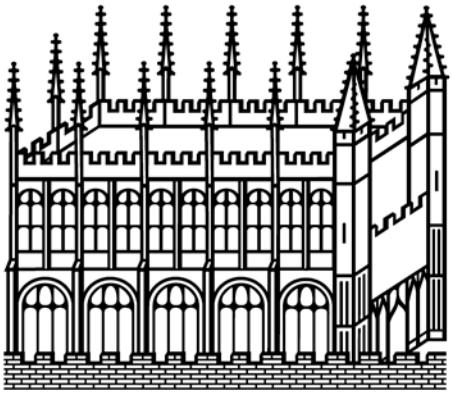


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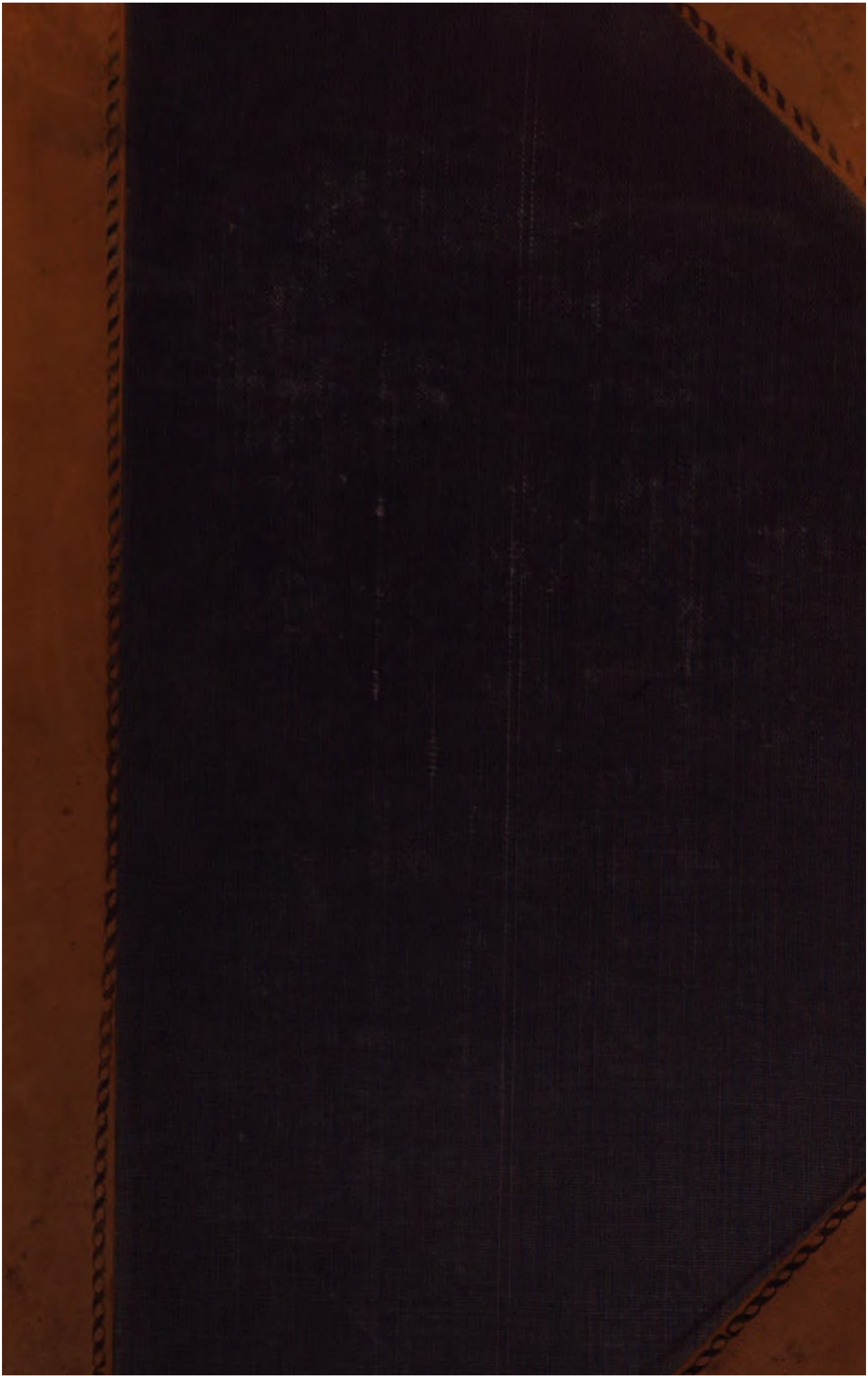
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THE

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OR

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OF

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JULY — OCTOBER, 1858.

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Bordeu, who was a man of humour and of sense, says: "I was trained to bring all my powers to bear against disease, even in its infancy. 'Principiis obsta, serò medicina paratur; sola remedia sanant; in extremis extrema; melius est dubium quam nullum,' &c. Such were my axioms. A happy chance cooled my desire to show to the amazed friends and the patient himself an array of bleeding basins. When

very young, I was called in as the fourth physician to a patient attacked with fever, pain in the side, and bloody expectoration. I had no advice to give. The first consultant proposed a third bleeding; the second, tartar emetic, followed by a purgative; the third, a blister to the legs. The contest was long, and nobody would give way. For my part, I thought they were all in the right. The dispute continued. Various partisans among the friends of the patient sided with the various physicians. Nothing was done. In spite of the terrible predictions of my three masters, the patient, reduced to fever diet, recovered. I watched his case with interest, and alone, for he was deserted by the rest."

Another anecdote. "The Seranes, father and son, were physicians to the hospital at Montpellier. The son was a mere theorist, who knew by heart, and repeated continually, all the formulæ for inflammation. Serane père had been taught by the great masters. The good man had learned to treat inflammations of the chest with tartarized antimony. He gave it at least every other day, in emetic doses, with or without the addition of two ounces of manna. That was his grand *cheval de bataille*. I have seen him order it a thousand times—everywhere, and for everybody. The son endeavoured to convert his father, to bring him into the fashion, and to make him dread phlogosis, erethism, and the laceration of the small vessels. The father fell into a singular species of indecision, and knew not what to do. He, notwithstanding, held firm against bleeding; but after looking at the patient, he would mutter to himself, order nothing, and pass on. I have seen him, when restrained from giving tartar emetic, apostrophize his son—'Mon fil, m'abès gastat! mon fils, vous m'avez gâté!' Never shall I forget this strange scene. I owe him much, and his patients owe him much also. They were cured, almost without being bled, because the elder Serane loved not bleeding; and without tartar emetic, because the younger Serane proved to his father that this remedy increases inflammation. The sick were cured, and I profited by the lesson; and concluded that the bleedings which Serane the son multiplied when left to himself, were at least as useless as the repeated emetics to the use of which Serane the father was so much addicted."

The elder Serane is the type of some perplexed physicians of the present day, and Bordeu himself of others—thinking men, who, watching disease, do not meddle with Nature unless she herself points out to them what to do.

The almost total abandonment of the lancet of late years has led to the controversy as to the treatment of the internal inflammations, and more especially pneumonia, which has been maintained in Edinburgh during the whole of the past year. Dr. Alison and the disciples of the older but advancing school of medicine account for the change in practice by a change in the type of disease; Dr. Bennett and the men of the "advanced" school attribute the change in practice to the sounder views of pathology now prevailing.

If this controversy were a mere attack upon, or defence of, the practice of former days, it would do harm rather than good; but it

will be of real service if it lead to a sounder and more discriminating practice, based on a strict knowledge of the laws of disease, of the powers of recovery, of the tendencies to death, and, above all, on the effect for good or evil of bleeding and other modes of treatment.

Although the treatment of inflammation is the avowed object of the controversy, it has practically almost narrowed itself to the treatment of pneumonia by bloodletting.

We shall consequently devote this article to the effect on pneumonia of practising or abstaining from bleeding.

However sanguine we may be as to the power of "an advanced diagnosis and pathology" to regulate treatment, we must submit the proof of that power to the rigid scrutiny of the actual effects in practice of the treatment of pneumonia, with and without bloodletting. We shall now therefore bring together the results arrived at by various observers, and then consider the whole subject.

The controversy itself has been carried on, with earnestness and signal ability on both sides, in the numbers for last year of the 'Edinburgh Medical Journal;' we give in a few words the position taken by the more prominent champions of both sides, and shall then proceed to examine the whole question as fully as our opportunities will permit.

Dr. Alison is convinced that the change which has taken place in the usual phenomena of inflammations, and the usual opinion of medical men as to the effect of bloodletting, is chiefly due to this, that inflammation now *very seldom* occurs *with such symptoms* of febrile reaction as Cullen has described as demanding and bearing full bleedings; and further, that very often it is attended with typhoid symptoms. If blood is taken in such cases, it wants the firm buffy coat of former days; faintness is brought on by a smaller loss; and there is no such encouragement to a repetition of the bleeding, from the pulse speedily regaining its strength, or from the local symptoms abating and quickly recurring. We must now, therefore, abstain from strong depletion, and, if needful, commence the use of stimuli. Gangrene is a more frequent result of pneumonia now than it was formerly.

While Dr. Alison thus sustains his original position, that the abandonment of the lancet is owing to a change in the character of inflammation from a sthenic to a low type, Dr. Bennett, in an important paper in reply to that of Dr. Alison, holds his original position, that the change in treatment is the result of an advanced pathology and improved powers of diagnosis.

In this paper, which has been prepared with great care and earnestness of purpose, and in the spirit of truth, Dr. Bennett makes the following series of propositions:

"Proposition 1.—That little reliance can be placed on the experience of those who, like Cullen and Gregory, were unacquainted with the nature of, and the mode of detecting, internal inflammations.

"Proposition 2.—That inflammation is the same now as it has ever been, and that the analogy sought to be established between it and the varying types of fevers is fallacious.

"Proposition 3.—That the principles on which bloodletting and antiphlogistic remedies have hitherto been practised are opposed to a sound pathology.

"Proposition 4.—That an inflammation once established cannot be cut short, and that the only end of judicious medical practice is to conduct it to a favourable termination.

"Proposition 5.—That all positive knowledge of the experience of the past, as well as the more exact observations of the present day, alike establish the truth of the preceding principles as guides for the future."

Dr. Bennett then gives an analysis of the statistics of the treatment of pneumonia by bleeding, the treatment by diet without bleeding, and his own treatment, directed to further the natural progress of the disease. To these we shall return when we review the whole discussion, and the extensive series of cases that we shall bring to bear upon these questions from various sources.

Dr. Alison and Dr. Bennett sustain their original views with great ability in the May number of the 'Edinburgh Medical Journal.' The former, at p. 978, gives an interesting anecdote in Dr. Gregory's own words of the extent to which that physician used bloodletting. He bled a young man for pleurisy. The remedy certainly was not spared. It was employed thrice in twenty-four hours, a pound at a time, without the least benefit. He bled him again on the second day to thirty-two ounces, the largest bleeding he had ever ordered. Next day he was worse than ever, so he took twenty ounces from him when sitting erect in bed. This time he bled him into convulsions; one or two pupils took fright, and ran out of the room. The doctor himself was uneasy, but not so disconcerted as his pupils. On laying him down the spasms went off, and from that moment he was in a manner cured. The man lost in all ninety-seven ounces of blood. Dr. Alison details two cases from his own practice. One, an old lady of eighty-six, who only the other day suffered severely from pneumonia. She was blistered, purged, and nauseated; morphia was administered and, after the pain subsided, a little wine. She resumed her usual active habits within a month from the seizure. The second case, a gentleman, aged sixty-seven, was treated, in November, 1841, for severe pneumonia. He was bled six times and cupped twice in five days! He had a second attack, under the care of Mr. Benjamin Bell, in November, 1848, when no blood was taken, and before the end of the week he was allowed porter several times in the day.

In the course of his paper, Dr. Alison makes the following remark:

"If Dr. Bennett's pathology leads necessarily to the belief, that the principle in Therapeutics which the great body of practitioners, since medicine has been a subject of reflection to mankind, have adopted in regard to the effect of bloodletting in the early stage of inflammatory disease, is false, I confess that I should think that a much better reason for setting aside his pathology than their therapeutics."

Dr. Watson, in a note to his Lecture on Inflammation, in the recent edition of his Lectures (vol. i. p. 231), sides with Dr. Alison in this important discussion. That note is in the hands of our readers, and will be carefully studied by them.

Dr. W. T. Gairdner, in an able paper in the September number of the Edinburgh Journal, attacks Dr. Bennett's view that "an advanced diagnosis and pathology" have been the chief causes of the change in

the practice of bloodletting. In support of his view, he relates a case of pneumonia treated by Dr. Bennett himself in 1845, with venesection, cupping, antimony, and opium and calomel to ptyalism, and that although there was little pain or fever, and almost no dyspnœa.

Dr. Markham, who gave an important paper bearing upon this subject in the 'Edinburgh Medical Journal' for 1855, has recently published in the 'Lancet' some well-written and closely-argued papers on the inflammation and bloodletting controversy.

Bouillaud's plan of bleeding *coup sur coup*, with the view of "jugulating" inflammation at once and by a "*coup de grace*," has given him the name of being the most sanguinary physician in Europe. This is his "formule:"

Bleed in the morning of the *first day* to sixteen, and in the evening to twelve or sixteen, ounces. In the interval, cup to the same amount, or apply thirty leeches. On the *second day* bleed again, and if pain still continues, cup or leech. The disease, fortunately, for the most part yields on the *third day*. If otherwise, don't hesitate, but bleed again. If by a rare chance it should resist to the fourth day, bleed again; but usually it is better to apply a large blister. As a rule, you must not give up bleeding until fever, pain, and dyspnœa have almost ceased.*

Bouillaud's treatment in 75 cases is reported by M. Pelletan. The only complication was in a case literally poisoned by tartar emetic. This man was bled once before he was seen by M. Bouillaud. He was too ill for the *coup sur coup* plan, so he took ten grains of the drug daily for eight days. On the ninth day he died. Ulcers were scattered along the whole track, from the œsophagus to the ilio-cæcal valve. In no other case was tartar emetic given. Indeed, all the other patients enjoyed the singular advantage of simple treatment, with the addition, however, of blisters in 43 cases.

In his article on Pneumonia in the 'Dictionnaire,' published in 1835, M. Bouillaud informs us that in 102 cases treated by him from 1831 to 1834, the deaths were 12, = 1 in $8\frac{1}{2}$; and he states that M. Lacaze treated 42 cases *par les saignées à haute dose*, and only lost one. Many of the cases were slight, and did not require large bleedings repeated *coup sur coup*. Notwithstanding this, it is rather amusing to see Bouillaud, with perfect candour, tack these cases on to his own, and so bring out a joint-stock mortality of 1 in 11.

Writing in 1844, M. Bouillaud† informs us that during the preceding twelve years he had the happiness to cure, with some exceptions which did but confirm the rule, upwards of 400 cases of pneumonia, a good number being over the age of sixty. The rare cases of exception were among those struck with pneumonia affecting nearly the whole of both lungs. In the following page he speaks of aiding by *la toute-puissance de l'art* between 500 and 600 cases. How are we to reconcile these discrepancies? He asserts at p. 507 that his plan singularly shortens the duration of the illness, and that the patients recover their strength with remarkable rapidity.

In the work just quoted, M. Bouillaud states that, on an average,

* Traité de Nosographie, tom. ii. p. 503. † Ibid., p. 496.

he takes away four or five pounds of blood, and that the largest quantity taken was ten pounds, which was abstracted from a patient who recovered. He objects to the plan in the third stage. Dr. Bennett informs us that Bouillaud, when he visited him at La Charité last August, still pursued the *coup sur coup* treatment in pneumonia. He saw several patients (all young persons) who were then convalescent. On asking him whether he had observed any change in the character of the pulse, or a more typhoid character of the fever in recent times, his answer was, "Certainly not." M. Bouillaud's plan is in some respects a modification of that of M. Sauvages, who prescribed bloodletting to half-a-pound every four hours during the first day, and repeated to ten times, and in an ardent case to eighteen times, with happy effect (*feliciter*)!*

Louis and Grisolle investigated under Chomel the treatment of pneumonia. Each of them was Chef de Clinique to Chomel at the Hôtel Dieu; each published a *résumé* of the cases of pneumonia treated by their master; each specially studied the effects of bleeding, tartar emetic, and blistering on that disease; each imbibed certain views in relation thereto from Chomel; each was able to put his views in practice and modify them, by being appointed physician to l'Hôpital de la Pitié. Louis preceded Grisolle, who dedicates his work 'On Pneumonia' to "Mes excellens maîtres, Messieurs Chomel et Louis." Louis' deductions were adopted and worked out by Grisolle, who began his labours when those of Louis were closed. Their researches, and those of Chomel, are so interwoven, that we shall regard them as one common inquiry.

Chomel says,† the earlier, the larger, and the more repeated the bleedings, the more useful are they in pneumonia. One or two very large early bleedings have sometimes at once arrested the disease, or shortened it to two or three days, whereas it rarely ends before the seventh day, and often much later. But in the great majority of cases it is not so. Since in them, whatever we do, the disease has a period of increase lasting several days, during which the bleedings only cause a short remission, scarcely marked.

Louis gives the following case:—A strong young man, ill twenty-four hours, was admitted with extreme breathlessness, pain, catching expiration, quick pulse, heat of skin, and inability to lie down; the sputa were rusty and viscid; resonance was imperfect behind, where crepitation and partial bronchophony were audible. He was bled to fainting, and lost twenty-seven ounces of blood. He soon felt great relief, and next day the amelioration was such that several medical men thought the disease had been *jugulée*. Far from it, for the phlegm was still rusty, and dulness on percussion and bronchophony had extended considerably. The disease increased in extent until the fifth day, and convalescence did not set in until the ninth. When the first bleeding is early the disease lasts some days, but when the bleeding is late it very soon yields. The fact is, that the disease then more or less ap-

* Nosologie, tom. ii. pp. 496–8.

