percussion; and on the Microscope as a means of Diagnosis. As the series is apparently to be continued, we shall defer the consideration of these topics; and shall conclude with the remark, that these Lectures are likely to prove useful in no common degree, from their practical tendencies, and from the brevity and simplicity of their style.

ART. XI.

1. *Essays and Notes on the Physiology and Diseases of Women, and on Practical Midwifery.* By JOHN ROBERTON.—*London*, 1851. 8vo, pp. 530.

2. *On the Preservation of the Health of Women at the Critical Periods of Life.* By E. J. TILT, M.D.—*London*, 1851. Fcap. 8vo, pp. 142.

THE two works before us,—the one, a goodly octavo volume, of sober professional aspect; the other, a little red book, adapted to catch the public eye,—are scarcely more unlike in size and form, than they are in the character and merits of their contents.

Mr. Roberton has long been known to the profession as a sound thinker, and as an able and industrious worker. Of his capacity for philosophical investigation, he early gave evidence in his 'Essays on Life and Mind;' and to his zeal in collecting facts, and his discrimination in reasoning upon them, his successive papers on the 'Epoch of Female Puberty in different Climates,' which elicited the high commendation of Dr. Prichard, bear ample testimony. These papers, brought together in a systematic form, with some additional Essays, constitute the First Part of Mr. Roberton's volume; and in reference to this he says, in his Preface—"The investigation is a novel one, or, rather, was so when I entered upon it upwards of twenty years ago; and, though I have spared neither labour nor expense in promoting it, much still remains to be done to render it complete." The Second Part is made up of a set of detached Essays or Notes on various practical subjects, the materials of which were chiefly furnished by the obstetric experience enjoyed by Mr. Roberton during his connection with the Manchester and Salford Lying-in Hospital; and of this he remarks, with the modesty of true merit, "considering the opportunities so long enjoyed for the cultivation of professional knowledge, I may well feel humbled that I have accomplished so little." It is obvious throughout the work that the writer has no ulterior view in its publication, save the honest desire to advance professional knowledge; and those who are acquainted with the author, and with the position he has long held in the city of his adoption, would be the last to suspect him of any other. The volume is inscribed to Professor Alison, in a manner not less creditable to the writer than to the object of the dedication; which we quote, alike for its own sake, as with the purpose of enabling our readers to compare with it a certain other dedication which we shall presently bring under their notice:

"MY DEAR SIR,—I inscribe this volume to you as an expression of unfeigned admiration of your character as a physician, a philanthropist, and a patriot. In you the profession has long had the noble example of one who, with the successful cultivation of medicine, has combined the most zealous and enlightened efforts to improve the condition of the neglected poor: their friend in sickness; the advocate of their rights; and, for their sake, an expounder (and the ablest) of those great principles of political science, which, by securing, universally, a legal provision for the relief of the destitute, promote the happiness and stability of nations. That your useful and honoured life may long be spared to mankind, is the sincere wish of—

"My dear Sir, your faithful friend,

"THE AUTHOR."

Our readers will recollect that we not long since reviewed a work by Dr. Tilt, 'On Diseases of Menstruation and Ovarian Inflammation;' and that, whilst we criticised some of the writer's doctrines, and noticed certain faults in his exposition of them—more especially his propensity to "fine writing,"—we gave him ample credit for both the desire and the ability to improve the knowledge of the profession in regard to the difficult topic which he had selected as his "speciality," and counselled him, "as his cordial well-wishers," "not to be in too great a hurry to publish again, but to wait until he has acquired for himself a larger experience in the particular investigations he has undertaken." No one, we think, who has read the review in question (to which Dr. Tilt himself refers, in his present treatise, as "courteous," though "severe,") will accuse us of any desire to lower Dr. Tilt in the estimation of the profession or the public; on the contrary, regarding him as a zealous and not unworthy aspirant for professional distinction, we gladly held out to him a helping hand, only hinting that he must not expect at once to attain that position, which it has cost others many years of laborious and well-directed exertion to acquire. It would seem, however, either as if practice did not come in fast enough, so that the public as well as the profession must be informed that Dr. Tilt is a great authority on the "Health of Women;" or that Dr. Tilt is troubled with a *cacoethes scribendi,* which will not allow him to remain quiet; for look into what medical periodical we may during the last twelve months, we are almost sure to see Dr. Tilt's name as a writer or speaker on some subject connected with Menstruation; and, not content with thus keeping himself continually under notice, he has now produced what he calls an "opuscule," embodying certain of these ephemeral contributions, in a guise which (as we have already remarked) is anything but becoming, and in a manner which we feel called upon to criticise severely, as not merely offensive to good taste, but as injurious to the credit of the profession. It will be apparent, in fact, to any one who turns over Dr. Tilt's pages, that he is continually in a state of vibration between the *medical* and the *general* public, his real aim being to impress the latter, whilst he ostensibly addresses himself to the former. The alternation, indeed, is very amusing.

Thus, whilst the general appearance of the book, with "Health of Women—Tilt" upon the back, may serve to relieve maternal solicitude by the promise of wholesome and intelligible advice, the Title-page is altogether professional in its aspect, bearing the impress (if we mistake not) of our worthy publisher's good taste. Turning the leaf, however, we come to the Dedication, about which there can be "no mistake."

"To whom (says Dr. Tilt) can I so appropriately inscribe a little work on the Preservation of the Health of Woman, as to her whose health is to me dearer than all earthly blessings? Still, I have an additional motive for doing so, in the pleasure of acknowledging, that to her sound judgment and maternal solicitude, I owe much of the wholesome advice offered in its pages relative to the education of the female sex.

"Accept then, my dear Wife, the dedication of this little volume, as a token of that regard, which, plighted early in life, has stood the test of years, and is fearless of diminution."

Now, if this be, as it professes to be, a strictly *medical* treatise, we can only say that Dr. Tilt's ideas of appropriateness and our own are very much at variance; and we hope and believe that our readers will be on our side. But it is clear enough, from what follows, that this dedication has a purpose; which is neither more nor less than to make it known to "mothers of families" that the book is intended for their perusal, for what is "proper" to be dedicated to one lady, must surely be "proper" for other ladies to read.

Passing on to the Table of Contents, we find, on the whole, a predominance of the medical aspect, the headings of the introduction indicating, 1st, that girls are badly managed; 2d, that this bad management of girls is chiefly due to the ignorance of mothers; and 3d, that this is an evil only to be remedied by medical influence; whilst at the end of each chapter we find an "Appeal to the Profession" to back its contents. Still, even here, the public taste is consulted, "What to avoid and what to do, in order to promote the monthly flow," being an obvious imitation of the too-well-known "What to eat, drink, and avoid" of the advertising quack. We can make nothing whatever of the following: "Sedatives and oscillations of medical practice: their utility proved by the natural history of menstruation." How the natural history of menstruation can prove the utility of oscillations of medical practice, with or without sedatives, is beyond our wisdom to discover.

At the commencement of the Preface we have the following grandiloquent announcement, which seems intended alike for both classes of readers:

"Those who have been engaged, for many years, in any important inquiry, will remember the feelings of despondency with which, at times, they view a vast amount of accumulated materials; for it seems as if no human power could infuse life into such a chaotic heap of notes and cases, memoranda and extracts, on papers of all sizes and colours, and in writing difficult sometimes for even the writer to decipher. They must also have felt the fruitlessness of the attempt to work up, at first, all their precious materials to the level of conceived perfection, and have therefore been induced, sometimes to preface the appearance of a greater work by one of smaller pretensions—a statue rough hewn, to be afterwards perfected." (p. xi.)

We learn from the ensuing paragraph, that the *opus magnum* which we are to expect from Dr. Tilt at some future time is a complete "*Natural History* of the functions peculiar to Women;" but we can only say, that, if the present *opuscule* is to be considered as a fair sample of it, we do not care, for the sake of the profession, how long its appearance may be delayed, although it may prove mightily edifying to the general public. The chief part of the Preface is obviously addressed to the *medical* reader; but towards the end, the *maternal* destination of the little book is again disclosed. "If it were worthwhile marking," says Dr. Tilt, "the *punctum saliens* of so unassuming an undertaking, I might say that, more than once, having been asked by mothers for some advice relative to the education of their daughters, in a medical point of view, I was compelled carefully to set down my views." The profession, however, is finally addressed; and the "flattering notice" bestowed by it on the author's previous papers is advanced as a reason for his present publication, of whose merits he seems to have a most comfortable appreciation. "To me," he informs us, "it is a spring-board (!) for the acquirement of greater knowledge; and to those of the profession who have time and disposition for meditation, I think it will be *eminently suggestive*. What, however, is of more importance to us all, is the fact of its being *stamped, from beginning to end, with practical utility*." One would think that Dr. Tilt had been writing a review of his book, instead of a preface to it. At the conclusion of his preface he tells us, that he has "placed the most valuable points of medical advice as aphorisms, *in order more forcibly to strike the attention of medical men*, to facilitate their retention, and thus to increase the chance of their being diffused to those placed within the sphere of their professional influence." Who would suppose from this, that the aphorisms are all addressed to "mothers of families?" Yet so it is, as will presently appear.

We shall have more to say, hereafter, touching the treatise itself; but having commented on its opening, let us now turn to its closing passages. The "Conclusion" is worthy of the Preface:

"A feeling of anxiety and a sensation of painful suspense generally comes over us on approaching the termination of any serious undertaking. The intimate acquaintance with the many failings of our own work—the fear lest the last step should not even be equal to the preceding ones—hovers over us all, and makes us understand the meaning of the Chinese proverb, "When ten steps are to be made, the ninth is half the distance." In this instance, I know not why I should fear to take the tenth, the last, the irretrievable step; for my works have been courteously received by the profession; and although this is but a little book, I know from personal experience that it contains a sufficient amount of practical matter, to prevent my publishing it being deemed entirely useless.

"Gathering honey from many flowers, the humble bee at last collects its treasures into some common cell, where they may be made serviceable for the public good. Imitating its example, I have, as I proceeded, gathered from every chapter the honey of good advice, which I now accumulate into one general store, trusting that many will appropriate and impart it to those placed within their sphere of professional influence." (pp. 140-1.)

And what may be the treasures so daintily culled by our "little busy bee," who has thus been so kind as to "gather honey all the day, from every opening flower," in the most disinterested manner, for the sole benefit of womankind? Our readers shall have the full benefit of them, in the style employed by Dr. Tilt himself to set them forth:

"I. TO KEEP A GIRL IN THE NURSERY AS LONG AS POSSIBLE.

II. NOT TO LET HER BE TAKEN UNAWARES.

III. NEVER TO SEEK TO DETERMINE FIRST MENSTRUATION BY FORCING MEDICINES.

IV. NEVER TO GIVE OR TAKE FORCING MEDICINES TO PROMOTE THE REAPPEARANCE OR THE INCREASE OF THE MENSTRUAL FLOW.

V. NEVER TO GIVE OR TAKE PURGATIVES DURING THE WEEK BEFORE AND AFTER MENSTRUATION, UNLESS BY MEDICAL ADVICE.

VI. NEVER TO ALLOW A DAUGHTER'S CONSTITUTION TO BE UNDERMINED BY PROFUSE OR VERY PAINFUL MENSTRUATION, OR BY PERMITTING IT TO RETURN AT SHORTER PERIODS THAN ONCE A MONTH.

VII. NEVER TO BE SO RECKLESS OF HER OWN, OR HER DAUGHTER'S, HEALTH, OR SO UNJUST TO HER MEDICAL ADVISER, AS TO LET HIM PRESCRIBE IN IGNORANCE OF HER STATE.

VIII. A RESPECT FOR WHATEVER MAY HAVE RECEIVED LIFE.

IX. A KNOWLEDGE OF THE POSSIBILITY OF MENSTRUATION CONTINUING DURING PREGNANCY, AND OF THE DANGER OF CONFOUNDING THIS WITH OTHER SANGUINEOUS UTERINE DISCHARGES.

X. A KNOWLEDGE OF THE POSSIBILITY OF MENSTRUATION CONTINUING DURING LACTATION, WITHOUT IN ANY WAY FORBIDDING IT.

XI. A KNOWLEDGE OF THE REALITY OF THE DANGERS BY WHICH THE CHANGE OF LIFE IS ATTENDED.

XII. A CONVICTION THAT THESE DANGERS CAN FOR THE MOST PART BE AVOIDED BY A JUDICIOUS LINE OF CONDUCT." (pp. 141-2.)

Now, if these aphorisms are intended for the professional reader, why address them to the maternal? If they are designed for the maternal, why give the book a professional aspect? We have a particular dislike for books on special departments of hygiène, which, when put forth (as they almost always are) by men practising the corresponding speciality of medicine or surgery, must be looked upon as "baits for patients." But considering how much ignorance prevails among even well-educated women, upon topics which cannot with propriety be introduced into a general treatise on the Art of Preserving Health, we think that much might be said in favour of a treatise on Female Hygiène, avowedly addressed to the sex in general; and had Dr. Tilt honestly stated such to be his object, we should have found no fault with him on this score, whatever critical objections we might have thought it our duty to make, either to his doctrines or to the manner of their enunciation.

Before putting aside Dr. Tilt's *opuscule,* to return to the much more agreeable task of directing the attention of our readers to the merits of Mr. Roberton's 'Essays,' we must notice a few of the more prominent faults of style and manner, by which the former is distinguished. In Dr. Tilt's previous work, these might be leniently passed over, in consideration of the real value of his matter; but the proportion is precisely reversed in the present instance, the book being charged with offensive absurdities, and the amount of matter worthy of notice being so small, that it can only be found after an attentive examination. A few specimens, taken from different parts of the book, will suffice for our purpose.

The function of menstruation seems, with Dr. Tilt, to be the "be all and end-all" of woman; and he affirms (p. 18) that *the proper management of it is the only sure foundation for her health.* We have been accustomed, on the other hand, to think that "the only sure foundation of health" lies in the "proper management" of *all* the functions, corporeal and mental; and that in proportion as this can be accomplished, menstruation may be left to take care of itself. For several pages, Dr. Tilt harps upon what seems a very favorite topic,—the ignorance of mothers, and the supineness of their doctors; and at last, after sundry less pointed inuendoes, he commits himself to the following very grave charge against his professional brethren:

"If at puberty, or during the persistence of menstruation, the diseases of this function are to be principally attributed to the ignorance or the prejudices of women, it seems to me, that the ailments and tedious infirmities to which they are liable, for a few years previous to and after the cessation of menstruation, are in a great measure owing to the indolence of the profession.

"We know that such diseases are not, generally speaking, fatal, and that time may work their cure; and, instead of seriously setting about the study of their pathology, we confine ourselves to giving some pill, or if too much pressed, to the prescribing of a *placebo;* whereas by a judicious combination of sanitary and therapeutical measures, this last critical epoch can be deprived of most of its tedious attendant infirmities." (pp. 21-2.)

This, be it remembered, occurs in a book which we have shown to be especially directed by its author to the maternal public; and it would not, of course, have its due effect, if it were not followed by a very decided hint of the author's peculiar qualifications to afford good advice to such as will seek it, fee in hand. "*Having given particular attention to this subject,*" he continues, "I may, perhaps, be permitted to observe, that it has seldom received from other authors the consideration it deserves; and if I have not been able to dispel all the darkness which clouds the subject, I have at least partially done so, and sown seeds which may be better fruit in other hands."—It happens that we have before us a Boston (N.E.) newspaper, in which one of the first things that struck our eye was an advertisement, of which the following is the commencement:

"Dr. F. Gay, 13, Howard Street, Boston, can be consulted by persons afflicted with diseases of any kind, confident of a cure being performed. Dr. Gay has paid particular attention to the study and practice of female complaints; and so perfectly has he become acquainted with their systems, that without boasting he can warrant a cure from all diseases peculiar to them, both by medical and surgical operations. So successful has been his course of treatment, that he has been called upon by the Directors of the Female Hospitals of France and England, to introduce his mode of practice, and visited those countries for the purpose, which answered the most sanguine expectation of the Medical Faculty there. In offering the results of his long study and practised experience to his native countrywomen, he is governed by feelings of humanity only; for so much have they been deceived by pretended Madams, and miserable pills and quack nostrums, that it is time that some healthy mode of practice should be introduced among them." &c. &c.

Now, we would ask, in what does the *spirit* of these two manifestoes differ? Dr. Tilt will no doubt pour forth a great deal of virtuous indignation, at the bare idea of being likened to an advertising quack. But we will put it to our readers, whether Dr. Tilt, as well as Dr. Gay, has not spoken disparagingly of his professional brethren, and exaltingly of himself? And we will ask *him*, whether he can honestly declare, that the idea of advancing medical science, and of improving the practice of his art, was uppermost in his mind, when he indited and sent forth into the world the passages we have quoted above? If he should reply in the affirmative, we can only say that he has a very strange mode of expressing himself, and that he would consult his own credit by the exercise of a little more caution.

The first chapter opens with a description of the indications of impending menstruation; and after enumerating the headache, pains in the back, uncertainty of temper, &c., which are the ordinary, though by no means the invariable forerunners of the first appearance of the catamenial flow, Dr. Tilt sums up the whole, by informing us, that "*in fact, when woman has become unto herself a mystery,* the medical attendant should lose no time in advising the mother ***not to let her daughter be taken unawares,*** but to prepare her to expect what is the common lot of her sex," &c. &c. The contrast between the two italicised portions of the foregoing sentence, is so charming, that we shall not attempt to heighten it by dwelling on its beauties.—In pointing out the importance of the early acquirement of self-control, "in enabling women to cope with the innumerable adverse vicissitudes of life," Dr. Tilt puts forth a very curious estimate of its uses to the doctor. "What assistance," he remarks, "would it not be to us, *as a lever,* to withstand and cure many of the diseases to which they are liable! We should thus be able," he continues, "to use the patient's will to throw off hysterical seizures," which, when thus dissipated, are rather got rid of, we opine, by the patient's own use of her will, than by the doctor's working of the lever. But Dr. Tilt's opinion of the powers of this lever goes yet further: for he avers that "it will sometimes arrest the advent of puerperal convulsions, which so often spread the silence of desolation around the domestic altar;" and in proof of this extraordinary position, he gives the following bit of personal experience, which is quite germane to the afore-cited dedication:

> "On one occasion I witnessed this power of the human mind exerted in one, bound to me by the dearest of all earthly ties, who, after a protracted labour, told me that it was only by the force of a continued effort of the will, that she did not go into fits long before the termination of her troubles. Her constitution was weak indeed, but her mind could bear pain, keep it at bay, and forbid its increase." (pp. 35-6.)

Now we venture unhesitatingly to affirm, that any "fits" which the amiable lady here so touchingly referred-to could keep at bay, by an effort of her will, could not have been deserving of the term of puerperal convulsions, but must have been simply hysteric. And we observe, moreover, that her will was exerted by herself, and not worked as a lever by Dr. Tilt. —Half-way down the next page, we find a special caution against "the habitual reading of hair-uplifting novels." What *does* Dr. Tilt mean? But still choicer *morceaux* occur a little further on:

> "This excessive development of the nervous system, the predisposing cause of so much disease, can be produced by an undue stimulation of all those nervous expansions which, underlaying the whole surface of the body, render it capable of sensation, and principally by the prolonged and exaggerated exercise of those portions of the body where nervous substance and nervous energy are concentrated to become senses, for the appreciation of special qualities: for the senses, and more particularly the ears and eyes, *are the mysterious portals through which mind and emotion enter a material structure,* to place themselves in communication with the mind and emotion of another being. Therefore the skin, by the habitual stimulation of heat; the palate, by luxurious feeding; the nostrils, by the indulgence in 'soul-dissolving scents;' the ears, by a superabundance of musical vibrations, may give an undue activity to the nervous system, can awaken the dormant powers of imagination, and by increasing the energy of the human passions, may react on the organs which they call into action." (pp. 36-7.)

In a note to this paragraph, Dr. Tilt expresses his dissent from Rousseau's doctrine, that olfaction is the sense of the imagination; but still, he says, "there may be some truth in the soul-dissolving influence ascribed by poets to perfumes; for the constant use of scents, ***applied to that portion of the brain which is least protected,*** must have an exciting effect on the nervous system." We scarcely know which to admire most, the profound psychology of the text, or the wonderful physiology of the note.

The one is quite worthy of the other. The man who can write about mind and emotion entering a material structure through those mysterious portals, the organs of sense, can scarcely astonish us by talking of odours applying themselves to that portion of the brain which is the least protected. Does Dr. Tilt imagine, that when he indulges in the "soul-dissolving scent" of a rose, the delicious *atar* finds its way through the cribriform plate of his ethmoid bone to the "unprotected" anterior lobe of his brain? If not, what *does* he mean?

After a few remarks on Dancing, we have the following delicious bit of nonsense about Music:

> "There are 'softly soothing Lydian measures' which, laden with emotion, travel lightning-like to some undiscovered bourne where similar emotions can be awakened —to that *ultima thule* of the human microcosm where emotion and reason, the two irreconcilable powers, meet, and where emotion can disturb the equilibrium of reason." (p. 38.)

One would think that Dr. Tilt had been attempting to "do into prose" Swift's well-known "Verses by a Lady of Quality."

We might go on multiplying quotations not less offensive than the foregoing; but we think that our readers must have had enough of such stuff; and we owe Mr. Roberton an apology for having so long kept his work on one side, in order that we might serve up Dr. Tilt's for their entertainment. To his Treatise, therefore, we shall now return; and shall first give a brief outline of his inquiries *On the Period of Female Puberty in Different Climates.* This topic, which, as he justly remarks, is "interesting alike to the ethnologist, the jurist, and the historian," he introduces by a chapter on "The Present Social Condition of Women not European," in which he points out the great importance of correct information respecting the physical and moral condition of the females of those varied races which are more or less beneath British control, "with a view to the elevation of these, our fellow-subjects, by the enactment of wise laws, by education, and by the free diffusion of religion." His illustrations are chiefly drawn from the habits of two races scarcely less widely removed from each other in the social scale, than they are in climatic position; and he shows upon reliable testimony, that the early corruption of females is common to both, and that "woman's rights" are as little thought of among the simple Esquimaux, as among the learned, subtle, and accomplished Hindus. The doctrine commonly-received amongst physiologists, since the time of Haller, regarding the influence of climate on the period of female puberty of women, is thus stated by Dr. Denman:

> "The early or late appearance of the *menses* may depend upon the climate, the constitution, the delicacy or hardness of living, and upon the manners of those with whom young persons converse. There seems to be some analogy between the effects of heat upon fruits and the female constitution with respect to menstruation, for, in general, the warmer the climate the sooner the *menses* appear. In Greece, and other hot countries, girls begin to menstruate at eight, nine, and ten years of age, but advancing to the northern climates, there is a gradual protraction of the time till we come to Lapland, where women do not menstruate till they become of maturer age; and, then, in small quantities, at long intervals, and sometimes only in summer." (pp. 19-20.)

The important consequences, both moral and political, which necessarily flow from the admission of such views, require that they should be grounded on an adequate basis of facts; and this, Mr. Roberton assures us, is far from being the case; indeed, he goes so far as to assert, that climate has no influence whatever. He first sets himself to demolish the physiological notion, that a high external temperature will more rapidly bring the body to maturity, than exposure to the frigid atmosphere of the Arctic regions; and urges various considerations which tend to undermine the idea so commonly entertained, of the analogy between the effect of heat in maturing the human female, and its power of ripening fruit. It does not seem to us, however, that he has put this question altogether upon its right footing. In the first place, it is certain that, as a general rule, both vegetables and *cold-blooded* animals have their vital activity augmented by heat and retarded by cold. It is true that different species have been adapted by the Creator to thrive under different degrees of temperature; and thus the pine-forests of Siberia and North America may rival even the vegetation of the tropics in the mass of vegetation they present. But if their *rate of life* be compared, and if the variety and high organisation of the denizens of warmer regions be taken into the account, there can be no question as to the side on which the balance will lie. It is interesting to remark, that of the whole class of reptiles, which are the highest animals that are dependent upon external warmth for the maintenance of their activity, few or none are found within the Arctic circle, whilst all the larger species are inhabitants of the tropical zone, or of countries immediately bordering upon it. But among *warm-blooded* animals, the case is altogether different; for *they* are rendered in great degree independent of external warmth, by their own heat-producing powers; and although we here, too, find particular species adapted to particular climates, yet there are some which, like man, are so independent of external heat or cold, as to be able to live in the extremes of both, and to endure the piercing cold of the arctic winter, and the scorching heat of the tropical summer, without being affected by either. And the *rationale* of this power of climatic adaptation obviously lies in *the constancy of the temperature of the body* itself, which is not changed (in the state of health) in any considerable degree, by the greatest elevation or depression of the external temperature which man is capable of sustaining. We should not anticipate, then, that heat could exert any such influence in accelerating, or cold in retarding, the maturation of the human organism, as it has upon vegetables and cold-blooded animals; but the question is, whether it can possess *any* influence; and this we should be inclined, upon *à priori* grounds, to think not improbable. For although we still want more ample information upon this subject, yet it appears pretty certain that the habitual temperature of the human body is a degree or two higher in tropical than in temperate climates; and it is probable that a similar difference exists between the heat of the body in the temperate and in the frigid zones. Hence, if heat possess any influence in promoting the development of the organism, which all sound physiology teaches that it has, we should expect that climatic influence may go for something in accelerating or retarding the epoch of puberty, which is the last phase in its history. Such an influence, however, may be inappreciably small; the difference in the temperature of the body on which it depends being so trifling.—We shall now follow Mr. Roberton into the results of his inquiries into the facts of this case.

The first point to be determined is the average age at the first menstrual crisis of females in this country. On this head we are sorry to see that Mr. Roberton has not extended his inquiries beyond the point to which he had brought them in 1830, when he published a table of 450 cases, derived exclusively from hospital practice. This table had its value, as showing that the natural period of puberty is spread over a more extended range of ages, and is more equally distributed through that range, than authors have generally stated; a result which has been confirmed by all who have subsequently investigated the subject. The mean age deduced by Mr. Roberton from this table is 15·204 years; Dr. Tilt, on the other hand, taking Mr. Roberton's own table, makes it out to be 14·09. We cannot understand the principle on which the latter computation is made; and such a marked inaccuracy throws a doubt over the other results of Dr. Tilt's computations. We are surprised that neither Mr. Roberton nor Dr. Tilt should mention the table of upwards of 4000 cases, collected indiscriminately among the poor and the rich, by Mr. Whitehead ('On Abortion and Sterility'), from which the mean age of 15·6 years was deducible; and it is to be regretted that Mr. Roberton should not have collated the data furnished by Dr. Guy, who collected 1498 cases, though he has noticed those obtained by Drs. Lee and Murphy, who collected 1719. The mean age of the latter series corresponds closely with his own, being 15·17, whilst that of the former is nearly a twelvemonth less, being 14·35. Dr. Tilt furnishes a series of 775, collected by himself, the mean of which also is 14·3. Without attempting to draw too precise a line, therefore, which the collection of an additional set of cases may displace, we may say that the mean age of the first menstruation in this country is not far, on the one side or the other, from fifteen years.

It is unfortunate that so few data should exist for accurately determining the epoch of the first menstruation among the inhabitants of Arctic regions. Mr. Roberton places his chief reliance upon a series of 16 cases furnished to him by Mr. Lundberg, one of the Labrador Moravian missionaries; from which it appears that the mean age among that people is 15·94 years. This series is of great value, from the authentication of the ages of these females, which the records of the Mission permitted; and although the number of cases is too small for satisfactory comparison with the mean of other countries, yet we can by no means agree with Dr. Tilt, that they are unworthy to "weigh against the contrary statements of travellers," which (as regards matters of this kind) are generally based upon very loose and imperfect evidence. In fact, the concurrent evidence of travellers in regard to the early licentiousness of all the unchristianised natives of these shores, would go to confirm the belief, that the period of puberty is not long postponed amongst them. By way of comparison, Mr. Roberton took out the first sixteen cases from three separate collections of cases in Manchester, and found that, whilst the mean of the Labrador cases was $15\frac{15}{16}$ years, the means of the three Manchester series were $16\frac{1}{16}$, $15\frac{6}{16}$, and $14\frac{14}{16}$ respectively. They afford no sufficient ground, therefore, for the assertion that the period of puberty is decidedly later in Labrador than in this country; but they rather favour the belief, that there may be a difference to the amount of a year, or thereabouts. Here again we have to remark an inaccuracy on the part of Dr. Tilt, for he makes the total number of Mr. Lundberg's cases 21, by including among them five individuals *who had not yet menstruated;* and, as these were below the mean age, this is brought down by Dr. Tilt's statement to 15·57. We do not question his honesty in the matter, for the real mean is more favorable to his views than the one which he has adopted; but we are additionally impressed with his want of statistical accuracy.—But what will our readers think of the next piece of management which Dr. Tilt has adopted to raise the mean age of his "cold climates," and to keep down that of his "temperate climates?" He has received from Dr. Ravn, of Copenhagen, a table of 3840 cases (of which 3429 have been already published in the 'Bibliothek for Lægr'), which make the mean age of puberty among the Danish females not less than 16·78 years; and this he puts among the "cold climates," notwithstanding that, according to his own showing, the mean annual temperature of Copenhagen is *not so much as a degree lower* than that of Manchester (which is placed among the "temperate"), the one being 47·7 and the other 46·8. Dr. Tilt also cites a table of 157 cases collected at Christiania, which gives 16·88 as the mean age; and, from the close coincidence of the Christiania and Copenhagen tables, notwithstanding that the difference in the mean annual temperature of the two places is 5° (which is as much as exists between Copenhagen and Paris), we are inclined to think that the cause of this late menstruation of the inhabitants of Denmark and Norway is rather to be looked for in their *race,* than in the *temperature* under which they live. With regard to the Laplanders, whose late menstruation is affirmed by Denman, on the authority of Linnæus, it is remarkable that Linnæus says nothing, in the passage of his "Flora Lapponica" to which reference is made, as to the age at which the menses appear, and merely speaks of the comparative scantiness of the flow, and of its occasional remission in the winter; and, like other travellers among the northern nations, he gives evidence of the strength of the sexual propensities of this people, and notices their early marriages. According to Clarke, the males among the Laplanders commonly marry at or before the age of eighteen, and the females at or before fifteen. This certainly does not look like a retarded puberty.