† Dictionnaire de Médecine, tom. xvii. p. 242. 1827.

proaches its natural termination; and that the utility of bleeding is really confined within very narrow limits.*

Chomel (p. 243) says that bleedings must be regulated in each case by the degree of inflammation and the powers of the patient. The first bleedings in strong adults may be from twelve to seventeen ounces, the later from eight to ten. In general, twelve or twenty-four hours may be allowed to elapse between the first and second bleedings. But you may often resort with advantage to a second bleeding a few hours after the first, and if the powers of the sick are good, a third in the same period after the second. By acting thus with energy early in the disease, you not only shorten the illness, but economize the blood, for two or three early bleedings will produce even more effect than six or eight made at intervals of twenty-four hours. A feeble pulse and great depression do not necessarily forbid bloodletting. The pulse often actually regains fulness, and the patient strength, while the blood is flowing. This was particularly marked in the epidemic of 1815, when the enfeebled powers and the pulse acquired strength even after a fifth or sixth bleeding! It is quite otherwise when the weakness instead of coming on suddenly at the beginning of the attack, has long preceded it, and when the patient has been exposed to weakening causes. In this truly adynamic pneumonia, bleeding increases the weakness, and we ought to abstain from it, or only employ it watchfully, and as a test (p. 244). It is particularly at the middle period of life that large bleedings are of service. In children, and in the old, they ought to be practised with reserve. Pinel, after a fatal experience, entirely abandoned bloodletting at the Salpêtrière in old women of seventy. Although, in the opinion of Chomel, bloodletting is, as a rule, greatly to be preferred to leeches, yet they often give complete relief when the pain in the side is very acute and superficial.

We shall see whether the precepts of Chomel tally with his practice by studying the reports of Louis in the 'Archives' for 1828, and of Grisolle in the 'Journal Hebdomadaire' for 1836, and that in the 'Gazette Médicale des Hôpitaux' for 1850.

Louis gives a *résumé* of 78 patients treated by Chomel. These were all in perfect health before the attack. He rejected 45 cases in persons previously affected for a certain period with pulmonary catarrh. All the 78 cases were bled: 28 died, or 1 in $2\frac{1}{14}$. 41 were bled for the first time during the first four days: 18 died, or 1 in $2\frac{5}{18}$; 36 from the fifth to the ninth day: 9 died, or 1 in 4. In the series of 41 bled early, 8 were above the age of sixty, of whom 7 died! In that of 36 bled later, 4 were above sixty, of whom 2 died. Notwithstanding Chomel's wholesome rule as to caution in the use of the lancet in old persons, he bled 12 aged patients, and some of these with unusual severity: 9 died. One patient of seventy-five was actually bled seven times! and another once at the extreme old age of eighty-five! We cannot wonder at the fatal issue in cases thus handled.

* Recherches sur les effets de La Saignée, pp. 38–47.

The same rigid plan of bleeding observed by Louis from 1821 to 1827, was carried out under the eyes of Grisolle from 1832 to 1836. The very old, from sixty to seventy-seven, were still bled, and *all* died! Those in hopeless depression in the third stage were still bled, and all (6) died. Two of these raised prune-juice sputa. They had all one or more bleedings, tartar emetic, and blisters on the chest and legs! The bleedings appeared to Grisolle to hasten the fatal end, especially in the cases raising prune-juice phlegm. The total amount of blood taken, varied from about one pound to nearly nine pounds, the average loss being between two and three pounds.

Chomel, in 1850, according to the reports in the 'Gazette Médicale des Hôpitaux,' still laid down the same discriminating rules, and still practised bleeding indiscriminately, even in the feeble and the very old. In one case there narrated, an old woman of seventy, who had been subject to catarrh for twenty years, was bled, and, being of course weakened by the bleeding, then blistered. It is clear that the practice of Chomel had not at that time been modified by any real or supposed change in the type of the inflammation. It is, however, too evident that Chomel, however sound his precept, is altogether wanting in discrimination at the bedside. It is painful to see the same fatal errors in practice repeated by him during three successive decades, and that the sad catalogue of fatal cases among the female, the weak, and the aged patients—among those affected with pneumonia of the whole of one, or portions of both lungs—among those already sinking under the disease in the third stage—is again and again and still swollen by a blind and sanguinary routine.

It is interesting to watch the career both of Louis and Grisolle when, separated from their master Chomel, they became physicians to the Hôpital de la Pitié. Louis added the results of his own treatment of pneumonia to those of Chomel in his work, 'Sur les Effets de la Saignée,' where he gives a *résumé* of 29 cases of pneumonia treated by himself in 1830–33. All these cases were in perfect health before the attack, and they presented every symptom and sign of pneumonia, including the characteristic viscid semi-transparent rusty sputa. These are therefore essentially picked cases, though picked on a rigid system; and they therefore, though comparable with each other, do not represent the actual success or mortality of Louis's practice in pneumonia. Louis, like Chomel, bled all his cases; but in two respects his treatment differed from that of his master. The first bleeding was larger —from thirteen ounces and a half to about twenty-seven, instead of from thirteen to seventeen; and in 20 of the cases he gave tartar emetic in large doses. Of the 29 cases 4 died, or 1 in $7\frac{1}{4}$. Tartar emetic, Louis states, caused a marked amelioration of the signs and symptoms. Six to twelve grains were given daily from the fourth to the seventh days of the illness. The convalescence of the cases taking the antimonial was however later by three days than in the other cases. This, as well as the great mortality under its use (3 in 20), Louis accounts for by the fact that it was not given until the repetition of bloodletting was forbidden. Louis is certain that blisters

do harm rather than good, at whatever stage they are applied. He remarks that the greater the extent of any inflammation, the greater the probability of some other organ suffering. A blister is an inflammation that does not subtract from the primary inflammation, and is therefore actually added to it. It is certain that blisters increase fever if applied when the skin is hot and dry.

These researches are certainly of value, but they omit many points a knowledge of which is necessary if we would compare them with other well-recorded groups of cases. The sex of the patients, and the stage and the extent of the disease, have, with age, the chief influence on the result. Louis enlightens us on none of these points excepting age, and in the *résumé* of Chomel's cases he does not even tell the age of the fatal cases; which he gives us incidentally in the memoir on his own cases published seven years later. As a specimen of the numerical method, these researches are therefore very defective. This memoir proves also that the numerical method is of little value, unless each case is a separate study, and unless the lifeblood of active thought penetrates the whole group of cases through and through. But though Louis' method is signally at fault in this work, the sagacity of Louis himself shines out. The narrow limits within which bloodletting is of use; the value of tartar emetic; the harm caused by blisters at the time, and afterwards; the causes, irrespectively of bleeding, of the quicker convalescence of cases admitted and bled early; the approach of the natural term of the disease as the cause of the more immediate yielding of the various symptoms and signs after late than early bleedings;—all these points, and others, are brought forth by Louis, but not through his *méthode numérique*. Not one of these points would he have made out by counting: they are the result of the Newtonian method of always thinking towards a foreseen aim.

In 1840, Grisolle (p. 460) treated 11 young persons affected with pneumonia of a favourable character with rest, *diète*, and diluents. He at the same time treated 13 similar cases with one or two bleedings, performed, on an average, on the fourth day. All the patients recovered. In those not bled, the sputa, on an average, ceased to be rusty on the ninth day; in those bled, on the sixth. The pain in the side, in those not bled, was better on the fourth or fifth day, but did not yield until the fifteenth. In 3 it lasted to the twentieth and twenty-fifth. Four of these cases were cupped, owing to the obstinacy of the pain. The pain was invariably relieved in the cases bled, and disappeared in from two to twelve days. Fever went in ten days in those not bled; in seven in those bled. The signs in the cases not bled began to yield towards the end of the second week, four or five days after the cessation of fever, and persisted to some degree until the twenty-second or thirtieth day. In those bled, the lung had recovered its permeability in ten or twelve days.

In Italy, Rasori,* in the early part of this century, carried bleeding to an extent greater than any of the French physicians, including even

* Archives Générales de Médecine, tom. iv.

Sauvages. Rasori bled many of his cases, a pound at a bleeding, night and morning, for days in succession. In the following case (p. 436) he carried his plan to the farthest stretch. A young man, aged nineteen, was admitted on the first day of an attack of pneumonia, which did not seem very formidable at first. In addition to the other symptoms, he bled from the nose, and vomited bile. He was bled that evening, and twelve grains of tartar emetic were given; on the second and third days the vomiting had ceased; he then took half a drachm of the drug. The symptoms increased in severity until the ninth: during that period he was bled night and morning, and took 1 drachm of the antimonial daily. On the twelfth day the fever and cough diminished. From the twelfth to the fifteenth the tartar emetic was replaced by kermes mineral. There were abundant stools on the fourteenth and fifteenth, when the kermes was omitted. The improvement continued, and on the twenty-seventh he left "*très bien guéri!*" This young man was bled seventeen times, to about 222 ounces, took nearly one ounce and a half of tartar emetic, and two drachms of kermes mineral. This is the largest amount of blood that Rasori had taken from any one patient.

The following Table shows the character and result of Rasori's practice, as put forth by himself:

Civil Hospital.

No. of bleedings.	Total cases.	Deaths.
0	70	9 = 1 in $7\frac{8}{9}$
1 to 2	213	11 = 1 in $19\frac{4}{11}$
3 to 5	215	48 = 1 in $4\frac{1}{2}$
6 to 9	129	62 = 1 in 2
9 to 16	25	17 = 1 in $1\frac{1}{2}$
	652	147 = 1 in $4\frac{1}{2}$

Military Hospital.

0	15	0
1 to 2	71	2 = 1 in $35\frac{1}{2}$
3 to 5	52	5 = 1 in $10\frac{2}{5}$
6 to 9	26	8 = 1 in $3\frac{1}{4}$
9 to 16	16	11 = 1 in $1\frac{1}{2}$
	180	26 = 1 in 7

Rasori gives this table to show the comparative severity of the cases. We give it to show the destructive effects of numerous bleedings. According to Rasori, patients affected with pneumonia tolerate the tartar emetic in enormous doses. As soon, however, as the disease lessens the tolerance ceases, and purging and perhaps vomiting come on. Rasori's statistics have been discredited from more than one quarter.

Prato and Strambio, quoted at length in an article in the 'Archives' for May, 1831, give a different account of the successes of Rasori. Prato gives 80 cases treated by Rasori, all of whom died but 9. One

of them was a robust young girl, three days ill with pneumonia, to whom he gave 270 grains of tartar emetic, and who on the fourth day of the treatment was much purged; she was calm, her cough less violent, and she was in a state of oppression and universal feebleness, the supposed signs of approaching convalescence: she died unexpectedly. Two fatal cases, who had taken respectively three and five ounces of the drug in five and ten days, with tolerance almost complete, presented nevertheless great prostration, indescribable anxiety, and a pulse more and more feeble. Another took daily a drachm of the antimonial for a month, was attacked with aphthous ulcers in the throat, anasarca, and sore back, and died suddenly when apparently recovering. One young man took the tartar emetic, and was bled on the fifth day of the attack; he continued vomiting till the twelfth, still taking one or two drachms daily. On the eleventh, no pain, breathing deep and easy, but so low that he could scarcely speak; pulse irregular; skin cold; tongue dry; thirst intense. Still, still bleeding and antimony! Death on the twelfth. In another, on the eleventh day, extreme depression; pulse feeble; features relaxed; rattle. Tartar emetic 144 grains, bleeding to ten ounces; at the end of the bleeding the sick man fainted and died! Now, these deaths were not from pneumonia. Strambio, Prato, and Macchi testify that very many left Rasori's wards cured of the pneumonia, but with a chronic, incurable, and fatal diarrhœa!

Acerbi* gives a summary of 142 cases of pneumonia treated in the hospital of Milan with bleeding, usually night and morning; and tartar emetic, four to eight grains daily. Of the 142 cases, 16 died, or 1 in 9; 4 of those who died had been bled from three to four times, 5 from five to eight times, and 7 from nine to thirteen times. Thirty of the 142 were bled from ten to twenty times, twelve ounces each time, who therefore lost from 120 to 240 ounces! One robust countryman was bled nineteen times in twelve days, and took tartar emetic for ten days, when it was exchanged for kermes mineral. He survived! But when he left the hospital after sixty-nine days he had œdema of the legs, diarrhœa, excessive pallor and weakness. One strong country fellow, who had been cured (!) of pneumonia on the previous January by eighteen bleedings, and who had been sickly during the intermediate four months, was readmitted in May, on the eighth day of a second attack. He was actually bled fifteen times in nine days, and took tartar emetic the while; he died on the twenty-eighth day, after being bled thirty-three times during the two attacks. But they had their successes. One strong high-coloured countryman went out perfectly well on the twenty-sixth day after being bled twelve times; another in thirty-three days, after eight bleedings and twenty-four leeches. A pale sempstress was bled nine times, lingered on in the hospital for fifty-six days, falling into a slow consumptive fever; she was then fortunately transferred to the infirmary for chronic diseases, where she was plentifully fed, and comforted with wine; she left at the end of a

* Annotazioni di Medicina Pratica.

month with a slight cough, but with a fine colour, joyous health, and in good flesh!

Laennec adopted and modified Rasori's method. He bled usually but once at first, taking from nine to seventeen ounces, and then gave one grain of tartar emetic every two hours for six doses, when a rest of seven or eight hours was allowed, unless the symptoms were urgent. He continued the treatment while tolerance lasted, or crepitation was heard. He thus treated 28 cases of pneumonia in the year 1824, of whom 1 died; and 34 in 1825, of whom 5 died; total mortality, 1 in $10\frac{1}{3}$. While he speaks of losing so few on this plan, he however states that he had seen several cases of pneumonia die complicated with severe pleurisy, and others with cancer, phthisis, heart disease, &c. M. Bouillaud, in the 'Dictionnaire de Médecine,' xvii. 398, throws a doubt on the accuracy of Laennec's statistics, on the testimony of Lecouteulx, Aide de Clinique, who states that 9 out of 17 cases of pneumonia admitted in 1825 died, and 3 out of 13 in 1826. The discrepancy can only be explained by the fact that Laennec admitted slight cases into his list; and, as we may infer from what he states, that he excluded complicated cases. Probably M. Lecouteulx knew more about the patients in the deadhouse than the wards.

Grisolle treated 44 cases with tartar emetic, and without bleeding. Six died, or 1 in $7\frac{1}{3}$; average age of the deaths, above fifty. The pain was never relieved by it. The pulse and breathing were less quick, the sputa were less tinged, and the signs were ameliorated in a considerable proportion at the end of twenty-four hours. Grisolle quotes the practice of the following physicians who employed tartar emetic, some with, some without, bleeding:—Bang, of Copenhagen, treated 54 cases with the antimonial, mostly after a single bleeding; of these 2 died, = 1 in 27; only 2 were above the age of sixty. Ruef treated 94 cases with tartar emetic, combined with bleeding; the cases were not complicated; he lost 5, = 1 in $18\frac{4}{5}$. Peschier, of Geneva, treated his cases with antimony, and without bleeding. He states that he lost but one. M. Peschier's memoir seems to be wanting in details; and the author of an article in the 'Archives' for 1831 treats him and his cases with ridicule. Trousseau treated 52 cases in l'Hôtel Dieu, under the eye of Grisolle, without bleeding, and with tartar emetic. Only 2 died, or 1 in 26. He remarks, that on this plan four days often sufficed to lead the patients to apparent health, though the local signs still remained.

Thielmann* treated 110 cases with tartar emetic, six grains daily, without bleeding. If there was diarrhœa, he gave opium also. Of the 110 cases, 12 died, = 1 in $9\frac{1}{6}$. Seventy-seven were males, 9 died, = 1 in $8\frac{5}{9}$; 36 females, 3 died, = 1 in 12. Fifty-seven of the cases were in the first stage, none died; 32 in the second stage, 3 died, = 1 in $10\frac{2}{3}$; 24 in the third stage, 8 died, = 1 in 3.

Schmidt† treated 37 out of 54 cases with tartar emetic, without

* Canstatt's Jahresbericht, Bd. iii. 231, 1852.
† Schmidt's Jahrbücher, Bd. lxxxix. p. 194.

bleeding. The mortality of the whole 54 was 12, = 1 in $4\frac{1}{2}$; that of 37 cases treated with antimony was 4, = 1 in $9\frac{1}{4}$. Fourteen of the cases took opium in some form, of these 6 died, = 1 in $2\frac{1}{3}$.

Skoda has only had occasional recourse to bleeding during the last twelve or thirteen years. Dr. Balfour* watched Skoda's treatment during May, June, and July, 1846. He employed no bleeding, but gave extr. graminis in scruple doses, or nitre five to twenty grains, or sublimate a quarter of a grain, with occasionally six grains of Dover's powder, in the course of the day. 45 cases were so treated; only 3 died, = 1 in 15. Skoda informed Dr. Balfour that his average mortality during the three preceding years, during which bloodletting had seldom or never been performed, and leeches or cupping glasses had not been applied, was 13·7 per cent. In 1840 he treated 64 females affected with pneumonia with large bleedings and large doses of tartar emetic, and only lost one; yet the deaths amongst the males in the same year made the total deaths amount to 1 in 8. That he conceived to be the general average under all modes of treatment. The great advantage of not bleeding was, he considered, the speedy recovery.

Dr. Balfour (*loc. cit.*) watched the treatment of 19 cases of pneumonia, examined by himself, at the Homœopathic Hospital under Fleischman; of these, 3 died, = 1 in $6\frac{1}{3}$.

The publication by Dietl of his comparatively successful treatment of pneumonia without bleeding, induced many physicians to follow his example. While, however, they refrained from bloodletting, they usually gave medicine of some kind or other. We have already seen that Thielmann and Schmidt, and previously to them Peschier, Trousseau, Bang, and Ruef, trusted to tartar emetic alone, in large doses.

Huss,† in 1852, treated 242, of whom 15 died, = 1 in 15. The larger proportion were between twenty and forty years of age; 8 were between sixty and seventy. Pneumonia was complicated with delirium tremens in 30, with pleuritic effusion in 27. Besides these 242 cases, 13 were admitted in a dying state. Huss at first gave up venesection, and only cupped; at length he deserted cupping. In a large portion of the cases, bleeding, general or local, had been already performed. The general effect of the treatment without bleeding was quick resolution and rapid convalescence. Huss considers that bleeding is only useful in the congestive stage, which is rare in his wards, and that it is injurious in the second stage unless congestion is still present elsewhere in the same or the opposite lung. Huss considers that what does good to pneumonia at one year or period, does harm at another.

Kissel‡ never bleeds. He considers that pneumonia lasts thirty-five days when treated with bleeding, twenty-nine days when with antimony, twenty-eight days when left to itself, and seven or eight days when the "direct treatment" is adopted! Kissel treated 112 cases

* British and Foreign Medical Review, vol. xxii. p. 590.
† Schmidt's Jahrbücher, Band lxxxiii. p. 311.
‡ Canstatt's Jahresbericht, Band iii. p. 229. 1852.

without bloodletting; 5 died, = 1 in 22⅖. He classes his patients into two groups. To one group, in which the urine is alkaline, he gives iron; to the other group, in which the urine is acid, and the crepitation and bronchial breathing are followed by sibilant and mucous rattles, he gives copper. Under this treatment he asserts that the dyspnœa soon lessens, the temperature falls, the thirst slackens, the cough is less troublesome, and the sputa become gradually less red. Appetite and strength return on the second day. Kissel states that in the epidemic pneumonia of 1795, observed by Schmiedtman,* the disease was treated without bleeding or nitre. The crisis lasted five to fourteen days. The sick returned to their work in two or three weeks. Of 68 cases, 2 died, = 1 in 34.

Wittich† abjures bleeding. He treated 23 cases, who all recovered, with calomel in three to six grain doses for adults, two to four for children. He pushed it until the gums were tender and the stools green.

Cohn‡ treated 57 cases without bleeding, and with infusion of crocus, antimonial wine, tinct. of opium, and benzoin; 24 died, = 1 in 2$\frac{9}{24}$. The cases, like the treatment, were complicated. Among the complications were old age, debility, drunkenness, meningitis, apoplexy, Bright's disease, bronchitis, and other maladies.

Gandini§ treated 25 cases without bleeding, and with calomel and opium. All recovered.

Niemeyer‖ only bleeds to relieve threatening suffocation, or to lessen the obstacle to the return of blood from the brain when it is congested. He never bleeds for the inflammation, and he agrees with Dietl that a more early, complete, and enduring restoration takes place than by the bleeding plan. He applies cold wet cloths to the chest, repeating them every few minutes, with great relief. To the weak and old he gives iron.

De Bordes¶ treated 90 cases, of whom 19 died, = 1 in 4$\frac{14}{19}$. 17 had double pneumonia, of whom 6 died. Of these, 2 were bled; both recovered. 11 were treated by local bleeding, of whom 2 died, = 1 in 5$\frac{1}{2}$. The local bleeding generally lessened the pain and relieved the anxiety in the chest. It did neither harm nor good to the inflamed part; in one case, however, cupping was followed by a decidedly quick resolution. 77 of the cases were not bled; of these 17 died, = 1 in 4$\frac{9}{17}$. Of the fatal cases, 2 were dying on admission, and some were lowered by ague, albuminuria, and heart disease. The mortality was due more to debility than to the want of bleeding. In 53 the recovery was very quick. De Bordes considers it proved that abstaining from bleeding is useful, since it leads to a quicker convalescence.

Chloroform came into use in Germany in the treatment of pneumonia, soon after its discovery as an anæsthetic agent. It remained in fashion from 1849 to 1856. Most of the chloroformers in pneumonia were also abstainers from bleeding.

* Hufeland's Journal, Band iii. p. 441.

† Canstatt's Jahresbericht, Band ii. p. 254. 1850. ‡ Ibid., Band iii. p. 214. 1854.

§ Ibid., Band iii. p. 256. 1856. ‖ Ibid., p. 254. ¶ Ibid., Band iii. p. 159. 1857.

Baumgärtner* employed the inhalation of chloroform, not carried to narcotism, in 30 cases of pneumonia, of whom 3 died, = 1 in 10. It relieves the pain in the side and oppression, lessens the cough, changes the glutinous into a mucous phlegm, reduces the fever, and lessens the pulse. The objections to it are, that it sometimes causes vertigo, headache, and lowering of the powers of life. As venesection is not mentioned, we may consider that it was not employed in Baumgärtner's cases. The same remark applies to the 90 cases of pneumonia treated by Wucherer in 1848† with the inhalation of ether or chloroform, 14 of them were private patients, 76 were soldiers—only 1 died, = 1 in 90! Helbing reports in the same volume (p. 256), 62 cases, who were treated like Wucherer's, with the inhalation of ether or of chloroform, but who were likewise bled once or twice, and took tartar emetic. Of the 62 cases, 5 died, = 1 in 12½.

The following table shows the average time at which various symptoms disappeared:

	Helbing's cases.	Wucherer's cases.
The fever disappeared in	... 6.7 days	... 5.2 days.
„ pain „	... 5·6 „	... 4·8 „
„ dyspnœa „	... 7·2 „	... 7·9 „
„ cough „	... 8·6 „	... 6·5 „
„ red sputa „	... 5·7 „	... 5·2 „
Vesicular breathing returned in	9·7 „	... 6·2 „

Varrentrapp gives, in a detailed and important memoir,‡ 23 cases of pneumonia treated by the inhalation of chloroform, of whom 1, aged 59, died, = 1 in 23. The average age of the patients was thirty-one, oldest sixty-two; 21 were males, 2 females; the pneumonia was double in 2; 2 of the cases had bronchitis, 1 jaundice; 1 was bled, 1 cupped, and 2 took calomel. In general, the inhalation while it was proceeding excited very soon a sense of increased warmth, perspiration, lessening of oppression and pain in the chest, diminution of cough, an effectual amelioration in the feelings of the patient, and a corresponding improvement of the fever, expectoration, and physical signs. The relief of pain and oppression lasted about an hour, when they returned nearly with the original severity, but less and less after each time. The pulse and respirations fell considerably under the influence of the treatment. After a few hours, when the symptoms had regained much of their intensity, the inhalation was resumed with the same happy effect. The rusty expectoration came afterwards, mixed with watery secretion. The daily number of inhalations varied from five to ten. On an average, seventy-four inhalations were practised in ten days and a-half; in one case, twenty-seven in five days, and in another, one hundred and sixty-two in fifteen days. One drachm of chloroform was used at each inhalation. The duration of the various signs and symptoms did not differ materially from that observed in Helbing's and Wucherer's cases. In most of the cases the treatment did good. Death was the immediate effect of the chloroform in the single fatal case.

* Canstatt, Band ii. p. 255. 1850. † Ibid. p. 256.
‡ Henle's Zeitschrift für rationelle Medicin, Neue Folge, Band i. p. 1.

Stohamll* treated 30 cases in like manner, with the happiest results. In these cases the effects of each inhalation went off in from four to six hours. Hutava treated 12; Frohnmüller, 12; Theile, 6; and Salawa, 15; in the same manner, with chloroform, and without the loss of any. The two first refrained from bleeding, and Salawa only bled the robust and plethoric. There is a remarkable uniformity in the description given by these various observers of the happy effects of the inhalation on their patients.

Although the tide had set so strongly against bleeding in Germany during the last ten years, it has within the last five years been resumed by several physicians with judgment and moderation.

Wossidlo† does not belong to the group of physicians we have just been speaking of, but he, like them, though at an earlier period, employed bleeding with great success in all his cases of pneumonia. Wossidlo employed both local and general bleeding, and gave tartar emetic. He treated 112 cases, of these 4 died, = 1 in 28; 34 were children below the age of five, deaths 3, = 1 in $11\frac{1}{3}$; 76 were between the ages of seventeen and seventy (5 above sixty), 0 died; 2 were above seventy, 1 died, = 1 in 2. Wossidlo ridicules the successes of Krüger Hansen, who boasted that he had cured 71 cases without bleeding, and with acetate of lead and opium. Of the 71 cases, 36, Wossidlo states, were treated on the mere report of friends!

Burkart‡ treated 60 cases of pneumonia, in 1854, with bleeding, when the medical constitution was decidedly inflammatory. Out of the 60 cases only one death, and in that case there were tubercles! Burkart makes some very sensible remarks as to the treatment of old people affected with pneumonia. They do not bear bleeding in any form. Tartar emetic is less influential on the pneumonia of the aged than of adults, and it often lowers them excessively.

Forget§ gives a *résumé* of the visitation of pneumonia at Strasburg in the spring of 1855, when the temperature, from being warm, suddenly became very cold. 32 cases were admitted, of whom 7 died, = 1 in $4\frac{4}{7}$; 2 had double pneumonia. All the fatal cases were complicated. Indeed, complications were the rule. 2 had tuberculosis, 1 followed typhus, 8 were in a typhoid condition, 5 had bronchitis, 3 albuminous urine, 1 meningitis. The illness lasted from one day to forty-eight, average fifteen and a half. General bleeding was practised in 21, local in 20 cases; 24 had tartar emetic, 17 had opiates, 11 were blistered.

Petters‖ gives a summary of 43 cases of pneumonia, 7 of whom died, = 1 in $6\frac{1}{7}$; in 2 the pneumonia was double. In 10 of the cases leeches were applied; more than half had no treatment beyond rest and deprival of food. None of them seem to have been bled from the arm.

Schützenberger¶ treated 48 cases of pneumonia in Strasburg during the winter six months of 1854–55; these 48 do not include the

* Schmidt, Band lxxxiii. p. 295.
† Ibid., Band li. p. 229.
‡ L'Union, tom. ix. p. 1855.
§ Canstatt, Band iii. p. 158. 1857.
‖ Prager Vierteljahrsschrift. B. i. p. 190, 1856.
¶ Canstatt, Band iii. p. 158. 1857.

typhoid cases. The number of patients rose and fell with the fall and rise of the temperature. Of 48 cases, 7 died, = 1 in $6\frac{6}{7}$.

Twelve acute cases recovered; bleeding and mercury were resorted to; 3 acute cases died, 2 admitted in agony: 1 had tartar emetic; 26 less acute cases had tartar emetic combined with acute phlogistic treatment; 25 recovered, 1 died; 2 less acute cases died, who had diffused hepatization, and were not treated; 6 cases of diffused engorgement treated with acetate of lead and local bleeding, recovered. This enumeration makes 49 cases with 6 deaths. An error must have crept in somewhere.

Eighteen of the above cases who, it is stated, were bled, locally or generally, recovered; 5 at least of the fatal cases were not bled. We shall reserve the consideration of Wunderlich's cases to a future part of this article.

We have now passed in review a large body of evidence bearing upon the treatment of pneumonia, and more especially upon the treatment of this disease by venesection. We may fairly consider ourselves in a position to arrive at some general conclusions, and, if they are attainable at all in a question of therapeutics, to lay down some guiding principles in spite of the apparent contradiction of authorities.

Bloodletting has been the main remedy relied upon for the treatment of pneumonia from the days of Hippocrates down to our own. The father of medicine himself, and such men as Sydenham and Huxham, used the *summum remedium* with moderation, apportioned it to the severity of the disease and the strength of the constitution, and refrained from it altogether when the patient was weak and the disease assumed a low type. They bled with discrimination. But from time to time reckless bleeders have, by their very excesses, caused others to rush into the opposite extreme, and abstain altogether from bloodletting. Nor need we wonder at these violent oscillations when we read that Heurnius took his four pounds of blood for pleurisy; that Bosquillon, the translator of Cullen, stimulated by that author, his three; that the elder Frank bled an octogenarian nine times; that Sauvages recommended for ardent pneumonia eighteen successive bleedings of eight ounces each, repeated at first every four hours; that Rasori, besides giving tartar emetic, bled one patient seventeen times in nine days; that Dr. Mackintosh says more mischief is done by bleeding too little than too much:—"I am not an advocate for the heroic practice of taking seventy or eighty ounces of blood at one operation; the largest bleeding that I can boast of was fifty-six ounces;"—that Dr. Alison in 1841, ordered an old gentleman of sixty-six, six bleedings and two cuppings in five days; and that Mr. Lawrence, in 1836, amused his class by informing them that he had bled "a slender young girl" to forty-eight ounces for supposed inflammation of the chest!

Must we still abandon ourselves to these extreme reactions, or, guided by knowledge, reason, and experience, may we not, standing on the vantage ground of truth, settle and hold fast to certain broad

grounds of practice? So far as mere counting is concerned, statistics do not help us, as a glance of the eye at the two rows of figures at the end of this article prove (p. 40). Take either side. The cases treated by bleeding show a mortality varying from 1 in 2 to 1 in 60. Examine the other set, in which bleeding was abstained from, and you will find the deaths vary from 1 in $2\frac{1}{2}$ to 1 in 90. So startling is the variety, that we are compelled either to discredit the observers, or to admit that, under the common name of pneumonia, they treated a totally different order of cases as regards severity of disease and tendency to death.

The disagreeable assumption, that some, at least, of the observers are not to be trusted, comes upon us with double force, when we find that Laennec's mortality is put down by himself at 1 in 10, and by Lecouteulx at 1 in $2\frac{1}{2}$. That Skoda's mortality in 1846, according to Dr. Balfour, was 1 in 15, while in 1852, according to Dr. Mitchell, it was 1 in $3\frac{3}{4}$. The second assumption, that the various physicians treated a totally different order of cases, as regards severity and tendency to death, recommends itself to us instinctively. We shall be able to answer this question if, instead of counting up these observations as so many figures, we study them closely as a body of vital facts. With these objects we shall examine the influence on the result as regards recovery and death; of the age, sex, and constitution of the patient; the season, the climate, the previous destitution of the patient, the early neglect of the disease; the extent, character, and stage of the disease; the complications; the change of type; and the hospital accommodation.

Age has a remarkable influence on the mortality of pneumonia. Between six and forty there is a strong tendency to recovery, so that patients during the fever can bear bleeding, tolerate poison, and endure starvation that would destroy a person in health. Many observers give the ages of the patients, very few those of the deaths. In the few instances of great success in which the ages are known, the patients had comparative youth on their side. Deaths are six or seven times more numerous after sixty than before thirty. Skoda's successful cases were chiefly between twenty and thirty; and those of Dr. Bennett, Dr. Bell, and Varrentrapp were aged on an average from thirty to thirty-one. This implies a small number of very old persons. A large proportion of Dietl's cases were above the age of sixty. We may safely infer that old persons would have a much better chance of recovery if, instead of being bled, starved, and antimonialized, they were fed and kept warm during their illness. The appalling mortality of Chomel's cases, as reported by Louis, proves to demonstration the destructive effect of repeated bleedings in old people. Eight of Louis's own cases were above the age of sixty; the 5 who survived were bled on an average 1·6 times: the number of bleedings practised on the 3 who died is, with the remarkable imperfection pervading Louis's *résumé*, not given. Twelve of Chomel's cases were above the age of sixty; the 3 who survived were bled on an average 1·66 times; the 9 who died were actually bled 2·86 times, the bleedings being smaller than in Louis's cases. Several of these 9 patients were bled to death.

Bouillaud's 75 cases, reported by Pelletan, when compared with those treated by Chomel and Louis, appear at first sight to be remarkably successful, seeing that while they lost respectively one-third and one-seventh of their patients, Bouillaud only lost one-eighth of his. But the tables are turned when we apply the test of age, since, while none of his cases were above the fatal age of sixty—and only one-tenth were between fifty and sixty—three-tenths of their cases were fifty years old and upwards, and nearly one-half of those were above sixty.

The mortality of pneumonia in the army is numerically very small, seeing that out of nearly 16,000 cases admitted into the military hospitals during the twenty years ending 1836, 621 died, or 1 in 25. It must not be lost sight of, that during one-half of the period referred to physical diagnosis was unknown; but the results during the last ten years, when it may be held that the returns approached to accuracy, were nearly as favourable as they were during the whole period—the proportion of deaths being 1 in 20·3. During the ten years ending 1846 the mortality was greater, being 1 in 13. The later returns do not, however, include the tropical stations, in which the mortality is very slight. One great reason for this remarkable success is the comparative youth of the patients, none of whom were over fifty, few above forty years of age.

We come down, then, upon these important facts, in relation to the influence of age on the mortality of pneumonia, from the examination of the statistics of bleeding and non-bleeding in that disease,—that to bleed old persons repeatedly, or in some cases at all, is mortal. The day surely never can come round again in medicine when men, with their eyes sealed by false theory and prejudice, shall return to this destructive practice in the aged.

That the apparent mortality among younger persons is unduly increased in the statistics of bleeding by the disproportionate number of deaths among the aged caused by that practice.

That, whatever be the treatment, the larger the proportion of young patients, the smaller the mortality; and that the extraordinary successes of new plans of treatment—as, for instance, *non*-bleeding and chloroform inhalation—are often due to the younger patients being selected who are enabled by the disease to bear almost any kind of treatment, especially if it tends to relax the tissues and reduce fever.

At first sight the influence of *sex* on the mortality appears to be great. Grisolle found the proportionate number of female deaths nearly twice as great as that of males during three consecutive years, excepting 1837. The average age of the female deaths was fifty-five, of the male forty-three. It was not so much, therefore, to the sex as to the age that this excessive female mortality was due. Pinel found that pneumonia was almost always mortal in old women of seventy when they were bled, and he therefore discontinued the practice in females of that advanced age. The deaths among females (1 in 2·7) were much greater than among males (1 in 4) in the Vienna Hospital during 1856. But though the age of the deaths is unfortunately not given, the excessive female mortality is manifestly due to the greater proportion of aged patients among the females, the proportion of patients

above fifty being 1 in 3 among the females, and 1 in 10 among the males. Briquet's observations as to the greater fatality of the disease among aged women, support those of Grisolle. The proportion of deaths among females was rather less than that of males in the Edinburgh Infirmary, and also in the practice of Thielmann, Schmidt, and Petters. It must not be lost sight of that women, if at all strong, are fitted by their periodic loss to bear bleeding better even than men. Indiscriminate bleeding would undoubtedly have the effect of increasing the mortality among females above the age of fifty; for they being weaker, and suffering from a less intense form of pneumonia than men, do not bear venesection so well.

Constitution of the patient.—The mortality of pneumonia is much greater in the weak, especially when bled indiscriminately, than in the robust. Thus in the practice of Briquet two-thirds of his deaths were among the feeble, although two-thirds of his patients were among the comparatively strong. He also found that the illness lasted a week longer in the weak than the strong.