The reputed early puberty of the Greek females has been shown by Mr. Roberton to rest chiefly upon the fact, that very early marriages are not uncommon among them, and that they sometimes become mothers at thirteen or fourteen years of age. But, so far as can be judged from the small number of cases furnished to Mr. Roberton by native practitioners, the mean age at puberty is not at the most a twelvemonth earlier than in this country, the difference lying chiefly in the larger proportion whose first menstruation occurs before fourteen, than in any general precocity. Cases of maternity before the age of thirteen sometimes present themselves in this country, and would doubtless be much more frequent, were it not that very early sexual connection is not only repressed by public opinion, but is held criminal in law.—The results of Mr. Roberton's inquiries respecting the inhabitants of Madeira, from which island he has procured a return of 242 cases, are far from favouring the climatic hypothesis; for the mean age which they afford is 15·44 years.

Passing on now from temperate to hot climates, we find that the information collected by Mr. Roberton in regard to the Negro race by no means bears out the common notion of their early puberty; for the mean age in 89 cases collected for him in Barbadoes, Demerara, and Jamaica, is 15·13 years, their distribution extending over much the same range as in this country; and the experience of medical men who could not afford the same definite statistical information, was to the same effect. All agree, too, that there is no difference in this particular between the Negro and European females.

Much attention has been given by Mr. Roberton to the allegation of very early puberty so commonly held in regard to the Hindu female; and the information he has collected on this subject is not a little curious.

The *mean age* at which the first menstruation occurs in the Hindu female, as deduced by Mr. Roberton from 597 cases collected by different observers, is nearly two years lower than that deduced by him from the 2169 cases collected in England by Drs. Lee and Murphy and himself; the former being 13 years, and the latter being 14 years 11 months. This does not arise, however, from the antedating (so to speak) of the puberty of the whole population, but only from the fact that *a larger proportion* of first menstruations takes place among Hindu females during the *earlier* years of that period over which they are spread in English females, the distribution in the latter being more equable. This will be apparent from the following Table: (p. 125.)

	Bengal.	Deckan.		
Ages.	Calcutta.	Bangalore, Toomkoor, and Bombay.	Both.	England.
8	3	—	3	—
9	7	1	8	14
10	14	4	18	55
11	37	43	80	77
12	66	79	145	142
13	49	90	139	263
14	41	64	105	396
15	11	34	45	417
16	6	18	24	340
17	3	15	18	215
18	1	4	5	138
19	—	3	3	65
20	1	—	1	33
21	—	2	2	9
22	—	—	—	4
23	—	1	1	1
	239	358	597	2169
	Mean Age.			
	12 ys. 6m.	13 ys. 5m.	13 ys.	14 ys. 11m.

A glance at this table will show that the greatest number of first menstruations occur among Hindu females in the 12th, 13th, and 14th years; whilst, among females in this country, the largest proportion presents itself in the 14th, 15th, and 16th years. The proportion of menstruations *under eleven years* is not at all remarkably different in Hindustan and in England; so that the current idea of the *very early puberty* of the Hindu females is altogether incorrect. Dr. Tilt furnishes, in addition, 37 cases collected by Dr. Stewart at Bombay, which do not materially affect the average; yet he is very wroth with Mr. Roberton for not having noticed these cases. On the other hand, Mr. Roberton has been informed by Professor Webb, that, in the Calcutta Government School, of 200 girls, Indo-British and British born, under his charge, *sixteen years* is the average age of menstruation.

It is evident, then, that climate cannot be the sole or even the chief determining cause of the difference in question; for Demerara and the West Indian Islands have a higher mean annual temperature than Calcutta and the Dekhan; and yet the Negresses of these colonies, like the Europeans and Indo-Europeans of Calcutta, are not at all earlier in their attainment of puberty, than females of the corresponding classes in this country. It is possible that it may be dependent upon difference of *race*, especially if, as now seems probable, the mass of the Hindu population belongs rather to the Mongolian stock than to that from which the European nations, as a whole, are offsets; but there is strong evidence that the cause partly lies in the peculiar habits of the natives of India, which tend, in more ways than one, to force forwards the period of puberty; and this view, as we shall presently see, is quite compatible with that just advanced:

"A fact, already repeatedly referred to, but hitherto, I apprehend, unknown to Europeans, is the law of the Shastras, that females shall be given in marriage *before* the occurrence of menstruation; and that, should consummation not take place until after this event, the marriage is a sin. Accordingly, it is the custom in Lower Bengal, to send the girl at the age of nine years to the house of her husband, unless the latter be so distant that it cannot be done: and two ancient Hindu sages are of opinion, that if the marriage is not consummated before the first appearance of the catamenia, the girl becomes "degraded in rank." Testimony to the same effect is given by Professor Webb; who likewise joins the Baboo in thinking that, in those alleged instances of very early menstruation, a discharge of blood from injury in premature intercourse is the real explanation of the occurrence; a supposition worthy of consideration, when endeavouring correctly to estimate the value of the Calcutta tables. At Bangalore it would seem, that this revolting custom of consummating the marriage before puberty, does not obtain, the husband refraining from taking his wife to his own house, till not less than sixteen days have elapsed subsequently to puberty. Customs, we know, differ in different parts of our great eastern empire. I had occasion formerly to refer to the diversity of manners and even morals, in so vast a country. In the present instance it might readily escape notice, when speaking of Calcutta and Bangalore, that these two cities are about as widely apart as to distance, and probably not less diverse in customs and manners, than are Copenhagen and Madrid, the capitals of Denmark and Spain." (pp. 130-1.)

Now, it is a confirmation of this view, that the difference in custom which thus obtains between Calcutta and the Dekhan should be accompanied by a marked difference in the age of the (reputed) first menstruation; for whilst in the former we have seen it in the table to be 12½ years, in the latter it is nearly 13⅓, and this notwithstanding a more equatorial latitude. It can scarcely be questioned that such a premature sexual excitement will have a tendency to accelerate the epoch of puberty; and that, when this is constantly acting through a long succession of generations, an early puberty may be established as a character of the race. But, besides this *modus operandi* of the custom in question, the following is pointed out by Mr. Roberton:

"When it is recollected, that the consummation of marriage among the Hindus has taken place, at the latest on the arrival of puberty, during the lapse of more than 3000 years, and that the practice is sanctioned by ancient laws and consecrated by custom, it is easy to conceive, that those females who were the latest in reaching puberty, would be the least sought after for wives;—that such women would not unlikely, in many instances, remain unmarried; and that thus, (owing to the origination of a preference on this ground in the selection of wives, operating through a long period of time,) Hindu women would gradually come to consist, in a proportion different from that in Europe or elsewhere, of such as by constitution are early nubile. To me there seems nothing extravagant or far-fetched in this supposition. The production of a like state of things in England, in any particular district, is quite conceivable. Nothing is better established than that early puberty is a family peculiarity. Let us, then, only suppose families, possessing this kind of constitution, to intermarry, and the peculiarity in question would be propagated, extended, and transmitted, and so a race, distinguished by it, would be produced." (pp. 128-9.)

How far a tendency of this kind may become established in *races*, may be judged of by what we witness among our own countrywomen in particular *families*, a tendency to very early or very late menstruation, to a very frequent or to a very tardy return, being often distinctly hereditary. Thus, Mr. Roberton mentions a case in which he found that the mother and grandmother of a girl who menstruated at twelve years old, had themselves become regular at the same age. In another, five sisters in one family menstruated at the age of eleven. So the fortnightly recurrence of menstruation not unfrequently presents itself as a family peculiarity; and Mr. Roberton mentions a case in which this peculiarity extends into collateral branches.

That the mean age at the first birth should be very early in India, is not at all surprising; but we should scarcely have expected it to present so great a contrast as it does, to that which is determined to be the average in the manufacturing districts of England, where marriages take place (at least when trade is brisk) at a very early period. In Bombay, the mean age at the time of the first birth is little short of 17 years; in Bangalore, 16 years and 5 months; while in Calcutta it is 14 years and 8 months. The mean for the whole of India deduced from these observations (265 in number) is 15 years 6½ months; whilst that of 500 females of the operative class in Manchester was found by Mr. Roberton to be 23 years. Out of the 265 Indian *primiparæ*, 1 was only 10 years of age, 7 had attained 11 years, 15 were 12 years old, 30 were 13 years of age, and the numbers for the ages of 14, 15, and 16 were 54, 50, and 31 respectively, the proportion then rapidly diminishing. On the other hand, in the Manchester table, the first *primiparæ* had attained the matronly age of 17 years, and the highest numbers fell between the ages of 19 and 23, a much larger number, however, being presented after the last of those years than before the first, so that the mean of all comes to be 23, as just stated.

It is clear enough, therefore, that this premature maternity is due much rather to social than to climatic influences; for the English females *might* become mothers (as is evident from the fact that they sometimes do) at nearly as early a period as the Hindus, if marriages were habitually practised at as early a period. Mr. Roberton has collected some very curious evidence to show that marriages, nearly as premature as those of the Hindu females, were formerly sanctioned both by law and by public opinion in this country; and that in Ireland they have been by no means unfrequent within a comparatively recent period. He very justly looks upon these infantile marriages as constituting a most important obstruction to social progress; and it gives us some hope of amendment to find that the more intelligent Hindus do not hesitate to pronounce the system to be one of the "monster evils" of their country. The following are the just remarks with which Mr. Roberton concludes this portion of his subject:

"If this inquiry into the age of female puberty in Hindustan answered no other purpose than to prove how little we have hitherto known, or even now, perhaps, know, of the physiological, social, and moral condition of our fellow-subjects in the East, it would not be altogether in vain. Our empire embraces every variety of climate and of race to be found on the globe; and yet, with the exception of the Esquimaux (of whom we have learned a great deal from the scientific men intrusted with the arctic voyages and journeys), what know we, comparatively, in regard to the above particulars of those races of people? little; with that fulness and accuracy of knowledge which our ample and long-enjoyed opportunities might have been expected to secure. Yet we legislate for them, we enact new and modify old laws, we change their immemorial customs, we set up schools for their instruction, we scatter amongst them our literature, and invite them to imitate our freedom of thought by the unrestricted use of the press. In short, we do not trouble ourselves to ascertain the various strongly-marked features whereby, in very many instances, they are so widely distinguished from each other, and from us, their governors. We know that they are men, and beyond this give ourselves little concern to study the wonderful diversities they present, the causes of those diversities, and the consideration that these demand at our hands. On the contrary, we content ourselves, so long as the dread of our power secures obedience, to treat them according to our own peculiar humour and prejudices. This state of things, in the East, may, perhaps, be endured for a long time; but it is neither honorable to us as a nation, nor favorable to the stability of our rule. The members of the medical profession, in our possessions and colonies, many of them accomplished men, have done a little to dissipate this state of ignorance; and may yet do a great deal more. With my very slender opportunities, I have attempted to do something towards a fuller and clearer knowledge of the physiology and social institutions of a most ancient people,—the earliest civilised of any on the face of the earth. The facts I have been able to collect I now present to my readers as a humble, yet not, I hope, unimportant contribution to the ethnology of the Hindus." (pp. 132-3.)

Such are the principal facts on which Mr. Roberton claims to have established his position, that the period of puberty is altogether unaffected by temperature; and he thus explains the circumstances which have led modern travellers and others to erroneous notions on this subject:

"1. The general licentiousness in that degraded condition of society, everywhere to be found south of the temperate zone, leading to sexual intercourse in childhood, is mistaken for evidence of general female precocity.

"2. The speedy decline of beauty and of youthful appearance where the women begin, while very young, to bear and suckle children. This we see even in our own climate in almost all cases of early prolific marriage; and, still more remarkably, where there happens to be superadded a life of toil and poverty." (p. 134.)

We fully agree with him, that the data which he has obtained afford no adequate support to the vulgar doctrine; on the other hand, they do not absolutely disprove it; and we are still permitted to believe, as it seems to us, that, under similar social conditions, the mean age at puberty is likely to be *somewhat* lower among the inhabitants of warm climates than it is in those of cold. As to Dr. Tilt's table, however, almost entirely compiled from the results collected by Mr. Roberton, it really proves nothing at all, notwithstanding his very definite assertion that this table "peremptorily refutes Mr. Roberton's opinion," the mean age in hot climates being 13·19 years, in temperate climates 14·74, and in cold climates 16·53; for, as we have already remarked, if the Copenhagen list be transferred from the "cold" to the "temperate," to which it properly belongs, it will raise the mean age of the latter to such a close approximation with the mean of the Labrador table (which is the only one deserving to be ranked in the former division), as to remove all decided difference between them. So, again, if we omit from the table of "hot" climates the Hindu returns, for the reasons advanced by Mr. Roberton, our comparison will be limited to the Negro table, which does not materially differ from the "temperate" mean. Dr. Tilt himself can only account for this conformity, by setting it down to the influence of *race*.

The inquiries of M. Brierre de Boismont, conducted in Paris, lead, as is well known, to the conclusion that menstruation occurs earlier, by some months, in females of the higher classes than in those of the lower; and Dr. Tilt states that the result of his own investigations is confirmatory of this view. We are very well disposed to accept it, as it would be quite in accordance with the general physiological principle, that the higher development of the *individual* diminishes the generative nisus; and there can be no doubt that, among what Dr. Tilt calls the "well-to-do working class," we meet with a much more healthful type of female adolescence than among the higher ranks, whose course of education is, generally speaking, very unfavorable to corporeal vigour. In particular we would remark, that the manner in which the cerebral portion of the nervous system is forced into undue activity, at the expense of the automatic apparatus and the muscular system, appears to us to have a special tendency to accelerate the development of the generative system; it being well known to the Comparative Physiologist, that there is a remarkable conformity between these two parts of the organism, the one advancing *pari passu* with the other.

It is much to be wished that more accurate data could be obtained on the whole subject; and we cannot conceive that any real difficulty can be in the way of the collection of such, except where there is no reliable record of the ages of the subjects observed. A large additional body of evidence is required, to establish the fact of a difference between the different classes of the community in our own country; and it would be interesting to note as many exceptional cases as possible, and to ascertain how far they might be considered as family peculiarities. And those members of our profession who are so widely, indeed universally, dispersed over the habitable globe, may, with very little trouble to themselves, do good service to science by collecting data, with regard both to the transplanted European population, and to the native races with which they are brought into contact. As Dr. Tilt remarks, the experience of the Jewish race would afford us important information in regard to the influence of climate, since this remarkable people maintains its ground, and preserves its customs unchanged, under a wide diversity of external conditions; and the experience of the Quaker and Moravian communities, whose daughters are not subject to the exciting influences of dancing, late hours, music, theatrical representations, &c., might elucidate the question how far such influences really possess any accelerating power.

Both Mr. Roberton and Dr. Tilt give us some information regarding the *periodic recurrence* of the menstrual discharge, which shows that the interval is less constant than is usually supposed. Mr. Roberton states, that, out of 100 women, he found only 61, or just three-fifths, in whom the catamenia recurred monthly; the remaining 39, or nearly two-fifths, menstruated at different intervals. In 28, the interval was regularly three weeks; in 1, regularly a fortnight; and in the remaining 10, the intervals were uncertain and irregular in their duration. Dr. Tilt informs us, that according to his observations,—he does not tell us, however, in what number of cases they were made,—the number that did not follow the monthly type was 23 per cent.; in 17 per cent., the type was three weeks; in 5 per cent., it was every six weeks; and in 1 per cent., the recurrence took place every fortnight. Putting aside the cases in which the intervals are irregular, which seem to have been excluded by Dr. Tilt, the correspondence between the two returns is pretty close, and shows that the three-weekly return occurs in from one-sixth to one-fourth of the whole number of females. We ourselves have met with cases, in which the flow has occurred with the utmost regularity every 27, 26, or 25 days, instead of every 28. It is obvious, then, that if Dr. Tyler Smith's doctrine of the occurrence of the parturient nisus at the tenth menstrual period after conception, had any foundation in truth, from one-fourth to one-sixth of child-bearing females would be brought to bed after a gestation of 210 days, instead of 280, which is assuredly not the case.

The question of the *Age at which Menstruation ceases* has also occupied Mr. Roberton's attention; and he has found this (even putting aside the *extraordinary* cases) to be spread over a longer series of years than is usually supposed. Out of 77 women, whose cases were tabulated at Manchester, the catamenia ceased in 1 at the age of 35 years; in 13, between 40 and 45 (both inclusive); in 46, between 47 and 50 (both inclusive); and in 17, above the latter age. Of these last, one went on to the 70th year, having ceased menstruating for a twelvemonth about the 50th, and having then recommenced and continued regular. Experience seems to have determined that menstruation always, or nearly always, continues up to the last pregnancy, a woman seldom or never becoming pregnant who has ceased menstruating; consequently, a knowledge of the ages at which pregnancies occur, affords approximative information on this point. Mr. Roberton has obtained, from the records of the Manchester Lying-in Hospital, the ages of 436 pregnant women, who had passed 40 years; and he found, that whilst 214, or nearly one-half, were either in their 41st or 42d years, 197, or nearly the same number, were between the 43d and the 47th years; 23 were between the 48th and the 50th; and 3 later than the 50th. This table, however, does not give us the dates of the *last* pregnancies; and it is, therefore, only of value as showing the relative frequency of pregnancy at rather advanced periods.—The results of Mr. Roberton's inquiries into the period of the last menstruation among the inhabitants of other climates, do not present any marked difference from the preceding; the concurrent testimony of all the observers being, that it commonly takes place at some time between 40 and 50 years, alike in the precocious Hindu, as in the (reputedly) tardy Laplander. On this point we do not find that Dr. Tilt has any information to give us.

The last subject connected with Menstruation, to which we shall direct the attention of our readers, is the *Influence of Lactation in retarding Conception.* This subject was first taken up systematically by Mr. Roberton in 1831, the number of cases upon which his deductions were founded being 160, and these being all from the class of operatives. Subsequently, Dr. Laycock has prosecuted similar inquiries upon 135 females. In both sets of cases, pregnancy had occurred during lactation in about half the total number; but, as Dr. Laycock remarks, these may be divided into two classes, the first consisting of those in which pregnancy occurred only once or twice, so as to be the exception rather than the rule; whilst the second included those in whom pregnancy, under such circumstances, ordinarily occurred. To the second class, in Mr. Roberton's experience, belong no fewer than 64 women, or two-fifths of the whole; 42 of them not having conceived until the lactation had been considerably protracted, whilst 22 were in the habit of conceiving soon after parturition. In Dr. Laycock's experience, however, the proportion of such cases was much smaller, being only 19 out of 135, or about one-seventh. Here, again, the distinction should be drawn between those who only become pregnant when lactation is unduly prolonged, and those in whom the aptitude for conception does not seem to be at all diminished by lactation; but Dr. Laycock does not give us any precise information on this head, only telling us that the period of lactation at which pregnancy took place varied from three months to two years, the average being twelve months and a half.

Thus it appears, that in a large majority of cases, pregnancy does not take place during the first year of lactation; but that, after that period, the inaptitude for conception seems to decrease, so that the continuance of lactation no longer affords the same security against a recurrence of pregnancy. Mr. Roberton tells us, that, of the above 160 women, 99 had never menstruated whilst suckling; whilst in 61, menstruation recurred once or more during that period. Of these 61, there were seven who only menstruated once each out of 41 lactations, and in whom the phenomenon was, therefore, exceptional; 31, or nearly one-fifth of the entire number registered, menstruated regularly during the whole period of lactation; 14 usually commenced menstruating between the fourth and the eleventh months of lactation; and 9, between the twelfth and fourteenth months. Mr. Roberton adds: "I ought not to omit remarking, that of those women who had usually conceived while yielding suck, *the majority did not menstruate;* that, in fact, the appearance of the catamenia during lactation, did not seem to have any influence in disposing to conception." This is a fact of great importance, as confirming the conviction which we have expressed on a former occasion, that those who assert menstruation to be *essentially* connected with the ovarian œstrum, go a great deal further than the facts of the case warrant. We may further remark, that the large majority of cases in which pregnancy took place after lactation had been continued for above twelve months, seems to show that lactation may be generally considered as a protective against too frequent conception, when it is not prolonged beyond what may be considered its natural limit.—Upon this subject, Dr. Tilt affords us no information; but he would seem to have met with a considerable number of cases of menstruation during lactation; for he states that he has interrogated no fewer than 100 females, with the view of ascertaining how far the mammary secretion was affected by the occurrence, and he gives, as the result of his inquiries, that, in 45, no change could be perceived; in 39, the flow of milk was increased; and in 16 only was it diminished or impoverished. He considers the notion, that a woman should leave off nursing if her catamenia reappear, to be an unfounded prejudice; and doubtless it is so, if it be regarded as an invariable rule. But there are, doubtless, a great number of cases, in which the system is quite unable to bear this double drain upon it, and in which the preservation of the health of the mother requires that lactation should be at once discontinued.

We should have been glad to have found some redeeming points in the detail of the Practical Management of the Diseases of Menstruation in Dr. Tilt's work; but here again we must leave him to praise himself, and to flatter himself, if he pleases, that he has "advanced one step further than previous authors." For our own part, we are bound to say, that we have rarely read a work in which the effort to appear practical was more constrained and awkward; and so overlaid is it with tawdry verbiage and dreamy sentimentality, that the siftings are hardly worth the reader's acceptance. When speaking even on so ordinary a subject as the use of purgatives in chlorosis, he says:—

"It is, however, better not to confide in purgatives alone, but to let them form the initial part of the treatment, as in nature diarrhœa often forms the initial part of menstruation. I always begin by producing a decided shock on the system of nutrition by an emeto-cathartic, and I then give steel and bitters; but if I find that the appetite does not improve, and that the bowels remain sluggish, I put aside steel and bitters, and seek to break in on a perverse concentration of forces by giving another emetic." (pp. 64-5.)

We are altogether too dull to understand why, because a diarrhœa occasionally precedes or accompanies the menstrual periods, a purgative should be given at the commencement of the treatment of chlorosis; and our own ardour pales before the grand idea of breaking through a perverse concentration of forces by an emeto-cathartic. But after all, excepting for the indulgence of the fancy, who but Dr. Tilt can make anything of the quotation? Again, the chapter on the Treatment of the Diseases at the change of Life begins with Bloodletting, which every one knows is rarely practised now as compared with what it was formerly. But to know this is a very tame affair. Dr. Tilt introduces it with the finest scenic effect: his description of this plain but important fact glows with the warm colouring of a dissolving view. Listen, reader!

"As the track of the caravan across the desert, of the fisherman by the sea-shore, and even as that of the peasant across a little field, is never a straight line, but is made up of a succession of undulations; so the pilgrim in his journey over the plains of scientific research seems to be constantly describing zigzags between extremes. The history of bloodletting well illustrates this oscillating tendency of the human mind." (p. 111.)

But enough of this. We have too much of what is really valuble in the Second Part of Mr. Roberton's work, to be detained any longer with Dr. Tilt. He has shown an entire ignorance of the wants and character of the medical profession in England, and has utterly mistaken his own capabilities in supposing that such trumpery could be made attractive enough to supersede the plain, concise, and pointed style, which alone aptly represents the observations at the bed-side. His book can only be attractive to a certain class of "pseudo-narcotized" women, for whom it was, in part at least, designed.

The Essay which concludes the first part of Mr. Roberton's work, is on the Hysteric Constitution, which he informs us is a reprint with one additional case, and a few verbal alterations, of one which he published in 1834. Associating it with its original date, it is an Essay of great practical value; as the hysteric character of several of the local disorders, which at that time were so constantly treated as inflammatory affections, was clearly indicated. It is, however, behind the knowledge of the present time.

One of the last of the practical Miscellaneous Essays, is an "Apology for the Study of Midwifery as a Science," in which the author combats the absurd notion, which the late Mr. Anthony Carlisle was, we would hope, the last person of any repute to maintain, viz., that childbirth, like parturition in the lower animals, is purely a natural process, the safety of which Divine Providence has most wisely secured; and, consequently, it is always mischievous to tamper with pregnant women, under the pretence of hastening, easing, or retarding their delivery." The premises on which this conclusion rests are well known to be fallacious; and Mr. Roberton shows that even in animals, in their wild state, there are occasional difficulties in the act of parturition, and some fœtal monstrosities; whilst in domesticated animals, the dangers of parturition are greatly augmented, and the fœtuses more liable to malformation. In the human female, when subject to the habitual toil of a savage life, which is not suspended or moderated during pregnancy, but which tends to harden their physical powers, the process of parturition is acknowledged to be less painful, and the puerperal state less dangerous, than in civilised life. But from some valuable incidental evidence of travellers, and some direct evidence obtained by Mr. Roberton, with reference to the females of the South Sea Islands, it appears that, so far from parturition being uniformly easy, expeditious, and safe, instances of protracted and even fatal labours are, perhaps, equally numerous as with us. It is hardly necessary for us, now that midwifery is recognised by all the great schools in Europe as a science of great practical import, to follow Mr. Roberton, or to make any addition to his arguments in counteracting the feeble attempts to decry it. The pure surgeons of this country, as they are called, are perhaps the insignificant few, who retain a remnant of their old colleague's prejudice; and within the last few years, their regulations for students have shown a disposition to lower the study of midwifery as though it were a subordinate pursuit, in a manner which called forth the remonstrance of the obstetric teachers of London.

"One cannot easily forget that some, even persons of note, and chartered bodies too, of high rank in the profession, have feigned to regard midwifery as a degrading pursuit! How is this? It is unquestionable that midwifery has repulsive features: the throes and sufferings of the feebler sex, abstractly considered, have nothing in them inviting. But what is there more attractive, I would gladly know, in the endless list of loathsome diseases engendered by vice and luxury, to the practical study of which those dignified men willingly give their days and nights? The truth is, this unkindly and absurd feeling ought rather to be regarded as a corporate prejudice, than as attaching itself individually to educated men. The names of Harvey, Smellie, Hunter, A. Hamilton, Denman, Burns, and many others of repute, who both practised and advanced the science of midwifery, are a guarantee that it is a pursuit neither mean nor unattractive." (p. 491.)

In a chapter on *the Bony Pelvis obstetrically considered,* the author recognises the relaxation of the joints of the pelvis during the latter month of pregnancy; and recounts some observations of his own on the separation of the pubic bones in the guinea-pig just before labour, which amounted to an inch in extent, the interspace being filled by a loose cellular membrane. The arguments which have been adduced, and still obtain with some persons, that the large size of the head in the human fœtus, and the obliquity of the pelvic inlet, are peculiar sources of difficulty in human parturition, as compared with brutes, are conclusively disproved by the author, and his observations on the labour of a bitch are detailed in support of his views. The following quotation sets forth the author's description of the comparative facility of human parturition.

"The size and figure of the human brim is as well fitted to give passage to the large head of the child, as the brim of the brute pelvis to allow the entrance of the comparatively smaller head of the fœtal brute. The long diameter of the child's head is so directed as to enter the brim with ease; while in the brute, the head does not enter alone, but along with the fore feet, upon which it rests: or, you have the bulky haunches when the hinder parts present, the head following couched upon the fore feet. In either kind of presentation, the long diameter of the presenting part enters in the direction of the long diameter of the brim, just as in woman. Besides, there are one or two circumstances which give woman an advantage over brutes in the parturient act,—I allude more particularly to the formation of the fœtal head in the two. In the child, ossification is incomplete, the bones of the cranium readily overlap, and the head elongates when the passage is narrow. In the brute fœtus there is nothing resembling this; the bones are completely ossified at birth, and the sutures so joined as not to admit of *moulding* during labour. Again, the human rectum is never loaded with fat so far as I have been able to ascertain; whereas, in the brute (I allude to animals under the control of man), this intestine is often buried in fat, which is one of the reasons why fat animals have dangerous parturition. For it is evident that this must, in some degree, diminish the natural capacity of the pelvis, on the same principle that a tumour, or other growth, lessens the capacity of the pelvis in the human female." (pp. 246-7.)

How to use the Midwifery Forceps with safety to the Mother and Child.—Under this title Mr. Roberton has written a chapter on the long forceps, which contains many sound precepts and sound cautions, the result evidently of considerable experience in their use. He remarks on the disadvantages which the learner has in the study of operative midwifery, as compared with operative surgery, on account of the latter being transacted, for the most part, on the surface of the body, appealing to the eye, and capable of being seen by all; while the obstetric student becomes a practitioner, in forty-nine cases out of fifty, without ever having witnessed a midwifery operation, or, even if he has had the good fortune to see one, it is but a minimum knowledge after all; for it is the touch, and not the sight, which can minister practical information. Hence Mr. Roberton concludes, that "accoucheurs qualified to undertake the different operations with safety to the mother and child, will always be comparatively few." Those whose duty it is to teach midwifery in the principal medical schools,—and most practitioners who have had to encounter, unassisted perhaps, from their locality, the responsibilities of difficult midwifery cases,—will readily assent to the truth of this observation. There can be no doubt that this grievous educational defect will never be effectually removed, until young men, after they have passed their examinations, have themselves the opportunity of performing these operations under the eye and guidance of their obstetric teachers. It is not enough for a student to have attended lectures, or be up to the midwifery books, or even to have delivered a large number of women; because nothing is more certain than that the passive routine experience, which may thus be acquired, may leave the mind singularly uninformed in the scientific practice of the art. A midwife, or a medical practitioner, may number up, and constantly do so, their one or two hundred cases, and yet remain the merest mechanical drudges; their experience, as it is called, having tested the endurance of their bodies, instead of storing their minds. If, however, having gone through the general study of his profession at the hospital, and the theoretical knowledge of midwifery having at least been acquired and tested by examination, the young practitioner has thrown upon him the official duties and responsibilities of a lying-in charity, subject to the control and supervision of the obstetric physician or surgeon at the head of it, he may, in the course of a few months, if his opportunities be good, enter upon general practice, with greater expertness in the use of instruments, and a better knowledge of the management of difficult midwifery, than a surgical student can acquire of the higher branches of operative surgery. We have seen the working of a lying-in charity attached to one of the Metropolitan hospitals, where these advantages are thrown open to diligent young men just before they enter practice; and we may safely say, that we have scarcely seen an instance where this opinion has not been confirmed. We sincerely hope, that the vast importance of this method of learning midwifery, to the welfare of the great mass of general practitioners, will commend itself to the executive authorities of our great public hospitals, and induce them to devote a portion of their funds to the liberal establishment of lying-in charities, which shall be so constituted, as to secure the benefit of a high order of educational experience to the youthful practitioner.