The Reports of Dr. Morehead and Mr. Stovin on the diseases in the European and native hospitals at Bombay, show in a conclusive manner the remarkable influence of weakness of constitution on the mortality of pneumonia. During the ten years from 1844 to 1853, Mr. Stovin met with but 22 cases of pneumonia out of about 12,000 European patients; 2 only died, or 1 in 11. According to Dr. Morehead, about 26,000 were admitted into the native hospital, during the six years ending 1853; of these, 313 were affected with pneumonia, of whom 121 died, or 1 in 2·6. The disease destroyed the fragile natives in much greater numbers and proportion than the robust Europeans, the former being more inured to the effects of heat and susceptible to the effects of cold, than the latter. These observations are borne out completely by the reports of the health of the Troops. The mortality being much greater among the Negroes in the West Indies, the Hottentots at the Cape, and the Malays in Ceylon, when attacked with pneumonia, than among the Europeans.

It is well remarked by Dr. Morehead, that though the disease is so much more fatal among the natives than the Europeans at Bombay, it causes much less pain, fever, and dyspnœa, and is consequently more apt to be overlooked. The disease, in fact, is altogether of a more adynamic form, presents grey more often than red hepatization, and frequently ends in gangrene.

When we take into account that the weak die from pneumonia so much more readily than the strong, we shall find that the cases treated by Chomel, Louis, and Bouillaud present even a worse aspect than they do at first sight; for those treated by Chomel, reported by Louis, and Louis's own cases, were exclusively selected from among the strong; yet 1 in 3 of Chomel's, and 1 in 7 of Louis's patients died. Although Bouillaud's cases, reported by Pelletan, treated on his plan of bleeding *coup sur coup*, were not selected, 8 of the 9 fatal cases were either strong or moderately strong!

When we recollect that so small a proportion of Bouillaud's patients were above the age of fifty, we almost anticipate that his deaths were among the robust; for if we inquire into the cause of the great mortality among the aged, as well as among infants below the age of five, we are obliged to refer it to the great natural weakness of the frame at the two opposite poles of infancy and old age.

We are reluctantly compelled to infer that Chomel, Louis, and Bouillaud, by excessive and repeated bleeding sometimes reduced their patients, when originally strong, and brought them into the same unfavourable condition as if they had been originally feeble.

Without calling in question the value of moderate bloodletting, local or general, proportioned to the severity of the case, in strong or even some feeble patients suffering severely from pneumonia in its early stage, we may consider it to be conclusively proved that large and repeated bleedings often extinguish the feeble, and by reducing the strong to the condition of the feeble, sometimes destroy the strong also.

Season.—To Briquet we owe the observation, that in winter, when the cases are most numerous, the comparative mortality is the greatest. This is readily accounted for by the greater intensity of the prime cause of pneumonia—dry cold, with an east wind, the difficulty of regaining warmth during the precursory chill, and the exposure afresh to cold, during the early days of neglect, after the incursion of the disease, but before admission into the wards. Briquet found that the cases admitted were three times, while the deaths were four times as numerous during the late winter months as in summer.

It may be laid down as a law, that the severity and fatality of a disease are in direct proportion to the intensity of its cause. Tropical marsh malaria cause yellow, remittent, and malignant fevers. Crowded, close dwellings, offensive with foul diseases and decaying matter, cause hospital, gaol, and ship fevers, dysentery, and inflammation of a low type. Small-pox, scarlet-fever, and the purely communicable diseases, are destructive or mild, according to the temper of the epidemic. These diseases, almost innocuous when they spring up under milder influences, are virulent and fatal when forced into action by virulent causes. So with pneumonia. It is "benignant" when excited by slight exposure in a young person of sound constitution. But it may be to the last degree malignant when caused by long-continued exposure to intense cold, the frame being naturally feeble or exhausted by fatigue, depressed by anxiety, broken down by starvation or overwork, shattered by excesses, or worn out by old age.

Climate exercises a remarkable and well-ascertained influence on the mortality as well as the frequency of pneumonia. The disease is not, like African, yellow, and remittent fevers, strictly limited within certain isothermal lines. But its geographical diffusion, intensity, and mortality, have been determined almost with the same precision as the tropical diseases which we have just referred to, and which inhabit the opposite pole of temperature. While they love intense heat with moisture, inflammatory pneumonia seizes on those dwelling in dry,

cold regions. It is not purely cold, but dry cold that excites it; thus it is more frequent and more mortal in Paris in March and April than in January and February, since, while the latter months are colder, the former are more dry. On the same principle, while it is absent or scarce along the tropical sea-shores, it abounds, and often with virulence, in the elevated regions. Dr. Lombard, in a recent paper translated in the 'Edinburgh New Philosophical Journal' for 1857, gave many interesting illustrations of this statement.

Tschudi, who lived long in the valley of Puna, in Peru, elevated more than 12,000 feet, informs us that pneumonia is very frequent in those lofty regions. It is rapid and acute. Even the apathetic Indian writhes under the pleuritic pain. But while meningitis, also common and fatal, demands active antiphlogistic treatment, pneumonia cannot be counteracted by bleeding. It is even dangerous, for patients often sink under the loss of blood. The stimulating treatment of the Indian with cayenne is therefore to be preferred. Humboldt found pneumonia frequent in Mexico.

Dr. Albert writes that at Briançon, elevated 4285 feet, the greater number of the deaths of adults are owing to pneumonia. Dr. Fuchs found the mortality from pulmonary inflammations in Thuringia increase rapidly with increasing elevation. The epidemic "Alpenstich," which often ravages Switzerland, is sometimes a pleurisy, at other times a pneumonia, accompanied with typhoid symptoms, running its course very rapidly, and attended with great danger. The disease may be compared to the pneumonia of Peru.

Upon examining the reports of the health of the British troops, we find that pneumonia is more fatal at some of the military stations than at others. At home, the mortality of pneumonia is about 1 in 13; at British North America it varies from 1 in 11 to 1 in 25; at the Cape, 1 dies in 12; at the Cape frontier, 1 in 20; in our possessions in the Mediterranean, the deaths range from 1 case of pneumonia in 15, to 1 in 47; and in the West Indies, from 1 in 15 to 1 in 28. The small proportion of deaths in some stations is an additional reason for the low aggregate mortality of pneumonia in the British army.

The proportion of cases of pneumonia to other medical cases admitted into hospital, differs much in various parts of the world, the extremes ranging between 1 in 5 at Chamounix, and 1 in 627 at Bone, in Africa. It is almost unknown at Madras. Paris, the North of Italy, certain parts of Germany, and the North of Europe, seem to be the head-quarters of pneumonia. It appears to be more frequent in hospital practice in Edinburgh, Dundee, and Dumfries, than in London or Aberdeen—in either, than in Inverness or Perth. It is less frequent in the United States than in Great Britain.

As a rule, inflammatory pneumonias which begin with rigor, and strike at once upon the air-cells, are more frequent in dry cold weather, as in spring, and dry cold climates, as in Paris; while the catarrhs, which seize first on the nostrils, and pass thence to the larynx, trachea,

and bronchi, and which, unlike the pneumonias, usually commence with sneezing, are relatively more frequent in damp cold weather, and damp cold climates, as in England. The sthenic pneumonias, as a rule, prevail in dry cold seasons and climates; while the asthenic pneumonias, which are more or less complicated with bronchitis, prevail in cold moist climates and seasons.

At first sight, one would expect that the proportion of deaths to recoveries from pneumonia would be greater in Paris, where they are more numerous, than in London, where they are less so; since the greater intensity of the cause must lead to a greater severity of the disease. Other things being equal, this is so. A healthy man would probably have a more severe attack of pneumonia in Paris than in London.

But other things are in many cases not equal. In Paris, where the air is comparatively dry, by far the largest proportion of the cases reported by Pelletan, Briquet, and Piorry were not complicated, and occurred in persons previously healthy. In London, where the air is often damp, and, as Dr. Jackson has shown, in Massachusets, a large proportion of the cases presented complications, and were not previously healthy. Now, these are the very cases in which the disease tends to a fatal issue. The result is, that though the proportion of deaths to recoveries from pneumonia among the strong is perhaps greater in Paris than in London, the actual proportion of deaths is greater in the latter city.

The effect of *previous hardship* and destitution on the fatality of the disease, is well illustrated by the returns of sickness and deaths in our army during the Crimean campaign, when one case of pneumonia died in 3·6, instead of 1 in 12 or 14. Other causes for this excessive mortality were at work, but the main cause was undoubtedly the want and distress among the men during the early days of the siege.

Neglect of the disease in the early stage has a very great influence on the mortality of pneumonia in hospitals. Grisolle states (p. 551) that only 1 died in 13 of those entering during the first three days, while of those admitted after the sixth day, from one-third to one-half died. The deaths of those natives in Bombay treated by Dr. Morehead, that were admitted early, were 1 in 11; of those admitted late, 1 in 2. One-fourth of Briquet's fatal cases were admitted in a dying state. Huss excludes 13 fatal cases from his returns, and Wunderlich 14 from his, for the same reason. 10 out of the 12 deaths among Schmidt's cases proved mortal within three days after admission. Military have a great advantage over civil hospitals, owing to the early admission of the cases. This is an additional reason for the comparatively small mortality of pneumonia among the soldiery.

Chomel's cases, reported by Louis, seem at first sight to contradict the law that, in pneumonia, the later the admission the greater the mortality, since almost one-half of those cases admitted during the first four days of their illness died, and only one-fourth of those admitted later. This apparent contradiction is nullified when we recollect that these patients were all selected from vigorous persons, the

disease being free from complication; that more than one-half of the deaths among the earlier admissions were above the age of fifty, while only one-third of those among the later were so; and that, unfortunately for them, there was more time for the practice of repeated bleeding to take fatal effect among the former than the latter class. This latter reason bears with especial force on those treated after the sixth day, of whom only one-sixth died, and who, besides being younger, were on an average bled only twice, while those treated earlier were bled thrice, their mortality being above one-third.

The discrepancy which sometimes exists between the reports of a hospital physician the devotee of some particular treatment, and the authorized returns of the same hospital, is doubtless owing to the matter-of-fact way in which the registrar enters all the cases, whether apparently dying or favourable on admission, and the involuntary selection of his cases for treatment and report by the physician himself.

Andral and Grisolle consider that pneumonia of the *upper* lobe is more often fatal than that of the *lower*. The observations of Briquet, Pelletan, and Schmidt, support the contrary view. There is no doubt that pneumonia of the upper lobe is more liable to be overlooked than that of the lower, and its apparent will therefore be greater than its real mortality. There are no grounds for the supposition that pneumonia of the right lung is more fatal than that of the left. We may safely, in comparing the statistics of the mortality of pneumonia furnished by various observers, put out of sight the situation of the disease, whether as regards lobe or lung. Lobular pneumonia is usually more asthenic than lobar or diffused pneumonia; and the mortality of the former, and that also of vesicular pneumonia—a form of the disease of which practically we know nothing—are probably greater than that of lobar pneumonia.

But while the situation of pneumonia exercises little or no influence on its mortality, the *extent* of the disease exercises a great influence. The proportion of deaths is from two to six times greater in cases of double than single pneumonia. The discrepancy is greatest in Briquet's cases, and in Bouillaud's reported by Pelletan. Nearly one-half of the double pneumonias treated by bleeding *coup sur coup* by Bouillaud, died, and only 1 in 14 of the single pneumonias. Dr. Bennett, Dietl, and others of the non-bleeding school, give the proportion of double pneumonias among their cases, but not among their deaths. De Bordes, who rarely resorted to local bleeding, and only twice, successfully, to bloodletting, lost in proportion twice as many cases of double as of single pneumonia. Varrentrapp, who treated his cases with chloroform, and with one exception refrained from bleeding, lost none of his double pneumonias, amounting to 5 out of 23; and Dr. Bennett lost none of the 11 uncomplicated cases of double pneumonia out of a total of 65 treated by him. It must be owned that excessive and repeated bloodletting seems to be much more fatal in cases of double than of single pneumonia.

The proportion of cases in which both lungs are affected varies from

1 in 4 to 1 in 18. Unless, therefore, the proportion of such cases, and of the deaths among them, are given, we cannot compare the returns with each other in estimating the success of any particular plan of treatment.

Pneumonia is much more fatal in cases affecting the whole lung, than in those affecting a portion of the lung or one lobe only. This result was very marked in Dr. Morehead's cases; in Bouillaud's, reported by Pelletan; and Chomel's, by Grisolle. We are of opinion that the mortality from pneumonia of the whole lung is even greater than it appears, for its existence to so great an extent is difficult to discover during life; the presence of pneumonia in all the lobes of either lung is far from infrequent, but it by no means follows that in such cases the whole lung is affected. The evidence relating to the effect of bloodletting on such cases is by no means so complete as in double pneumonia; but, so far as it goes, it confirms the inference that large and numerous bleedings exercise a most unfavourable influence on those cases in which the extent of the disease is great. Indeed, the frequency of such cases, as well as of double pneumonias, in the hands of those who resort to repeated bloodletting, leads us to infer that the extent of the disease increases in consequence of that practice. Facts are still required to prove this inference, but those we possess strongly corroborate it.

The *stage* of the disease at which the treatment commences exercises a weighty influence on the result. This is illustrated by the position already demonstrated, that the later the admission the greater the danger; which may be re-stated thus: the later the stage on admission, the greater the danger. Cases are more likely to prove fatal when admitted during the second stage than the first. This rule has tenfold force when applied to the third stage, or that of grey hepatization, when compared with the second.

Grey hepatization is reported by Grisolle in Chomel's, and by Pelletan in Bouillaud's cases, to be always fatal. No author ranks any of his cases of recovery in the third stage, or that of grey hepatization. Should this be? Do not those cases that pass from the first through the second, necessarily pass from the second through the third stage to recovery? Why, then, do so many observers regard that as the fatal stage? The proportion of deaths presenting grey and red hepatization varies greatly in the experience of different observers. While about one-half show grey and the other half red hepatization in Chomel's and Bouillaud's cases, seven-eighths exhibit grey and one-eighth red in those of Dietl and Briquet. If we take Dietl's returns as a test, it speaks unfavourably for the repeated bloodlettings of Bouillaud and Chomel, that so large a proportion of their cases died during the second stage; it suggests, in fact, that their career was cut short prematurely by the bleeding.

Pneumonia of a *low* type is, in a vital sense, diametrically opposite to pneumonia of a *sthenic* type in essential character, result, and fitting treatment. They are really two opposite diseased conditions, though identified by a common name. The anatomical appearance of

the affected lung itself in the two types is wonderfully alike, the sthenic being firmer, ruddier, and less easily broken down than the asthenic. The chief post-mortem difference is the character of the pleuritic exudation, which forms a firm, thin, almost dry membrane in the one case, and in the other a thick, loose, flabby, often creamy film, which, on rough handling, breaks down and peels off from the pleura, and which is bathed in a yellow, turbid, or even puriform fluid. Although the pneumonic lung of the asthenic type closely resembles to the eye that of the sthenic type, yet the low form, especially when associated with kidney disease, tends to run through its stages so rapidly, that even on the sixth day the whole lung may present purulent infiltration.

But during life the opposite types present the most opposite pictures. In the one, a healthy labourer, exposed, when fatigued, to cold, is seized with the "cold shivers," followed by burning fever, a hot, pungent skin, thirst, pain in the side, tightness at the chest, cough—dry at first, then expelling with difficulty a streaky, rusty phlegm; this state, after lasting seven or eight days, yields to a moist skin, a more liquid puriform expectoration, freer breathing, slower pulse, and complete resolution, and in a fortnight ends in perfect recovery.

In the other, a man of broken-down constitution, of loose, flabby fibre, takes a chill, becomes listless, depressed, perhaps delirious; the features relaxed, the eyes dull, the skin dusky and thick, and covered with an unpleasant sweat; the pain sometimes sharp, sometimes absent; the disease in local symptoms perhaps quite latent; the whole frame nerveless, prostrate, and sinking. The progress of the disease is sometimes very rapid, sometimes slow; death is frequent; convalescence tedious and imperfect. The disease often ends in gangrene. In this kind, bleeding, general or local, antimony and calomel, increase the prostration and hasten the fatal end.

These two opposite types of pneumonia melt into each other by every intermediate shade of difference. The low type of pneumonia is painfully frequent in our London hospitals. Dr. Hughes informs us that at Guy's Hospital, during the five or six years ending 1849, out of 54 post-mortem examinations of pneumonia, gangrene was present in one-third. The low type of pneumonia prevails also in Dublin. But in Paris, and perhaps to a less extent in Edinburgh, the sthenic is the prevailing type.

Although the asthenic form is often more marked by its low character during life, than by the degradation of the pneumonic lung, there are cases not essentially asthenic that present themselves early in a typhoid form. There are scarcely any symptoms of pneumonia, but the patient presents great depression and low muttering delirium, and at the first glance appears to be sinking under low fever. Physical examination exhibits pneumonia, perhaps occupying, as in our own cases, the upper lobe. The patient, supported by food and moderate stimulation, passes rapidly to a recovery, perhaps raising but little phlegm. The dulness on percussion disappears in three or four days, vesicular respiration is restored, and the patient, perhaps the very day

after that of the greatest collapse, looks bright and cheerful, and seems literally to waken up suddenly out of the disease.

Secondary pneumonia, occurring frequently after measles, occasionally after scarlet fever and small-pox, is the most formidable and fatal form of the disease. More than one-third of Becquerel's cases of measles were attacked with pneumonia. Of these patients, amounting to 21, 1 only recovered. In the practice of Rilliet and Barthez, four-fifths of the like cases died. The mortality was as great in proportion in the few cases following scarlet-fever and small-pox.

In all these secondary pneumonias the disease is of a low type.

Pneumonia consequent on operations and injuries is a very frequent cause of death in the surgical wards. Mr. Erichsen discovered pneumonia in 28 out of 62 deaths from operations, injuries, and surgical diseases. These cases were none of them dependent on the absorption of pus. The greater number of the cases were of an asthenic type, the disease being double in no fewer than 22 of the patients. We cannot rank the fatal so-called lobular pneumonia so frequently following phlebitis, with the true "croupous" pneumonia, whether lobar, lobular, or vesicular. The latter consists essentially in an exudation which fills the air-cells, passes through red and grey hepatization and purulent infiltration, and is finally evacuated, leaving the air-cells uninjured, and capable of resuming their respiratory functions in a fortnight or three weeks from the seizure. The pneumonia of phlebitis, on the contrary, occupies a small and irregular group of air-cells or lobules, attacks the walls of the cells themselves, and ends in suppurative destruction of the tissue of the lung.

Symptomatic pneumonia, occurring in the progress of acute rheumatism, ague, typhus, and typhoid fever, is in some of these cases less fatal, in others more fatal, than uncomplicated pneumonia.

Pneumonia is frequent in acute rheumatism, especially when complicated with pericarditis. Dr. Latham met with 18 cases of pneumonia, 3 of which were fatal, in 136 cases of acute rheumatism, in St. Bartholomew's Hospital; Dr. Fuller reports 28 pneumonias, 4 being fatal, in 246 cases of acute rheumatism, in St. George's Hospital; and Dr. Taylor gives 7—perhaps 9—cases of pneumonia (1 of which died) in 22 cases of rheumatic pericarditis, in University College Hospital, not including 1 death from lobular, and 1 from hypostatic pneumonia. The average mortality of pneumonia in St. George's Hospital was, according to Dr. Barclay, 26·8 per cent. during three years; that of rheumatic pneumonia, according to Dr. Fuller, was only 14·3 per cent. These statements prove that pneumonia in acute rheumatism, even when complicated with endo-pericarditis, is less fatal than in the average of cases.

Symptomatic pneumonia in connexion with ague, occurring as an epidemic in April, appears not to have been fatal in the experience of Delvaux, and Morère met with 5 cases of adynamic intermittent pneumonia during a very fatal epidemic of pneumonia in the Hautes Pyrenees during the winter of 1854.

The symptomatic pneumonia of typhus and typhoid fever

cannot properly be classed with the various forms of croupous pneumonia.

The symptomatic pneumonia of influenza is not usually frequent. Dr. Peacock only observed seven cases during the epidemic visitation of 1847. He does not state that one of these died. Dr. Jackson saw several cases of pneumonia consecutive to influenza in Boston, all of which recovered.

Complicated pneumonia is, as a rule, more fatal than idiopathic pneumonia. The proportion of deaths ought therefore, *cæteris paribus*, to be greater or less in proportion to the number of complicated cases.

The number of cases of pneumonia complicated with diseases of other organs, presents a remarkable difference in different places. In Paris, Edinburgh, and Glasgow, the complications appear to be few. Thus, of Bouillaud's 75 cases given by Pelletan, 16 of which are imperfectly reported, the health had been previously indifferent in 15; 6 of the 10 deaths were free from complications. Nineteen out of 82 cases observed by Briquet were deteriorated by previous disease, old age, or occupation; and 5 out of 21 deaths presented complications. Dr. Bennett's cases in Edinburgh, and Dr. Bell's in Glasgow, present a similar small return of complicated cases. The former enumerates 55 uncomplicated cases out of 65. He omits, however, 7 cases of pneumonia secondary to other diseases. Dr. Bell states that 21 out of his 71 cases had pleuritis in addition, and he enumerates no other complications.

In London, the complications are much more numerous. Thus, at St. George's Hospital, Dr. Barclay reports that 20 cases out of 34 presented complications; and while 8 of the 20 complicated cases died, only 2 of the 14 uncomplicated did so. At Guy's Hospital, Dr. Hughes observed 49 cases of pneumonia with complications, not including pleuritis, in a total of 101. Dr. Sanderson states that at St. Mary's Hospital, 47 cases of pneumonia presented complications in a total of 103, the mortality of the complicated cases being 1 in 3; of the uncomplicated, 1 in 5.

Forget observed 32 cases at Strasburg, in 18 of whom there were complications, including the whole of the deaths, amounting to 7.

The reports of the German physicians vary much as to the proportion of cases with complications. Thus Rubach only reports 6 cases of complicated pneumonia observed by him at Wurzburg in a total of 38 cases. Dietl, on the other hand, at Vienna, met with 389 cases of complicated pneumonia in a total of 750; and informs us that not one of the fatal cases, amounting to 69, was free from complication.

Certain complications are much more fatal than others. The recent paper of Dr. M'Dowel has called attention to the connexion of pneumonia with Bright's disease, the most fatal of all the complications. Dr. M'Dowel gives 11 such cases, occurring in Dublin during the years 1853–56. In 8 at least out of the 9 cases in which the state of the kidneys is described, the disease was unquestionably of some standing. In 7 of the cases there was extensive purulent

infiltration of the lung, in 4 there was gangrene. The rapidity with which the exudation broke up into pus was remarkable. One man died on the Thursday, after being exposed all night to wind and rain on the previous Sunday. The right lung was solidified throughout, and infiltrated with dark-coloured pus, the left was solidified and infiltrated in patches.

Dr. Gordon relates 3 cases in which albumen appeared in the urine after the invasion of pneumonia, and he infers that albuminuria, instead of preceding the disease, sometimes follows. Dr. Christison details a case in which albuminuria, lasting some time, came on after pneumonia in a case that was profusely salivated. Dr. George Johnson informs us that he thinks it probable that the kidney disease often springs up simultaneously with the pneumonia.

We have been favoured by Dr. George Johnson with the following important analysis of 163 fatal cases of Bright's disease observed by him:

"131 chronic cases—i.e., three months and upwards; 32 acute cases, from a few days to three months. Of the 131 chronic cases, there was pneumonia in 6, bronchitis in 10, engorgement of lungs in 38, tubercles in 13. Of the 32 acute cases, there was pneumonia in 4, engorgement of lung in 7, tubercles in 2. Besides these fatal cases, 3 cases of acute Bright's disease complicated with pneumonia have recovered. Pneumonia certainly occurs more frequently in connexion with acute than chronic Bright's disease."

Dr. Hughes observed Bright's disease in 6 of the 54 fatal cases of pneumonia examined by him post-mortem during the few years preceding.

It would almost appear that pneumonia has been more frequently associated with disease of the kidney at Guy's Hospital of late years, since of 34 cases of renal disease reported in the fifth volume of the 'Guy's Hospital Reports' by Dr. Bright and Dr. Barlow, only 1 presented pneumonia, and that to a slight degree in the left upper lobe. Many other physicians have observed pneumonia in connexion with Bright's disease. Dr. Christison noticed 3, in addition to the 1 just referred to. Forget met with 3 in 32 cases; Rubach, 1 in 38; Dr. Morehead, 3 in 103; and Dietl, 3 (all fatal) in 750 cases of pneumonia.

Pneumonia may be complicated with a great number of diseases. Dietl, with laborious minuteness, enumerates 18 acute and 39 chronic affections with which 389 of his 750 cases were complicated, and these do not exhaust the catalogue.

The most important complications for fatality or frequency are—phthisis, bronchitis, emphysema, pleuritis of a low form with turbid effusion, heart disease, aortic aneurism, pericarditis, meningitis, delirium tremens (Huss met with 30 cases out of 242 in Sweden); and affections of the liver, the spleen, and the stomach and intestines. These complications, as well as the previous existence of lung disease, exercise a pernicious influence on the mortality.

The *change of type* is almost the last cause influencing the mortality of pneumonia that we shall inquire into. We have already seen that the prevailing type of the disease differs in different countries, and that while

it is sthenic in France, it is comparatively asthenic in London; that while there the proportion of pneumonias of an inflammatory type are in excess of those of a low type, here it is the reverse. We shall find that the character of the disease alters in the same place at different periods, so that the proportion of cases of a low type, and consequently the fatality of the disease, is greater at one time than at another.

Dr. Hughes informs us that of 145 inspections of pneumonia published by him in 1842, 11 were gangrenous, or only 1 in 13; while of 54 inspections observed between that year and 1849, 18 were gangrenous, or 1 in 3. During the latter period pneumonia partook of the general low type of disease especially prevailing during the epidemic of influenza. In fact, pneumonia, while it retains in each case the characteristics of the disease, partakes more or less of the epidemic constitution of the day.

The fatality of the disease varies very much in different years. Thus, in St. Mary's Hospital, 1 died in 8·6 cases in 1854, 1 in 2·15 in 1855; in the Edinburgh Infirmary, 1 died in 2·4 in 1847–48, 1 in 5·2 in 1848–49; in the Aberdeen Infirmary, 1 died in 2·7 in 1853, 1 in 10 in 1855, and not one of 18 cases in 1856; and in the Vienna "Krankenhause," 1 died in 7·3 among the male cases, 1 in 3 among the females, in 1851, while 1 died in 3·6 among the males, 1 in 2·6 among the females, in 1855. The great discrepancy in the proportion of deaths in the same institution in adjacent years must be mainly accounted for, not by difference in treatment but by the variation in the type of disease, and other causes, for the treatment conducted usually at adjoining periods by the same physicians, would not, as a rule, differ materially. The above statements are taken from the authorized reports of the several hospitals, and embrace every case of pneumonia admitted during the respective years.

The great example set by Sydenham of recording the characters and history of disease in London during successive years, in connexion with the weather, plenty or scarcity, and other influences, has been worthily followed by an almost lineal succession of eminent men, beginning with himself and ending with Dr. Bateman. We shall select from these nearly consecutive records a few leading instances of the shifting type of pneumonia. It may be objected that those observers were unable to distinguish the disease. We grant that they could not decipher it with the nicety of the accomplished adepts in physical diagnosis of the present day; but we affirm that whoever studies their life-like narratives in an unbiassed spirit, will convince himself that though many latent cases escaped them, they really did recognise the disease; and that though the minute local changes patent to us were hidden from them, they took a larger view than we do of the disease in its vital characteristics. We refer particularly to the descriptions of Sydenham, and still more of Huxham, Hoffman, Boerhaave, Van Swieten, Black,* Morgagni, Sauvages, Cullen, Gregory, J. P. and Joseph Frank, and Acerbi. Their pathology, too, was more true than we are wont to

* On Mortality, p. 145.

give them credit for. Some of them even recognised "exudation of coagulable lymph into the pulmonary cellular tissue." They do not, however, seem to have been aware of the important changes by which the exudation was evacuated from the air-cells, after passing through the stages of red and grey hepatization and suppuration. They all recognised that the disease presented itself in two opposite types—the sthenic and asthenic—or, as some of them expressed it, the inflammatory and the typhoid.

Sydenham did not distinguish so thoroughly as his successors between pleurisy and peripneumony. He informs us that those diseases partook of the epidemic constitution of the prevailing fever in the spring of 1664 and the autumn of 1675; the latter not bearing repeated bleedings, as in ordinary pleurisy, but one or at most two bloodlettings. He also recognised two kinds of peripneumony as occurring every year, the one true, the other bastard; the one falling frequently on country people accustomed to hard labour, the other seizing those of a gross habit, and middle aged or older, or too much addicted to brandy.

Huxham, who recognised the true character of pneumonia, describes the diseases prevailing from year to year in Plymouth. In May, 1730, peripneumonic fevers were rife, but the patients freely and largely expectorated a yellow-reddish matter, which very soon proved critical, so that, unless in the very beginning of the disease, there was no occasion for bleeding, at least for a repetition of it. Pneumonia presented itself in its more or less inflammatory form most years during the early spring; but the bastard or low type prevailed along with the true in April, 1730, March, 1733, and March, 1737; and in excess of the true in March, 1732, March, 1736, and January, 1737. Huxham says, that though in the peripneumonia notha, or bastard form, the load at the breast is very great, the breathing difficult, and cough importunate; yet the fever and heat are small, the pulse quick and weak, or sluggish and oppressed, so that it is quite contrary to a true peripneumony in several respects, arises from a different cause, and requires a different method of cure. It commonly seizes the old and phlegmatic, the weak and lax, and is most rife in wet, flabby, foggy weather; whereas the inflammatory peripneumony attacks the robust and active, and is most frequent in cold, dry weather, during north-east winds. In the low form, "a serous, acrid *colluvies* may fall on the lungs, be diffused into and among the inmost recesses of their vesicular and cellular cavities." If the blood is drawn, and appears loose and not buffed, the patient soon sinks and grows considerably weaker after such an evacuation.*

Dr. Fothergill describes an insidious species of pneumonia which occurred in 1751. "They have great composure and ease, which imposes on those about them, till they are suddenly alarmed with some unexpected alteration which carries the patient off in a short time."

The prevailing type of pneumonia in London, as observed by Dr.

* On Fevers, pp. 222 et seq.

Willan, was low in 1796–97 and 1799, inflammatory in 1798, and still more so in 1800, when venesection was employed more than once before the acute pain and sense of constriction about the chest could be relieved; whereas, in the year 1796–7, bleeding proved fatal to the young as well as the aged in the peripneumonia notha then prevailing.

Dr. Bateman describes the diseases of London from 1804 to 1816. Pneumonia was chiefly of a low type during the winters from 1804 to 1810. It was inflammatory, but not severe, in 1810–11 and 1811–12. During the severe winters of 1813–14 and 1815–16, the disease was severe and highly inflammatory, and demanded repeated bleedings; whereas general bloodletting was prohibited by experience during the earlier part of this century.

The Edinburgh physicians of the last century published no works of the Sydenham and Huxham class containing a narrative of the varying characters of disease during successive years. We are compelled therefore to get what information we can by scraps, to help us in ascertaining whether the type of pneumonia has changed in Edinburgh since the days of Cullen and Gregory. There are several MS. volumes of the clinical lectures delivered by both Cullen and Gregory, in the Library of the Medical and Chirurgical Society. These volumes contain several well-told cases of pneumonia, to which we shall again allude. One at least of these cases was of a low type, and the rest were far from being of an intensely inflammatory character. They do not indeed seem to differ from the ordinary run of uncomplicated cases that we meet with in the London hospitals. There is no doubt, however, that then, as now, the pneumonia of Edinburgh was of a more sthenic type than the pneumonia of London.

There appears to be no reason to think that pneumonia has altered to a lower type during recent years in Paris. Chomel, after a lapse of thirty years, continued to treat his patients by repeated bleeding in 1850; Bouillaud his *coup sur coup* in 1846, and again in 1857, when Dr. Bennett was in Paris; Grisolle advises bleeding with the same decision in 1852 that he did in 1841 and 1837. We may safely, therefore, infer that there is no reason for supposing that the type of pneumonia is less sthenic now in Paris than it was a quarter or half a century ago.

The abandonment of bloodletting for pneumonia by Dietl, Skoda, and the Vienna school might lead us to think that the type of the disease is lower in Germany now than it was formerly; but Wunderlich and others have recently returned to venesection, which might lead to the idea that the disease is becoming again more sthenic. This is, however, mere guess-work; and in the want of more precise facts, we must leave the question unsettled in relation to Germany. The mortality of pneumonia in the Vienna Hospital varies, as we have shown, each year; but this variation is zigzag, and there is but little difference between the fatality of the disease in 1847 and 1857. Such difference as there is, however, is in favour of the former year—the deaths being 1 in 4·1 in 1847, and 1 in 3·4 in 1856.

The last cause influencing the mortality of pneumonia, irrespective

of treatment that we shall consider is hospital accommodation. We have no information as to the precise influence on the fatality of that disease from the cause in question. But the reality of such an influence cannot be doubted, when we find that the proportion of deaths to admissions varies very greatly in different institutions, and in the same institution at different times. Thus the mortality in the Hôtel Dieu de Paris was 1 in 4½ in 1787, 1 in 7 in 1822, 1 in 9 in 1839, 1 in 7·32 in 1848; that of the Hôtel-Dieu, or hospitals in Lyons, was 1 in 13 to 1 in 11 in 1787, 1 in 11 in 1822; that of the hospitals at Milan, 1 in 7 in 1822; at Padua, 1 in 15 in 1821; that of the hospital at Vienna, 1 in 12 to 13 in 1787, 1 in 6 in 1824, 1 in 7·2 in 1856; that of the Edinburgh Infirmary, 1 in 25½ in 1787, 1 in 16·6 in 1817, and 1 in 8·2 in 1842; that of the Inverness Infirmary, 1 in 23 in 1842; that of St. Bartholomew's, London, 1 in 8½ in 1787, 1 in 11·3 in 1819, 1 in 12·6 in 1834, and that of the Suffolk Hospital, 1 in 50 in 1835. The mortality is in all countries, greater in the town than the country hospitals. Two causes are at work to produce this effect: the feebler constitutions of the town inmates, and the greater crowding and worse air of the town hospitals. The increase of mortality in the Edinburgh and some other infirmaries, is in great part due to the exclusion formerly of many patients likely to prove fatal, and their admission of late years.

We see, then, how futile it is to attempt to prove the superiority and success of any particular treatment by a flattering list of cures. We have demonstrated that numerous distinct causes may operate to increase the fatality of the disease, whatever be the treatment pursued. If any one or several of these causes be in undue proportion absent or present, the results, *cæteris paribus*, will in like proportion be happy or unfortunate. It is only, then, by the close observation of each case, the rigorous analysis of the whole series, and the scientific comparison of the various returns, that we can even approximately estimate the comparative success or failure of two opposite plans of treatment.

Considered practically, however, the above causes all work together so as to stamp upon each case of pneumonia a sthenic or an asthenic type. In considering the treatment of pneumonia, we must keep these two opposite tendencies of the disease steadily before us. The low type, we must remember, is by far the most fatal of the two, and we must beware that we do not convert the sthenic into the asthenic form by a lowering practice.

We have now, after an extended examination of the evidence, arrived at the main object of this article,—the treatment of pneumonia, more especially the bad and good effects of its treatment, by practising or abstaining from bleeding.

We have already given numerous instances of the *bad effects of bloodletting* in pneumonia. Early and repeated bleedings were in fashion with Cullen. His practice is well illustrated by a case in the MS. clinical lectures before alluded to. A young man who had been

bled once, was admitted four days after a rigor, suffering from heat, headache, pain in the right breast, aggravated by coughing, dyspnœa, and expectoration. His pulse was full and *soft;* he was bled five times in two days, and died within forty-eight hours. There was extensive double pneumonia, and turbid greenish serum in the pleura.

A female, aged forty, suffering from pneumonia, who had been ill four days, was bled twice without relief, and again a third time, still without relief, and then died. These two cases, the last probably, the first certainly, of a low type, illustrate the force of Cullen's own maxim, that unless bleeding is performed during the first three days, it is commonly fatal. Cullen avowedly adopted his practice of early and repeated bleedings from Dr. Cleghorn, who, at Minorca, successfully abstracted from fifty-five to sixty-three ounces of blood from the patient during the first twenty-four hours of treatment, if seen during the first three days. Dr. Cleghorn seems to have acquired the practice from Dr. Font, of Minorca.*

There is no doubt that the strong very generally recovered, and that often speedily and well, who were treated by Chomel, Louis, Bouillaud, Briquet, and Grisolle, with repeated bleedings, especially when performed early in the disease.