Mr. Roberton describes and figures a pair of forceps, which he had constructed for him in 1831, so closely resembling the ordinary long forceps of Dr. F. Ramsbotham, which were first described in his lectures, published in 1834, that they might have been supposed to be a copy of them. It is enough to say that they are good working forceps; and, at the time they were made, must have been a great improvement upon those generally in vogue. Mr. Roberton, like many obstetric practitioners, discards the use of the short forceps; and we quite agree with him in thinking, that they are not so good as what are commonly termed long forceps (something under 14 inches in length), even for the purposes of delivery at the outlet. As an illustration of the mode of applying and delivering with the forceps, Mr. Roberton supposes the head to be at the brim in the first position, and having noticed the usual precautions, with respect to the catheter, &c., he thus describes the way of using them.

"The under blade is introduced first (the *back* of it *only* slightly greased), and carried slowly upwards, the tip sliding along, in contact with the passage over that surface which corresponds to the roof of the acetabulum, the handle being brought backwards as the blade is advanced, until the tip has passed the head; then the handle is given to an assistant. The upper blade I introduce after the same manner, carrying it on behind the right acetabulum, and bringing the handle backwards, until it will lock with its fellow; which done (and this may always be done, if the forceps have been rightly applied, though it will require, the head being thus high, a little cautious management), the operator pauses to examine how the blades adapt themselves, and that they are within the lips of the womb. As he cannot employ his eyesight, he must use the mind's eye, with his hand; and being satisfied that he has laid hold of the head diagonally, as was formerly described, seated at a proper elevation behind the patient, he makes cautious traction in the axis of the brim; never suffering himself to forget, that the perinæum is in danger from any jerk of the instrument, whether by slipping or by the rapid descent of the head. Much force he ought not to employ; it is neither safe, nor, on principle, allowable. He must persevere, in a cautious, gentle manner, until the head descends into the cavity, or, by resisting his efforts, he is convinced that this is not the *means* that will succeed. When, however, there is no deformity of the brim, or morbid enlargement of the head, I do not remember having failed of success. The head being brought into the cavity of the pelvis, and the face turned more or less towards the sacrum, I readjust the blades; not withdrawing, but having unlocked them, I move them so as to hold the head over the parietal bones. It is now that the lever action of the forceps begins: I no longer draw the handles towards me, but, seizing them with the right hand, I carry (that is push) them slowly forwards, in a line between the thighs of the patient, it being now necessary, that the right knee should be elevated by an assistant. The mere friction of the blades between the surfaces of the passage and those of the fœtal head, when the handles are thus slowly and cautiously carried forward, is generally of itself sufficient to bring the head to the external orifice, without the aid of a fulcrum; but when, from the greater tightness of the passage, as there will often be in a first labour, a fulcrum is required, the open left hand of the operator is the fulcrum. As the vertex advances along its curved line of exit, the handles have to be carried over the pubis; and in a first labour the ends of them may come at last, as the base of the head is escaping, to press upon the abdomen of the mother." (pp. 266-8.)

We cordially concur with the author in the importance of seizing the head diagonally; and we cannot but reprobate the practice of some modern authors, who still recommend the application of the forceps in the conjugate diameter of the brim. Dr. Churchill's and Dr. Murphy's Plates thus represent them; and it appears to us a serious fault in the Dublin School, notoriously disinclined to the use of the forceps, to select that diameter, which, in the majority of cases, is the one morbidly contracted; and in front of which is the bladder and urethra. It appears almost to be a perverse selection of difficulty and danger, when both may be avoided by the other plan; our own experience is against Mr. Roberton's in passing the blades in front, behind the acetabula; our rule is to introduce first one, then the other, towards the sacro-iliac synchondroses, and move them respectively forwards to the sides of the pelvis, catching the head diagonally over the occiput and the brow and forehead, if there be a fair space in the conjugate diameter, or more centrally over the forehead and occiput, if the conjugate diameter be contracted. We confess, too, that we still retain this hold by preference, until the head is delivered, without shifting the blades, as recommended by our author, so as to embrace the parietal bones. Mr. Roberton insists at great length on the lever action of the forceps, which he justly says cannot be obtained with short forceps, until the head is nearly born, as there is not length of handle enough to effect it. Whether there is any advantage in adopting this method of pushing the handles of the forceps forward, over the plan of continuing traction, directing the head completely forward as it advances to the external orifice, by shifting our own position to correspond with it, we cannot say, as our habit has been to follow the latter exclusively.

Among the incidental remarks which Mr. Roberton makes as the result of his experience, we infer that he has been surprised at finding so little soreness or swelling of the vulva after forceps delivery; also, too, that he is somewhat "shy of using the ergot, unless where the head is in a position to be readily expelled;" and, lastly, we quote, in his own words, a salutary caution, which has the weight of great experience to recommend it:

"And here I would advise my professional brethren not to use instruments, whether the forceps or the perforator, without calling in, whenever it is possible, the assistance of another practitioner. This has generally been my own course, and I have had no reason to regret that I pursued it. Worn out with fatigue and watching, as the accoucheur will often be in lingering cases such as these, it is a comfort to himself, and an inestimable advantage to the sufferer intrusted to his skill, to have the opinion of a friend whose mind is fresh, and free from the perturbing influence of anxiety. It is a too common feeling, perhaps, with the younger members of the profession, that to request a consultation is to own and to proclaim their own incapacity. So far am I from being of this opinion, that I am persuaded the patient and her friends will, in a great majority of instances, attribute the step to a different motive,—to an honorable and conscientious solicitude for the welfare of the patient. Few have been in the habit of calling consultations more than myself, and I am able to declare, that I never suffered in reputation or in my interests from having done so; rather the contrary in regard to both." (p. 270.)

Mr. Roberton relates cases to illustrate the use of the forceps, for the especial preservation of the child;—one case where, after a long labour, the perinæum ruptured, the child being dead; another, where the perinæum was probably saved by the timely removal of the forceps, and the use of the perforator, the child being dead; and others, one of which is detailed, where chloroform was freely used, and with the most benign influence. In 43 cases of forceps, with head presentation, occurring in Mr. Roberton's practice, he says, "the 43 mothers completely recovered, and in 36 instances the children were alive. The catheter was required after labour in seven of the 43 cases." In only one was the perinæum torn, and that not through the sphincter ani. We conclude our notice of this chapter, with quoting one of Mr. Roberton's practical inferences, which is full of truthful import. "In nearly all the operations of surgery, quick despatch is desirable to save suffering; but in forceps-delivery, its being *slowly* is necessary to its being *skilfully* performed."

Laceration of the Uterus in Complicated Labours.—Mr. Roberton narrates ten cases from his own practice, and he has collected twenty-seven from the various authors, of laceration of the uterus, which were probably caused by contraction of the brim of the pelvis, or at any rate were associated with it. He does not extend the Essay beyond the consideration of this particular cause of laceration of the uterus; and his cases abundantly confirm the view which he entertains, and which is generally admitted by systematic writers. Three out of Mr. Roberton's ten cases recovered.

The deductions which he derives from a consideration of the whole number of cases, viz., 37, are—

1st. That labour was not unduly protracted in them; and in illustration of this interesting point, he thus tabulates them:

"The duration of labour, previous to the occurrence of laceration, was:

In 2	cases	4	hours.	In 1	case	17	hours.
In 1	„	6	„	In 1	„	19	„
In 2	„	7	„	In 1	„	20	„
In 5	„	8	„	In 1	„	21	„
In 3	„	10	„	In 1	„	22	„
In 2	„	11	„	In 1	„	24	„
In 5	„	12	„	In 2	„	30	„
In 2	„	13	„	In 1	„	32	„
In 2	„	15	„	In 1	„	35	„
In 2	„	16	„	In 1	„	40	„

"In the major proportion, therefore, this period was under thirteen hours. The duration of labour too, it ought to be remembered, is calculated pretty nearly from the occurrence of the earliest signs of parturition in all the cases, and not from the commencement of active labour pains; otherwise its average duration would have been scarcely half what it appears in the table. This is a highly important fact of a practical kind." (p. 311.)

2d. Laceration rarely takes place in first labours, but most frequently in second, third, fourth and fifth. In Mr. Roberton's statistics of 30 cases, there was only one which occurred in a first pregnancy.

3d. The uterus (in the cervix) seems to give way with nearly equal readiness in all parts of its circumference.

4th. The kind of deformity of the brim which particularly disposes to this accident, cannot with certainty be stated. A narrowing of the conjugate diameter from a projection of the promontory, is numerically the most frequent. And next to this, the sharpening or irregularity of the anterior part of the brim from the inner edge of the pubes.

The sign of impending danger in these cases which Mr. Roberton thinks the best pronounced, is a feeling of crampy pain and tenderness on pressure in some particular part of the lower abdomen; and he explains the cause of the crampy pain by referring to a case in which it occurred as the result of the cervix being held by a "vice-like grip" between the head and the brim.

The practice which Mr. Roberton counsels when laceration threatens, is to procure a consultation, if at all practicable, and then to determine whether there is space enough—allowing for a moderate compression and moulding—for the head to pass. If so, to watch the case attentively, to apply a binder, and, perhaps, to raise the caught lip of the uterus. If the space be deemed insufficient, and especially if the child be dead, to have recourse to embryulcia. The practitioner must be guided by the effect of labour upon the head, as to the length of time he may leave the case to the natural powers. If the head and the cranial bones deepen in their descent into the pelvic cavity with each pain, the progress is favorable; but if the scalp only bulges out, or the head after the pain is loose and retires under the pressure of the finger above the brim, instrumental aid will probably be required, and the election will be between the forceps and craniotomy.

How to Turn with the least suffering to the Mother.—The three indications which are to be attended to for this purpose, are:—1. To suspend or allay uterine contraction. 2. To render the os uteri dilatable, if need be. 3. To prepare the vagina for the ready admission of the hand and arm. The first two indications are, in Mr. Roberton's judgment, best fulfilled by venesection and laudanum. Chloroform, too, by producing insensibility to pain and muscular relaxation, is spoken of as invaluable. For the last indication Mr. Roberton recommends, that the vagina be conveniently filled with lard, which is far more effectually lubricating than the mere anointing the hand of the operator, and is a great protector of the soft parts, and diminishes the irritability of the sphincter vaginæ.

It is a question in which practitioners differ, as to which hand should be used in turning; and Mr. Roberton's recommendation is, that the "hand should correspond to the side of the body on which the patient is lying." In this we quite coincide, and it has been a rule with us for years past. We quote Mr. Roberton's description:

"After anointing his left arm, he sits down, or kneels behind his patient; and folding the fingers and thumb together, in the form of a cone, he slowly and gently passes the hand in the axis of the outlet, until the wrist is embraced by the os externum, when the sense of distension and pain, if such there has been, ceases. In this manner the hand may be introduced, with little suffering to the patient; but in no other without acute pain, spasmodic action of the passage, and bearing down efforts. When, instead of the left, the right hand is used, the patient being on her left side, the moment it has passed, the flexures of the wrist and of the palm are found not to correspond with the axis of the uterus. In order to obviate this, the fore-arm has to be depressed against the perinæum, an effort that not only gives pain, but excites, except in tranquil relaxed subjects, the powerful opposition of the levator ani, which strives to carry the arm into its first position, that is, into the axis of the outlet. Moreover, whoever has practised with the right hand must have remarked how difficult it is, even after the hand is in the uterus (if the uterine action is powerful) to turn the palm towards the child, so as to grasp any part of it. Occasionally, too, there is difficulty in passing the *right* hand sufficiently high to reach the feet: for, in carrying it onwards in the womb, he necessarily strives to bring the forearm as nearly as possible in a line with the hand—a manœuvre opposed, as I before mentioned, by the levator ani. In operating with the left hand, it is obvious that few of these difficulties will be experienced. It is true, when the feet of the fœtus lie, as they commonly do, in the anterior part of the womb, which is readily discovered by the palm of the presenting hand being towards the abdomen of the mother, the operator may have slight difficulty, especially if there be much uterine action, in turning his left hand round to grasp the feet. In practice I have not found this a difficulty of any moment." (pp. 329-30.)

Mr. Roberton agrees in the practice of Dr. Radford, which is, we believe, very generally adopted, to bring down one foot; but he cautions the inexperienced practitioner against concluding, that because the foot is seized and brought into the vagina, or even to the external parts, the fœtus is turned. On the contrary, it may still remain transverse, and the shoulder be forced lower by labour-pains; and Mr. Roberton says, that he has known three instances in which great ultimate difficulty to the operator, hazard to the mother, and destruction of the life of the children, resulted from ignorance of the fact. When the *knee* is at the os externum, turning *has* been effected.

Causes of Prolapsus of the Funis.—The manner by which, in the mechanism of parturition, this accident, so hazardous to fœtal life, is prevented, is by the subsidence of the uterus before labour, and the sinking of the fœtal head over its lower segment, so that when the liquor amnii escapes, the head so occupies the lower part of the uterus as to prevent the escape of the funis, as well as of a reserve of the liquor amnii, which gathers in the upper part of the uterus, and prevents undue pressure of the body of the child. Anything which interferes with this provision, may allow the funis—unless it be coiled round the fœtus—to float down into the vagina with the escape of the water. Mr. Roberton details 19 cases of this complication, and refers to 22, in all of which the inlet of the pelvis was not properly secured. If the head presented, it was not in the brim, but above it; the pelvis, in some cases, was narrow, and the head could not enter; whilst, in others, the presentation was preternatural (the feet, the hand, or the shoulder), allowing the funis to pass between or beyond them. In 6 of these cases, the membranes ruptured when the patient was either in a standing or sitting posture.

The practice adopted by Mr. Roberton included the usual resources of turning, raising the funis above the head, and forceps delivery. Out of the 22 cases, the children were still-born in 15.

A chapter on the best mode of securing the speedy expulsion of the placenta will repay perusal. The practice of overlapping the fundus with the hand, and pressing it with movements of the expanded fingers, has not novelty, at least, to recommend it. Mr. Roberton's general directions for the management of this stage of delivery are quite sound, and his criticism of Dr. Gooch's case of a peculiar form of hæmorrhage, satisfactory.

Secondary Uterine Hæmorrhage.—Fourteen instances of this accident are related, which are intended to illustrate one kind of case, to the exclusion of other sources of secondary hæmorrhage. A woman has had a favorable confinement—the expulsion of the placenta not being followed by bleeding; and yet, after one, two, three, or even four weeks, is seized with uterine hæmorrhage. The following is Mr. Roberton's summary:

"Concerning these fourteen cases of uterine hæmorrhage, it is worthy of remark, that in none had the labour preceding been severe; nor does it appear that in any the placenta was extracted by the hand, or that there was much hæmorrhage accompanying or immediately following the expulsion of the placenta. In one case, the hæmorrhage occurred on the seventh day after delivery, in two on the ninth, in two on the tenth, in one on the eleventh, in one on the twelfth, in two on the fourteenth, in two on the sixteenth, in one on the nineteenth, in one on the twenty-second, and one on the twenty-seventh. In five of the fourteen cases there was a single attack only of hæmorrhage; in the remaining nine, hæmorrhage occurred oftener than once." (pp. 374-5.)

In none of these cases was the hæmorrhage fatal; and Mr. Roberton relates one instance only, furnished to him by Mr. Windsor, in which a female died from a sudden hæmorrhage one month after delivery; but, as there was no *post-mortem* examination, and no previous history is related, the case is valueless in relation to this subject.

Mr. Roberton does not think that this species of secondary hæmorrhage arises from relaxation of the uterus, or from patients getting up too soon; but he inclines to regard it as an expression of a menorrhagic diathesis.

He speaks well of ergot of rye, in small repeated doses as a remedy; but he does not refer to gallic acid, which, in some cases under our own care, has been of great value.

The Cases and Observations in illustration of the Signs of Pregnancy, are just those which must have occurred to most practical men who have been extensively occupied in obstetric pursuits:

"My involuntary familiarity," writes Mr. Roberton, "with the subject of fancied, doubtful, and concealed pregnancy, has led me to lay down, at length, the following rules for my own guidance, the importance of which (commonplace though they may seem) my more experienced professional brethren will admit. First. Always *doubt* alleged pregnancy in the married, when there is a regular, periodical, bloody discharge, however slight it may be. Second. Always *suspect* pregnancy in the married, in the child-bearing period of life, whenever the menses are unduly absent. Third. *Suspect* pregnancy on being called to prescribe for the unmarried, in the rank of servant, when the menses have been suppressed for a couple of months." (pp. 408-9.)

It is, perhaps, only those who have been, or are, in the habit of being called upon suddenly to form a diagnosis on uterine disease, who can feel the full force of these observations. To doubt pregnancy when the catamenia are regular; to suspect it, in the face of the most seeming innocence, and as the most unlikely event, when they are suppressed, and to test the doubt and suspicion, without, perhaps, being at liberty to mention them, will frequently try to the utmost the tact and discrimination of the medical man. To bear the possibility of pregnancy in mind, and not to let the attention shift away from it, until satisfactory proofs for or against it are adduced, is, in the majority of cases which come before us, almost all that is requisite, because the signs of pregnancy are generally clear and appreciable. But still the obscure cases come, which need the most careful attention, to form the differential diagnosis between pregnancy and uterine disease. Mr. Roberton's chapter not only adds nothing to our knowledge of the means of diagnosis; but he appears to us, as we read and realise his cases, to be a good deal behindhand in the resources which the modern accoucheur has at his command. We have no mention of kiestin, or of the feel of the developing uterus in the even swelling of its anterior wall, as signs in the early months; nor of fœtal auscultation, which, in nineteen cases out of twenty, will dissipate all doubt in the latter months. There is an instructive case at the end of this chapter, in which a large ventral omental hernia was mistaken for an extra-uterine pregnancy, and an exploratory incision made.

The Puerperal State and its Dangers.—Mr. Roberton draws an interesting and instructive picture of the daily life of active toil, which the wife of the artisan and labourer leads in Manchester. Her incessant round of multifarious duties from five in the morning until night, is not interrupted by pregnancy, and only partly suspended after delivery; on the fifth day after which event she is about her usual avocations. "And with regard to parturition and the after-recovery in these women, when, a number of years ago, a government commissioner was in Manchester, inquiring into the state of the Irish portion of the inhabitants, he examined several of the more intelligent midwives of the Lying-in Hospital; and the conclusion he and myself arrived at, from the information thus obtained, was, that probably about one only in from seven to eight hundred of the patients of the charity (supposing no epidemic puerperal fever prevailed) died in labour or from the consequences. This conclusion, of course, is somewhat conjectural; but I am persuaded, from long observation, we made a near approximation to the truth." (p. 435.)

A more precise statistical inference, regarding the exemption of the poor from the dangers of parturition, as compared with the classes above them, is given by the author, and embraces ten years, ending in December, 1849. In the township of Hulme, representing a great labouring community, 11,001 deaths were registered, 56 of which were in childbed, or in the proportion of 1 in 196½ of the total deaths. In the townships of Moss-side, Cheetham, Broughton, and Crumpsall, composed, as largely as any that could be found, of the middle and affluent classes, the deaths were 2778, and in childbed 33, or 1 in 84 of the total deaths. It is, however, when epidemic puerperal fever prevails, that the dangers of the poorer classes and the comparative exemption of the affluent become strikingly apparent. Mr. Roberton refers to an outbreak of puerperal fever at Manchester, in the spring of 1831, when the mortality was very great, although, from the absence of registration at the time, no accurate account of the numbers lost was kept. "In company with my colleague, Mr. Fawdington," says Mr. Roberton, "I was in the habit of wandering till midnight, and sometimes later, from house to house, visiting patients of the Lying-in Charity stricken by this malady."

In this epidemic, one of the midwives appeared to have been the direct channel of communicating the disease to a frightful proportion of her cases, no less than 16 women, out of 30 whom she attended, having caught it and died. The medical officers suspended her from further attendance. This is only one more out of the numerous instances of the spread of this frightful malady, through the medium of a healthy person. In this epidemic, Mr. Roberton says, that phlebitis, although diligently sought for as a morbid appearance, was not detected in a single instance. Peritonitis, more or less severe effusion into the pleura, with softening and disorganisation of the ovaria, were the usual appearances.

"The cases of puerperal fever, according to my observation, were resolvable into three classes; first, those—the most numerous, certainly—in which no medical treatment was of avail; where the pulse was 140 and upwards, resembling, in the most striking manner, the pulse when rupture of the uterus has taken place in labour; and where the heat of the surface never rose to the natural standard. Second, those where leeching, calomel and opium, blisters, and other counter-irritants were indicated, and occasionally proved successful. Third, those cases in which bleeding by the lancet, owing to the complete development of the heat of the body, the acuteness of local pain, and the distinctness and comparative strength and hardness of the pulse, was clearly indicated; and which, when employed early in this variety of the disease, was almost uniformly successful. The latter class of cases became more prevalent towards the decline of the epidemic. During the first two months of its prevalence, on the contrary, few recovered." (pp. 443-4.)

For twenty years, though there have been occasional cases of sporadic puerperal peritonitis, the complaint has never returned in Manchester in its epidemic form.

The last subject which our space permits us to notice, is the use of *Anæsthetics in labour*. Mr. Roberton's views upon this subject are sensible and judicious, and, so far as we can judge, represent very much the opinions which prevail amongst unprejudiced obstetricians. He thinks that its indiscriminate use in natural labour does not admit even "plausible justification." He mentions a case or two in which labour was clearly a time of happiness to the females, and he well puts it, that "to induce a loss of consciousness in cases like these would plainly be to diminish pleasure and enjoyment in the endeavour to assuage pain." There are some women, too, who have a dread of losing consciousness, and Mr. Roberton recognises most properly the wish of the patient when expressed against it, as a reason for its non-employment. His conclusions are thus summarily expressed.

"Where there is harassing metastatic labour, the suffering from which is often all but intolerable, situated, as it is, in the abdominal muscles, the back, the hips, often in the sphincter ani, and occasionally in the neck of the bladder, chloroform, given from time to time, to produce intervals of sleep, answers infinitely better than laudanum, as it does not interrupt the progress of the labour,—rather the contrary,—and is far more immediate and certain in the relief it gives.

"In reverse position of the head, the most painful of any; and this not alone in first labours. Here chloroform, by its remarkable power in relaxing the passage, would be useful, independently of lessening suffering.

"In the last stage of a first labour, when the preceding stages have been long, and the patient is jaded and melancholy, chloroform is allowable, even if only to cause one deep, refreshing sleep. Its relaxing power, with respect to the passage, has been already referred to.

"Generally speaking, in forceps, and in other kinds of instrumental delivery.

"In turning, when the waters have been for some time evacuated, and resistance, both in the passage and in the neck of the uterus, is to be expected, chloroform is more serviceable than in any obstetric operation whatever.

"In extracting the placenta, when there is spasm of the uterus, *i.e.* hour-glass contraction.

"Not more than a drachm, that is, a teaspoonful, should be administered at once; and this to be inhaled, at first, with plenty of atmospheric air. I have known a larger quantity, suddenly given, rather alarmingly reduce the frequency of the pulse.

"A fasting stomach, moist skin, expended excitability, and a subdued circulation, favour the benign influence of chloroform; hence it acts best when administered *late* in the progress of labour." (pp. 463-4.)

Our readers will notice from the character of our remarks on the practical Essays of our author, that our task has not been one of critical disputation; but of general acquiescence in his sound, we had almost said, sedate views. We feel throughout, as we have before hinted, that there is a stamp of age about it, which makes us almost wish that we had known it ten or fifteen years ago. Still we are thankful and happy to meet with it now, and to acknowledge a respectful obligation to a provincial accoucheur of candour, worth, and integrity. It is a book which will bear appealing-to on all points which are stated in it, with the authority of its truthfulness, and with the great merit of the author's industry, more perhaps than of his intellectual acuteness. There is no man who is in earnest about his duties as an obstetric practitioner, but will read it with pleasure and profit; and there is no one, however high may be his position in the profession, and however anxious his hopes may be of doing something for its advancement, who might not gracefully yield to the spirit of unpretending modesty which pervades and adorns it.

ART. XII.

Principles of Physiology, General and Comparative. By WILLIAM B. CARPENTER, M.D. F.R.S. F.G.S., Examiner in Physiology and Comparative Anatomy in the University of London, &c. &c. Third Edition.—*London*, 1851. 8vo, pp. 1098. With 321 Wood-Engravings.

THE general character of the treatise, of which the goodly volume before us constitutes a new and greatly-enlarged edition, is sufficiently well known, we presume, to our readers, to have rendered it unnecessary for us to have occupied much space in directing their attention to it, had it been *merely* a new and enlarged edition. But it comes before us in some degree with the claim of a new work; as will be apparent from the following extract from the author's preface.

"The second edition of his 'Principles of General and Comparative Physiology' having been for some time out of print, the author commenced, about two years ago, the task of preparing for the re-appearance of that work; in the expectation that, although much addition and modification would be requisite, in order to embody in it the most important of those discoveries in which the Physiological researches of recent times have been so fertile, yet that it would be found possible to interweave these with the original matter, in such a mode as to preserve the unity of the whole. On proceeding with his task, however, he soon perceived that it would be impossible to do it the justice he desired, without putting aside his former work altogether, and entirely reconstructing the treatise; only employing the materials of the original, where they might chance to be appropriate to his purpose. And thus it happens that the present work may be considered as essentially a new one; since, out of the 1080 pages of which it consists, not above 150, or less than one-seventh, belong to the previous edition. The author hopes that this fact may be admitted as evidence of his desire to spare no pains, in order to render his treatise a faithful exposition of the present state of physiological science; and that it will be deemed a sufficient justification of the slight change, by which he considers that the title has been made to represent more appropriately the present character of the work." (p. vii.)

We feel it right, therefore, to give our readers some opportunity of judging for themselves as to its scope and character.

The author further informs us, in his Preface, that "notwithstanding this almost complete renewal of the *materials* of his Treatise, he has found no important modification to be requisite in its *plan;*" for, in fact, he had framed his physiological system as if with an anticipation of subsequent discoveries. The reader of the first edition, published in 1839, will be at no loss to discern that the leading idea of the work was that of reducing every form of organic structure, and every functional change, to its simplest expression; and that the lowest vegetable organism, consisting of a single cell, was continually recurred-to as the starting point from which to trace out the essential character of each physiological action, through its successive complications in the higher forms of vegetable and animal life. In no other treatise, that we are aware of, had this been even hinted at; and we have high authority for saying, that the introduction of this method of study, and the elevation of the comparative physiology of Plants to the rank of an integral portion of general physiology, made this publication the commencement of a new era in the science. Certain it is, that much of what has since been done to extend and consolidate it, has been in the path thus opened up; so that, again to use the author's language, "the 'cell-doctrine' of Schwann and Schleiden, perfected and extended by the labours of the numerous observers, who have subsequently prosecuted the same line of research, has fallen (so to speak) into its appropriate place, and now serves as a complete confirmation of the principles which the author had previously propounded." (Preface.)

It is obvious from the list given by the author in his Preface, of the doctrines which he regards as more particularly his own, that what he has himself done for the advancement of Physiological Science has been chiefly in the way of reasoning upon the facts collected by others. Such, for example, was his application of the doctrines of Reflex action to the Nervous System of Invertebrated Animals, contained in his Prize Thesis for 1839; the validity of which is now universally recognised, we believe, by Physiologists, although it is not always attributed to its original author. Such, also, is his application of the "Correlation" idea to the Vital Forces, and to their relation with the Physical, of which we gave a sketch in our last number. Several other doctrines, first advanced by him (some of which we shall presently notice), are, in like manner, based upon the observations of others; but are not the less new in themselves, or less likely to take their due rank in Physiological Science. One important original investigation, however, has been entirely worked out by himself, namely, the inquiry into the Organic Structure of the Shells of the Mollusca, Echinodermata, and Crustacea, which was published in the Reports of the British Association for 1844 and 1847.

The Treatise is divided into Two Books, of nearly equal extent; the first being the "General," and the second the "Special and Comparative" Physiology. The following are the titles of the Chapters included in the First Book:—I. On the Nature and Objects of the Science of Physiology. II. Of the General Characters of Organised Structures. III. Of the Nature and Conditions of Vital Phenomena. IV. Of the Component Structures of Organised Fabrics. V. Of the Distinctive Characteristics of the Vegetable and Animal Kingdoms. VI. General View of the Vegetable Kingdom. VII. General View of the Animal Kingdom. VIII. On the General Plan of Organic Structure and Development.