But we have proved to demonstration, that under the same and other physicians, the practice of repeated bleedings was appallingly fatal when performed on the aged, especially aged females; on the weak, destitute, and drunken; on those who inhabit mountain districts, and some crowded cities, such as London; on those who have been ill above four or five days; on those who are affected with secondary, double, or complicated pneumonia, especially if complicated with Bright's disease; in short, on all those in whom the disease is of a more or less low type. Although the robust so generally escape the ill effects of repeated bleedings, yet even they are often so much lowered by them, that the disease, from being originally sthenic, may assume a low type, ending fatally. It is almost proved that in such cases a single is often converted by the repeated venesections into a double pneumonia, or one affecting the whole lung.

The extraordinary returns of Rasori prove—on the showing, too, of a partisan of the practice—that the greater the number of the bleedings the greater is the mortality. Thus, only one-third of those patients recovered who were bled more than ten times, while only one-twentieth of those died who were bled but once or twice.

The "heroic practice," not long since the fashion in this kingdom, of abstracting several pounds of blood at one time, if performed only once, and that soon after the onset of the fever, seems to be less injurious than smaller bleedings frequently repeated. Dr. Mackintosh, as we have already seen, took fifty-six ounces of blood at once, and hints that others had abstracted seventy or eighty; and Mr. Law-

* "Hic morbus (peripneumonia), ut vehemens, largis et repetitis venæsectionibus, in herbâ felicissime jugulatur." .... "Quoad curationem; ubi tempestive vocatus sum, primo scilicet die, vel secundi initio, depositâ morâ, sanguinis circiter quatuor libras, partitis vicibus, nuchthemeri spatio, si æger robustus sit, detrahere jubeo; quâ subitâ, et copiosâ evacuatione, morbus quandoque in herbâ resolvitur."—Cleghorn on the Epidemical Diseases of Minorca: correspondence with Dr. Font, p. 286–8.

rence also, speaking in 1836, actually drained "a slender young girl" of an ounce or two more than three pounds of blood, two or three hours after being attacked with supposed inflammation of the chest!

Many robust persons have the wonderful faculty of speedily making fresh blood after large and repeated bloodlettings; but others apparently as strong are not so fortunate. They remain bleached, weak, and nerveless, fall into fresh attacks of a low form, or into consumption—either finally fatal, or hang on for years with the mere remnants of their former strength. Dr. Mackintosh informs us of one man who lost 192 ounces. Several months afterwards he was weak and miserable, and it appeared very doubtful that he ever could regain his health. The same physician had seen several cases in which great injury was inflicted by very large bleedings, the continuance of dyspnœa, which increased after each abstraction of blood, having misdirected the medical attendants. One patient was on the brink of the grave, with a pale, sunk countenance and cold extremities. The strongest stimulants were administered along with large opiates. All these cases eventually recovered.

A first or even a second bleeding rarely or never produces an injurious effect on cases of pneumonia, unless the disease is of a low type or in an advanced stage, or the constitution is deteriorated by old age, destitution, drunkenness, or disease,—especially Bright's disease.

The physiological causes of the injurious effects of repeated bloodletting in pneumonia, more particularly of the low form, are many of them understood. Repeated bleedings lessen the red corpuscles, but not the fibrin. The thinned blood, with its excess of fibrin, tends therefore to exude more readily into the air-cells, and hence the pneumonia, from being single may become double—from centering itself in one lobe, may diffuse itself over the whole lung. This especially applies to later and more repeated bleedings. The tension of the blood in the vessels leading to the affected capillaries is rendered inadequate and oscillating under the diminished amount of the fluid and the lessened force of the right ventricle. Consequently the proper renewal of the circulation through the affected portion of lung is checked, the supplies and vital force necessary to enable the exudation to go rapidly through its stages to suppuration and evacuation are lessened, and the restoration of the respiratory functions are dangerously delayed. These are the very dangers that render the disease so formidable when asthenic.

Dr. Bennett has stated the ascertained and some of the probable changes which take place in the exudation during its various stages with great clearness.

But we must now put the important question, What are *the good effects of bleeding* in pneumonia? Have we not neglected its employment of late years too much?

Dr. Gregory bled a man, admitted early in the disease, with hot skin and intermittent pulse, to sixteen, and again in the evening of the day of admission, to eight ounces, with the effect of carrying off the fever, moistening the skin, and promoting the expectoration. All are agreed

that early venesection reduces the fever, relieves the oppression, and often lessens pain for a time, though it does not stop the progress of the disease.

Wunderlich has recently employed the bleeding and the expectant plans in two series of parallel cases. He found that when the fever rapidly disappeared, the convalescence was quick and recovery complete; when the fever was prolonged, the process of exudation was slow, the augury bad, convalescence often incomplete, and ending sometimes in induration or tuberculization of lung. Bloodletting shortened the fever, lessened the danger, and hastened the favourable result, the mortality being 1 in $15\frac{2}{3}$. On the expectant plan the fever was prolonged, the convalescence delayed and unsatisfactory, the result often fatal, the mortality in 76 cases so treated being 1 in $5\frac{11}{13}$. Local bleeding and spontaneous hæmorrhage, as from epistaxis and menstruation, shortened the fever and improved the result, but not to the same extent as venesection. The fatality in 114 cases that lost blood generally, locally, or spontaneously, was 1 in $12\frac{2}{3}$. We are unable to scrutinize Wunderlich's inferences, as we want the age, extent of disease, time of the attack, and other important details. Wunderlich has quite a genius for subdivision; he classes his patients into six classes, according to the rate and completeness of the cessation of fever; he classes his treatment into—expectant, with local bleeding, with general bleeding, with spontaneous bleeding, and he subdivides some classes accordingly as they are treated with or without tartar-emetic and digitalis. Then, after docketing his patients and his plans in this bureaucratic way, he sets off the cases under one treatment, each in one or other of the six classes of "*defervescenz*," with exact precision, case for case against the cases under another treatment, and thinks himself able to take the rule and measure of these complex vital changes in living beings, just as if he had them oscillating in his chemical balance, and weighing against each other even to the one-thousandth of a grain. We look upon this as merely the extravagance of over-refinement. The mind fixed on all these minute self-created subdivisions loses sight of the great vital differences of each case from all others, and forgets that there is a natural difference in each as to severity, fever, duration, and convalescence, that overrides all minor differences of treatment. We should have esteemed Wunderlich's researches more if they had been larger in spirit and more practicable in aim. We consider, however, that his results are decidedly favourable to the judicious and moderate employment of venesection.

The records of pneumonia, both in this country and on the Continent, teem with cases in which bloodletting, when performed early and in the stage of fever, lightened oppression, released the breathing, cooled the skin, quieted and softened the pulse when hard, or rendered it fuller when depressed, somewhat mitigated the pain, and gave general and grateful relief. These good effects are not, however, permanent, though the fever and oppression rarely retain their prime intensity. It must not be forgotten that the disease is a reality, and must pass through its natural stages to a favourable issue, or the

reverse. It is never really "jugulated" by the most heroic bleeding, but the augmenting distress and the intensity of the fever are subdued by it. No one has proved that the mortality has been increased, or mischief done by a single or even a second early moderate bleeding, in sthenic pneumonia affecting young and healthy persons.

The relief afforded by early bloodlettings has been too great a temptation to the physician when he found the distressing symptoms resuming their sway. He had the power of again giving certain relief in his hands, and it was natural that he should use it. But in doing so again and again, as we have amply proved, he paid dearly for present relief by future mischief, and even destruction.

But because of the certain evil of repeated bleedings, are we to withhold the certain relief of an early bleeding in sthenic cases, especially when we find no proof whatever that it has done mischief, and numerous evidences that it has done good? The evidence in favour of such relief is immeasurably strengthened, when we find that seven-eighths of Bouillaud's cases did well, although they were treated on the inexorable plan of indiscriminate bleeding *coup sur coup*—whether in the young or old, the strong or weak, the sthenic or asthenic case. What are the physiological reasons for or against a single early bleeding for a sthenic pneumonia in a healthy subject? During the early period the channels for the flow of blood through the lungs are narrowed. Now the lungs are like no other organ. You cannot divert from them one drop of the blood circulating throughout the system. The only way in which you can accommodate the circulation to the bloodvessels, is by abstracting blood from the mass, and so lessening its total volume. But this only applies to early venesection, which we should feel called upon to perform, on the grounds just established, whenever there are great tightness, dyspnœa, and fever in sthenic pneumonia occurring in robust young persons previously in good health. Dr. Bennett himself sanctions small and moderate bleedings directed to palliate certain symptoms, and especially excessive pain and dyspnœa.

Local bleeding, whether by leeching or cupping, has at all times been employed in pneumonia. Dr. Bell treated 71 cases of pneumonia in the Glasgow Infirmary: 36 of them he bled without the loss of a single patient. Venesection was performed in only two of the cases, but four had been bled before admission; the remainder were bled locally—chiefly by cupping. The local bleeding was preferred to the general, because the majority of them had passed into the stage of exudation. He, in fact, bled the sthenic cases only, and with the exception of four in the second stage, those exclusively that had not overpassed the first stage.

Experience has shown that while early venesection often removes tightness in the chest and dyspnœa caused by the pneumonia, local bleeding relieves pain in the side caused by the pleurisy. According to the predominance of oppression on the one hand, or pain on the other, ought general or local bleeding to be preferred; the former for the pneumonia itself, the latter for the attendant pleuritis.

Local bleeding is found to exhaust less, in proportion to its effects, than venesection. It may be safely employed, too, at a later period of the disease; and after general bloodletting can no longer be practised with safety, cupping or leeching may be resorted to.

Local bleeding is also less injurious than venesection in cases of an intermediate character between the asthenic and the sthenic. It ought to be employed with caution or not at all in secondary pneumonias, in cases of a decidedly low type, in the later stages of the disease, and in very old persons.

The *good effects* of *abstaining from bleeding* may be inferred from what has just been said with regard to the bad and good effects of bloodletting.

Dietl treated 380 case of pneumonia: 85 by venesection; 106 by tartar emetic; 189 by diet only. The mortality was 1 in 5 among those bled; 1 in 13½ among those treated by diet only. At a later period, Dietl treated 750 cases without bleeding, when the mortality was 1 in 10·8. Although he has given in the summary a great deal of elaborate detail as to the cases, showing the sex, age, and constitution; the previous ailments; the duration of the premonitory stage, fever, and convalescence; the situation and extent of the disease; the amount of dyspnœa, cough, and expectoration; the state of the urine; the complications; and season; yet he has only given information as to the death, in relation to complications, sex, season, and character of hepatization. We are unable, therefore, to compare his returns with those of Pelletan, Briquet, and other partisans of repeated bloodletting.

Although these deficiencies prevent us from making strict comparative deductions as to Dietl's cases, yet we are able to say that the proportion of aged persons was up to the average, and that the number of complications was unusual. We consider that the comparatively small mortality in Dietl's cases is owing to the non-bleeding of aged, weak persons, of cases of double pneumonia, of those admitted long after the onset of the disease, and especially of cases of a low type.

Dr. Bennett's treatment, which was without bleeding, and was directed to further the natural progress of the disease, has been very successful, the mortality being only 1 in 21⅔. The patients were comparatively young, the average age being thirty-one. 10 only out of 65 presented complications, including the 3 deaths. In this respect Dr. Bennett's cases approximate to those of Bouillaud, Chomel, and Louis, and they were apparently at a disadvantage in this respect when compared with Dr. Bell's cases, seeing that Dr. Bell does not state that any of his were complicated, and that he specially excluded those associated either with phthisis or Bright's disease. Dr. Bennett also excludes seven fatal complicated cases, "in which, as the result of chronic, cerebral, cardiac, renal, or other pulmonary disease, such as phthisis, pneumonia appeared before death." The contrast in this respect between Dr. Bennett's and the London Hospital returns is very great, seeing that while one-half of their cases were complicated, only one-sixth of his were so. It is remarkable, and something more

than a coincidence, that during the years 1855 and 1856 the cases of pneumonia admitted into the Edinburgh (Dr. Bennett's), Glasgow (Dr. Bell's), and Aberdeen infirmaries exhibited a very small proportion of deaths. The diminution in the mortality in Aberdeen was rapidly progressive, the deaths being 1 in 2·7 in 1853, 1 in 5 in 1854, 1 in 10 in 1855, and not one out of 18 cases in 1856. There is no reason to think that the treatment was materially modified during that period; and we may consider it as proved that in Aberdeen at least, the tendency to death from pneumonia was nearly four times as great in 1853 as in 1855, and more than that in 1856. It may be almost inferred that in the three principal towns of Scotland pneumonia exhibited a remarkable tendency to get well during the years 1855 and 1856, and that the non-bleeding plan of treatment pursued by Dr. Bennett in Edinburgh, and the mixed plan by Dr. Bell in Glasgow, were equally and remarkably successful. While, therefore, these facts do not detract from the great credit due to Dr. Bennett for the judicious and, at all events, successful management of his cases, it must be admitted that the returns presented by himself on the one hand, and by Dr. Bell on the other, cannot be set on the same parallel of comparison with the cases treated either with or without bleeding in Paris, Vienna, Leipsic (Wunderlich), Milan, or London.

From the great similarity of the usual type of pneumonia in Edinburgh and Paris, we are entitled to compare Dr. Bennett's non-bleeding practice with Chomel's and Bouillaud's plan of repeated bleeding, and here we have a striking proof of what we have again and again urged—the destructive effect of that plan on the aged and on those having double, extensive, or asthenic pneumonias. Dietl's cases may also be brought into the comparison, although they are of a somewhat lower type, and more often complicated than the Paris cases. Dr. Bennett had 11 cases of uncomplicated double pneumonia, all of which recovered. Bouillaud and Chomel, on the other hand, lost almost one-half of their double pneumonias. Dietl gives the number of patients affected with double pneumonia, but not the number of deaths. We cannot therefore, in this respect, compare his returns with theirs, but we can in relation to the proportion of fatal cases presenting red and grey hepatization. The proportion of red to grey in Dietl's deaths was 1 in 6, in Bouillaud's and Chomel's they were equal. Hence we infer that, under the treatment by repeated bleeding, death was hastened in several of those cases that had not passed from the red to the grey stage.

We may consider it to be proved that the non-bleeding treatment is much more successful than the indiscriminate employment of repeated venesections.

One of the most important good effects of refraining from bleeding in pneumonia, claimed for it by Skoda, Dietl, and all its advocates, is the comparatively short convalescence of the patients and their rapid return to vigorous health.

The result of non-bleeding in pneumonia in the Vienna Hospital in 1856 was very *unfavourable,* the deaths being 1 in 4 among the males,

1 in 2·7 among the females. Only a few of these cases were bled. It is to be inferred that fewer of the asthenic cases, especially among the aged, died than would have been the case if they had been bled repeatedly, or even at all. When we deduct this favourable set-off, we are compelled to infer that the mortality was greater among the sthenic cases than it would have been under a discriminating plan of moderate early bleeding.

While, then, the non-bleeding plan has a demonstrable advantage over that of indiscriminate and repeated bleedings, we maintain that the discriminating practice of moderate early bleeding, general or local, in cases of more or less sthenic pneumonia, and of refraining from it altogether in asthenic pneumonia, whether as regards the character of the disease or the constitution of the patient, is pressed upon us both by experience and science.

PLANS OF TREATMENT.

Bleeding Plan.

		No. of cases.	No. of deaths.	One death in
Repeated or large bleedings.	Bouillaud (Pelletan)	71	9	7·9
	Ditto (own report)	102	12	8·5
	Lacaze	42	1	42
	Briquet	129	17	7
	Chomel (Louis)	78	28	2·8
	Ditto (Grisolle)	50	14	3·6
	Ditto (ditto) [T.E.]	24	13	1·8
	Louis (20 cases T.E.)	29	4	7·2
	Grisolle	232	37	6·3
	Piorry	30	2	15
	Rasori [T.E.]	747	164	4·5
	Ditto Wagner [T.E.]	13	7	1·9
	Acerbi [T.E.]	142	16	8·9
	Skoda [T.E.] females	64	1	64
	Do. [do.] males & fem.	—	—	8
Few bleedings.	Laennec [T.E.]	62	6	10·3
	Do. [T.E.] Leconteulx	32	12	2·7
	Grisolle [T.E.]	110	28	4
	Ruef [T.E.]	94	5	18·8
	Bang [T.E.]	54	2	27
	Gerhard (children 6 to 16)	26	1	26
	Becquerel (children)	12	8	1·5
	Ditto, ditto (secondary)	16	15 or 16	
	Dietl	85	17	5
	Wunderlich	47	3	15·6
	Ditto*	114	9	12·7
	Dr. Bell [M.]	36	0	0
	Wossidlo	112	4	28
	Burkart	60	1	60
	De Bordes†	13	2	6·5
	Müller†	10	0	0
	Forget†	32	7	4·6
	Schmidt‡	6	1	6
	Morehead, Dr. (local)	57	11	5·2
	Ditto, total (bled or not)	103	32	3·2

Non-Bleeding Plan.

	No. of cases.	No. of deaths.	One death in
Trousseau [T.E.]	52	2	26
Grisolle [T.E.]	44	6	7·3
Thielmann [T.E.]	110	12	9·2
Schmidt [T.E.]	87	4	9·1
Ditto [O.]	13	6	2·1
Ditto [T.E., O.]	46	9	5·1
Dietl [T.E.]	106	22	4·8
Skoda (Balfour), 1846	45	3	15
Ditto (Mitchell), 1852	54	14	3·9
Oppolzer (ditto)	32	5	6·4
Vienna Hospital (ditto)	240	40	6
Sigmund, Pr. & Ho. (ditto)	743	104	7
Vienna Hospital (ditto)§	5909	1439	4·1
Ditto, 1856	658	187	3·5
Wittich [M.]	23	0	0
Bell, Dr. [M.]	35	4	8·7
Gandini [M., O.]	25	0	0
Cohn [T.E., O.]	57	24	2·4
De Bordes [O.]	77	17	4·5
Burkart [O., L.]	60	1	60
Baumgärtner [Ch.]	30	3	10
Wucherer [Ch.]	90	1	90
Helbing [Ch.]	62	5	12
Varrentrapp [Ch.]§	23	1	23
Stohaml [Ch.]	30	0	0
Frohmuller [Ch.]	12	0	0
Hutava [Ch.]	12	0	0
Theile [Ch.]	6	0	0
Salawa [Ch.]§	15	0	0
Dietl [diet only], 1848	189	14	13·5
Ditto [ditto], 1852	750	69	10·9
WiedenHos. Vienna, 1855	—	—	4·8
Bennett, Dr.	65	3	21·7
Wunderlich	76	13	5·8
Kissel [I., Co.]	112	5	22

* Combined general and local bleeding, and spontaneous hæmorrhage.
† Part bled generally, part locally. ‡ Four bled before admission. § Very few bled.

T.E. Tartar-emetic. M. Mercury. O. Opium.
Ch. Chloroform. I. Iron. Co. Copper. L. Lead.

REVIEW II.

A Treatise on Rheumatic Gout, or Chronic Rheumatic Arthritis of all the Joints. By ROBERT ADAMS, M.D., A.M., Ex-President of the College of Surgeons in Ireland, and of the Pathological Society; Surgeon to the Richmond Hospital, Dublin. Illustrated by Woodcuts and an Atlas of Plates.—*London*, 1857. 8vo, pp. 362.

THE volume presented to us by Dr. Adams "embodies the substance of clinical lectures" on rheumatic arthritis, delivered by him to the pupils of the Richmond Hospital during a series of years, and "comprises the principal part of various communications" he has from time to time published relative to this disease. Consequently, he observes it cannot be expected the work will be found to suggest much that is new, or that has not already, by himself or by others, been laid before the profession. But we must do Dr. Adams the justice to state, that having carefully collected from the various medical periodicals all the information on this subject, and having added this to his own experience, he has produced a work complete as far as it goes, on the characters and effects of this disease—a volume which well deserves the careful perusal of every practitioner.

"Chronic Rheumatic Arthritis," is the term Dr. Adams has selected to designate an affection of the joints which, whether following gout or rheumatism, or what authors term "Rheumatic Gout," has long been known, and will be found described by Cruveilhier and some few others, as a chronic, unmanageable, distressing, and persistent malady, affecting the articulations, and rendering the sufferer in every sense of the word "crippled" in his joints. Every one familiar with the dissecting rooms of London hospitals, has had opportunities of examining every species of joint, in every stage of the disease described by Dr. Adams; and we can individually bear testimony to its frequent occurrence amongst the poorer classes of this metropolis, and to the accuracy of Dr. Adams's descriptions of the disease affecting the various articulations of the body. How far the terms "Rheumatic Gout," or "Chronic Rheumatic Arthritis," are justified in their application to this malady, may be questioned by several of our readers; but, in justice to Dr. Adams, we must remind them that he confesses to the difficulty of proposing a name entirely unobjectionable. Dr. Barclay, in his recent work on 'Medical Diagnosis,' speaking of rheumatic gout, says:

"We cannot refuse a separate place in our classification to a disease which, though its place in pathology be as yet undetermined, is very well marked in particular cases. The peculiar twisting and distortion of the joints in persons who have suffered for any length of time from its effects, is such as cannot pass unobserved by any one who is familiar with the aspect of disease.

"In its early history it partakes most of the character of subacute rheumatism. It differs from an acute attack chiefly in the absence of fever, and in the circumstance that comparatively few of the joints are under its influence at the period of its commencement; there is a good deal of swelling, and perhaps of redness, of one or two joints, but these are not marked by the

extreme tenderness and pain so distinctive of rheumatic fever or of gout; while the local inflammation is more decided than in the subacute form of the disease, if we except a few cases which we have characterized as abortive attacks of acute rheumatism. If due consideration be given to these circumstances, the practitioner will be prepared for the incursion of a most inveterate and most hopeless malady. And let it be remembered, that diagnosis has in this case very much to do with prognosis; where we recognise rheumatic fever, we know that, except the heart become implicated, the patient will be in a few weeks at most as well as ever; and that he is not very much more liable to a second attack than his neighbour. When gout is clearly established, we are sure that the patient will be, for a time, in better health than usual, after the present pain and distress have subsided; but that all the care possible will scarcely serve to ward off a second attack. When we have only subacute rheumatism to deal with, we look for either a trivial and passing affection, or for a lingering illness, as we find less or more of local action; but with rheumatic gout we ought to know that our patient is exposed to protracted suffering, and is liable to remain a cripple for life. When this is not foreseen, much discredit may unnecessarily be brought on the profession—much undue praise be given to the quack, into whose hands such cases are very apt to fall; he will not scruple to throw on the regular attendant the blame of all the mischief which has happened, and claim for himself the credit of any improvement which, under favourable circumstances, nature herself may slowly produce.

"As the disease proceeds, its peculiar characters begin to develop themselves; the swelling subsides in some measure; the redness, if any, is gone from the joints first attacked, but they remain stiff, tender, and useless; while others, in succession, become the seat of inflammatory action; until at length the unfortunate patient is reduced to a condition of utter helplessness. When convalescence has slowly been established, as it may be after either months or years of suffering, considerable distortion and permanent stiffness are the invariable result."

The symptoms of this disease, like those of chronic rheumatism, sub-inflammatory in their nature, are located in the joints, whether small or large; but the disease certainly appears to possess this one peculiar characteristic of clinical importance, that it will be found sometimes to be simply local and affecting but one joint, and that in all probability the hip or the shoulder; or it will be constitutional, and affecting any and several of the joints. In the latter case it generally pursues a steady, undeviating, and deteriorating course, rendering the patient in time crippled and helpless. In the former case the disease will most frequently be traced to some local injury, or to long-continued and excessive exercise or exertion. When constitutional, it will often be attributed to some attack of rheumatic fever which has continued on to a chronic state, accompanied with gradual rigidity of the affected joints. Dr. Haygarth, one of the first to draw attention to this affection, considered it as "one of a peculiar nature, and clearly distinguishable from all others, by symptoms manifestly different from gout, and from both acute and chronic rheumatism."

In what pathological changes does this malady differ from the other inflammatory affections of articulations? Dr. Adams has most carefully noted the former, and in the following order describes them:—viz., those which occur—1. In the fibrous and synovial tissues; 2. In the cartilaginous and fibro-cartilaginous; 3. In the osseous structures.

In order to detect the primary alterations in the fibrous and synovial

membranes, it is necessary to examine an articulation in the early stage of the disease—in which case the capsular covering and the lining membrane of the joint afford evidence of chronic inflammatory action. The sac will be more or less distended with fluid, and the membrane itself thickened; the synovial lining will present a red colour, and "vascular tufts, red and hypertrophied synovial fimbriæ, will be seen in the joint." At a later stage the fluid is less in quantity, but the capsule more dense, and hypertrophied to such an extent, that the capsule of the hip-joint sometimes resembles, in structure and thickness, that of intervertebral cartilage, and frequently has deposited in its fibres masses of bony structure. The cartilaginous and fibro-cartilaginous portions of the joints become affected as the disease advances, and will be invariably found considerably deteriorated in the latter stages of this affection. The articular cartilage becomes by degrees worn off, leaving the surface of the extremities of the bones entirely divested of their natural covering. The inter-articular fibro-cartilages in their respective joints also disappear; "not a vestige" of them will be found in the joints in which they naturally exist, "if they have been long and severely affected by the disease."

The inter-articular ligaments will also be found, under similar circumstances, almost entirely wanting, or softened, shreddy, and torn; the round ligament of the hip, or the long head of the biceps, afford ample evidence of this damaging process.

Dr. Adams mentions that the semilunar cartilages of the knee-joint sometimes become hypertrophied, or even partly ossified; but as a general rule, it will be found that, as with all other interarticular fibro-cartilages, so are they more affected by the process of absorption than by that of hypertrophy.

With the alteration in the condition of the fibrous, synovial, and cartilaginous tissues, are combined the very interesting development and growth of the "foreign bodies" *within* the joints; of cartilaginous growth, and sometimes becoming osseous, they have been found in most of the joints in varying numbers; they may be either in immediate contact with the synovial membrane lining the wall, or growing in some fringe or prolongation of the lining membrane into the cavity of the joint. When detached, as they frequently are by accidental circumstances, they constitute the "loose cartilages" of joints, the simple existence of which in any joint, Dr. Adams looks upon as but a symptom of rheumatic arthritis in the individual affected. Cruveilhier, Haller, Morgagni, have each noticed the presence of these "foreign bodies," and in most of the articulations. Dr. Adams's remarks on their formation and progress are clear and practical, and our experience, after the examination of many cases, entirely supports all he says on the subject.

The changes undergone in the osseous system affected by this malady, are almost entirely confined to the articular extremities of bones, and are remarkable in the substitution of the ivory-like enamel over their surfaces for that of healthy natural articular cartilage. But there is one peculiar difference between some of the conditions affecting the articular extremities of bones—i.e., between those bones which form

ginglymoid joints, and those of the enarthrodial variety. In the latter, the socket becomes deepened by osseous and fibroid deposit round its margin, and the head of the round bone becomes expanded and enlarged in an equal proportion; whereas in the hinge joints, the trochleæ by their constant friction in time wear away the corresponding surfaces of the opposed bones, and thus form grooves for their reception. The lower extremity of the femur will often be found worn into ridges, and the articular surfaces of other bones similarly irregular. The shafts of bones are sometimes affected in this disease, though much less frequently in proportion to their epiphysal extremities. When this is the case, they will be found enlarged in different parts, and their structure rendered dense and heavy. Dr. Adams mentions an interesting case, in which the ramus of the lower jaw-bone was found to be an inch longer on one side than on the other, the corresponding condyle being three times the size of its fellow.

The chapter On Treatment is the only really defective portion of this volume. We are disappointed with the superficial manner in which the author disposes of it. The few observations made by him are to the purpose, but with his experience of the disease, we think he might have entered more minutely into the question of therapeutical assistance. In our own experience, the administration of colchicum and blue pill in the early stages, with the continued use of the iodide of iron, has proved of much benefit, and given considerable comfort to the patient. We should have been more satisfied, and perhaps the subject would have been better elucidated, had Dr. Adams informed us, what were the results of the chymical and microscopical examinations of the altered tissues, or whether Dr. Garrod's uric-acid test had been employed in any of the earlier stages of the disease; for there is an ample field for investigation and improvement, in the question of treatment in the earlier conditions of this grave malady.

Dr. Adams commences with the special consideration of the disease affecting the hip-joint, in consequence of the latter being the joint most frequently attacked; and as he considers the profession are more familiar with its occurrence in this locality. His observations here are highly practical, and well deserve attentive perusal. More particularly does he point out the importance of careful examination in such a case, in order to avoid the mistake apt to be made—and one which often has been made — between the effects of the disease on the head and neck of the femur, and the conditions resulting from fracture of this part. The distinctions, also, between scrofulous affections of the joints, sciatica, &c., are well considered.

When confined to the hip-joint, it appears from Dr. Adams's experience that the disease seldom exhibits itself under forty years of age; but we well know that, when of a constitutional character, neither youth nor age is any protection against its ravages.

The disappearance of the ligamentum teres in the advanced stages of this disease, is analogous to the process of absorption and destruction in other joints, in which either inter-articular ligaments or fibro-

cartilages exist; and points to the fact, that though the disease may be confined to this one joint, and so far considered a local malady, yet in all its features and effects it strictly adheres to rules, by which we may be said to detect its constitutional character, when implicating any or all other articulations. We see the same thickening of the capsular covering whether the disease be found in one or many joints. There is the same expansion of the heads of the bones, and corresponding enlargements of their sockets; there are the deposits of bony masses round the margins of the articular surfaces or cavities: we find the same thickening, vascularity, and prominent fringe-like projections of synovial membrane; we observe the usual absorption of articular and inter-articular cartilage, and the eburnation and grooving of articular extremities of bone after the disappearance of articular cartilage; and lastly, we invariably find uniform absence of all evidence of suppuration. The consideration of the changes in the shoulder-joint when affected by this disease, taken next in order, forms a very interesting chapter. There is, however, only one point to which we need draw attention. It is invariably found—at least so frequently is it the case that we may lay it down as a rule—that when the shoulder is affected, and the disease continues and increases, the tendon of the long head of the biceps muscle (the intra-capsular portion) becomes shreddy and worn away, and at last entirely cut through, analogous to the destruction of the ligamentum teres. Attendant upon this condition, a partial displacement of the head of the humerus occurs upwards; the deltoid, no longer restrained in its lifting action, draws the head of the bone partially over the upper margin of the glenoid cavity, and in contact with the under surface of the acromion and coraco-acromial ligament; the under surface of these parts becomes covered with an ivory-like coating, whilst a corresponding change takes place in the opposed surface of the head of the humerus.

Dr. Adams carefully indicates how readily this condition, the result of disease, may be mistaken for the effects of an injury; and though the true nature of these cases has been known for some time to many surgeons, and though we have personally examined and satisfied ourselves in many instances of the true character of this lesion, we quite agree with Dr. Adams, that hasty conclusions have frequently allowed practitioners to mark it down as a partial dislocation, following an accidental displacement of the biceps tendon. It is not necessary for us to enter more minutely into this question, but we venture to express a hope that Dr. Adams's careful summary of this disease affecting the shoulder-joint, will be well studied by every one interested in the pathology of diseases of the joints. One case of this partial dislocation is mentioned on the authority of Professor Smith of Dublin, in which the long tendon of the biceps was found

"In its place, and both it and the capsular ligament in this instance maintained their continuity, the loss of which has been referred to as the lesion permitting of the displacement upwards of the humerus."

The numerous cases that have occurred in the Dublin hospitals and

workhouses, have enabled Dr. Adams to draw up a very complete list of the various diseased conditions in all the articulations of the body, and cases are given in the work which illustrate every variety.

One interesting fact remains to be mentioned, which is, that in this disease, as the mischief advances in a joint, there appears to be a tendency in some bones, to a separation of the epiphyses from the body of the bone, long after ossification must have consolidated the pieces together. This is especially observed in the acromion, and Dr. Adams has in his illustrations shown several specimens of it.

We cannot conclude our observations, without drawing attention to the remarkably well-illustrated specimens of diseased joints, which accompany the volume in a separate atlas, and which have been produced regardless of expense. They are most truthful in detail and abundant in character; they form a most valuable addition to Dr. Adams's work, and the profession have much to thank him for, in preparing and presenting to them such interesting and instructive volumes.

Review III.

1. *Fifteenth Annual Report of the Registrar-General of Births, Deaths, and Marriages in England.—London*, 1855.
2. *The Census of Ireland for the Year* 1851. Eleven Volumes. Presented to both Houses of Parliament by command of her Majesty.
3. *A Selection from the Physiological and Horticultural Papers, published in the Transactions of the Royal and Horticultural Societies.* By the late Thomas Andrew Knight, Esq.—*London*, 1841.
4. *An Account of some Experiments on certain Seaweeds of an Edible Kind.* By John Davy, M.D., F.R.S. ('Edinburgh New Philosophical Journal,' New Series, vol. iv.)

In a former article we passed in review the last Census of Ireland chiefly in relation to its bearings on medical science. It was then seen how direful were the effects of famine, owing to failure of the main article of sustenance, the potato, in reducing an excessive population; and also the happy consequences resulting from the removal of that excess, and the introduction of a more varied diet, in increasing the prosperity of the remainder.

As a correlative subject, we propose now to consider briefly the food of the people, viewing it in its greatest generality, affecting as it does the whole human race for weal or for woe. As preliminary, it may not be amiss to advert to the animal, man, and his structure—the very elementary part of the subject. We are told in holy writ that man's destiny was to people and subdue the earth, not specifying any particular region; and accordingly the original man was endowed with an organization and qualities fitting him for this great object. In relation to food he was, we know, so provided with teeth, stomach, and an entire alimentary canal and digestive system, as to render him capable of deriving support either from an animal diet alone, or a vegetable diet, or from a mixture of these in almost any proportions,

bountiful Nature at the same time supplying, both in the animal matter and the vegetable matter, in their composite nature, in their ultimate elements, and more especially in their proximate principles, all the ingredients absolutely requisite for the sustentation of the animal being; and ethnological experience accords with this. Every region of the earth's surface that enterprising man has hitherto explored—and how few are those left unexplored!—has been found more or less inhabited by his fellow men,—in the Arctic regions, subsisting on an animal diet, fat supplying the place of vegetable matter, and animal life abounding there where vegetable life is most feeble and rare; in the tropical regions, subsisting chiefly on a vegetable diet, vegetable life there being in surpassing abundance, and the azotized proximate principles—gluten, vegetable albumen, &c.—supplying the place of the animal matter, the muscular fibre. Notwithstanding, however, this wide supply of food compatible with the existence of our race, there are circumstances, both as regards the earth's productions, and organization in the instance of man, tending to show that all regions are not equally suitable to his well-being, nor all kinds of diet; that there is a happy medium diet, and a happy medium region and climate, on and in which he attains his highest animal and mental qualities, and as a sensuous intellectual being, is capable of experiencing the greatest enjoyment in life. These regions, need we say, are the temperate, and the diet a mixed one of animal and vegetable matter. Further, as regards animal and vegetable substances in relation to diet, there are well-known and marked differences, some being more nutritive and sustaining than others; some more decidedly wholesome than others; and again, amongst the animals reared and slaughtered for food, and the vegetables cultivated for the same purpose, some can be reared with greater ease than others, and at a less expense, and some can be grown and brought to maturity with less risk of failure than others, and afford (occupying the same space of ground) a far greater yield. These are matters of vital interest to a people, yet, though obviously so, how little have they had the general attention which their importance deserves.