We do not intend to give any regular analyses of these chapters; but shall make a few selections from some of them, that may serve to show the mode in which their subjects are treated. As the first two chapters do not afford us any passages capable of such isolation, and as we have already brought under the notice of our readers the views which our author has recently enunciated respecting the "Nature and Conditions of Vital Phenomena," we shall pass on to the fourth, from which we shall, in the first place, extract a statement, that will show how much closer is the alliance between the Plant and the Animal, in regard to the essential composition of their primary tissues, than has been until recently supposed. In describing the vegetable cell, the author remarks:

"Although we have hitherto spoken of the *cell-wall* as a simple membrane, yet it is now well known to be made up, in most if not in all instances, of two layers of very different composition and properties. The inner of these layers, which has received the name of 'primordial utricle,' appears to be the one first formed, and most essential to the existence of the cell; it is extremely thin and delicate, so that it escapes attention so long as it remains in contact with the external layer, and is only brought into view when circumstances occasion its separation from this; it seems to consist of an azotised compound, probably of an albuminous nature; and it appears to participate actively in the vital operations of the cell. The external layer, on the other hand, though commonly regarded as the proper 'cell-wall,' seems to be generated on the external surface of the primordial utricle, after the latter has completely enclosed the cavity and its contents, so that it cannot be regarded as essential to the cell; it is usually thick and strong in comparison with the other, but it may possess various degrees of consolidation, from mere mucus to a firm and tenacious substance; it is composed of cellulose, a substance nearly identical with starch; and it does not appear to take any active share in the vital operations of the cell, its principal office being to protect, locate, and isolate the matter it contains. This external layer may consist of many laminæ, the results of successive deposits from the surface of the primordial utricle; but it still usually remains readily permeable to fluid, although no pores can be distinguished in it, even under the highest magnifying power." (pp. 88-9.)

Hence it appears that the *real* cell-wall of the Plant has the same albuminous composition as that of the Animal; and that the envelope of cellulose by which it is generally invested has no more to do with the cell itself, than has the envelope of chondrine which encloses and connects the cartilage-cells of the latter, or than that of horny matter which constitutes the firm intercellular substance that gives tenacity to the otherwise delicate cellular residue left after the decalcification of certain shelly structures.

On the minute structure of the Vegetable and Animal tissues, we do not find any new observations of importance; and it is obvious that the limits imposed upon the author have prevented him from entering into many details that find their appropriate place in treatises of less comprehensiveness. We would direct the attention of our readers, however, to some interesting observations, too long for extract, which present themselves under the head of "Transformation of Tissues;" wherein a parallel is sketched out between the histological metamorphosis observed in ascending the scale of organisation, and that which is witnessed in the successive stages of embryonic development, this parallel being, for various reasons, most complete in the vegetable kingdom. We here find a doctrine explicitly stated, with regard to the real character of the lower tribes of Infusorial Animalcules, which has been hinted at by many preceding writers, but which has not been, so far as we know, so clearly expressed before:

"Many of the simpler forms of (so called) Animalcules must be probably regarded as nothing else than simple cells, which are endowed by their ciliary appendages with the power of free locomotion, but which contain no variety of organs, and multiply themselves (like other independent cells) by spontaneous fission. Now such beings cannot be properly compared to any of the higher Animals, in the fully-developed state of the latter; but they do present a very close correspondence to their earliest embryonic condition. For the life of the highest commences in a single cell, in which not the least vestige of its future organs or tissues can be traced; and this cell speedily multiplies itself by a process of binary subdivision, until a homogeneous mass of cells is produced, bearing a very close resemblance to some of those composite clusters of Animalcules, which are produced in like manner by the repeated bi-partition of a single individual." (p. 177.)

The parallel was first drawn, we believe, by Dr. Barry; but, among those who have since noticed it, these Animalcules have been still described as possessing sensibility, and as executing their movements under the guidance of an intelligent will, or, at any rate, of a consciously-exerted instinct. We are satisfied that such an assumption is altogether illogical; and that, so long as it is the fashion to interpret the actions of the simplest organisms by those of the most complex, instead of using the former to enable us rightly to comprehend the latter, we shall have no sound physiology. This part of the subject is more fully discussed, however, in the ensuing chapters, in which the author applies himself to the determination of the distinctive characters of the Animal and Vegetable kingdoms; and, as the question is one of much interest, and has recently been the subject of much debate, we shall take this as a sample of the mode in which the higher departments of general physiology are discussed in the present work. We may premise, however, that some naturalists of our time assert that no real distinction between the two kingdoms exists in nature; others, that there must be an intermediate kingdom for the reception of a group of organisms that strictly belongs to neither; whilst there are others who yet sustain the antiquated notion, that there are beings which are Vegetables in one part of their lives, and Animals in the other.

Dr. Carpenter first inquires into our ideas of a *typical* Plant and a *typical* Animal; and he comes to the conclusion that the existence of consciousness and the power of spontaneous motion must be regarded as the chief attributes, which must be superadded in our "idea" of an Animal to those which, being involved also in our "idea" of a Plant, are therefore the characteristics of organised beings in general; so that, if we always possessed the means of determining where consciousness and spontaneity *do* and *do not* exist, we should have comparatively little difficulty in drawing a definite line of demarcation between the two. "But," he continues,—

"We have no other means of judging of the presence or absence of these psychical endowments, than by studying the *movements* of the beings whose character we seek to determine; and it is frequently impossible to say, with anything like certainty, from what source these movements proceed. This difficulty is increased by the fact, that the most remarkable movements exhibited by undoubted Plants are seen in a group whose lowest forms present the nearest approximation to the simplest types of the Animal kingdom; and hence it has happened that beings have been invested with the attributes of Animality, whose subsequent history has clearly indicated their Vegetable nature. On the other hand, there are beings to which, from the absence of sensible movements, we should be disposed to refuse the endowment of consciousness; although the general characters of their structure would lead us to suppose their place to be rather in the Animal than in the Vegetable Kingdom. Such beings, however, present a strong resemblance to the embryonic or transitory forms of higher types of organisation; and as the (presumed) absence of consciousness in the early embryo does not lead us to exclude it from the Animal Kingdom, so it should not warrant the exclusion of a class of beings, the *tendency* of whose development is the same with that of undoubted animals, although the higher stages of that development are never attained. Here, as elsewhere, it happens that in the separation and circumscription of really 'natural' groups, we are obliged to leave a margin around each, for the reception of forms to which the definitions that we frame to express their most characteristic features are not applicable; and in the assignment of such 'aberrant' forms to one or the other of these groups, we must be rather guided by their general resemblance to those which display its typical characters, than by the degree in which they may themselves possess them. Thus, when we meet with beings possessing the general structure of Plants, and corresponding with them in the mode of their development, we are justified in referring them to the Vegetable Kingdom, although their movements may bear a strong resemblance to those of Animals. On the other hand, when we encounter beings possessing the general structure of Animals, and corresponding with them in the mode of their development, we seem justified in referring them to the Animal Kingdom, even if we never witness any such movements in them as can be fairly considered indicative of consciousness." (pp. 182-3.)

According to this view, then, although the presence of consciousness is the most important feature of the Animal kingdom, taken as a whole, yet it is not that on which the most satisfactory diagnosis can be founded; and we must look elsewhere for characters of more ready applicability. These may be found, in the Author's opinion, in the chemical composition of the doubtful fabric, and in the mode in which it acquires its food; provided that due discrimination be employed.

"If its cell-walls and fibres be found to consist solely of albuminous or gelatinous matter, the presumption is strongly in favour of its Animal nature. If, on the other hand, the tissues should be chiefly composed of cellulose, the presumption is equally strong in favour of its Vegetable character. In the application of such a test, however, great discrimination is required; for, as already pointed out, albuminous matter is not merely stored up in the cell-cavities of Plants, but even forms the most essential part of their cell-walls; whilst, on the other hand, it is quite certain that cellulose (contrary to what was formerly maintained) may be an integral part of the bodies of certain animals. A distinction is to be drawn between the cases in which, as in the *Corallines* and *Nullipores*, it forms part of the proper cell-walls; and those in which it is merely deposited within or around the cells, as happens in certain Compound Tunicata whose food consists largely of starchy matters, the cellulose appearing to be stored up in certain parts of their fabric, just as fatty matter is in other instances. Thus, although the mere presence of starchy substances in the fabric is not of itself a sufficient proof of the Vegetable nature of a doubtful being, it may be held to be so when there is no distinct proof to the contrary, and when the starch is found as an integral part of the solid fabric, and is generated by the being itself, instead of being a mere deposit derived from external sources.—This leads us to consider, in the *second* place, the inferences derivable from the mode in which the nutritive materials are taken in. For if the aliment of a being whose other characters leave its place doubtful, be *formed* by it at the expense of carbonic acid, water, and ammonia,—as is indicated especially by its decomposition of carbonic acid, and its liberation of oxygen under the influence of light,—there is a strong presumption of its Vegetable nature: whilst, on the other hand, if it be dependent for its support upon organic compounds previously formed by other beings, and especially if these be received into internal cavities for the purpose of digestion, the presumption is equally strong in favour of its Animal character. Here, too, discrimination is needed; for Plants may seem to live at the expense of matters previously organised (as is the case with the whole group of Fungi), when they really only thrive by the decay of these, taking in the carbonic acid and ammonia which are thus set free; whilst, on the other hand, there are Animals which appear to subsist on inorganic matters, such as sand or earth, although they are really supported by the small quantity of organic substances which these may include." (p. 191.)

Now it is quite true that many *reputed* Animalcules possess the power of decomposing carbonic acid under the influence of light; but it is also certain that the tendency of microscopic research has been recently to refer these, on other grounds, to the vegetable kingdom. The most active movement, effected by the agency of cilia, cannot be admitted as any evidence of animality, since we know that the 'zoospores' of many undoubted

Algæ are equally active, their propulsion being effected by the very same instruments; and when no other proof than this can be given that a certain organism is an 'animalcule,' it seems to us obvious that agreement with the vegetable kingdom in a fundamental condition of its organic existence, ought at once to determine its true place to be on that side of the boundary.—It is curious to observe how strongly the current of opinion is now taking this direction. The authority of Ehrenberg, who withdrew from the domain of Botany a very large number of the doubtful forms, to find a place among his "Infusionsthierchen," for a long time bore down every other; so great was the weight attached to his character as a most industrious and accurate observer, and as a sagacious and discriminating reasoner. But gradually that authority has been undermined by the labours of the vast number of observers who have devoted themselves to this field of inquiry; and it now becomes apparent that the whole work has to be done over again, in order to complete what Ehrenberg has left undone, to correct much of what he was supposed to have done, and thus to afford data for satisfactory generalisation on points which he is found to have determined under the influence of strong prejudice, and, not unfrequently, in the very teeth of evidence. The consequence has already been, the transfer of several important groups (including most of the so-called "fossil-animalcules") back to the Vegetable kingdom, from which he removed them; and the probable allocation in the same division of a large proportion of those so called Polygastrica, which are distinguished by the greenness of their colour, notwithstanding that some of these have hitherto ranked among the most indubitable "Animalcules." What will our readers say, for example, if the *volvox globator*, so familiar to every microscopist, should prove to be a plant? Yet such may now be considered as all but demonstrated. We find the following notice on this subject in a subsequent chapter of Dr. Carpenter's book.

"It is not by any means certain that the *Volvocinæ* are not to be transferred to the Vegetable Kingdom. Their green colour leads to the suspicion that they decompose carbonic acid; and the stomachs described by Ehrenberg in the component Monads, are not more distinct than the stomachs which he has represented as existing in several other beings, whose Vegetable nature is now generally admitted. It is considered by Braun, who has paid much attention to the development of the inferior Algæ, that the *Volvocinæ* are of the same type with certain 'Zoospores,' which become composite by fissiparous multiplication." (p. 251.)

We have recently seen a series of preparations by Mr. Williamson of Manchester, who has paid much attention to the development of the Volvox, which leave not the slightest doubt in our minds that the entire hollow sphere is originally composed of cells, formed by the multiplication of a single one; and that these cells are of the same essential character with those of the inferior Algæ generally; it being a part of their nature to secrete a great thickness of pellucid mucilaginous matter around the primordial utricle, and thus the green utricles, which are originally in close approximation, become separated from each other by its intervention, still remaining connected, however, by certain thread-like prolongations, which Ehrenberg has erroneously described as vessels.

The chapters which contain the general view of the Vegetable and Animal Kingdoms occupy a far larger proportion of the Treatise than they did, in the previous editions, together making, in fact, not less than 365 pages. Had he been able to refer to any concise outline that suited his purpose, the author would probably have forborne to enlarge his work by the allotment of so much space to what must be considered rather as Natural History than as Physiology; but to those who desire to become acquainted with those types of organic structure, which are of special interest to the Physiologist, but which are insufficiently or not at all described in elementary treatises, without going through the vast mass of (to him) useless matter contained in the larger systematic works on Botany, Zoology, and Comparative Anatomy, these outline views, filled-in where most requisite for the purpose intended, seem almost indispensable. Novelty is not here aimed at by the author; a faithful sketch being all that he aims at presenting. And we shall therefore be extremely brief in our notice of this portion of his work.

We find the Animal kingdom subdivided into *five*, not *four*, sub-kingdoms; the fifth being termed *Protozoa*,—a designation which has been of late much employed by German writers, but which has been very variously applied. Dr. Carpenter limits them to those in which the very simplest type of form and structure presents itself, but which must yet be regarded as deserving a place in the animal kingdom, on account of their chemical composition, and their mode of obtaining their aliment, and also on account of their resemblance to the embryonic condition of higher animals; and he thus gives them a corresponding rank with those equally simple vegetable organisms, to which the name *Protophyta* has been given. The best idea of the characters of this group may be derived, he considers, from an examination of the *Amœba*, commonly known as the 'Proteus,' on account of the unlimited variety of forms which it may assume, but superseded in that designation by the well-known perennibranchiate Batrachian. This creature is a not uncommon inhabitant of fresh waters and stagnant infusions; and the following account of it, given by the author in great part from his own observations, contains so many points of interest, that we shall place it in full before our readers:

"At first sight, this creature seems to be nothing else than a minute particle of gelatinous matter; but its spontaneous changes of shape soon give evidence of its possession of vitality; and it becomes evident, on further examination, that it may be considered as a *simple cell*, of unusual dimensions (being sometimes, in the largest species, as much as 1-70th of an inch in diameter), analogous to that which constitutes each individual of the simplest Protophytes. Like these, it lives for and by itself, and it imbibes its nutriment through its cell-wall, having no oral orifice; but it possesses a motor power with which they are not endowed; and this power is obviously related to the character of the food on which it is to be supported. For, whilst *they* can obtain, wherever they may be situated, the materials required for their development, this Animal cell, being dependent upon matter previously assimilated, and not endowed with any means of drawing such matter towards itself, must go (as it were) in search of it. This movement is accomplished by the continual changes of form that take place in its body, the typical figure of which may be considered globular, but which may assume almost any shape whatever. The change of form seems due rather to actions taking place in the interior of the cell, than to any irritability of the cell-wall; for if the movements of an Amœba be attentively watched, the extension of the gelatinous body in any particular direction (so as to form one of the digitate prolongations) will be seen to be preceded by the setting of a current of the moving molecules within the cell, in that direction; to which current the protrusion of the cell-wall is really due. A continuation of the same current distends the prolongation, and the whole mass of the body is gradually carried onwards (so to speak) into it, so that its place in the field of the microscope is slowly changed. After a short time, the particles in the interior of the cell are again seen to move in a definite course; a new prolongation is thrown out, in the same or some other direction; and the body is again absorbed into it. Thus its locomotion is effected by the agency of the currents which traverse the interior of the cell, and which are obviously connected (as in the cells of *Chara* and other plants) with the organic processes concerned in its growth and maintenance; and it must consequently be regarded as entirely independent of any spontaneous control on the part of the individual, and cannot even be allowed to afford the least indication of consciousness. The nutrition of the Amœba is derived from animal and vegetable substances; but these are not received into an internal cavity (as affirmed by Ehrenberg), their enclosure within the gelatinous body being rather apparent than real. When the creature, in the course of its progress, meets with a particle capable of affording it nutriment, its body spreads itself over or around this, so as even to invest it completely; and the particle thus seems to be included in a digestive cavity, while it is really in contact with the external surface only, through which its nutritive elements are absorbed into the cavity of the proteus-cell. It is interesting to see such a creature thus manifesting the peculiar *nisus* of animal development; *making*, as it were, a stomach for itself, by wrapping its body round the alimentary matter, which it is not able to receive into its interior." (pp. 244-5.)

This account closely corresponds with that which Mr. Carter has given of the *sponge-proteus*, from his observation of the fresh-water sponges of Bombay. The fact that the change of form of the cell appears to be consequent upon the direction of currents in its interior, is one of great physiological interest, in reference to the production of the stellate, caudate, and other prolongations of cells, which are so frequent in the pigmentary, nervous, and other tissues.—Further, we have in this animal, as Dr. Carpenter remarks, just such a manifestation of animal *tendencies*, rather than of animal *powers*, as might be expected in beings that constitute the transition-group between the two kingdoms; and these tendencies are still further carried out in the three higher groups, which he comprehends under the general designation Protozoa,—namely, the true *Animalcules*, the *Rhizopods*, (corresponding with the group commonly designated as *Foraminifera*,) and the *Sponges*.

"Thus in the (so-called) *Polygastric Animalcules*, we seem to find the highest development which animal cells can attain as individuals; their nourishment being directly received into their interior by an orifice in the cell-wall; and the movements of the cilia with which they are beset, being adapted not merely for the propulsion of the body through the water, but also for the introduction of food through this orifice. But this is a type of structure, which does not seem capable of any further development; for, although we meet with forms among these Animalcules, which seem like sketches of fabrics much more elevated in the scale, we do not meet with any which present a decided approximation towards higher groups. So in the *Rhizopods*, we have composite fabrics, presenting a considerable regularity of form, which seem to have their origin in the gemmiparous multiplication of beings, closely allied to the Amœba in their general structure and mode of life, each individual still living for and by itself. In the *Sponges* and their allies, on the other hand, we meet with vast aggregations of proteiform cells, united into composite fabrics; of which the several parts, though almost precisely similar to each other, have yet a certain degree of interdependence; and through these we are led towards the true *Zoophytes*, in which the individuality of the component cells and fibres is merged (as it were) in that of those composite structures which are termed Polypes." (pp. 245-6.)

The section on *Entozoa* contains several facts and doctrines which will probably be new to most of our readers; we shall pass it over, however, on the present occasion, as we intend to take an early opportunity of reviewing in detail some of the more interesting amongst recent contributions to our knowledge on this subject, which differ in this from what had been previously brought together,—viz. that the history of their *development* is now made the object of most attentive research, and, when fully made out, has not merely removed many difficulties and cleared up many obscurities, but has revealed a new order of facts, of the most remarkable character.

A large proportion of this chapter is occupied with a general account of the organisation of the different classes of vertebrated animals; and in this is included an outline of the "Vertebral Theory" of Professor Owen, a description and figure of whose "Archetype Skeleton" is given in the introductory account of the vertebrata generally; whilst under the several heads of Fishes, Reptiles, and Birds, the chief modifications which this archetype undergo are pointed out. In treating of the cranium of the Mammalia, however, a different plan is followed; each bone being analysed, so to speak, and reduced to its proper elements in the vertebral system, which are generally found to be marked in the embryonic condition of the bone, by so many distinct centres of ossification. The whole of this exposition, as we are informed in the Preface, has had the advantage of Professor Owen's revision; and, consequently, it may be recommended with confidence to those who desire to gain a general acquaintance with the system, as presenting an authorised outline of it.

In the last chapter of the First Book, "On the General Plan of Organic Structure and Development," we find an exposition of the much-canvassed doctrine of *Unity of Composition*, in a form which, we believe, is perfectly consistent with the known facts of anatomical science, and which, even in its present imperfect development, enables these to be far more readily apprehended than they could otherwise be. In assigning to any particular being its place in the organised creation, our author remarks, we always proceed *from the general to the special;* for each group, in a perfect classification, should have its "archetype" or ideal model; and in proportion as we pass from the larger to the smaller groups, from those most comprehensive to those most restricted, should we find the general characters of the archetype presenting modifications of successively increasing degrees of speciality, until we arrive at those distinctions which are characteristic of the most restricted groups. All scientific classification, in fact, aims at the determination of such archetypes; and it is in helping us to the knowledge of what are the most general or fundamental characters of groups, and what are the more special and (so to speak) accidental, that the study of development seems likely to exert its most important influence upon zoological science. That the real history of the development of the higher organisms is *not* expressed by saying that they pass through forms or conditions which are permanent in the lower, was shown by Dr. Carpenter in his former edition. The true law is here, too, that of progress *from the general to the special*, as first propounded by Von Bär, and more fully set forth by Dr. Martin Barry; and this is fully elucidated by the general remarks on the subject contained in this chapter, and by the more detailed account of the evolution of the several organs and systems given under various heads in the Special Physiology.

The doctrine of the 'progressive development' of the organised creation peopling this earth, during successive geological epochs, has been, under one form or another, pretty generally held, from the time when a scientific knowledge of Palæontology began to prevail, until a comparatively recent period; for however few there may have been, who gave credence to the hypothesis of the actual transmutation of the lower species of one epoch into the higher species of another, yet still the fact of a *succession* of organic life is indisputable, as is also that of the *general tendency* towards a more elevated set of types as we come down in geological time from the remoter epochs towards the more recent. But when this general doctrine is carried into detail, it is immediately met by such a vast number of difficulties, that many palæontologists have of late altogether refused to acknowledge its validity; and the fact of the succession has of late stood by itself, no law or general expression of the phenomena having been proposed. By Prof. Agassiz and others, indeed, it has been maintained that the general plan of development of terrestrial organic life corresponds with that of the embryonic development of the higher organisms; but they have not been guided by a correct notion of what that plan is. Dr. Carpenter suggests that the law of progress from the more general to the more special, will here also be found applicable; for, as he remarks, "many indications present themselves that the types of each principal group first introduced were *not* the lowest, but that they presented *in combination* those characters which are found to be *separately* distributed, and more distinctly manifested, among groups that subsequently made their appearance." As an example of this law, he specially cites the very peculiar distribution *in time* of the several orders of the class Echinodermata. This class seems to have been first represented, during the Palæozoic epoch, by an order that combined some of the most remarkable characters of all the orders which have since shown themselves; and this order, *Cystidea,* must be regarded as superior in general organisation to the Crinoidea, notwithstanding that the greatest multiplication of the latter occurred at a later period. Moreover, such of the other orders as showed themselves during the Palæozoic period, exhibited a decided approximation to the cystidean type; and it was not until the secondary period, when the Cystidea disappeared altogether, that the most characteristic types of Crinoidea, Asteriada, Ophiurida, and Echinida presented themselves; these orders dividing among them (so to speak) the characters possessed in combination by the Cystidea, and carrying them out separately as the distinctive peculiarities of their respective types.—In the extraordinary multiplication of the Tetrabranchiate or Nautiloid Cephalopods, too, during the Palæozoic period, our author finds another example of the same general plan; for these seem to have occupied the place of two other groups which came into consequence as it dwindled away, namely, the carnivorous Gasteropods and the higher Cephalopods.—The class of Fishes presents many indications of the same kind; and it serves to indicate the difference between the view of Prof. Agassiz and that of Dr. Carpenter, that the former continually represents the 'homocercal' tail, of which the earliest fishes were almost invariably possessed, as being an embryonic character, because it is found at an early period of the development of existing fishes, whose tail subsequently becomes 'heterocercal;' whilst by Dr. Carpenter, the homocercal tail is considered as the "most general" character in the class of Fishes, because it is common to them all in some stage of their development; whilst the heterocercal is peculiar to certain sections of the fishes of later epochs.—It is in the Reptilian class, however, that we find the most curious exemplifications of this principle; for there is every reason to believe that this class, at one time, occupied the place, and performed the duties, of the whole series of air-breathing Vertebrates, including groups which represented Fishes, Birds, and Mammals respectively, and thus having a *more general* character than the class at present exhibits. These groups, which can in no degree be regarded as paralleled by any embryonic conditions of existing reptiles, subsequently gave place to those *more special* forms, which carry out most exclusively the reptilian type. And when, as Dr. Carpenter remarks, we look at the earliest forms of reptilian life of which we have any cognizance, we find them to present very remarkable combinations of the characters which are now distributed among different groups. Thus, at the very commencement of the secondary period, we meet with the extraordinary Labyrinthodon, which, whilst essentially a gigantic Batrachian, was in many respects formed after the Crocodilian model. So, again, the same formation contains the remains of Reptiles, which, while essentially Saurian in their general structure, had the horny mandibles, and probably many other characters, of the Turtle tribe.—It seems to us that there is a sufficiently strong *primâ facie* case in favour of this doctrine, to warrant its being systematically put on its trial. The one in place of which it is proposed, cannot stand a rigorous criticism for a moment; and it will be a strong indication of unity of design, if the palæontologist should be able to trace, throughout the long succession of geological epochs, and the repeated metamorphoses which have occurred in the types of organic life which prevailed during each, one harmonious plan, and that plan the same as the embryologist discerns in watching the development of any single organism.

The Second Book commences with a "General View of the Functions of Animated Beings, and of their Mutual Relations;" and the author then proceeds with each function in detail, according to the following plan. Under the head of "General Considerations," the essential nature of the functional change is considered, and the conditions on which it is dependent are set forth. The function is then traced, in its various stages of complication, from the lowest to the highest forms of the Vegetable kingdom, and its gradual *specialisation* pointed out; and a similar specialisation is traced in the history of the embryonic development of the higher forms of plants. The same course is then followed in regard to Animals; the function being first examined under its simplest conditions, and then traced upwards through the ascending scale of Animal existence, so as to show its gradual limitation to a certain set of organs, which, whilst becoming more and more distinct from the remainder of the organism, and more and more devoted to that purpose only, perform it with more and more of energy and completeness; and the progress of the evolution of these organs in the embryonic state of the higher types, being then exhibited, by way of showing the essential conformity in *plan* between the two series of phases. Such having been the scheme on which this portion of the work was originally constructed, we do not think it requisite now to give any detailed exemplification of it; since, although it is much more fully and elaborately carried out in the present edition, the readers of the former will here find no essential departure from the original plan. We may, however, notice a few of the more interesting facts, which we now, for the first time, encounter.

It was originally laid down by our author, that the development of the special circulating apparatus is proportional,—not so much to the limitation of the respiratory surface to a particular part of the organism, as stated by Cuvier and others,—as to the degree of restriction of the digestive cavity and of the power of absorption, whereby the parts imbibing aliment are removed from those requiring supplies. In Zoophytes, we find that no part of the organism is sufficiently far removed from the parietes of the digestive cavity, to prevent it from obtaining the requisite amount of alimentary matter, by imbibition through them; and in the Acalephæ and Trematode Entozoa, we frequently find the digestive cavity extended through the body in a very remarkable manner, so as to supersede the necessity of any special circulating apparatus. On the other hand, in the Echinodermata among the Radiated classes, and in all but the lowest Mollusca and Articulata, we find the digestive cavity no longer channelled out among the tissues, but constituting a distinct sac or canal, with which a large part of the body has no close proximity; and a special absorbing and circulating apparatus, of greater or less complexity, is interposed, to take up the nutrient materials from the walls of the digestive cavity, and to convey them to the parts which are destined to appropriate them. Now the very simplest condition of this apparatus would be that in which the general visceral cavity, instead of a distinct system of vessels, is employed for the purpose; and such has been observed in a group of animals, whose general type of structure would give them a comparatively high place in the scale, although, in the evolution of their apparatus of organic life, they present a remarkable degradation. We refer to the tribe of *Pycnogonidæ*, which, in regard to their habits of life, seem to be, among Crustaceans, something what the Sloths are among Mammals. They are found lying inertly upon sea-weeds, with their long, sprawling limbs spread out around the body; and when they change their position, it is not by walking, or springing, or swimming, but by fastening one of their hooked feet to some fixed point, and then drawing the body towards it by the flexure of the limb, just as the Sloth will do when placed upon the ground. Like the Sloths, again, these little animals seem naturally to live in situations where they can imbibe their food with the least possible trouble; for their mouth is suctorial, and it seems probable that their aliment consists of the gelatinous mucus covering the fronds of the sea-weeds on which they live. The mode in which the alimentary matter is conveyed to the tissues, constitutes the most curious feature in the organisation of this animal; and it is thus described by Dr. Carpenter, from his own observations, which coincide with those of M. de Quatrefages. We should mention, however, that the extension of the digestive cavity into the limbs was first noticed by M. Milne-Edwards.

"The mouth leads to a very narrow œsophagus, which passes backwards to the central stomach, situated in the middle of the thorax; and from the posterior part of this cavity, a narrow intestine passes off, to terminate at the posterior extremity of the body. From this central stomach, however, five pairs of cæcal prolongations radiate, one pair entering the feet-jaws, and the other four penetrating the thoracic legs, and passing along them as far as the last joint but one; when these cæca are distended with fluid, they present a series of contractions and dilatations, which correspond with the articulations of the limbs. That these curious organs are veritable extensions of the stomach into the limbs, and not mere secreting cæca, is proved by the curious peristaltic action which the transparency of the body permits to be observed in them; for they all contract and dilate alternately, so as to produce a continual flow of the contents of this curious digestive cavity from one part into another. Their membranous walls are covered, however, with a layer of brownish-yellow granulations; and these, which are scarcely perceptible on the wall of the central stomach, and not at all upon that of the intestine, may probably be regarded as a diffused and rudimentary condition of the liver. In the fluid which is transmitted from one portion of the digestive cavity to another, minute granular particles can be seen, which are probably of a fæcal nature. The digestive sac is freely suspended in the general solid cavity of the body, by occasional fibrous bands; and nothing intervenes between its external surface and the muscular lining of the crustaceous envelope. Thus a wide *lacuna* is left around it, occupying the greater part of the thoracic cavity, and also the hollow of the legs; and this lacuna is filled with a transparent liquid, in which are seen floating a number of minute transparent corpuscles, of irregular size. This fluid is kept in continual motion, not only by the general movements of the body and limbs, but by the more special and constant action of the different parts of the digestive sac; for, when the cæcum in any one of the legs undergoes dilatation, a part of the circumambient liquid will be pressed out from the cavity of the limb, either into the thorax or into some other limb whose cæcum is contracting; and when, in its turn, the first cæcum contracts, the space around it will be again filled with liquid, forced into it by the dilatation of the central stomach or of some other cæcum. There is no other circulating apparatus whatever; and it cannot be doubted that the liquid which fills the great cavity of the body must be regarded as the nutrient material derived from the digestive organs, that is, blood; and that its movement from one part of the body or limbs to another is to be regarded as a great lacunar circulation, no part of it being as yet circumscribed within distinct vessels, and no special organ of impulsion being developed. There is no special organ of respiration; but the aeration of the circulating fluid must be accomplished by exposure to the surrounding medium through the crustaceous envelope, which does not possess great density in any of these animals." (pp. 402-3.)

This condition of the circulating system is not so exceptional as it may at first appear; for the visceral cavity seems to be the sole channel for the conveyance of the nutritious fluid from the digestive sac to the tissues, both among the Bryozoa (which now rank as Mollusks) and the Rotifera; and even among the higher Mollusca and Articulata, it usually forms part of the circuit. The curious discovery that in Invertebrated animals, with probably no exception, the circulation of nutritious fluids is partly *lacunar*, the blood escaping from the vessels to meander through the interstitial spaces, and employing, even in animals as high as Cephalopods, the peritoneal and pericardiac cavities as great venous sinuses, is chiefly due to the researches of Professor Milne-Edwards. Generally speaking, the arterial circulation is truly *vascular;* but there is no proper system of systemic veins, the passage of the blood from the tissues to the respiratory organs taking place entirely through lacunar channels, and it being only whilst returning to the heart after passing through the gills, that it is again conveyed by proper vessels. As justly observed by Professor Milne-Edwards, there is no part of the circulating apparatus in the Vertebrata which can be properly compared with the lacunar portion of that of the Invertebrata, except the commencement of the lymphatic system; for that superfluous fluid, which, having transuded the walls of the capillary blood-vessels, is not appropriated by the surrounding tissues, appears to find its way through their interstices into the peripheral portions of the lymphatic system, whose origin has not yet been clearly made out. It seems almost impossible to contemplate the peculiar arrangement which has been described as prevalent in the Invertebrata, without perceiving that the branchial circulation must be maintained by some other force than the heart's action, which can no longer be imagined to communicate a *vis a tergo* to the blood, when this is meandering through the lacunæ of the tissues. In the Cephalopoda and higher Crustacea, as well as in certain among the Annelida, we find distinct contractile cavities (apparently of a propulsive character) at the base of the gills; but where these are wanting, there would seem to be no other sustaining power than that for which Professor Alison has always contended, as arising out of the changes to which the blood is rendered subservient in passing through the capillaries themselves.

Some of the most interesting novelties in the Second Part will be found in the Chapter on *Generation and Development;* and of one or two of these we may stop to take a brief notice. The general view of the antagonistic relation between the act of *sexual* Reproduction, or true Generation, and reproduction or multiplication by gemmation, is that which has been already expressed in our own pages; and it has received full confirmation from the researches of Mr. Huxley, who has had ample opportunity of studying the wonderful aberrant forms of the Acalephan class with which tropical seas abound, and who has succeeded, with the aid of the microscope, in unravelling their structure, and in making out their real character, to an extent that is really surprising. In fact, Mr. Huxley had arrived at precisely the same conclusions, when studying these creatures on the coast of New Guinea, that Dr. Carpenter had formed from the analysis and comparison of the facts previously known to him. He considers the *Medusæ,* whatever their degree of independent activity and self-nutrition, as nothing else than the detached generative apparatus of some polypoid organism, which, without the medusa-bud, cannot be regarded as a complete individual; and thus he holds, with Dr. Carpenter, that the polype-stock and free medusa, instead of being "alternate generations," constitute in reality but a single generation. He considers, in fact, that a metamorphosis will be required in our ideas, and, perhaps, also in our language, in consequence of our having formed our ideas of *individuality* entirely upon the higher organisms, in which the whole series of parts evolved as the product of a single act of generation, is so mutually dependent as to constitute but one individual. Among the lower tribes, this is the exception rather than the rule; for the same germ may be developed by gemmation into a great number of parts, which, being able to live independently of each other, commonly take rank as distinct individuals. These may be all equal and similar one to another, or one may be, so to speak, the complement of the other; and of both kinds of gemmation do we see examples among many of the lower tribes. For the generative apparatus to be separate as a distinct gemma, and to be made more or less self-supporting, is really a very common occurrence; and when looked at in this point of view, even the most aberrant and apparently complex forms among the Acalephæ are resolved into simplicity. They are really Zoophytic in their character, being composed of a repetition of a number of similar polype-like organs developed from each other by gemmation; and what shows that we must not regard each of these parts, but rather the entire stock, as the real *individual*, is that the generative medusa-buds are not developed from the polypoid bodies, but from the common stem which bears them. Mr. Huxley has proposed the term *Zooids* for these separable segments; and we believe that it might be adopted with advantage.

This gemmiparous power seems greater, as pointed out by Mr. Paget, in the inverse proportion to the grade of development which the organism has already attained; and consequently, in the higher tribes, we should expect that the reproductive nisus, at an early period, might be in some degree comparable to that which normally shows itself at a later period in the lower. Thus we find, in General Physiology, the probable explanation of certain Teratological phenomena, which have hitherto been a source of much perplexity. For, as Dr. Carpenter remarks:

"By the knowledge of these facts and principles, we seem justified in the surmise, that the occurrence of supernumerary or multiple parts is not always due (as usually supposed) to the 'fusion' of two germs, but that it may result from the subdivision of one; for if it be supposed that this subdivision has taken place when the developmental process has advanced no further than in a Hydra or a Planaria, it seems by no means impossible that each part might, as in those creatures, advance in its development up to the attainment of its complete form." (p. 873.)

This view has much to recommend and support it. The separation of the primitive germ-mass into two or more parts, does not seem a very uncommon phenomenon among the lower animals; and even where the complete organism is not multiplied by this process of fission, imperfect but temporarily-independent bodies are sometimes formed. The following are the very curious observations made by Nordnann on the embryonic development of *Tergipes*, one of the Nudibranchiate Gasteropoda.

"It is not unfrequently seen that some of the cells of the vitelline mass detach themselves from the principal cluster, become clothed with long cilia, and continue to move about actively within the egg, until the escape of the embryo. It is even affirmed by Nordnann, that they increase by partial subdivision, and that thus from a single detached cell may be produced a cluster having a very definite form, and furnished with long cilia, so as very strongly to resemble a parasitic animal. It has not been shown, however, that these bodies ever advance to a higher condition, or are capable of generating their kind; and the correct view is probably to regard them (with Vogt) simply as portions of the embryonic mass, exactly resembling those that form the ciliated lobes, which, being detached from the rest, preserve their vitality for an unusually long time,—such vitality, however, not being different in kind from that of an ordinary ciliated epithelium-cell, though greater in degree." (p. 949.)

Now Professor Simpson exhibited to the Physiological Section of the British Association, at its meeting in Edinburgh last year, two living subjects, and several preserved specimens, exhibiting an obvious attempt at the reparation of limbs, after their spontaneous amputation *in utero;* thus fully supporting the view, that the gemmiparous power exists, even in the human subject, during the earlier period of development; and justifying the surmise, that the primitive germ-mass might undergo such a complete subdivision as to produce two connected bodies, or that it might bud forth more than its accustomed number of appendages. It has always seemed to us a most extravagant supposition, that the least excess (for example, a double thumb,) must be due to the partial incorporation or fusion of a second germ; and if a second thumb or a sixth finger should bud forth from the hands, why should not a second arm with all its appendages bud forth from the shoulder, as in the case which has been recently noticed in the public prints; or why should not a second head be developed by spontaneous fission of the germ-mass at its cephalic extremity, or an additional trunk and lower extremities proceed from a single head by fission at the posterior part of the primitive trace, the anterior remaining single? These monstrosities are usually explained on a principle for which we have no warrant in any other part of the animal kingdom; whilst for their production by the abnormal occurrence of that process in man and the higher animals, which is normal in the lower, we seem to have a strong *à priori* probability. And to the whole class of "monstrosities by inclusion," such a view is obviously more applicable than any other; it being very easy to understand that the abnormal growth may be the result of an imperfect gemmation, commenced at a very early period; and this not less readily in the male subject than in the female. The occurrence of cysts containing hair, teeth, bones, &c., is really not very unfrequent; and their origin seems to us to find a ready solution in the view here propounded.

The inquiry into the history of the *Development* of Animals may be regarded as only now being brought into the prominence it deserves; and with respect to that of the lower Invertebrata, it may be said that we, as yet, know next to nothing. Unfortunately, it is only by a prolonged residence near the seacoast, and by patient and attentive watchfulness for many months or even years, that the history of even a single species can be fully made out. Among the recent contributions of this kind, one of the most curious is the account given by Professor Müller of the early condition of a species of *Star-fish.* The body first developed from the embryonic mass is a larva, of which little remains in the permanent structure; and the star-fish is subsequently developed out of a mass of unconverted yolk at its anterior extremity. The larva is of elongated form, and has seven arm-like appendages on either side, which are fringed with cilia, and its bilobed tail is furnished with the same appendages; by these it moves actively through the water, whilst as yet the mass at the extremity does not present the least indication of what it is permanently to become. Moreover, this larva possesses not only a mouth, but an intestinal canal and anal orifice of its own; and these have no relation to the mouth which the star-fish is to possess, this being afterwards formed by the thinning-away of the integuments at the part most remote from the attachment of the larva. The young star-fish, when sufficiently matured to maintain its own existence, is separated from the larva (which from its shape has received the name of *Bipinnaria*) by the forcible contractions of the connecting pedicle; and the larva continues active for some days after its detachment. Strange as this process may seem, it is not so great a departure from the plan of development with which we are familiar in the higher animals, as it at first sight appears; for, as Dr. Carpenter remarks,—

"We here find the portion of the yolk-mass that first undergoes organisation, converted into a structure which is destined only to possess a transient existence, and which disappears entirely by the time that the development of the principal part of the yolk-mass has advanced so far, that it begins to assume the characters of the permanent organism. This, however, is what takes place in the higher Vertebrata; for the structures first developed in the egg of the Bird hold nearly the same relation to the rudimentary chick, that the 'Pluteus' bears to the incipient Echinus or Ophiura, or the 'Bipinnaria' to the incipient Star-fish. The only essential difference consists in this,—that the development of these temporary structures proceeds so much further in the latter case, as to give them more the character of distinct individuals; and they are endowed with self-moving powers, whereby they are dispersed through the water in this stage of their existence, so as to prevent that accumulation in particular localities which would otherwise result from the comparatively sluggish habits of these animals in their adult condition." (p. 939.)

In fact, in the Mammalia, we may regard the fœtal portion of the placental apparatus as nearly corresponding, homologically, with the bipinnarian larva of the star-fish; and the separation of the former at birth may be considered as representing the detachment of the latter. In each case we find that the portion of the germinal mass which first undergoes organisation, is adapted to provide nutriment for that which is to be more permanent, rather than to be itself converted into it; and the early condition of the embryo is scarcely more unlike what it is afterwards to become, in the one case, than it is in the other.

We do not think it necessary to detain our readers with any account of our author's views on the offices of the Nervous system and organs of sense, which constitute the concluding portion of the work, since they have been already placed before the readers of this journal and of one of its predecessors; and we have only to add, in reference to the external characters of this volume, that it is one of the handsomest, best printed, and best illustrated, that has ever issued from the medical press. A large part of its 321 wood-engravings have been executed by Mr. W. Bagg, whose well-known reputation is a guarantee for their artistic excellence; and the author informs us that he has been careful to have them taken direct, where practicable, from the original monographs of the authors who have specially treated of the several subjects. To all these, reference is made in a separate list; and there are besides, numerous references in the foot-notes, chiefly to the sources of those new facts and doctrines which cannot be yet said to have become part of the common stock of physiological science. The author has followed the plan originally adopted by him, of numbering the paragraphs, and of making frequent reference, by means of these numbers, from one part of the volume to another. And when we add that there is a copious index, we think we have said enough to show that he has spared no pains to make the work available to those for whose use it is intended. We shall end, as we began, with an extract from his Preface, which we think, in justice to him, ought to be placed before our readers.

"The Author thinks it but due to himself to state, that as the almost constant occupation of his time in other indispensable avocations leaves him much less opportunity than he would desire for the prosecution of those original researches in which he feels the greatest satisfaction, so he trusts that the same plea may in some degree avail him in regard to any deficiencies which may be found in the present work. The whole of his disposable time has been devoted to it, for nearly two years, not merely without the anticipation of the slightest pecuniary reward, but under the certain loss involved in the relinquishment of other literary engagements of a remunerative character; and the sale of the entire edition will not do more than remunerate his liberal Publisher for the very large outlay which has been incurred in the production of the work." (*Preface*, p. x.)

ART. XIII.

The Wisdom and Beneficence of the Almighty, as displayed in the Sense of Vision. (*The Actonian Prize-Treatise for* 1851.) By T. WHARTON JONES, F.R.S., Ophthalmic Surgeon to University College Hospital.

THE subject selected for the Second Septennial award of the Actonian Prize, was the Physiology of any one of the Senses ; and it was but natural that the Sense of Vision should be chosen by a writer who has made its normal and morbid conditions a special object of pursuit, as the one best fitted to afford the required illustrations. Considering Mr. Wharton Jones's acknowledged qualifications for the task, we must confess to having experienced a certain degree of disappointment in the perusal of this Prize Essay; since, although we have no special fault to find with it, we cannot discern in it any marked superiority to the mode in which the same subject is treated in our well-known works on Natural Theology; no new point (so far as we can perceive) is brought forwards, no new fact stated, no new inference deduced. It may be said that it would be unreasonable to expect such, on a topic so hackneyed as this ; but we would reply that, as every physiologist is aware, there is a multitude of questions on which original investigation is more than ever called for, besides a vast stock of materials accumulated in readiness to be digested and arranged. The Comparative Anatomy and Embryonic Development of the Organ of Vision, for instance, suggest themselves to us as a wide field of research, which would have probably enabled the Essayist to have unveiled somewhat of the *plan* whereon the complete fabric has been evolved ; but this is a part of the subject which Mr. Wharton Jones has almost entirely passed by, the Essay being almost entirely devoted to the Human Eye, and the eyes of the lower Vertebrata and the Compound Eyes of the Articulata being only incidentally noticed towards the conclusion. The subject of the movements of the Eyeballs, again, is most imperfectly treated ; although, as Professor Alison has shown, it is capable of being discussed in a way that opens up one of the most fundamental questions in the Physiology of Motion, namely, the necessity of guiding sensations for the performance of voluntary actions. On the other hand, all that relates to the Sense of Vision in Man,—the "outness of visual perceptions," "erect vision from inverted perceptions," "single vision with two eyes," &c.,—is extremely well treated, though with great conciseness.

Notwithstanding that we have failed to derive from this Essay the instruction which we thought we had reason to anticipate, we can recommend it to our readers as a pleasingly-written performance, well adapted for those who come fresh to the subject; and this is, perhaps, all that its author aimed at producing.

PART SECOND.

Bibliographical Notices.

ART. I.—*On Articulate Sounds; and on the Causes and Cure of Impediments of Speech.* By JOHN BISHOP, F.R.C.S., &c. &c.—*London*, 1851. 8vo, pp. 179.

THE purpose of this little Treatise is to inculcate what may be considered as a *rational* method of treatment of impediments of speech, on the basis of what is known of the physiology of the organs of voice and articulation. Mr. Bishop, as many of our readers must be aware, has carefully and scientifically studied this subject; and whatever he writes upon it, therefore, must possess a sterling value.

The First Part, on Articulate Sounds, contains a very exact account of the various elementary sounds of which our spoken language is composed; and this is, as Mr. Bishop justly remarks, "a subject of high practical importance; for, unless the normal action of the vocal mechanism is thoroughly understood, it is impossible for the medical practitioner to undertake, with any probability of success, the cure of those distressing cases of defective pronunciation and hesitation of speech, which are so frequently committed to his care." It is obvious that the very nature of the subject must prevent us from giving any more detailed account of this part of the treatise.

Near the commencement of the Second Part, on Impediments of Speech, we find the apposite remark, that considering the multifariousness of the parts concerned in the mechanism of speech, and the number of conditions requisite for their normal action, "we need not wonder that they should so frequently present a great variety of irregular and disordered actions; indeed, the more the subject is investigated, the more must the investigator be impressed with the conviction that it is more wonderful that the majority of persons speak properly, than that a comparatively small number speak imperfectly." The most common cause, in the opinion of Mr. Bishop, as in that of most other writers who have treated the subject scientifically, lies in the want of perfect voluntary control over the muscles concerned in vocalization; and efforts at cure should be mainly directed to its attainment. It seems to us that the views which we enunciated on a former occasion (vol. V, p. 14) respecting the real mechanism of voluntary actions, have a peculiar applicability to those concerned in the act of speech. For every one must be conscious that he has no *direct* voluntary power over a single one of these muscles, so as to be able to single it out and put it in action independently of the rest; in fact, except by the sound he produces, he has no self-consciousness of the action he is performing, unless when an unusual effort causes a tension which communicates a disagreeable sensation that is referred to the muscles themselves. In fact, we simply *will* the result, namely, a sound which we conceive, however momentarily, and which we aim to produce; and it is the automatic apparatus, which (as we formerly showed) is the real instrument in executing that mandate. It is difficult to comprehend, on the ordinary hypothesis, now there can be any break or interruption between the brain and the muscle, a set of nerve-fibres being supposed to run continuously from the one to the other; but when we consider that, in the execution of a voluntary movement, the cerebrum has to play down (so to speak) upon the automatic centres, and that, with regard to all movements not originally automatic, the combinations have to be gradually made and brought into use by habit, it seems far more easy to understand that there should, in certain cases, be a want of consentaneousness between the two central organs, and that, however strongly the mind may will, and however perfectly the cerebrum may act, the automatic apparatus has not been efficiently trained to perfect respondence to its mandates, and executes irregular and even spasmodic movements, when it should be acting as a well-tuned instrument under the fingers of the performer. The marked influence of the emotions, also, finds a ready explanation on this view; for as they, too, play down upon the automatic apparatus after a method of their own, it is obvious that they are liable to disturb or modify the voluntary effort. They may, indeed, intensify, as they sometimes paralyse it; as we perceive in cases where mental agony lends an almost inconceivable force to cries for assistance, whilst, on the other hand, the strongest effort of the will is sometimes rendered powerless by awe or terror, and no sound comes forth, even though the lips may be seen to move. But with those who are in the least disposed to lose command over the muscles of speech, the slightest emotional excitement is sufficient to produce a complete perturbation; the very fear that stammering will occur, particularly under circumstances in which it will be especially annoying, being quite sufficient to produce it in a predisposed subject; and the tendency to consensual imitation being sometimes sufficient to produce it in certain individuals, who never show the slightest tendency to stammer, except when they witness the difficulty in others.—We commend these remarks to Mr. Bishop, and to others who are working-out the conditions of these impediments.

Mr. Bishop considers, that "the interruptions of vocalised breath in stammering may take place by the irregular actions of four different sets of organs:—1st. By the closing of the valve of the glottis. 2dly. By the closing of the isthmus of the fauces. 3dly. By the dorsum of the tongue being brought into contact with the palate. 4thly. By the closing of the lips and posterior nares." Notwithstanding that the first of these localities is specified as even the most frequent seat of the obstruction by some high authorities, we are very doubtful if it ever really is so. If it were, the *explosion* that takes place when the obstruction is overcome, should have somewhat the sound of a *cough*, such as is always heard when the glottis is suddenly burst open after being closed during the expiratory movement; and this, so far as our experience extends, is never the case. Moreover, rom our own experience in occasional imitative stammering, we have been ed to judge the obstruction to lie in the organs of *speech*, not in those of *voice*. That such is by far the most frequently the case, seems to be also Mr. Bishop's opinion.

We are very glad to find Mr. Bishop adding the force of his opinion opposition to the sanguinary methods of treatment of impediments of ·ch, which have been recommended by authorities in high esteem as ical operators, but of no weight as phy·iologists:

'ases have been described in which the motions of the tongue are said to have restricted by the genio-hyo-glossi muscles; and it has been asserted by at, and others, that the division of the genio-glossi portions has been attended ıparent relief. There is most probably some fallacy in these views, and the ·f articulation should always be tested on the principles already described, making the patient articulate slowly in a *continuous* vocal tone, as in before any attempt is made to relieve the defect by operating with the Vhen we reflect on the severity and gravity of the methods pursued by ch for the relief of stammering; which consists either in the division of th ·es of the tongue at its root, cutting at the same time through the linguales, ge ·-hyo-glossi, and stylo-glossi muscles, with their blood-vessels and nerves; or in cutting a transverse wedge-shaped slice out of the dorsum of the tongue, we may easily conceive the danger of hæmorrhage and sloughing, which must result from such operations. It appears, indeed, to be wholly unjustifiable for surgeons thus to inflict wounds, and mutilate organs, upon mere hypothesis, more especially when the practice is at variance with the physiology of the parts concerned in the defects of speech intended to be relieved." (p. 52.)

Precisely the same remarks apply to the tonsil-cutting practised by certain English surgeons. The temporary improvement which has followed many of these operations is easy to be understood, when it is considered how much the patient's *faith* has to do with his command of the muscles concerned in articulation.—We strongly commend Mr. Bishop's Treatise to the attention of our readers.

Art. II.—*Surgical Anatomy.* By Joseph Maclise, F.R.C.S. Fasciculus IX.—*London*, 1851. Folio. Three Plates.

With the present Number, this admirable production is brought to a conclusion, in a manner that fully sustains the character which the previous fasciculi had acquired; and we heartily congratulate Mr. Maclise on the well-deserved success which his work has attained. The First Plate contains seventeen figures, illustrating deformities of the Urinary Bladder, and the operations of sounding for stone, of catheterism, and of puncturing the bladder above the pubes; and of these figures, all but the last, which is of the nature of a diagram, are stated by Mr. Maclise to have been drawn by himself from natural specimens in the hospitals and museums of London and Paris. They constitute an extremely valuable series, and we can only regret that they are so closely crowded together. If Mr. Maclise had dispersed the drawings on these and some of his former plates, to which we made the same objection, over four more stones, he would have required but one additional fasciculus to have rendered the work of uniform appearance, as well as of uniform excellence, throughout; and to this we feel sure that none of his subscribers would have objected, considering at what a marvellously small cost they are obtaining so splendid a volume. In the Second Plate is given, in two figures, the Surgical Dissection of the Popliteal Space and of the Posterior Crural Region; and in the Third, we have the Surgical Dissection of the Anterior Crural Region, the Ankle, and the Foot, all drawn in Mr. Maclise's best style.

The Anatomical Commentaries on these Plates are clearly and accurately written; and we have, in addition, a concluding Commentary 'On the Form and Distribution of the Vascular System as a whole—Anomalies—Ramification—Anastomosis,' in which are embodied some general considerations on what may be called the Philosophical Anatomy of the Sanguiferous System. These are certainly much less objectionable in such an Appendix, than they would have been in the Commentaries on the Plates, through which Mr. Maclise at first seemed inclined to disperse them; but still we do not see what place they have in a work on *Surgical* Anatomy, which we suppose to be especially intended to assist the student and practitioner in applying his knowledge of anatomical details to the requirements of the surgical art; and they contain some doctrines which we hold to be altogether fallacious, such as, that, "since the liver on the right side has no counterpart *as a liver* on the left, and since the spleen on the left has no counterpart *as a spleen* on the right; so these two organs (the liver and spleen) must themselves correspond to each other, and, as such, express their respective significations." We might have been at a loss to understand the meaning of this mysterious sentence, if we had not found, further on, that Mr. Maclise actually regards the spleen as a ductless portion of the liver, of no use whatever in the economy. We should not have thought that any one could have gone beyond Paley's notion, that the spleen is merely a stuffing to fill up a void space in the abdominal cavity; but Mr. Maclise does not even give it the benefit of this hypothesis, treating it as "a thing degenerate and functionless."

We have another fault to find with Mr. Maclise, before we finally dismiss him from our critical bar. He has ventured, in the present number, to broach the strange hypothesis, that "physical impediments to the passage of the urine from the vesical reservoir through the urinary conduit, are sufficient to account for the formation of stone in the bladder, or in any other part of the urinary apparatus, without the necessity of ascribing it to a constitutional disease, such as that named *lithic diathesis* by the humoral pathologists." Now we do not at all deny that such obstructions will favour the deposit of the less soluble matters of the urine within the bladder; but to assert that such is a *sufficient* cause, is to betray a degree of ignorance of the subject he goes out of his way to notice, which a prudent care for his reputation would have prevented him from displaying.

In thus pointedly taking notice of these aberrations, we have had no desire save that of doing equal justice, to those, on the one hand, who are likely to make use of Mr. Maclise's book, and to that gentleman himself on the other. We should have gladly spoken of the work with unmingled laudation; and we are far from thinking that the faults which have been pointed out seriously detract from its value, consisting, as this chiefly does, in merits to which they are quite foreign. In fact, if they had not been in a degree forced upon our attention by the obvious partiality of the author for their display, we should not have noticed them at all. We have cheerfully and heartily given Mr. Maclise that meed of praise to which we deem him entitled, for having produced a system of Surgical Anatomy, which is a credit to his country as well as to himself, and at a price which must have been far from remunerating him for his manual labour alone; and the best advice we can now offer him, is, that he should steadily pursue the subjects in which he has shown himself so well fitted to succeed, leaving transcendental anatomy and physiology to those whose genius lies more decidedly in that direction. He will find, moreover, that the former, in this practical country, are decidedly more remunerative than the latter, as he will have doubtless found out by the difference in the amount of sale between his 'Surgical Anatomy' and his 'Archetype Skeleton;' and therefore we are consulting his interests in every way, in tendering him, as our best expression of gratitude for what he has now so well accomplished, our hope to meet him again, ere long, in some other department of the same field.

ART. III.—*De Arteriarum Notione, Structura, Constitutione Chemica, et Vita. Disquisitio Critica, Experimentis fulta, Præmio ornata.* Auctore MAXIMIL. JOH. SIGISM. SCHULTZE.—*Gryphiæ*, 1850.

On the Character, Structure, Chemical Constitution, and Vital Properties of Arteries. A Critical and Experimental Dissertation. By MAX. JOH. SIG. SCHULTZE.—*Griefswald*, 1850. 8vo, pp. 59.

THE author of this dissertation, the son of the well-known physiologist, thinks it necessary to commence with the inquiry how an artery is to be defined. On this point he does not say anything new; adopting the ordinary definition of an artery as a blood-vessel in which the blood passes from the trunk into the branches; and informing us that though it is easy to say where an artery begins, it is not so easy to say how or where it ends. We need scarcely remark that the portal vein, after its formation by the confluence of the veins of the chylopoietic viscera, is anatomically as much an artery, as is the aortic trunk of fishes, formed by the confluence of the branchial veins. On the component tissues of arteries, we find no new information. Like all recent inquirers, the author finds both yellow elastic tissue and contractile fibres in the walls of the arteries; the former predominating in the great trunks, the latter in the branches. Regarding the chemical composition of the arterial tunics, he has made some original experiments, which have led to some rather novel results. The contractile tissue of arteries he affirms to consist, like muscular fibre in general, of a proteine-compound; but this compound has rather the characters of casein than of fibrin, and in this respect it differs from organic muscular fibre elsewhere. He states that the elastic tissue may be resolved into a gelatinous substance by long boiling; and that this bears a closer resemblance to the gelatine of bones, than it does to chondrin. We suspect, however, that he has not sufficiently isolated the proper elastic fibres from the areolar tissue with which they are largely mingled. In common with other recent experimenters, he has fully satisfied himself that arteries are contractile under the influence of stimuli; and, like Weber, he has found the magneto-electric machine most effectual in demonstrating this, not merely in the small arteries, but also in the larger trunks, such as those of the carotids, which gradually contract to the size of mere threads. This contraction may be produced 12 or 14 hours after the removal of the vessel from the body, provided that it be not isolated from the neighbouring tissues, but they be removed with it.

The Dissertation is, on the whole, very creditable to the learning and sagacity of the author.

ART. IV.—*Memorials of James Mackness, Esq., M.D.* Edited by the Author of 'Brampton Rectory,' &c.—*London*, 1851. Fcap. 8vo, pp. 271.

WE are not among those who object to the multiplication of biographies, when the subjects of them are *real men*, who may be worthily held up for imitation to those who are labouring in similar spheres. We consider a really good man to be one whom Divine Providence has sent as a guide and helper in the attainment of that ideal, which is placed before the Christian as his highest aim; and as "the Word became flesh," to show us the perfect model of the Divine Life in a human form, so does the manifestation of the true Christian spirit in one who is engaged like ourselves in the active duties of ordinary life, tend to encourage, in their aspiration after its godlikeness, those who might otherwise despair of presenting even a faint reflection of it. It is not among the so-called "eminent Christians," that *we* should look for such assistance and guidance, for in them, self-consciousness is too apt to grow up into spiritual pride; but among those who, in a comparatively humble and retired path, have devoted themselves to the duties of the time, with no thought of self, and with the inwardly-cherished hope, rather than with the outwardly-expressed expectation, of Divine acceptance.

It is too often the case, however, that such memoirs do not give us the real men, but only one side of them, and that the most favorable, viewed through a medium which exaggerates every excellence, and casts every failing into the shade; and, however interesting and beautiful may be the ideal portrait thus presented, it is deficient in that which is to us its greatest excellence and value. We have in our minds a recent medical biography, extensively circulated in a particular religious connection, which affords a notable example of this kind of error; and nothing but the desire to avoid giving pain to the sincere and estimable believers in its truth, has prevented us from laying bare its fallacies, and placing the subject of it in what we believe to have been his true light.

Far different is it, however, we rejoice to say, in the present case. Dr. Mackness, we have every reason to believe, was, in every sense of the word, a true man and a true Christian; one who pre-eminently desired to realize in his life the religion he professed, and who considered that realization as the only test of its influence on his spirit. And the 'Memorials,' collected and edited by a grateful friend, touched with a kindred spirit, present us with a simple and faithful portrait, far more interesting and useful than a more ambitious biography would have been. His character, as partly delineated in his own letters and journals, and partly in a series of letters from friends who had become attached to him in his professional relation, is thus worthily summed up:

"From these letters it will not be difficult for the reader to construct in the mind an image of him whose life has here been faintly traced; but that image will be essentially defective, if it do not present the idea of a man of clear, quick, perception, close concentration of mind, and untiring energy and perseverance. Yet more, of one singularly endowed with the power of loving, and of making himself beloved, tender as a woman, yet firm, decided, and fearless as a man. Most of all, the image of a Christian, not fitted, indeed, with close precision to any mechanical type, but touched with living fire, and moulded in silence and suffering into the essential form of humility, faith, and love." (p. 262.)

Dr. Mackness first became known to the profession at large by his 'Moral Aspects of Medical Life;' a work which, though founded on the 'Akesios' of Professor Marx, was so amplified by himself, as to constitute in great degree a reflex of his own character. He subsequently published a small treatise on 'Dysphonia Clericorum,' which was noticed by us in our Second Volume (p. 227), and 'Hastings considered as a Resort for Invalids.' We must confess that we were not struck, in any of these works, with the indications of any remarkable power or originality of mind; and it is not, therefore, as an author, that we would hold him up for imitation. But the simple narrative of his life, as for the most part contained in his own letters, more especially those portions of it which relate to the severe and protracted, and at one time almost hopeless illness which led to his abandonment of general practice, and to his settlement as a physician at Hastings, and to his struggles with many adverse circumstances during the early period of his residence there, contain lessons of Christian faith and patience, which are the more valuable in our estimation, because they are not obtrusively put forth *as* lessons. And in the affectionate remembrance which is obviously entertained of him, by a large circle of those who had at various times been brought into relation with him, we have the best testimony to that *unselfishness*, which seems to have left an universal impression upon all who came in contact with him, and which, more than any other part of his character, recommends itself to the imitation of his professional brethren.

We consider such a production to be scarcely a fair subject for literary criticism; but we would remark, that many of the trivial details in Dr. Mackness's diaries add nothing to the fidelity of the portrait, and detract from the pleasure with which we believe that it would otherwise be contemplated.

Art. V.—*Lectures on the German Mineral Waters, and on their Rational Employment for the Cure of certain Chronic Diseases.* By Sigismund Sutro, m.d., Senior Physician to the German Hospital, &c. &c.—*London*, 1851. Fcap. 8vo, pp 430.

The lectures embodied in this compact little volume contain a large body of information upon the subject of the German Spas, the greater part of it collected by the personal inquiries of the author; and its special merit is, that he has done his best to determine the relative advantages of the respective springs in different classes of cases, by the careful analysis of recorded experience, so as to afford most valuable guidance in the choice of a mineral water, to those who are embarrassed and undecided by the very variety of the remedial agents submitted to their choice. The volume is interspersed with cases which illustrate what is believed by Dr. Sutro to be the *characteristic efficacy* of each Spa; and he states that he has been careful to sift the adventitious cures from those properly due to the prominent curative powers of the Springs. We do not profess to be able to speak critically as to the accuracy of all the views at which Dr. Sutro has arrived; but the work bears evidence of so much careful research, that we feel justified in recommending it as the safest guide our readers can adopt.

ART. VI.—*The First Step in Chemistry*. By ROBERT GALLOWAY, F.C.S., Lecturer on Chemistry, Putney College.—*London*, 1851. Post 8vo, pp. 91.

"In the pages of this work" says the author, "I have attempted, by a series of progressive exercises, to render the pupil familiar with chemical nomenclature, the laws of combining proportion, the construction of chemical formulæ, the mode of expressing chemical changes, the influence which cohesion and repulsion exercise over affinity, &c." The treatise is admirably adapted for self-instruction, and may be safely recommended to those who are commencing the study of Chemistry, whether by attendance on Lectures, or in anticipation of them. It is one of the great advantages of the pursuit, which the method here inculcated will assist the student to realize, that it thus trains the mind to trace the relations of theory and practical results.

ART. VII.—*On Gout; its History, its Causes, and its Cure*. By WILLIAM GAIRDNER, M.D. Second Edition.—*London*, 1851. Post 8vo, pp. 300.

WE are glad to find the early call for a new edition of this excellent treatise, fully justifying the opinion we expressed of its merits on its first appearance scarcely two years since. A considerable amount of new matter has now been added, chiefly having reference, however, to the physiological considerations with which the subject of Gout is most intimately allied. The author has taken up a very strong, and we believe a perfectly just, opinion, "that to represent the great functions of respiration and sanguification as a mere chemical process for casting out carbon and generating heat, is a very imperfect view of the most important office of living beings." We do not believe, however, that any judicious physiologist would dissent from this opinion, although it may be the fashion with those who think that chemistry is to explain every vital phenomenon, to disregard it. We have always been accustomed to hear and to teach, that whatever may be the special instrumentality employed for effecting a change, the crude elementary materials taken up from the digestive cavity are in a state of progressive metamorphosis up to the time when they are applied to the nutrition of the tissues; that fibrin is one of the intermediate phases of the metamorphoses; and that for every stage in this *assimilating* process, the exposure of the blood to the air is a necessary condition. We really do not see that Dr. Gairdner has made these truths in any respect more clear; and in fact we think that he has kept in the background the most essential differences in the conditions of albumen and fibrin, by representing the conversion of the former to the latter as a simple process of oxidation. Moreover, his formula for the production of gelatine from albumen by "the removal of a portion of its carbon, a further process of oxygenation, and the addition of some atoms of nitrogen," is conceived in the spirit of the chemical school, which he so much deprecates. We altogether dissent from him, too, in considering that albumen and fibrin are *elevated* in character by being converted into gelatine and chondrin.

In making these remarks on this new portion of Dr. Gairdner's treatise, we would not be understood as in the least degree desirous to qualify what we said on a former occasion, as to the sterling practical value of the treatise, which has been at once recognised by the profession generally. We observe that, in the present edition, the author alludes to the unanimity of assent with which his proposition of the great frequency of the gouty diathesis was received; and he quotes Sir Benjamin Brodie as especially confirming this view from his own large experience. We are well satisfied that it is founded in fact, and that the disorder is really far more prevalent than those who judge only by its characteristic local manifestations have any idea of. But bearing in mind that those who see most of the gouty diathesis, are medical men whose practice lies chiefly, if not entirely, among the middle and higher ranks of society, and that it comparatively rarely presents itself among hospital patients, whilst nothing is more common in the latter than scrofula in all its forms, we cannot but believe that Dr. Gairdner goes rather too far in asserting that "the strumous is not more frequent than the gouty habit."

The practical portion of the work has undergone various improvements, suggested by the author's later experience, or more extended knowledge; and, though he has failed to convince us of the validity of all his objections to the humoral pathology of gout, he appears fully to have weighed all that is to be urged in its favour, and states both sides with laudable impartiality.

Art. VIII.—*Hand-book of Natural Philosophy and Astronomy.* By Dionysius Lardner, D.C.L. First Course, including Mechanics, Hydrostatics, Hydraulics, Pneumatics, Sound, Optics.—*London*, 1851. 12mo, pp. 824. With upwards of 400 Illustrations.

The advertisement to this Treatise will best explain its plan and objects:

"In the composition of this work, the author has had in view the satisfaction of those who desire to obtain a knowledge of the elements of physics without pursuing them through their mathematical consequences and details. The methods of demonstration and illustration have accordingly been adapted to such readers. The work has been also composed with the object of supplying that information, relating to physical and mechanical science, which is required by the Medical and Law student, the Engineer, and Artisan; by those who are preparing for the Universities, and, in short, by those, who, having already entered upon the active pursuits of business, are still desirous to sustain and improve their knowledge of the general truths of physics, and of those laws by which the order and stability of the material world are maintained."

The work appears to us extremely well adapted for these purposes, and will, we doubt not, become very useful to those for whose benefit it is designed. We cannot accord to it a higher scientific character than that which Dr. Golding Bird's excellent 'Manual of Natural Philosophy,' possesses; but it is much fuller in details, being fully double its bulk, whilst it only passes over half its ground; and it is, consequently, better adapted for self-instruction, whilst it contains a larger body of facts of a directly practical character. Still it appears to us to be written too much upon the older models, and scarcely to take sufficient cognisance of those views, with regard to the fundamental relations of Force and Matter, which are destined, we feel sure, to bring about a revolution in the forms (to say the least) of scientific thought and expression, and to remove many difficulties which must have continually obtruded themselves upon the attention of thoughtful minds. In particular we regret to notice that the fiction of designating Light, Heat, &c., as "imponderable forms of matter," is still kept up.

The greatest novelty that we meet with, is a chapter on the Theory of Undulation, which affords an excellent summary of a body of doctrines that has grown up within a comparatively recent period into special importance, being not merely related to the subjects of Sound, Light, &c., but even to such questions as the diffusion of Earthquakes and the elevation of Mountain-chains.

ART. IX.—1. *The Laws of Health, in relation to Mind and Body: a Series of Letters from an Old Practitioner to a Patient.* By LIONEL JOHN BEALE, M.R.C.S.—*London*, 1851. 12mo, pp. 306.

2. *Physiology of Human Nature; being an Investigation of the Physical and Moral Condition of Man, in his Relations to the Inspired Word of God.* By ROBERT CROSS, M.D.—*London*, 1851. Post 8vo, pp. 366.

THESE two works, which are nearly of the same dimensions, are chiefly directed to the same purpose; but their methods are very different. Mr. Beale takes his stand upon the recognised principles of Physiology and Psychology, and upon the phenomena with which his experience as a practitioner has made him familiar; and has produced a very sensible treatise, which may be advantageously placed by any of our readers in the hands of such of their patients as may be disposed to profit by it. A particularly valuable portion of the work consists in the series of chapters treating of the Hygiène of the different periods of life, from Infancy to Old Age.

Dr. Cross, on the other hand, treats of Physiology and Hygiène in connection with his Theological views of man's nature and destination; and endeavours to establish a harmony between them, which it seems to us very difficult, if not impossible, to bring about. Thus, regarding "the resurrection of the body" in its literal sense as a Scriptural dogma, he aims to show that this is quite compatible with our existing physiological knowledge of the process of organisation; and holding the doctrine that the soul is altogether independent in its nature and essence, and that the body is only its temporary instrument, he affirms that insanity is only a disease of the latter, and that the former must remain intact. To ourselves, this mixing up of two entirely distinct sets of considerations is anything but satisfactory; and we must regard the book as suitable to those alone, who, participating with the author in his theological opinions, desire to engraft upon them such a knowledge of man's material nature as may guide them in the best use of their own bodily fabrics.

ART. X.—*Experimental Researches, illustrative of the Functional Oneness, Unity, and Diffusion of Nervous Action; in opposition to the Anatomical Assumption of Four Sets of Nerves, and a Fourfold Set of Functions and Transmitted Impressions; with a Brief Exposition of the Philosophy of Vivisection, and of Sensation.* By BENNETT DOWLER, M.D., &c. &c.—*New Orleans,* 1851. 8vo, pp. 34.

Dr. Dowler has made himself conspicuous among his brethren, by his refusal to receive certain of these Neurological doctrines, which, under one form or another, are now generally admitted amongst well-informed physiologists. We do not quarrel with him for declining to accept the double system of excito-motor and of sensori-volitional nerves, such having, as we now believe, no real existence in nature; and we have a strong sympathy with his objection to the new terms—diastaltic, esodic, exodic, anodic, cathodic, paltodic, panthodic, anastaltic, catastaltic, peristaltic, &c., by the adoption of which, we venture to think, a comparatively easy subject would be rendered obscure. But we are not on that account less convinced that he is mistaken in his notion, that all the acts which we attribute to the reflex power of the spinal cord, involve not merely sensation, but volition. Dr. Dowler has a fine field for experiment, being able to procure alligators for purposes for which European physiologists must content themselves with frogs; and the former animals not only exhibiting the phenomena of reflex action upon a much larger scale, but also possessing a most extraordinary tenacity of life. His accounts of his experiments are very graphically drawn; but it is necessary for the reader to be constantly on his guard against confounding Dr. Dowler's inferences with the facts themselves. Thus, a decapitated alligator "*seemed to be aware* of the nature of the touching body, which, if free from a pain-giving property, was borne without any violent efforts to escape from it; but fire, punctures, and skinning called into *agonised action* the body, limbs, and tail. The body curved in such a manner as to recede from the offending agent; the limbs were directed so as to remove it, and even the short stump of the headless neck was turned in the proper direction as if to bite, *as if forgetful that it had lost its head.* . . . . . . From its action, far more impressive than words, it was evident that it judged accurately as to the nature, degree, duration, and place of painful or painless impressions." (p. 8.) Dr. Dowler found that on applying a lighted match perpendicularly over the spine, the animal went straight forward, and would have crawled off the table if it had not been prevented; which experiment, he affirms, having been frequently repeated with the same result, "seems quite sufficient to establish the principle of possible forward *voluntary* motion after decapitation." Of course, we put a very different interpretation of these phenomena, and merely regard them as showing how large a share *automatic* action has in the production of movements, to which, in the unmutilated animal, we are accustomed to attribute a voluntary character.

But Dr. Dowler further informs us that complete division of the spinal cord between the anterior and posterior extremities "did not prevent the mutual and simultaneous action of the two divisions in warding off injuries of either;" but the instance he gives is by no means satisfactory: "When a bit of burning paper was applied to the flanks, the fore-legs were directed strongly to the seat of the pain, and in a straight line (that is, in the utmost degree of extension) along the sides of the body, without any lateral flexure common to the trunk and tail, as seen before the second division, though the tail, hind-legs, and body, as far as the division of the spine, continued to act in concert with the fore-legs." From all that is told us of the seat of irritation, it might very well be a point supplied with nerves from both the upper and lower divisions of the spinal cord. Further on we are told, among the results of experiments on another alligator, from which a portion of the spinal cord (though we are not informed at what part) had been removed, that "the indicants of a double life, a double sensation, and a double volition, continued in the tail below that portion of the cord that had been removed, and in the body above—the one independent of the other." After a probe had been passed for some way down the caudal canal, the lower half of the tail, Dr. Dowler assures us, was still directed in an *intelligential* manner.

Now, in all these statements, Dr. Dowler has left out of view the essential and fundamental fact, that movements equally purposive or intelligential with these are continually taking place in ourselves, without any exercise of our volition, and even without our consciousness; and that when accident makes the experiment, which we cannot make designedly, of injuring the spinal cord in the dorsal region, movements answering to those of his alligator are executed by the legs, in respondence to irritations of which the individual, or at least the owner of the brain, is unconscious. But, according to Dr. Dowler, a second *ego*, or independent centre of sensation and volition, is created by the very act of division, and is located, we presume, somewhere about the *cauda equina*. Is not this a *reductio ad absurdum* of his whole system?

ART. XI.—1. *Letters on the Laws of Man's Nature and Development.* By HENRY GEORGE ATKINSON, F.G.S., and HARRIET MARTINEAU.—*London*, 1851. 12mo, pp. 390.

2. *Miss Martineau and Her Master.* By J. STEVENSON BUSHNAN, M.D. F.R.C.P.E., Physician to the Metropolitan Free Hospital, &c. &c.—*London*, 1851. Fcap. 8vo, pp. 173.

THE appearance of the first of these works, however much in itself to be regretted, strikes us as, in some respects, a wholesome sign of the times. We hold that any man who has honestly arrived, by the use of his reason, at a certain opinion or set of opinions, and believes that these opinions are important to the world at large, ought to be allowed to publish them, without subjecting himself to those penalties which were formerly inflicted on the advocates of all unpopular creeds. The time was, when the authors of this work would have been roasted by a slow fire, or have been subjected to some other equally barbarous refinement of cruelty; and from what we know of one of them, we believe that she would not have shrunk from it, but would have glories in her martyrdom. Even within our own day, a criminal information, fine, and imprisonment, would almost certainly have visited both authors and publisher; and the consequence would have been, that their book would have immediately risen in public estimation, that the prohibition of its sale would have only made men more curious to read it, and that its deleterious doctrines would have taken a firmer hold on their minds. At the present day, men are becoming more and more convinced of the omnipotence of TRUTH, and less and less afraid of the effects of temporary perversions or backslidings, even on the part of those to whom they have been accustomed to look up to as their guides. And the consequence is, that publications like the present soon come to be estimated at their true worth, or rather worthlessness; and after reaching the elevation of a nine days' wonder, which they gain only by the previous reputation which their authors may have acquired, sink into their merited oblivion.

In another point of view, too, we must honestly say that we scarcely regret this publication. We have long been aware that a certain class of minds not destitute of intelligence, but wanting in clear habits of thought, has been hovering on the brink of the gulf into which Mr. Atkinson has fallen, dragging Miss Martineau after him. And we are not at all sure, therefore, that an exhibition of the principles really professed by these apostles of the new philosophy, in all their nakedness and deformity, will not have the effect of warning off many who might have been disposed to follow in their path, had the ultimate issue of their doctrines been less apparent. For ourselves we can only say, that we cannot imagine any persons of ordinary common sense, still less any who are in the least imbued with the principles of correct reasoning, to have gone carefully through this work, without being so much disgusted with the arrogant dogmatism, and intense self-confidence of both its authors, their flippant denials of whatever *they do not choose* to believe, their marvellous credulity as to what *they do*, their perpetual self-contradiction, their disregard of the commonest rules of logic, and (we must add) their flagrant distortion of authorities, especially that of Bacon, in their own favour, without thanking Heaven that those who have taken upon themselves to declare that there is no personal Deity, (which is the same as saying that there is no God at all,) that man and external nature are everything, that this world is all, and that we are utterly destitute of all power to shape our own course, but are entirely what our organisation and circumstances make us, should have been forced, by the weakness of their cause, to make what is a practical *reductio ad absurdum* of their whole system.

We had intended to have served up for our readers' entertainment a few of the most monstrous of these absurdities; but really, we think, that the less that is said about the book, the sooner will its unwholesome influence subside. And we shall content ourselves with entering our protest against the use of the term *Law*, as giving us any definite explanation of phenomena, or any account of their existence. We are told that the man of science, in reducing phenomena to laws, makes the idea of a Creator unnecessary; a statement which seems to us to betray an utter ignorance of the logic of science, and to show that the authors have made the not uncommon mistake of confusing the *law* of the Physical Philosopher with the *law* of the Legislator. The former is simply a generalised expression of facts, and has no *coercive* power whatever. Even the law of gravitation, which is the highest generalisation yet known to us, has no other meaning. It is absurd to say that it gives an account of, or a reason for, a single phenomenon; when all that it can do, is to place it in the same category with others which we before knew. To say that a stone falls to the ground *because* of the law of gravitation, is merely to say that one stone falls because others do; or, extending our generalisation further, that bodies fall towards the earth, *because* the earth and other planets tend to fall towards the sun and towards each other,—which is obviously no reason at all. A law can possess no *coercive* character, can do nothing, nor make anything or anybody else do anything, except it be the expression of *a power*, such as that of a Government or Legislator; and all our experience of such powers leads us to the conception that they must be *mental*, and that there is no other *fons et origo* of power, save Mind. Viewed in this light, therefore, the "Laws of Nature," so far as known to us, are, on the one hand, generalised expressions of phenomena; on the other, they are expressions of the *will* of the Divine Author and Governor of the Universe. Strip them of the latter character, and they have no more right to be spoken of as *coercive*, than have the empirical computations of the Statistician; although they may afford a more certain power of prediction, in virtue of the wider and more certain base on which they are founded.

There is nothing in the leading doctrines of this volume, but what has been so frequently put forth and refuted, that we have scarcely felt that a formal reply to it, or critique upon it, is either necessary or desirable. There are many cases in which we hold that the common consciousness of mankind is the best reply to the absurdities of specious but shallow pretenders to philosophy; and this we believe to be one of them. When we are told that we have no control whatever over the current of our thoughts, we, as Medical Philosophers, know that the authors can have no experience of Insanity, of which this want is a very general characteristic; and we simply say that they do not know the existence of the power they deny, through not having witnessed the results of its deficiency. We wonder, however, that the case of dreaming did not occur to them. According to our authors, our whole life is an acted dream; and, like the "mesmerised," "hypnotised," or "biologised," patients, who can be played upon by any suggestion that is brought vividly before their consciousness, we are the sport of "circumstances," and can do nothing of ourselves!

But we are falling into the very error we have deprecated. Those who desire a formal refutation of such nonsense, will find it in Dr. Bushnan's cleverly written little book; which will, we trust, assist in setting free such of the readers of 'Man's Nature and Development,' as may have been entangled by its sophistries, from the web which has been spun round them.

PART THIRD.

Periscope.

ANATOMY, PHYSIOLOGY, AND ORGANIC CHEMISTRY.

On the Reunion of Wounds of the Spinal Cord, with Restoration of its lost Functions. By M. Brown-Séquard.

During the last three years, M. Brown-Séquard has made a considerable number of experiments, with the view of determining the degree of reparative power which exists in the Spinal Cord; the results of which are very remarkable. The following is one of the most striking:—The spinal cord of a Pigeon was *entirely divided* between the 5th and 6th dorsal vertebræ; and the operation was followed by complete paralysis of the posterior part of the body, as regarded sensibility and voluntary movement. At the end of three months, voluntary movements began to show themselves, in the midst of reflex actions; and sensibility also reappeared. These powers gradually augmented; and six months after the operation, the bird could stand for some minutes, but fell if it attempted to walk. In the course of the seventh month it began to walk, but unsteadily, helping itself by its wings. By the end of the eighth month, it could walk slowly without support; but if it attempted to walk fast, it fell over, unless it supported itself by its wings. Twelve months after the operation, it could run; and when the account of the case was drawn up, fifteen months after the section had been made, its progression seemed in all respects normal, save that a certain degree of stiffness remained in its gait.

In several Guinea-pigs, in which the section had only been made through one-half of the spinal cord, an incomplete return of voluntary power was observed within seven or eight months after the operation. In the case of one Guinea-pig, which had been subjected to this operation a year before, and in which sensibility appeared to have been completely restored, and voluntary movement less completely, a careful examination was made of the injured part. It was found that the section had traversed both the posterior columns, as well as the anterior and lateral columns, and a portion of the grey substance on the right side; all of which parts exhibited a sort of contraction, the continuity of the divided parts being re-established by a whitish cicatrix. On examining the substance of this cicatrix, it was found to be in great part made up of fibres of areolar tissue, the direction of which was transverse or oblique; but these were crossed by great numbers of nerve-fibres running in a longitudinal direction, which exhibited a double contour, and were uninterruptedly continuous through the whole extent of the cicatrix. Amongst these were scattered some ganglionic corpuscles. A like reproduction of nerve-fibres in the cicatrix of the spinal cord, has been substantiated by M. Brown-Séquard in two other cases.—*Gazette Médicale*, No. 30.

Researches on the Histology of the Nervous System; with Remarks on the Muscular Fibre and on the Movements of the Heart. By Dr. Schaffner.

The author has applied himself to the detection of the mode of termination of the nerves in the auricle of the frog's heart, portions of which, taken from the living animal, he has examined without the addition of any liquid; and he confirms the statements of those who maintain, that from the so-called terminal loops, there proceed other nerve-fibrils, of much smaller diameter. He has also ascertained that, contrary to what obtains in higher Vertebrata, the fibres of the *auricles* in the Amphibia are *non-striated;* those of the ventricles being striated, as elsewhere. The same arrangement exists in Fishes; whilst in Mollusca and Crustacea, the entire heart is composed of non-striated fibres.—The author finds, as others have done, numerous ganglionic corpuscles in connection with the nerve-fibres; some of these being prolonged into the peripheral fibres, and others having commissural connections with each other; and, like many other physiologists, he attributes the heart's rhythmical movements to their reflex action, disregarding the fact, that these movements commence when, as yet, the walls of the cavities are nothing but a layer of cells. The lower character of the muscular fibre in the heart of Amphibia and Fishes, seems connected, as he justly remarks, with their longer tenacity of life.—*Henle and Pfeufer's Zeitsehrift.*

On the Oxidation of Ammonia in the Human Body; with some Remarks on Nitrification. By HENRY BENCE JONES, M.D. F.R.S., &c.

IT is shown in this paper, that when ammoniacal salts are taken into the body, nitric acid is excreted by the urine, although no trace of that substance could previously be detected in it. The author was then led to investigate other cases of combustion, in which ammonia is present, and came to the conclusion, that nitric acid is formed out of the body as well as in it; and he further ascertained, that even the nitrogen of the atmosphere is not indifferent in ordinary cases of combustion, but that it gives rise to minute quantities of nitric acid.—He found that a mixture of starch with a drop or two of hydriodate of potash and hydrochloric acid was a more delicate test of the presence of nitric acid than either the indigo test or the protosulphate of iron test; and that he was able to detect, by its means, as little as 1 grain of nitre in 10 oz. of urine, which neither of the other tests would indicate.—*Proceedings of the Royal Society.*

On the Distribution of the Blood-vessels in the Mucous Membrane of the Stomach. By HENRY FREY.

THE distribution of the blood-vessels in the gastric mucous membrane has an interesting relation to its double function; for the vessels of the surface, which are those most concerned in absorption, are veins, and have a large diameter; whilst those of the deeper portions of the membrane, which are subservient to secretion, are arteries, which form very delicate net-works around the gastric follicles.—*Henle and Pfeufer's Zeitschrift.*

Further Experiments on Cadaveric Rigidity. By Dr. BROWN-SÉQUARD.

FOLLOWING up the researches on which he has been for some time engaged, the author has ascertained that if a current of arterial blood be re-established through muscles in which cadaveric rigidity has already begun to show itself, they cease to be rigid and recover their irritability. He found that when he connected the aorta and vena cava of the body of a rabbit, in which the cadaveric rigidity had already manifested itself for between ten and twenty minutes, with the corresponding vessels of a living rabbit, so as to re-establish the circulation in the lower extremities, the rigidity disappeared in from six to ten minutes, and that in two or three minutes afterwards, muscular contractions took place when the nerve-trunks were irritated. These experiments have been repeated in various ways with the same result; and they fully justify the opinion of those who maintain that cadaveric rigidity is a vital phenomenon, and not an indication of the death of the muscles, which does not take place until the rigidity passes off. He has even succeeded in removing the cadaveric rigidity from the muscles of the decapitated body of a criminal, thirteen hours after execution, and two hours after the supervention of the rigidity, by the injection of defibrinated human blood. The muscles lost their rigidity, and continued to contract on irritation, during several hours.—*Gazette Médicale*, Nos. 24, 27.

[Fully recognising the interest and importance of the series of experiments on which M. Brown-Séquard is engaged, we would yet express the earnest hope that he renders them, by the use of anæsthetic agents, as little productive as possible of animal suffering. He speaks so coolly of cutting a live rabbit or guinea-pig in two, leaving the anterior and posterior portions connected only by the aorta and vena cava, that we fear that he must be ranked among those who have been so inured to the manifestations of pain, that they cease to take account of them, save as scientific phenomena, as indications of sensibility.]

Anatomical Examination of an Infant born without Eyes. By M. LISSA.

THE palpebral fissures were very small, not being above two lines in length; but the lids and lachrymal apparatus were perfectly developed, and the conjunctival membrane covered the contents of the orbit. There was not a vestige of the globe of the eye in either orbital cavity; its place being occupied by areolar tissue, in which the optic nerves seemed to lose themselves. The intra-cranial portion of these nerves followed its usual course; but the tubercula quadrigemina and the thalami optici were of very small size; thus confirming the view that the latter, as well as the former, are the ganglionic centres of the visual sense.

Gazetta Medica Italiana, July 1850.

On the Post-mortem Duration of the Ciliary Movements in the Human Subject. By M. GOSSELIN.

THE body of a decapitated criminal having been conveyed to the École Pratique, the ciliary movement was recognised on the mucous membrane of the trachea, of the nasal fossæ, and on that lining the maxillary, frontal, and sphenoidal sinuses, 8 hours after death. The movements were still distinguishable, especially on the mucous membrane of the trachea, 32 hours after death. The movement had ceased on the mucous membrane of the nasal fossæ and of the sinuses, 56 hours after death; but this was perhaps due to the free exposure of these parts to the air; for the vibration was still active on the mucous membrane of the trachea, where it was distinctly seen to the 168th hour after death, after which putrefaction came on, and the movement ceased. In another case of the same nature, the ciliary movements were much less durable; and this seemed to be consequent upon the earlier supervention of putrefaction, brought about by a higher temperature, the thermometer having ranged from 46° to 54° in the first case, and having risen to 68° in the second.—*Gazette Médicale*, No. 26.

On the Nerves of the Uterus. By M. BOULARD.

THE author states that his dissections were carried on without any knowledge of the 'Memoirs' of Dr. Robert Lee and Mr. Snow Beck; which he only consulted after the termination of his own inquiries. He states that these have led him, in all essential particulars, to concur with the latter anatomist; and he particularly affirms that the nerves do not augment during pregnancy. He has made two comparative preparations of the uterus of a girl of 12 years old, and of a woman who died near the end of pregnancy; and he affirms that there is no difference in the arrangement of their nerves, except that which arises from the closeness of the elements of the plexuses in the first case, and their separation in the second.

Gazette Médicale, No. 33.

On the Iodine of the Atmosphere. By M. CHATIN.

THE constant dispersion of iodine, through the slow, spontaneous evaporation of the waters which contain it, and its more rapid volatilisation when heat is applied to these; its elimination from hard waters, which is so speedy that it can seldom be detected therein, even when they spring from highly iodined soils; and the results, though incomplete, which have been obtained by operating on rain water, are so many circumstances which have led M. Chatin to conclude that this substance must exist in the atmosphere. He estimates the 4000 *litres* of air, which traverse the lungs of a man in 12 hours, as containing $\frac{1}{15}$ milligramme, *i. e.*, the same quantity that is found in a *litre* of potable water moderately iodined. This iodine becomes fixed during the act of respiration, the expired gases exhibiting about $\frac{1}{5}$ of the iodine contained in the inspired air. The atmosphere of ill-ventilated and crowded places is in part deprived of its iodine. The proportion of iodine contained in the waters of a given locality indicates approximatively the quantity contained in its atmosphere. Rain is notably more iodined in the interior than in the vicinity of the coast, inasmuch as the iodine of fresh waters is much more completely dispersed than is that of sea-water. Great differences, due to causes not yet appreciated, exist in the amount of iodine contained in the rain of the same locality; the proportion, however, always diminishing when the rains are very prolonged. As rain always loses its iodine on falling, this might be fixed for useful purposes by placing in cisterns a millionth or half-millionth part of carbonate of potash. Snow is iodined; but, *cæteris paribus*, less so than rain. Dew contains iodine. Additional observations are required to decide whether iodine exists in the air in the free state, as hydriodic acid, as hydriodate of ammonia, or as forming a volatile combination with certain organic elements.—*Gaz. Méd.*, 1851, No. 19, p. 300.

PATHOLOGY AND PRACTICE OF MEDICINE.

On Cretinism. By M. FERRUS.

M. FERRUS, in virtue of his office of Inspector of the Condition of the Insane in France, has, during his excursions into the Alpine, Pyrenean, and other infected regions, investigated the condition of the Cretins, and has read an interesting memoir on the subject to the Academy of Medicine. As much of what he says is only corroborative of the conclusions of the Sardinian commission, with which our readers have been made acquainted, we shall only advert to some few points.

M. Ferrus observes, that the number of cretins may seem to be more diminished than they really are, as, owing to the alteration of opinion that has taken place respecting them, their friends drive them away, instead of exhibiting them as heretofore,—one good result of this being that the encouragement of sexual intercourse between them and sound persons has ceased to prevail. After a vivid picture of the condition of the cretins he saw at Sion in 1837, he shows that, in many of their characteristics, they differ from idiots; the latter being far from exhibiting the animation and *bizarreries* he witnessed among the cretins, who, he believes, are more susceptible of education than they are. The peculiarity which especially struck him was the mode of development of the cranium, which, in all, had more or less of a hydrocephalic character. Even among the more advanced cretins, some remains of memory exist; and in the demicretins it may be considerable, and is more marked than in idiots.

M. Ferrus quotes, at considerable length, Stahl's account of the pathological anatomy of cretinism, which confirms him in his opinion as to the intimate nature of the disease. "I have sought," he says, "to render prominent two orders of essential phenomena. 1. A constitutional condition of the entire economy, a peculiar temperament, a lymphatic or cretinous cachexia. 2. A moderate but permanent degree of cerebral compression, shown by the obtuse state of the senses and faculties, the general engourdissement of the economy, the unusual size and the continuous vacillation of the head." The author considers the most exact definition of cretinism would be, *a chronic œdematous hydrocephalus, diffused hydrocephalus* or *cerebral œdema*,—the considerable effusion into the ventricles and upon the surface of the brain being, in his view, essential features. When the affection is generally developed in a country under the influence of continuous local causes and generative transmission, the disease affects more or less the entire mass of the brain of the cretin, though it may not at once abolish all the functions. Pathological anatomy may much more frequently show in *idiots* local affections of the brain, but the remainder of its substance has not undergone any appreciable change;—so that in them we sometimes observe isolated faculties nearly untouched, while others are absent; and certain portions of the body paralysed or atrophied, while others are active and useful.

In regard to the *causes* of the affection, M. Ferrus is in considerable accordance with the Sardinian commission; but he attributes far greater influence, in the cretin regions of France (Brittany, the French Jura, borders of the Rhine, Lorraine, and the Pyrennees), to the absence of free ventilation by a pure air, than to bad diet, inasmuch as this is quite equal to that of various other parts of the country where cretinism does not prevail. He denies that the views of M. Grange and others, of the ill-effects of magnesian waters and soils, are founded on fact; and asks how, in such case, the disease has become eradicated from these identical soils, by the vigorous adoption of hygienic measures. He does not deny the remedial power of the iodined food that has been recommended, but he doubts its preventive agency.

M. Ferrus considers that the propagation of the affection by sexual intercourse should be prevented; and that, as regards their sequestration and responsibility, cretins should be administratively and juridically assimilated with idiots.

In respect to *education*, after referring to the part he had taken in applying this to idiots and the gratifying results, M. Ferrus stated his belief, that even yet more encouraging ones are to be anticipated with respect to cretins, and that for the following reasons:—(1.) Because the disease affecting them, contrary to what is the case in idiots, depends upon the general disposition of the economy, which can be advantageously modified by change of place, regimen, and habits. (2.) Because the pathological alteration of the brain consists in a general modification of the texture of the organ, or rather in the abnormal quantity of fluid which it contains; and that this modification is much more accessible to art than are the arrests of formation and partial alterations of cerebral substance, which are so frequently met with in idiots. (3.) Because, while in the idiot the faculties are radically extinct, or exist only in a rudimentary state, they would have acquired in the cretin with the integral development of the organ an equally complete activity, had not disease interrupted this. Although now oppressed and obtuse, they are not absolutely obliterated.

Entertaining the above view of the nature of the disease, M. Ferrus, besides hygienic and educational treatment, would resort to means calculated to relieve the diseased cerebral condition, viz., revulsive remedies, whether acting as purgatives or external irritants.

(In the discussion which followed the reading of the paper, it was objected to M. Ferrus, that his distinction between cretinism and idiocy, founded upon the pathological appearances hitherto recorded, is based upon very insufficient data. M. Grange's statements concerning the influence of magnesian soils, too, meet with but little favour at the Academy, though he has accompanied their exposition by an elaborate geological map, the correctness of which is testified to by M. Elie de Beaumont. M. Niepce, who has been investigating the subject in the cretin regions during the last three years, and has just published a work upon it, states that he has repeatedly analysed the waters of the most infected districts, and has hardly ever found them containing the magnesian salts in question. M. Bouchardat believes that the Sardinian commission examined this part of the question in the most superficial manner, though reporting on it so confidently. He observes also, that although it may be true, as M. Grange has brought so much evidence to prove, that the disease especially prevails in magnesian soils, yet it is not probably owing to so innocent a substance as magnesia itself. He thinks it much more likely that the gypsum, so prevalent in such soils, may prove injurious. In localities where these soils are present, and goitre and cretinism do not prevail, this may be due to the counteracting presence of iodine. The Academy has referred the matter to a commission.)—*Bullet. de l'Acad.*, xvi, pp. 200—282, 381, 436, 473; *Rév. Méd.-Chir.*, viii, 223.

History of a Remarkable Attack of Measles in a Family at Padua.
By Dr. ARGENTI.

THE following fearful occurrences took place in the family of Signor Graziani, a respectable councillor of Padua. Measles had prevailed to some extent in the city, when *Joseph* Graziani, æt. 17, took them on the 31st of May, and recovered in a few days. On the 31st his married sister, *Theresa* (second case), æt. 28, called with her child, and on learning the nature of the disease, hurried away, much alarmed lest her child should take it, being then herself the prey to excessive grief from the recent death of her husband. She was engaged in a very fatiguing occupation, the management of silkworms; and attributed some febrile indisposition, which she experienced on the 12th and 13th of June, to over-exertion. Getting worse she took to bed, and on the 14th the eruption appeared. The removal of her child, to which she was devotedly attached, caused her great grief. The eruption was profuse and red; the accompanying fever was intense; and she suffered much from dyspnœa, and pain at the epigastrium. On the 17th she was bled twice, with some relief to the pain, but the fever continued excessive; on the 18th she was furiously delirious. The skin was hot, but the eruption had become pale. She was seized with tremors of the lips, convulsions of the limbs, and stertorous breathing, amidst which she expired. *Nina* (third case), æt. 3, was her child, and, though removed from its mother on the 14th of June, became the subject of the disease on the 25th. This pursued a favorable course, though the fever was intense, and the convalescence tedious. *Annetta* (fourth case), æt. 16, of a lymphatic habit, enjoyed good health, and was also employed in managing silkworms. She had severely felt the loss of Theresa, and, with her other sisters, was incessantly engaged in anxiously watching little Nina during this period. On the 8th and 9th of July, the eruption appeared, became confluent, and was accompanied by great swelling of the head, and epistaxis. She was doing well, when, on the 11th, she arose from bed, and suppressed a copious sweat, the urine being, however, abundant. Hearing of her sister's death on the 12th, she became the subject of epileptiform convulsions and delirium, and in three quarters of an hour died. The autopsy was conducted in the presence of several able practitioners, who all agreed that no appearance explanatory of death was observed. *Fanny* (fifth case), æt. 14, of a nervous temperament and lymphatic habit, exhibited the eruption on the same days as Annetta (8th and 9th of July), and by the 30th was convalescent. *Laura* (sixth case), æt. 22, of nervous temperament and scrofulous habit, and participating in the fatiguing employment and depressing emotions of her sisters, also exhibited the eruption on the 8th of July, it coming well out, but being less confluent than in the others. She went on very well till the 12th, when she was seized with violent delirium and epileptiform convulsions, and in an hour she was dead. In the autopsy, no change in the brain or other important organs (the spinal marrow, however not being examined in these cases) could be discovered. *Josephine* (seventh case), æt. 19, of nervous temperament and scrofulous habit, but in tolerable health, felt much alarmed at these occurrences in the family, and on the 9th and 10th of July, the eruption appeared. Her removal from the presence of her dying sisters on the 12th, caused her great dismay and anguish. The eruption came well out; but as there was much fever and great disposition to lethargy, some leeches were applied to the head, and were followed by blisters, (which had also been freely used in the other cases.) She was more tranquillised in the afternoon, and there was less somnolence; but early in the evening she was seized with epigastric pain, as her sisters had been, and then with convulsions and delirium, expiring in about an hour after. The autopsy furnished similar negative results. *Maurice* (eighth case), æt. 12, exhibited the eruption on the 8th and 9th of July, and had become convalescent by the 18th. *Bartholomew* (ninth case), æt. 20, of plethoric habit, and accustomed to frequent bleeding, manifested such high febrile action on the 12th and 13th of July, as to require two venesections. Later the febrile action took on an intermittent form, and quinine was given. He was convalescent by the 24th.

The eruption in this attack was quite normal, though very intense and confluent, and the disease presented nothing peculiar in its mode of invasion or complications; and yet four of the cases perished within an hour from the time that really dangerous symptoms set in; the morbid action seeming here to concentrate itself with all its force in the cerebro-spinal axis. In three of these, the autopsies, most carefully conducted, revealed nothing.

In regard to the ages of the victims it may be observed, that while Borsieri, Frank, and Raimann believed there is greater danger for adults, Dr. Lees found in the Dublin epidemics, 1840—4, that it was in inverse proportion to the age. Levy, in his account of the epidemics among the military in 1837—47,states that fewer adults than boys died. In the present cases, the ages varied from 3 to 28. Of the five recoveries, four took place among the youngest; and all who died had attained puberty.

As concurring to impress upon these cases their remarkable fatality, may be their nervous-lymphatic temperament, scrofulous habit, physical debility, great sensibility, excessive alarm, and inordinate fatigue.—*Omedei Annali,* vol. cxxxiv, pp. 449—504.

On some of the Histological Characteristics of Malignant Growths.
By Professor ALBERS, of Bonn.

1. No form of growth other than the malignant consists so exclusively, even to the acquisition of a large size, in cell-formation, all non-malignant ones containing a great abundance of fibre-formations. It may be objected that *epithelial* tumours consist of cells, and yet remain innocent. It is to be observed, however, that such tumours always remain small, and have not proved so generally innocent as the polypus and fibroid. Epithelial tumour, too, frequently relapses, and is sometimes as destructive as cancer itself. Among other innocent tumours, the *fatty* especially exhibit cells, but the regular fibrous network, which is also present, essentially distinguishes them from all malignant tumours.

2. In innocent growths the cells decrease with the duration of these, while in malignant ones they increase. At the commencement of the so-called tumours of the cellular tissue, among the predominant fibres, cells are to be seen, which at an older date are entirely absent; and the same is observed in polypus and fibroid. In malignant tumours a great number of fibres are found at first; but the longer the tumours exist and the larger they become, the more completely do such fibres disappear, leaving the cells as the sole histological element.

3. Certain peculiarities are observed in these cell-formations, among which may be mentioned the incomplete formation of the greater part of the cells, when the tumour is old and large, and especially in the case of relapsing and secondary formations. The cells exhibit either a different form, an unequal size, or an irregular degree of development. The equal development of the structural elements of polypus, fibroid or fatty tumour, furnishes an entirely different general impression from that derived from any kind of malignant tumour.

4. Besides the incompleteness and irregularity of the development of cells in malignant growths, they are found in these to undergo a rapid disintegration, examples of which, though more frequently met with in the older tumours, are not wanting in the younger ones, showing the retrograde changes which are taking place. The elements proving this, are granules, granular bodies, and granular cells; and these are to be found in a greater or less number in every cancerous tumour proportionate to its age. If, on the other hand, we consider the regular and unchanged condition of the cells in fatty tumours or polypus, in which scarcely any granular bodies or cells are found, it becomes certain that the duration of the life of a cell is much longer in innocent than in malignant tumours.

5. Malignant tumours are remarkable for the rapidity of their cell growth. In a few days an entire lung may undergo tubercular transformation, or a cancerous tumour acquire double its size. A relapse may occur in five or six days, and a few days later may attain enormous dimensions. No innocent tumour comports itself thus.

6. In malignant swellings we always find a more abundant juice, which flows out on pressure, and contains some of the elements of the disease, as the cells, and the same fluid blastema is obtainable from tubercular lungs. When fluid is pressed out from a polypus, it contains no cells or fibres, or very few, while in that obtained from cancer there are numerous cells in every stage of development. It follows from this, that the textural connection in the malignant tumour is always looser, and the proportion of fluid blastema always larger, than in the innocent; and that these slightly connected elements are easily separable, and are incapable of the degree of development observed in the innocent, being, therefore, endowed with a shorter duration of life than these.

It results from the above observations, that there is less *vital energy and durability* in malignant growths, as is shown by the fewer stages of development they are capable of; and by the great disposition of the cells to terminate their life, and to pass into granular bodies and granule-cells. This retrograde course explains the inordinate increase of cells, just as we see an immense reproductive power in animals placed low down in the scale. The lower its vital energy sinks, the more rapidly does the growth increase, so that the second or third relapse takes on a much larger and more rapid development than did the original tumour—a point well deserving the attention of the operator, lest, by his interference, he lowers the amount of vital energy, and hastens death more rapidly than it would have occurred had the case been left to nature. It is to this diminution of vital activity, that the peculiar softening of these tumours is due. In the softened mass are found the elements of the degenerated structure with incompletely formed pus globules; and when the vital power is increased, and, as in tubercle, a stationary condition of the disease produced, a more complete pus formation takes place.—*Canstatt's Jahrb.*, 1850, v, 248.

On Facial Paralysis in Children. By M. ROMBERG.

A CHILD having been recently admitted to Professor Romberg's *clinique*, suffering from facial paralysis, coming on from exposure to a current of air, he took the occasion to make a few remarks upon the affection.

Paralysis of the facial nerve does not manifest itself by exactly the same characters as in the adult; for while in the latter the constant play of the features exhibits continuously the want of symmetry in the two sides of the face, this is only observable in the infant when it screams, or its emotions are otherwise excited. In the adult the forehead is more or less wrinkled, but its smooth state in the child prevents its two sides being so remarkably contrasted as they are in the adult. If the child be examined during a state of calm, nothing remarkable is observable in its countenance; but if we make it cry, the deformity of the lineaments is seen, since the mouth is drawn to the left and upwards, while the right eye is not closed. The parents state, that in sleep this eye is almost entirely closed, which is an important point, signifying that the nervous directing power is not completely abolished, as in complete paralysis the eye remains open during sleep. The collapsed condition of the ala nasi is not observed as in adults, and it is only when the child sneezes that we can remark it, as upon the palsied side the peculiar motion which this action calls forth is wanting.

As the present case is of short duration, and evidently of a peripheric nature, the prognosis is favorable, though a rapid cure may not be procurable. M. Romberg has sometimes seen such paralyses spontaneously cured, after he has abandoned the use of remedial means for weeks. Still we must not rely on this, since, as a rule, the hope of cure diminishes with the prolonged duration of the case. When general symptoms calling for treatment are not present, a blister should be applied to the angle of the jaw, and from one twelfth to one third of a grain of strychnine applied endermically daily, the palsied parts being treated by friction with flannel and a "nervine" ointment.—*Journ. für Kinderkrank.*, xv, 125.

SURGERY.

On Treatment of Paraphymosis in Children. By Dr. RAU.

SINCE 1848, six cases of this affection have come under Dr. Rau's notice, the paraphymosis having existed from twelve to twenty-four, and in one instance for thirty-six hours, so that very considerable tumefaction and inflammation of the glans and prepuce were present. Attempts at reduction by Walther's and other methods proved fruitless; but this was easily accomplished after the application, for from twelve to twenty-four hours, of the following ointment:—*Ung. Hyd. Ciner.*, ℥½; *Ext. Conii vel Belladon.*, ʒj *ad* ʒij. In the case which had continued for thirty-six hours, *Aq. saturni* was also, on account of the excoriations, applied for thirty-six hours, after which the reduction was easily effected.—*Casper's Wochenschrift*, 1851, No. 21.

On Fracture of the Os Hyoides. By Dr. GRÜNDER.

THE following case proves that this affection is not always so easy of recognition as it is usually considered to be. A labourer, æt. 63, fell from a waggon on his face, and discharged a large quantity of blood by the mouth. He found he could not swallow, and when seen twelve hours afterwards complained of severe pain in the neck and nape, with inability to turn his head, though no injury of the vertebræ could be detected. His voice was hoarse and difficult. On attempting to drink, the fluid was rejected with violent coughing, the patient declaring he felt it as if entering the air passages. An examination of the fauces led to no explanation of this condition. The epiglottis did not, however, appear to completely close the larynx, or to be in its exact position. The tongue was moveable in all directions, and pressing it down with a spatula caused no inconvenience. The hyoid seemed to possess its continuity. No crepitation or abnormal moveability could be perceived, and no pain at the root of the tongue occurred on attempting to swallow. After repeated examination, the case was concluded to be one in which the functions of the nervus vagus had undergone great disturbance, or the muscles of the larynx had become torn or paralysed. Medicine and food were administered by means of an elastic tube. The patient had a good appetite and slept well, the pain of the neck was lost, and its motion recovered; a hectic cough, from which he had long suffered, alone remaining. After continuing, however, to go on thus well for six days, the cough increased, the appetite failed, strength was lost, the voice was scarcely audible, and in five more days the patient died exhausted. At the autopsy, a fracture of the os hyoides was found. One of the large cornua was broken, and had become firmly imbedded between the epiglottis and rima glottidis, inducing the raised position of the epiglottis, loss of voice, and difficulty in swallowing. The fracture was probably produced by muscular action, a cause first assigned in a case occurring to Ollivier d'Angers.—*Schmidt's Jahrbuch*, vol. lxviii, p. 215.

On Spintheropia, or Sparkling Synchisis. By M. SICHEL.

M. SICHEL in the present paper publishes the conclusions he has arrived at respecting *sparkling synchisis*, after a reconsideration of the particulars of all the published cases, now amounting to eleven. He thinks the term synchisis should be abandoned, bestowed as it was under the idea that a softened state of the vitreous humour is an essential feature, which subsequent observation has rendered doubtful—although, in the bulk of the cases, such softening is present. He proposes, as a better name, applicable to the various species, and indicating the pathognomonic sign of the affection, without prejudging the nature and seat of this, the appellation *Spintheropia*, from the Greek word σπινθηρ, a spark.

In *true spintheropia*, the shining particles are unattached, at their circumference, to any part of the eye; and are therefore liable, on certain conditions, to displacement from their ordinary situation at the lower parts of the organ. Although in two cases the particles were found in both chambers, and in two in the anterior, M. Sichel believes that they always originate in the deeper structures of the eye, finding their way only exceptionally into the anterior chamber. There would seem to be an *imperfect* or *pseudo spintheropia*, in which the particles are attached to some portion of the circumference of the eye. But only one example of this (observed by M. Robert, Gaz. des Hôp., 1847, p. 371,) has been recorded; and of the nature of this, various opinions are entertained. M. Sichel believes it to have been formed by the deposit of cholesterine upon the debris of a ruptured crystalline membrane.

Cause of the Scintillation.—Although this was declared, at first, to be a mere optical illusion, the actual passage of the shining bodies, and subsequently their extraction, placed their material character beyond all doubt. M. Malgaigne having found *cholesterine* in the eye, first suspected that this might be the matter furnishing the brilliant appearance; and M. Blasius referred it to the same cause. Subsequent examination of these bodies, after removal by Lebert, Mandl, &c., has proved the opinion to be correct. The floating particles probably consist entirely of cholesterine; but they have hitherto been examined only when deposited in albuminous substance, or around the debris of the crystallised and hyaloid membranes.

However agreed observers may be as to the material nature and composition of these particles, great difference of opinion prevails as regards their *seat*. M. Sichel believes that in all observations hitherto made, their primary locality has been behind the pupil; but does not agree with M. Blasius, that they are always found within the crystalline capsule, although he believes they may sometimes be developed there; and in M. Robert's case, they were formed on its anterior surface. Their exact locality posterior to the pupil cannot, however, be always assigned; but, in the majority of cases, the vitreous humour has been their seat, as might have been presumed from their cholesterinic composition, and from the fact of that body containing fatty matters. In those cases the vitreous humour usually manifested undoubted signs of liquefaction.—*Annales d'Oculistique*, t. xxiv, pp. 145-60.

On the Abortive Treatment of Gonorrhœa by Chloroform. By M. Venot.

M. Venot, of Bordeaux, states, as the result of a twelvemonth's experience, that injections of chloroform, though of little avail in confirmed gonorrhœa, are possessed of a complete abortive efficacy, if employed during the first week.—*Bull. de Thérap.*, tom. xl, p. 184.

Statistics of Operations.

As this subject has of late excited much attention, a contribution or two, even on a small scale, may not be without their utility. One of these is furnished by M. Schöning, a practitioner of Copenhagen.

Amputations.—In the course of several years, he has performed 54 amputations of large limbs, with the following results. Of these, 43 occurred in men, of whom 5 died; and 11 in women, of whom 2 died. They were as follows:

		Recovered.	Died.
Thigh . . .	19	16	3
Leg . . .	25	22	3
Arm . . .	3	2	1
Forearm . .	7	7	0
	54	47	7

All the fatal cases occurred in winter or spring (viz. 7 out of 27 amputations). In 48 cases, the amputation was performed for chronic disease, 42 recovering and 6 dying. In 5, it was resorted to after injuries, and all recovered. In 1, it was performed for club-foot, and proved fatal.

Hernia Operations were 81 in number, of which 29 died. They were :—

	Died.		Recovered.
23 inguinal in the male	10	...	13
7 inguinal in the female	4	...	3
51 crural in the female	15	...	36
81	29		52

In the winter months the mortality was 56 per cent., and in the summer months 20 per cent. The contents of the sac were :—

	Died.		Recovered.
In 52 intestine alone	21	...	31
In 3 omentum alone	1	...	2
In 26 intestine and omentum . .	7	...	19
81	29		52

The influence of the duration of the incarceration upon the mortality is thus shown :—In 39, with incarceration for *less* than three days, 8 died, or 22 per cent.; while in 39, with incarceration for *more* than three days, 20 died, or 51 per cent. Of 6 cases, in which it continued less than a day, none died; of 15, in which it continued from one to two days, 4 died; and of 10, in which it lasted from two to three days, 4 died.

Lithotomy.—Of 47 operations, 44 were in men, of whom 7 died, and 3 in women, none of whom died. In 29 cases, the lateral section (6 deaths) was performed; in 11, the bilateral (1 death); and in 4, the urethra alone was opened.—*Oppenheim's Zeitschrift*, Bd. xliv, p. 216.

Amputations.—Mr. Hayward, in an interesting paper, informs us, that from the period of the foundation of the Massachusetts Hospital until 1850, there have been performed 146 amputations upon 141 patients, of which number 32 have died. Of the 141, 85 were operated on for disease (10 dying), and 56 for injury (22 dying). The following is the account of the parts amputated :

	Died.		Recovered.
69 Thigh	19	...	50
50 Leg	10	...	40
11 Above the elbow	1	...	10
11 Below the elbow	2	...	9
141	32		109

The ages of the patients were as follow :—26 under 20 (4 died); 56 between 20 and 30 (11 died); 28 between 30 and 40 (10 died); 18 between 40 and 50 (5 died); 7 between 50 and 60 (1 died); 4 between 60 and 70 (1 died); 2 above 70 (neither died).—*Boston Med. and Surg. Journal*, vol. xliii, p. 181.

MIDWIFERY, &c.

On Kiestein. By Dr. VEIT.

IN consequence of the discrepancy of opinion which prevails among observers as to the value to be attached to the appearance of the urine termed Kiestein, as diagnostic of pregnancy, Dr. Veit has, during a year and a half, been conducting a series of experiments at the Halle Lying-in Institution. He has examined for this purpose the urine of 10 men, of 4 non-pregnant females, and of 48 women in various stages of pregnancy. He comes to the same conclusion as Höfle (*Chemie und Mikroskop am Krankenbette*) and, recently, Lehmann, viz., that the so-called pellicle of Kiestein is no peculiar matter at all, and is not of the slightest value as a sign of pregnancy. In urine of both non-pregnant and pregnant women, pellicles are formed containing vibriones, and frequently the triple phosphate; the chief difference between the respective urines being, that in that of pregnant women, alkaline, and in that of non-pregnant women acid, reaction more frequently manifests itself. This may in some measure depend upon the greater concentration of the urine in pregnancy, and the larger proportion of mucus mixed with it, as a consequence of the changes induced in the condition of the mucous membrane of the bladder by the passive hyperæmia of that organ during pregnancy. Persons partaking of a more nitrogenous diet than did the poor pregnant women whose urine was examined, might furnish different results in this respect.—*Zeitsch. für Geburt.*, vol. xxx, pp. 257—278.

Separation of the Cervix Uteri during Labour. By Dr. Johnston.

In this case, a first labour, the anterior lip of the os uteri was much prolonged and carried before the head. The pulse being frequent and hard, bleeding was resorted to, and belladonna was applied to the cervix uteri. About thirty-five hours after the commencement of labour, the head was suddenly delivered, under the influence of energetic pain; and carried before it a portion of the cervix uteri, separated in its entire circumference, and measuring at various parts from 1 to 1½ inches. Convalescence took place speedily; and the author learned that twelve months after this, the woman had been delivered of a second child in ten minutes, her labour coming on without any premonition.—*Am. Journ. Med. Sc.*, vol. xxi, p. 342.

Absence of the Uterus. By M. Depaul.

M. Depaul recently related a case at the Medical Society of Emulation, which he regarded as an example of absence of the uterus and vagina. It occurred in the person of a woman, æt. 22, having well-developed breasts and external sexual organs, together with marked venereal desire. In place of the vaginal opening was a simple depression; and by the simultaneous introduction of a finger into the rectum, and a catheter into the bladder, no body having any analogy to the uterus could be felt. The right ovary was supposed to be detected. Every month, she has all the symptoms of menstruation, except the flux.

M. Depaul, by careful examination, convinced himself that this case was not one of mere imperforate vagina; but, as the woman is still living, the exact state of the parts is not demonstrable. In a case, however, recently related by Dr. Ziehl, an autopsy was possible. This person, æt. 57, had been married for thirty-two years, but had never menstruated. She was feminine in appearance and inclinations, and the external sexual organs were completely developed. The vagina was very narrow, so as only to admit the finger for an inch, when it terminated in a blind sac. No uterus could be felt. The Fallopian tubes lay in the broad ligaments behind the bladder, the fimbriæ being normal. The ostium abdominale was open in both, but of the ostium uterinum no trace could be found. Behind and below the tubes lay the ovaries, somewhat wasted, wrinkled on their surface, dry and firm in structure, and containing only small thick nodules. Not a rudiment of the uterus existed.

L'Union Méd., No. lxxix, 1851; *Canstatt's Jahr.*, 1850, vol. iv, p. 322.

Examples of Large Infants.

Dr. Siebold, in a recent paper in the *Zeitschrift für Geburtsk.* (vol. xxix, p. 178), observes, that when new-born infants are not actually weighed, the most ridiculous exaggerations prevail in respect to the estimates of the weight of the larger ones. Since 1825, he has had all the children weighed at the Berlin, Marburg, and Göttingen Institutions, with which he has been successively connected, and the heaviest he has met with only reached $11\frac{3}{4}$ lbs., notwithstanding we peruse fabulous statements of 20 lbs. being attained.

That such statements, however, are not always fabulous, is seen from the fact of a recent instance recorded in the American Journal by Dr. Johnston, in which the child weighed exactly 20 lbs., and the placenta 3lbs. Its length was $25\frac{1}{4}$ inches, the breadth of the shoulders $8\frac{1}{2}$, and of the hips $7\frac{3}{8}$ inches. The occipito-mental diameter was $6\frac{3}{4}$ inches; the occipito-frontal $5\frac{3}{4}$, and the biparietal $4\frac{3}{4}$ inches. The labour was accomplished in eight hours; but, owing to the great delay which the passage of the shoulders and hips entailed, the child was still-born.

In another case recently observed by M. Depaul, the child which was born dead, with the epidermis detached, after version, weighed $6\frac{1}{2}$ kilogrammes (nearly $14\frac{1}{2}$ lbs.), and measured 62 centimetres (about 21 inches) in place of from 45 to 48, from head to foot.—*Amer. Journ. Med. Sc.*, vol. xxi, p. 341; *L'Union Médicale*, 1851, No. 22.

Compression of the Aorta in Uterine Hæmorrhage. By M. CHAILLY-HONORÉ.

M. CHAILLY-HONORÉ considers that this practice is not resorted to so frequently as from its merits it deserves to be; and believes, that had it been employed in one or two cases in which transfusion has been lately performed, it would have rendered that *dernier ressort* unnecessary, or would have enabled it to save life when employed. Rudiger employed compression so long back as 1797; but Ulsamer first advised its being applied through the wall of the abdomen in place of through the uterus. The practitioner, standing at the left side, passes his right hand between the uterus and intestines, seizes the vessel between the index and medius finger, fixing it firmly against the vertebral column, and pressing on his right with his left hand. If in 13 cases in which this practice has been resorted to, half the women died, this arose from its being deferred until they were *in extremis*, and all other means had failed. To these cases M. Chailly opposes 18 others, occurring in his own practice, and among which only one woman died, in whom also the application had been too long delayed. In some of these, compression was maintained for two hours without inconvenience. In the former series of cases the compression was delayed too long, and employed without rule, confidence, or patience. In the latter it was resorted to in time, and methodically continued. Of course the practice is not advocated as curative, but as a means of gaining time in an emergency, wherein time is everything.—*Bull. de l'Academie*, xvi, 731.

MATERIA MEDICA AND THERAPEUTICS.

On Medicinal Cigars. By DR. LANDERER.

THE employment of various organic and inorganic substances of a volatilisable nature in the cigar form, has frequently been resorted to. In this way stramonium, cicuta, Raspail's camphor, and corrosive sublimate, have been used by means of tobacco deprived of its nicotin. The great efficacy of this last substance in ulcerated syphilitic throat, in Dr. Landerer's hands, has rendered him very desirous of extending this form of medication. He prepared cigars, therefore, by moistening tobacco freed from nicotin with tinct. of iodine, a solution of iodide of mercury in sulphuric æther, or a solution of iodide of potassium. He found these cigars of great utility in syphilitic ulceration of the throat and in ozæna. So, too, by moistening the tobacco with an æthereal solution of hyoscyamin, he has relieved most obstinate spasmodic cough without inducing any narcotism. Among other substances tried, he found a solution of creasote in spirit of wine and æther, a very useful form in scorbutic ulceration of the gums. Cigars moistened with *tinct. moschi* relieved hysterical and spasmodic coughs; and a case of severe hysterical paroxysms, occurring in an irritable subject, was advantageously treated by the alcoholic solution of the acetate of morphia. Cigars formed of this substance are also very useful in the toothache. Arsenical cigars, formed by steeping the tobacco in Fowler's solution, have also been employed; and Dr. Landerer believes that this form of medication might be extended to a great variety of substances.—*Buchner's Repert.*, B. vi, p. 347.

On the Filix Mas in Tænia. By Professor ALBERS.

PROFESSOR ALBERS, while noticing the great success that has attended the use of kousso in England, observes, that he has derived so much benefit from the employment of the *Extractum Filicis maris æthereum*, that even putting aside the difference in price, he does not think the kousso ought to supplant it. For two or three days the patient lives sparingly, and the day before he commences the filix he takes a dose of Glauber salts. Next morning 30 grs. of the extract are given, and the dose is repeated in one hour, a dose of castor-oil being given one or two hours yet later. In from six to eight hours after the first dose of extract, the worm is expelled; and in only two out of about 100 cases has Dr. Albers known this prompt action to be wanting. In one of these the dose required to be repeated, and in the other to be much increased.

Much of the efficacy of the extract depends upon its efficient preparation. Only the fresh root should be used. Collected in May or June, it must be macerated in ether for several days, the infusion being frequently shaken. This extract is more easily taken than is the kousso.—*Casper's Wochenschrift*, 1850, No. 31.

On Ferruginous Preparations. By M. MARTENS.

THE following are the conclusions of an Essay recently read by M. Martens at the Belgian Academy of Medicine. 1. That as a general rule the *lactate of iron* is the best preparation. This conclusion, founded on the chemical fact of the conversion of the other preparations of iron into lactates in the stomach, was disputed as regards its therapeutical truth, by M. Lombard and other members, who denied that the lactate possessed any superiority. 2. It may be advantageously replaced by the carbonate given in water, or in pills made with honey, so that superoxidation may be prevented. 3. All ferruginous pills in which the metal is liable to superoxidation should be rejected, because they soon become indurated, so as to be soluble neither in water nor in the juices of the stomach. 4. Insoluble ferruginous preparations ought always to be administered at meal-time, in order that they may become dissolved in the acid juices then existing in the stomach. 5. Those preparations should be chosen which cannot be precipitated, or rather rendered quite insoluble by the alkaline juices of the duodenum, especially during intestinal digestion. 6. The most active are those which having penetrated into the blood in their liquid state, are there most easily assimilated with the hæmatosine, so as to form with it the red colouring matter of the blood. 7. In the treatment of chlorosis or anæmia, it does not suffice to prescribe preparations of iron, but their assimilation should be aided by residence in the country, or in localities well exposed to the sun's rays. 8. The regimen in chlorosis should, as far as possible, be composed of succulent and dark coloured meats, and not of white alimentary substances, in which the oxide of iron is usually defective. 9. Slight or recent chlorosis may be generally cured by animal regimen alone, in combination with exercise in the open air, and insolation. 10. The habitual use of meat introduces into the economy sufficiency of iron for the formation of the red globules, and may give rise even to their excessive formation. 11. On the other hand, the exclusive use of potatoes, white bread, vegetables, and fatty substances, the ordinary regimen of the working and poorer classes, predisposes to chlorosis or an anæmic alteration of the blood, because such aliments contain too little iron to concur efficiently in the formation of red globules. 12. Wheaten bread may be rendered much more restorative, by adding, prior to panification, a little *sulphate of iron*, and it is only thus that an alimentation entirely capable of replacing meat can be furnished. 13. We may estimate approximatively at a *minimum* of two grains the quantity of oxide of iron that is daily required for the restoration or renewal of the blood; and for alimentation to suffice for the maintenance of health, it must contain this quantity. 14. All persons who in consequence of a too slightly animalized regimen, or of residing in badly-lighted localities, are disposed to anæmic vitiation of the blood, should employ such ferruginous bread to favour the formation of red globules. 15. Manganese only entering into the constitution of the blood-globules in an infinitesimal quantity, does not appear necessary for sanguification. Compounds of this substance cannot be considered as antichlorotic medicines, like ferruginous preparations. At least they do not concur directly in the restoration of the blood.—*Gaz. Méd.*, No. 48, 1850.

On the Medicinal Employment of Extracts of Flesh and Blood. By MM. BRESLAU and MAUTHNER.

DR. BUCHNER states, that such great benefit has been derived from the employment of Dr. Breslau's *extractum carnis*, as a remedy in the diseases of exhaustion in children, that it ought to find a place in the materia medica. Fresh ox-flesh, freed from fat, first finely chopped up, and then well beaten in a stone mortar, with a little cold or luke-warm distilled water, is afterwards submitted to a good press. The cake is again similarly treated, and when the juice is thus pressed out of it, it may still, when seasoned, be advantageously employed as food. The juice, reddish in colour, is immediately heated sufficiently to coagulate the albumen, and is then evaporated in a water bath to the ordinary consistency of an extract. As ordinary ox-flesh contains only 1 in 1000 of Kreatin, while that of the heart, according to Gregory, contains from 1·37 to 1·41, this is the part employed by Dr. Breslau at the chief apothecary establishment in Munich. The extract is of agreeable odour and taste, and is easily soluble in water, when it reddens litmus. By the addition of caramel to the juice, the taste and consistency of the extract is much improved.

In the exhausting diseases of children, Dr. Mauthner strongly recommends his *extractum sanguinis bovis*. Fresh blood caught from the slaughtered animal is passed through a sieve, and then evaporated in a water-bath to dryness, rubbing it up into powder when cold. From 10 to 20 grains are given per diem in a little water, the solubility being increased by the addition of a few drops of spirit of wine. Dr. Mauthner has now employed it with great success in about twenty cases, several of which were reduced to an apparently desperate condition before commencing with it. Four cases are related as examples. 1. A girl, æt. 7, had suffered from catarrhal diarrhœa during eight days, which completely reduced her. She took ℈j of the extract daily, from the 28th of August to the 10th of September, when she left quite well. 2. A girl, æt. 12, was reduced to a mere skeleton by diarrhœa; and after being treated by various means, and constantly getting worse, she commenced the extract on the 8th of September, and was quite cured by the 27th. 3. A child, æt. 7, very liable to scrofulous ophthalmia, and now reduced to the lowest point by diarrhœa supervening on hip-joint disease, continued the extract from the 8th to the 22d of September, when he left the hospital cured as regards his immediate cause of exhaustion. 4. A child, æt. 4, suffering from hectic and manifesting bronchophony, had a *fistula ani* form, and was reduced to a complete state of anæmia. He recovered by continuing the extract from the 1st to the 12th of September.—It is by no means a disagreeable remedy; and a child will take it when it will not take or rejects ordinary medicines. It does not appear in the stools, scarcely a trace is found in the urine, and it is never vomited. It is especially useful in what Dr. Mauthner terms *exhaustio scrofulosa*, and the child will take it much better than the *ol. jecoris*. It is of no use in the acute marasmus and anæmia of young infants, due to bringing up by hand, who are brought to the hospital during the last days of their wretched existence.—*Buchner's Repert.*: No. 19, p. 90; *Journ. für Kinderkrank.*, xvi, p. 56.

On the Employment of Tannin. By Dr. Cummings.

Dr. Cummings states, as the result of several years' experience, that he has found tannin the most valuable of *astringents*. Thus, whenever, in dysentery, medicines of this class are indicated, it acts admirably, either given alone or combined with opium. He says, he could refer to more than a thousand cases of dysentery, diarrhœa, cholera infantum, &c., in which he has employed it, never with regret, and almost always with advantage; while other practitioners, with whom he has communicated concerning it, express similar opinions. In the sweating or last stage of phthisis, or low continued typhus, and even in the worst cases, this accompaniment of diseases of debility has been entirely or in part relieved. It is useful in almost all forms of hæmorrhage, and most remarkably so in hæmoptysis; and when combined with opium and ipecac., it forms a medicament very preferable to acetate of lead and other similar substances. Among other forms of hæmorrhage, over which it exerts great power, is that from the bowels resulting from dysentery, and that which occurs in threatened abortion. In hæmorrhoids, it is of great use as an outward wash. In epistaxis, it may be snuffed up or blown through a quill, and will almost always arrest the bleeding. No article in the whole class of astringents acts like it in severe salivation. In aphthæ and other diseases of the mouth, in which there are spongy or bleeding gums, it possesses no equal. Used as a gargle in relaxed uvula and tonsils, its efficacy is great. As an *antiseptic*, for cleaning old, foul ulcers, the author has extensively used it in the form of a powder, especially when there is disposition to hæmorrhage. As an astringent collyrium, it is, in his opinion, preferable to all other substances in the purulent ophthalmia of infants. He administers it internally in 2-grain doses.—*Boston Med. and Surg. Journ.*, vol. xliii, p. 40.

FORENSIC MEDICINE AND MEDICAL STATISTICS.

On the Effects of Poisoning by Alcohol, considered in relation to Juridical Medicine. By M. Rösch.

The alcoholic fluid which M. Rösch's observations chiefly relate to, is *brandy;* and he considers, first of all, the consequences of *slow or chronic poisoning* by this substance, as observed in the bodies of persons submitted to official inspection, who have met with their death from accident or suicide. The changes which have been, to a greater or less degree, found in the bodies of all spirit-drinkers, are thus summarily mentioned.

1. The *brain* itself has exhibited no constant changes of sufficient account; but its *membranes* have always manifested more or less diseased conditions. Of these the partial *thickening of the arachnoid,* giving it a milky-white appearance, has been especially observed. Commonly, too, colourless fluid, though in general not in very large quantities, was effused between its layers, and was also found in the spinal canal. In several cases, some serum was found in the cavities of the brain, and the spinal marrow had become softened by imbibition of such fluid. In several cases the membranes of the brain had grown together, but in others the dura mater was only adherent to the cranium. These changes have all been observed in cases in which, during life, no signs of inflammatory action or of effusion were present, —unless we are to consider as such the decrease of mental activity, and the blunting of all sensibility, both general and special.

2. The *lungs* exhibited various diseased appearances. Of these *œdema* was a frequent one, a colourless or reddish frothy fluid flowing out on incision, and escaping in large quantities when pressure was applied, the compressed parts retaining the impression of the fingers. In several cases, lobular *emphysema* was observed. Adhesions of variable extent to the ribs and diaphragm occurred; and in certain places the investing membrane of the lungs was thickened.

3. The mucous membrane of the *stomach* exhibited isolated, bright red, punctated spots, and this especially near the pylorus. Similar groups were observed in the duodenum, jejunum, and ileum. The mucous membrane of the small intestine was much thinned; the muscular, likewise, in a less degree; but the serous remained unchanged. The mucous glands of the small intestines were enlarged.

4. General emaciation, and a whitened appearance of the muscles was observed, as well as laxity and thinness of the walls of the heart. On the other hand, a considerable quantity of fat was found deposited under the skin and between the muscles. The mesentery, heart, and kidneys were covered with fat; and the liver so penetrated with it, that, in many cases, its texture seemed as if converted into adipose substance.

5. The blood in the vessels was dark and diffluent. The spleen, as a rule, was softened, and, in several cases, pappy.

(2.) *Acute Alcoholic Poisoning.*—In strict language every intoxication and stupefaction by spirits should be called poisoning; but as intoxication is of daily occurrence without danger to life, it is only so considered here, when urgent symptoms, requiring medical aid, are present. Cases are, however, not wanting, in which paralysis, soon ending in death, has followed this undue stimulation by alcohol; and the author supplies the particulars of such as have come under his notice. In these, besides the appearances due to chronic poisoning, others due to the rapid influence of the spirit upon the body were observed,—viz., a considerable repletion of the brain and its membranes with blood, and a congested state of the lungs (in one case acute œdema pulmonum being present). The immediate cause of death in those who die soon after taking a large quantity of spirit, is arrest of blood in the central organ of circulation and the respiratory organs,—a state of asphyxia. Such effect upon the circulation and respiration is, however, but a consequence of the repletion and paralysis of the brain by blood containing alcohol. In acute alcohol-poisoning, not only is the ingested spirit found in the digestive canal, but the various visceral structures and fluids of the body strongly smell of it, and are, therefore, penetrated by it.

In violent deaths it may often become a matter of importance and difficulty to state what part alcoholic fluids have exerted in producing the fatal termination. Two cases are given by the author, in one of which an effusion of blood was supposed to be due to external violence; but that this was the case could not be positively stated, since, during the state of distension of the blood-vessels in drunkenness, they are ill capable of resistance, while the blood itself is in a dissolved condition. The effects upon the brain do not arise from a simple excess of healthy blood, but of a blood which has undergone change, which in acute spirit-poison still contains the substance inducing this.

While the nervous system is stimulated and enfeebled through this changed condition of the blood, so also, in a reverse order, the blood, heart, and circulation are disturbed and enfeebled by the condition of the brain and nerves; so that here is a continual reciprocal mischievous influence of the blood and venous system going on, until the disturbance of the economy becomes complete, physical disease prostrates the body, and all controlling power and mental activity are destroyed.—*Henke's Zeitsch.*, Band lx, pp. 241—279.

On the Influence of the Manufacture of Phosphorus-Matches on the Health of the Work-people. By Dr. Ebel.

DR. EBEL first gives a summary account of all that has been written upon the subject, and then states the result of his five years' experience in the factory at Waldmichelberg, whence large quantities of matches are exported to all parts of Europe and America. He agrees in opinion with Dupasquier, Jünken, Helftt, and others, that the disease is not produced merely by the influence of the phosphoric vapours. He describes the factory at large, in consequence of its excellent arrangement, as respects ventilation and other particulars. There are about 200 individuals employed, partly male and partly female, most of whom are between 14 and 18 years of age. The hours of employment are from five to seven, with two for meals, which are taken either in the open air, or in a separate part of the establishment. No person suffering from constitutional disease is admitted among the work-people; and the general state of health of those employed has been highly satisfactory,—no scrofulous or cachectic manifestations having occurred, and not a trace of necrosis of the jaw-bone being discoverable.

As the disease of the jaw has been usually believed to commence with a diseased state of the teeth, forming the *route*, as it were, for the admission of the phosphorus vapour, the author examined the mouths of all the work-people with great care, and exhibits the result in a table. Caries of the teeth is a very common affection of the locality, and these young people offered no exception; for of the 200, 153 exhibited one or more, and in the majority of cases the caries had existed for a longer period than they had been employed in the factory. The health of these was as good as that of the others; their appearance being healthy, and their dispositions lively. Experience has repeatedly shown, that after a long employment in the factory, (two, three, or four years,) the young people assumed a better appearance and development than their relatives who were exposed to the domestic discomforts of their station. The work is no-wise laborious, but easy; and the work-rooms are spacious, clean, and airy; and the vapours of phosphorus seem far less unhealthy than the emanations of the low, dirty, and damp dwellings of the locality. Since the establishment of the factory, some of the diseases of the locality have become less prevalent, especially the itch, which, once so common, is now, probably in consequence of the sulphur impregnations, rarely met with. The proprietor of the establishment, who has conducted it for eleven years, has never met with a single case of disease of the jaw.—*Casper's Wochenschrift*, 1851, Nos. 10, 11.

[We regard this paper as a very important contribution, exhibiting, as it does, the great power which sanitary precautions possess in warding off the noxious effects of vapours, which, under other circumstances, produce such fearful consequences.]

On the Detection of Lead in a Body that had been interred nearly Two Years. By DR. MAYR.

ALTHOUGH lead-poisoning is of such common occurrence, it seldom takes place in a manner calling for judicial investigation. Dr. Mayr has, however, recently published an interesting case of this kind. The body of a clergyman was disinterred after twenty-one months' burial, as it was reported he had been poisoned. The head had become a complete skeleton. To the chest hung some dark green fleshy substance. Within its cavity more remains were found of the right than of the left lung. Below the diaphragm a mouldering mass was found, apparently formed of the remains of the liver, stomach, spleen, and small intestines,—a yellow spot, the size of a hen's egg, resembling effused bile existing in the region of the liver. Of the ascending colon little remained; but the rectum, and a small portion of the sigmoid flexure were observed. The fæcal remains were of a blueish-white colour, and shone remarkably through the serous membrane. The inner lining of the intestines seemed thickened, and in parts ulcerated. After all had been removed, the cavity of the pelvis exhibited a shining white, almost silvery appearance. On testing the remains of the abdomen, lead was freely detected and reduced, no arsenic, which was also sought for, being present. Several grains of lead were reduced from 3 oz. of the lung substance, and a drachm from the same quantity of the intestinal contents,—although no flux was employed to facilitate the reduction.

When the history of the case was investigated, it was found that the poisoning had been going on for ten months of the year 1844. Four physicians, who had attended him under supposed attacks of gastric fever, and latterly an ulcerated condition of the bowels, now deposed to the existence of symptoms of lead poisoning, though this was not suspected during life. As severe abdominal pain always came on after the performance of mass, it was now supposed that the wine used had been poisoned. No lead was prescribed for him among his medicines during his long illness.

From the fact of the poison having been found in the lungs, it must have been introduced into the economy as a soluble salt, which, from its solubility and tastelessness, was probably the acetate. Large quantities must have been administered for a long period, or it would not have been found in such abundance, and diffused over so great an extent; it being estimated, from the quantity obtained, that at least 30 drachms must have been contained in the body. It is of importance to remark, that although the body had been buried for twenty-one months, and no traces of the brain, heart, or other muscles remained, the poison was yet detectible in the remaining portions of the lungs and intestines, and that it had undergone a partial reduction through the putrefactive process in the body itself. That the acetate was not, however, completely decomposed, was seen by procuring from the intestinal contents a soluble salt. This shows also that the acetate, like other metallic salts soluble in water, more or less resists the putrefactive process, and that the decay of organic structures impregnated with it may be delayed,—the putrefactive process, which ordinarily commences in the abdominal organs, being in this case much less advanced in the large intestine than in other parts of the body. The finding so considerable a portion of the lead in the lungs, is also therapeutically interesting, in regard to the administration of this substance in hæmoptysis and phthisis.

It seems that the minister, who was formerly mild in his manners, became latterly morose and impatient, exhibiting signs of the peculiar melancholy generated in *tabes metallica;* while with this were conjoined all the ordinary symptoms of gradual lead-poisoning, even to the occurrence of attacks of paralysis.—*Buchner's Repert.*, vii, 187—202.

On the Rupture of the Internal Coats of the Carotid in Hanged Persons.
By Dr. MILDNER.

AMUSSAT first observed the inner and middle coats of the carotid divided in a hanged person, as if the vessel had been tied. Devergie, examining the bodies of thirteen persons, found it ruptured only once, and then the vessel was superficially placed, and pressure had been especially directed to the side (left) on which the rupture had occurred. Kloz relates a case in the *Med. Zeit.*, 1850, in which the left carotid was similarly ruptured; and, in the present communication, Dr. Mildner furnishes a new example. A strong and heavy woman, æt. 48, hung herself by means of a hempen cord as thick as the little finger. The brown-yellow, parchmenty, dry mark passed backwards between the hyoid and larynx, and then rose towards the head. The right side comparatively escaped from the pressure and was unmarked, while the left side was strongly marked, and at the upper part excoriated. The portion of the left carotid corresponding to the lower edge of this mark, had its internal coats divided in two places, situated at half an inch from each other. The edges of the rupture were everted, slightly irregular, and very red from imbibition. The base of the rupture was formed by the cellular coat, which, for about the size of a bean, was of a blueish-red colour, covered by a thin layer of effused blood, and much injected and infiltrated with bloody serum. The left large cornu of the os hyoides was broken obliquely. The woman having suspended herself from a door-post, the pressure of the cord was concentrated on the left side of the neck. The rigid state of the vessel diminished its elastic power of resistance; and experiments made by the traction of arteries, prove that rupture most readily takes place in those which are maintained fixed. Whatever tends to diminish their elasticity favours their rupture, and hence the greater liability of the aged. The rarity of the coincidence of some of these conditions must explain why the appearance is so rare in hanged persons, and so seldom produced in experiments,—Mallett only having induced it in two out of eighty-two bodies.

Dr. Mildner adverts to the question sometimes debated by jurists, as to whether this rupture is a proof of hanging having taken place *during life;* and, from the above experiments of Mallet, he agrees with Devergie in thinking this very doubtful. If, however, we compare the rupture caused during life with that made experimentally, we may observe essential differences. In the first case, there is surrounding injection into the cellular coat, swelling and infiltration of this, together with thin layers of effused blood; which appearances are not observed in the other. The condition of the edges of the wound is alike in both cases, the redness from imbibition being as easily produced after as before death.—*Schmidt's Jahrb.*, lxviii, 240.

On the Detection of Mercury in the Body of a Person Dying of Mercurial Cachexy. By M. GORUP-BESANEZ.

THAT quicksilver is one of the metals capable of absorption into the economy is a well-known fact, detected as it has been by various chemists, not only in the blood, but in the secretions of various organs, and especially the saliva, and in the structure of the organs themselves. But as to the mode of its distribution, the duration of its presence in the various organs, and whether it is found in all or certain tissues only, are points yet to be investigated. Dr. Gorup-Besanez relates the results of a recent investigation of the body of a woman, who was long (twenty-five years) laboriously engaged in silvering looking-glasses, but who, from the convulsive tremors that were induced, had been obliged to desist from her occupation for a year prior to death.

The somewhat collapsed brain did not entirely fill the skull, and the dura mater was of a reddish-blue from venous congestion. The consistency of the brain was firmer than usual. The lungs were hepatized, loaded with dark-coloured blood, and non-crepitant.

The chemical results obtained by following the processes of Fresenius and Babo were as follows. The lungs and heart gave no traces of mercury; a very small quantity was detected in the liver, and none in the bile. A doubtful precipitate was thrown down upon the gold plate by the brain, while the spinal column presented no traces. That any remains at all should be found after a year is remarkable, and is confirmatory of other facts, proving how long certain metals, *e. g.* antimony, may be retained in the economy. That the liver was the only organ in which it could then be detected, confirms the doctrine that metallic poisonous substances are usually longest found in that organ.—*Buchner's Repert.*, vii, 178—186.

On the Duration of Medical Life in Prussia. By Dr. CASPER.

DR. CASPER has, in former publications, exhibited the short probability of life attached to the medical calling, as compared with that of various other occupations; and the present communication forms a kind of confirmatory appendix. The source whence it is drawn is the 'Prussian Medical Calendar, for 1851,' which gives a list of all the Prussian physicians, and the dates of their diplomas. Dr. Casper, assuming the diploma to have been granted, upon an average, at the age of 23, thence deduces the present ages of the 3462 doctors. Of these, thirty-three only received their diplomas in the last century, *i. e.* not 1 per cent. of living Prussian physicians are 74 years old and upwards. The Nestor of the profession is Dr. Druffel, of Münster, his diploma dating 1786, and his age being 87.

The next fact that results is the remarkable preponderance of young physicians, —not less than a fourth part of the entire number being between the ages of 24 and 29, the proportion in Berlin reaching to one third, (31 per cent.) Again, almost the half of the entire number ($47\frac{1}{3}$ per cent.) are between 24 and 34. The age of ripest experience and mental activity of men in general is from 45 to 60; but within this period hardly a fifth ($21\frac{1}{4}$ per cent.) of the Prussian physicians are found, and only one sixteenth part ($6\frac{1}{3}$ per cent.) of the body are above 60. Seeing, too, that the elder practitioners are chiefly consulted by the well-to-do portions of society, it results that the great mass of the public are in the hands of young physicians, as is also the carrying on of the literature and scientific progress of the profession. If we consider that almost one half of the profession is below 34, and that at this age the *nisus scribendi* especially prevails, on perusing any new work or article in a journal on practical medicine, we may estimate the probability at more than two to one that the writer who is instructing us is between 24 and 34 years of age, and we must appreciate his recommendations accordingly.—*Casper's Wochenschrift*, 1851, No. 3.

[Judging from the length of time that his valuable writings have been before the public, the learned author of this communication must have long since attained the age which he regards as that of medical maturity. But we think he is somewhat hard upon the younger members of the profession; and can scarcely pity a public that can command, at a moderate rate of payment, the active, mental, and bodily exertions which are brought into play prior to the age of 45. Certainly it is often to be wished that the writing part of the business were somewhat delayed.]

Statistics of the Medical Profession in Paris and Russia.

On the 1st of January, 1849, there were 1389 doctors of medicine in Paris, being 53 less than in 1847; and now, in 1851, the number is reduced to 1351, being a still further diminution of 38. During the last two years 65 have died, while, during the two former years, only 56 died, and still fewer during the prior periods. In 1843—7, the mean annual mortality was 1 in 75; in 1848-9, 1 in 50; and in 1850-1, 1 in 42. There have emigrated 86, of whom 12 have repaired to California. Towards following up the vacancies, 113 new doctors have been made during the last two years.

Officiers de Santé have, however, increased from 156 in 1849, to 178 in 1851, and the *pharmaciens* from 363 to 381. *Sage-femmes* have diminished from 480 in 1847, to 385 in 1849, and 350 in 1851.—*Révue Medico-Chirurgicale*, ix, 46.

According to the official lists, published January 1851, there were, in the entire Russian empire, 7957 doctors possessing the right to practise, 552 veterinary surgeons, and 132 oculists, dentists, and others possessing restricted rights of practice. There were also 714 *pharmaciens* having authority to sell medicines, viz. 77 in the two capitals, 150 in the governmental towns, and 487 in other parts. Siberia and the oriental governments of the empire only possess 19 civil practitioners; and the insufficiency of this number has given rise to the establishment of a medical school at the university of Kazan. In the course of the year 1850, there were treated 737,442 patients in the hospitals, of whom 609,564 are returned as cured, and 91,545 as dead, *i.e.* a mortality of 1 in 13.—*L'Union Médicale*, 1851, No. 37.

[We frequently see statements quoted in the foreign journals with respect to the medical statistics of our own country, in which great discrepancies prevail. The inquiries made during the late taking of the census furnish the means of giving authoritative information upon this point, which it is to be hoped will at once be made public.]

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