If we regard the different regions of our globe, instances offer of a very instructive kind, showing how, with advancing civilization and practical science, as the minds of men become better informed and their prejudices removed on the subject of food, population increases with diminished risk of suffering, and with greater security of comfort and enjoyment. What a contrast is presented in that extraordinary country, New Holland, between the natives, few and sparse, living wretchedly as hunters chiefly on the flesh of animals killed in the chase by means of their feeble instruments, and the European colonists, dependent for food on flocks already naturalized and so wonderfully increased, and on grain —wheat of their own growing, or imported with other imported grains—and various vegetables and fruits, most of them exotic, obtained by garden culture, and vegetable products almost as various, obtained by manufacturing skill. The one race, the lowest in the

scale of humanity, not increasing, but rather decreasing, and likely soon to become extinct; the other, an advancing people, self-governing, rapidly multiplying, and in progress to become a great nation. In New Zealand, in the United States, in North and a great part of South America, we have similar examples. In New Zealand the contrast between the past and the present is especially striking, a country in which, when first discovered, cannibalism prevailed, arising probably from a want of animals for food, and the occasional scarcity of the ordinary vegetable food, as when their male inhabitants were collected in large war-parties and were subjected to the temptation of making their prisoners their victims,—like what we sometimes read of when shipwrecked men, pressed by starvation, have become cannibals, the pressing sense of hunger and dread of death overcoming all those natural feelings opposed to the abhorred act.

In the countries mentioned we witness the changes which have taken place in connexion with a change of diet in recent times, the most remarkable even within our own memory. In other countries, as those longer known historically—Asiatic, European, and African—not less instructive lessons are afforded. In India, in those vast fertile regions included between the Indus and the Ganges, the Himalayan Alps and the Indian Ocean, we see a numerous people chained by superstition and the influence of caste—the dread of becoming outcasts—to a restricted diet, and that chiefly vegetable, of a low sustaining power, and in consequence, we may say, though to a certain degree advanced in civilization, not an advancing people—low in science, low in vigour, and subject to a great occasional suffering from desolating famine, whenever, owing to unfavourable seasons, the crop fails on which they chiefly depend. With these may be contrasted an almost conterminous people, the Chinese, offering the opposite extreme in relation to diet, and probably with some, though less, bad effect,—a people free, it would appear, from all prejudice in the matter of diet, making no distinctions whatever between clean and unclean, trying all things eatable and using them all,—a people vastly numerous, wonderfully advanced in the mechanical arts, and yet little advanced in the sciences and the fine arts, and now in a manner stationary, or even perhaps retrograde, in the scale of civilization; robust in body, unrefined in mind, and seemingly deficient in the higher mental faculties.

Leaving Asia for Europe, and confining our regards to the nations with which we are most familiar, examples offer, though not so striking. We shall select only for brief comparison England and France, and these with Ireland. The English people perhaps approach the nearest to the Chinese in the variety of their diet, but with this difference, that they are somewhat more fastidious as to the articles of which it is constituted, and their quality. Their diet is essentially a very mixed one, even amongst the labouring class, some kind of animal matter forming a part of it, though in a proportion often less than could be wished. What the people are in relation to animal power, mental energy, and intellect, it is unnecessary to state, or how rarely

we have been afflicted with famine, our resources being so many, home and foreign, and the arms of commerce so far-stretching even beyond our pressing wants; and owing to a tolerable absence of prejudice, having in the produce of other countries so many substitutes for those of our own in case of failing crops. In France we see a rival people, our rivals in war, in science, the arts, especially the fine arts, more restrictedas to diet—we speak of the mass of the people—and depending chiefly on the produce of their own lands, their corn lands, and living principally on wheaten bread. As a peasantry, in vigour of body we believe they are inferior to ours, and also in longevity and the ratio of increase. From a recent census, the population appears to be diminishing. According to statistical tables considered worthy of credit, the average length of life in France scarcely exceeds two-thirds that in England;* and this difference has been attributed by a very eminent French physiologist, M. Dutrochet, to the food being chiefly bread, every adult peasant eating, according to his calculation, two pounds daily. Whether this inference be strictly just or not, whether other circumstances may not be concerned in the effect, this at least is certain, that when a people are dependent mainly on one crop, and that people not a commercial one, they are subject to suffer in years of scarcity; and an impoverished diet we know has invariably the effect of lowering strength, and not only of body, but also of mind. In Belgium, where the peasantry live very much on the same kind of food as the French peasantry, it would appear from Dr. Webster's report on the institutions for the insane in that country, noticed in our number for July, 1857, that the large numerical amount of insane there was referrible in part to a poor and deficient diet; and if so, if capable of producing or conducing to insanity, is it at all surprising that the same cause should conduce to irregular and vicious conduct, and to violent outrages; and these, in their turn, to a diminishing population. It is acknowledged that scarcity was one of the roots of the French Revolution, an extreme centralizing system, destructive of stability, preceding as it has also followed. To come nearer home, if we compare England and Ireland, or Ireland and France, how wide is the contrast. Ireland, before the late famine, with its potato-fed population, rapidly increasing beyond safe bounds—France, with its wheat-fed population, almost stationary. The former, with a sufficiency of a peculiar food peculiarly wholesome, remarkably healthy, prolific, and long-lived, but always in danger of famine, and when famine came, more than decimated, notwithstanding the vast exertions made to give relief, affording a terrible example, not too often to be dwelt on, of the danger of dependency on one article of food, however salubrious; and more, showing how, from the facility of obtaining that one article, the little agricultural skill necessary for its culture, and the less culinary

* For further information on this subject, see the Registrar-General's Reports for 1855. In "1853 the deaths in France exceeded the births by 69,318; and that time preceded the Russian war, and the high price of provisions which heralded and accompanied the war." In 1854, "in France, to 1000 of the population 26 children were born; in England and Wales the same population gave birth to 34 children;" the difference, "not referrible to unusual abstinence from marriage, but to difference of fecundity of married women."

skill requisite for its preparation, it becomes a barrier to the improved condition of a people and the advance of their social condition.

Leaving these examples of the influences of diet on peoples, admitting it proved that a mixed diet is the best, and the best in all climates, with certain modifications, seemingly pointed out by Nature in the profusion of the vegetable productions in the warmer regions of the globe, and of animal life in the colder, let us give attention to particular kinds of diet. And first of animal food. Of whatever class this is, whether butcher's-meat, poultry, or fish, we know that it consists chiefly of muscle, and that for ordinary use any one of these may be received as an equivalent nearly for the other, the differences as to supporting strength and nourishing power not being very strongly marked, varying chiefly, it may be inferred, with the proportion of solid matter and water in each; butcher's-meat containing from about 27 to 30 per cent. solid matter—i.e., as obtained by thorough desiccation; poultry about the same; fish, including those abounding in fat or oil, from 17 per cent. to 23 per cent. And we come to this conclusion, that they are so nearly equivalent—not forgetting what we are told of Lord Bacon by his only trustworthy biographer, Dr. Rowley—that we quote the words of the latter:

"In his younger years he was much given to the finer and lighter sorts of meat, as of fowls and such like; but afterwards, when grown more judicious, he preferred the stronger meats, such as the shambles afforded, as those meats which bred the more firm and substantial juices of the body, and less *dissipable*, upon which he would often make his meal, though he had other meats upon the table."

Assigned reasons, these, for a preference hardly borne out by facts, and founded, we think, on too limited an induction, according to the Baconian philosophy.

Next of vegetable food. The several, the multitudinous articles in use of this kind, differ far more from each other in composition than the various kinds of animal food, whether we regard the proportions of solid matter and water which they contain, or the proximate principles of which the solid matter consists. Tubers and roots generally, such as the potato,* the carrot, the parsnip, the turnip, are, as it is well known, far more succulent than the grains or seeds, such as wheat, barley, oats, rice, ripe beans and peas, &c., containing a vastly larger proportion of

* Mr. J. W. Rogers, in a paper read at the British Association for the Advancement of Science (Dublin, Aug. 26th), On the Chemical Properties of the Potato, and its Uses as a General Article of Commerce if properly Manipulated, undertook to show that the matter of the potato is really equal in nutritive value to the dry matter of wheat, whilst the quantum of food produced from a given quantity of land is nearly four times that produced from wheat. The results of his analyses are the following:

	Starch. lbs.	Gluten. lbs.	Oil. lbs.
Components of the potato, per cwt....	84·077	14·818	1·004
„ wheat „	78·199	17·536	4·265
Of food from an acre of land—			
Dry matter of potato	3427	604	45
„ wheat	825	185	45

Edinburgh Philosophical Journal, Oct. 1857.

water—from 74 to 88 per cent., against from 11 to 18 per cent. They also appear to be more compounded—that is, to consist of a greater number of proximate principles, on which account probably they are peculiarly wholesome and antiscorbutic. Owing to so wide a difference, any one of them cannot be considered an equivalent for any other, whether weight for weight or article for article. Thus, in the instances of wheaten bread and potato, at least four times the weight of the latter is required to satisfy the appetite of a working man and preserve him in health; but it would appear at the same time, judging from the vital statistics of France and Ireland, that the more concentrated and stronger diet is inferior in degree of health and life-sustaining power. The same remark applies to different kinds of grain—for instance, wheat and oats, rice and maize; their qualities, no doubt, as alimentary substances, varying according to their composition, the most wholesome, if used exclusively, probably being oats and maize, and the least nutritive and supporting, rice; oats and maize being most compounded, rice least so, and containing the smallest proportion of azotized matter, or matter analogous to animal matter.

The varieties of food and their equivalents are matters of special interest—again, we may say, of vital interest, and yet have hitherto been neglected by mankind generally. How few of the people are tolerably acquainted, rationally, even with that elementary food, milk, and its analogue, eggs; and understand, that as regards man, milk, his first food, is the type of his appropriate food—milk being essentially composed of the three important elements of a wholesome diet—an azotized matter, curd, casein, in composition differing but little from muscle; saccharine matter, sugar of milk; and fatty matter, the ingredient of cream and butter. It is curious to see how, under different circumstances, man, without exact knowledge, is, as it were, guided by an instinctive taste to select, when he has the power of selection, such articles of diet as are most suitable, most in accordance with the principles of his first food. Thus, if deprived of vegetables, as are the Esquimaux, these rude people prize oil and animal fat; if a pastoral people, such as the Caffres, without cultivated vegetables and fruits, dependent chiefly on preparations of milk for their support, they strictly prohibit themselves the use of fresh milk; the drinking it is a capital offence—an acid must be formed in it, it must become sour and coagulated before it can be lawfully taken, thus tending to prevent waste and gluttony, and at the same time securing the benefit of an acid which did not exist in the milk. In the instance of the Hindoos, though in regard to diet the very opposite of the Esquimaux, yet the same taste exists for oily matter; they appear to be as fond of gee (clarified butter) as the latter are of blubber; and is it not because in the staple food of the former, rice, the proportion of oily matter (if any) is so minute? The same instinct in the Brahminical caste, who of all Indians abstain most strictly from animal food, seems to have created in them a taste for pulse as well as gee, in quest of the nitrogenous as well as the oleaginous element.*

* See on this subject Dr. Forbes Watson's remarks in the 'Sanitary Review' for Jan., 1858.

A word more respecting equivalents: were a knowledge of them diffused, such as modern science affords, how great would be the advantage, especially to families with limited means. We witness a striking example of the want of such knowledge in the instance of starch; a proximate principle, possessing the same qualities, however varied in the form of its particles, from whatever plant produced—whether from the potato, arrow-root, or wheat, yet as sold in the shops how different in price. This ignorance leads to two evils; on the part of the consumer, if fairly dealt with, to an unnecessary outlay; on the part of the seller, tempting to fraudulent practices. Were the doctrine of equivalents but tolerably understood, we might have been spared those idle discussions in the House of Commons respecting chicory, and that mischievous legislation, at one time followed, of allowing chicory to be sold mixed with coffee, as if it possessed the same qualities, and were in degree a true substitute for it. England has well been described as the paradise of quacks: we have charlatanism of all kinds, and not least remarkable as regards our food. Witness the various advertisements sounding the praises of revalenta, semolina, and other simple and compound vegetable preparations from time to time brought into the market solely with a view to gain, the venders calculating on the love of mankind for novelty, and on their credulity, remembering—"Quod mavult homo esse verum id facile credit." But to proceed.

As we have taken examples from races inhabiting different regions of the globe, it may not be without advantage to select some in illustration of the effects of diet from our home population, choosing those classes in which the difference of diet is most marked, and the effects of that difference best seen, such as the gentry and labouring peasantry, the army and navy, fishermen and miners.

As belonging to the first class may be mentioned, whether gentle or simple, high-born or low-born, all those families possessed of sufficient means to enable them to live independent of labour, head-labour or hand-labour, and to provide themselves with whatever food they desire, the comforts and luxuries of life. Their diet is essentially a mixed diet, into which animal food of the best kind enters largely, and, if they err as to quantity, their error almost invariably is one of excess. Their health is generally vigorous, their length of life long, their powers, bodily and mental, high. The diseases they are most subject to are of the acute kind—inflammations, gout, apoplexy.

The peasantry may be designated as those who earn their subsistence by farm labour, who commonly marry young, live from hand to mouth, making no provision for the future; are oftener under-fed than over-fed; and whose diet, though of a mixed kind, is principally vegetable, formed of bread and potatoes, with some cheese or bacon, some milk and less butter, and little butcher's meat. Where well off, as they are in some counties, with fair wages, they are generally strong and healthy; but when, as too often is the case, they are under-fed, they are comparatively spiritless and feeble, unequal to hard work, and nowise disposed to exert themselves, and altogether, as a class, it would

appear that their average duration of life is less than that of the gentry; and also that they are less healthy, not subject indeed to gout, but especially subject to rheumatism and dyspepsia, and very liable to suffer from any prevailing endemic or epidemic disease, such as typhus and cholera.

The army and navy exhibit almost as great a contrast as the two classes last considered. Science of late years has to a considerable extent regulated the dietary of the Royal Navy; the rations of the men are now of a mixed kind, and ample in quantity, consisting, in long voyages, of salt meats and biscuit, with flour, peas, raisins, sugar, tea, cocoa; and when in harbour, or within reach of frequent supplies, chiefly of fresh meat and soft bread, with the other additions. The same compliment, we regret, cannot be paid to the army administrators, for science certainly has not guided them in the rationing of the troops. The regulated diet of the soldier, wherever serving, whether in the coldest climate or the hottest, is much the same—a pound of meat daily, and a pound of bread—and the meat, provided at a low contract price, commonly of inferior quality; and even within the tropics, often alternated with salt meat, and generally without any of those wholesome additions given to the sailor as a regular allowance. The difference of the men of the two services in point of health is remarkable. Since the diet of the navy has been improved, those diseases which formerly were so fatal in our fleets, scurvy especially, have disappeared, and the mortality of the crews afloat has become even less than that of the civil population on shore. In the army, on the contrary, even when healthiest, the mortality exceeds that of the home population, and on most foreign stations greatly exceeds it, ranging from 20 per thousand to 120, and occasionally rising as high as 250, and even 300. Their diseases are peculiar—the diseases of camps, principally fevers and dysentery, occasionally scurvy and purpura; the latter intimately connected, as to production, with defective and unwholesome nourishment. In the navy, the effects of the old and new system of victualling are well illustrated in the contrast between Lord Anson's crew and Captain Cook's, in their memorable voyages of circumnavigation; the one, mainly owing to bad provisions, with neglect of sanitary measures, crippled, and more than decimated; the other, amply provided with good provisions, with a watchful and most judicious attention to ward off the causes of disease, preserved in admirable health. A like contrast, and one not less instructive, is presented in recent times, in our army before Sebastopol, and in our fleet lying off that fortress, in the winter of 1854–55. The condition of both is too well known to need description; it may be sufficient to say, that the troops, on the verge of starvation, suffered as much as Lord Anson's crew, and from much the same diseases; whilst the crews of the adjoining fleets being, as usual, well fed, escaped those diseases entirely, and enjoyed uninterrupted health. And the example is in a remarkable manner confirmed, and rendered more impressive by the altered condition of the former—from a state of remarkable sickliness to that of high health, as soon as, from being

half-starved, they were well fed. The French troops, at the same time similarly situated, but ill fed, in their turn becoming the victims of destructive disease. Wherever there is much fatal disease, the average length of life of course must be low. Respecting this, in either service, we cannot speak with any precision, owing to the men being discharged when no longer equal to their duties, whether from failing strength or existing disease; but this we know for certain as regards our troops, that the proportion of men remaining efficient after attaining the age of forty is very small, an age in civil life at which man may be considered as having attained his full strength, and when his labour, except where great activity is required, is of most value. Further, in the instance of the troops, a comparison might be made with advantage between the health of the regiments and their diseases in different stations. Widely scattered as the latter are over the surface of the globe, we could show how diet and climate operate in relation to health and disease, and how, were science consulted, the diet should to a great extent be regulated by the climate, more solid food given in a cold than in a hot climate; in Canada, in winter, than in Bengal; in England than in the West Indies; then we should not, we believe, find fatty liver and hepatic disease so prevalent in India, nor yellow fever and purpura in the West Indies. We would refer for some interesting facts on this subject to our number for July, 1856, in which notice is taken of Mr. Macnamara's paper on the higher rate of mortality of European troops in India in connexion with fatty degeneration of organs. The importance of the dieting of troops can hardly be too highly rated, when we consider the effects. The author whom we have just quoted, estimates the mortality of the soldiers in the regiment to which he belonged at 80 per cent. in seven years, that of the officers at 11 per cent., and the life of the soldier at 27 years. And now that our force in India is about to be increased from 20,000 to more than 50,000, and it may be to double that amount, how much more important it becomes, if we can reckon by degrees in such a matter. We remember reading a short time ago, in a leading article in the 'Times,' a confident expression, that of 30,000 troops then supposed to be required for Indian service, two-thirds would return home to give an account of their adventures; without pretending to the gift of prophecy, we can say with confidence, that one-third would be nearer the truth, unless precautions are taken as to diet, clothing, &c., very different from the old usages.

On fishermen and miners our remarks must be very brief. They, too, exhibit a well-marked contrast: the former, fully exposed to the action of atmospheric influences, the latter, in a degree excluded from them, working in a confined and often in a contaminated atmosphere, in which the only light is the feeble light of their candles; the former commonly having abundance of food, consisting chiefly of fish and the potato, the latter generally better fed than the agricultural labourer, and with a greater variety of kinds of food. The fishermen are commonly an active and vigorous race, long-lived, and the women prolific, little subject to disease, and we believe especially exempt from scro-

fulous ailments and from pulmonary consumption. The miners are less healthy, shorter lived, and seem to be especially subject to pulmonary consumption. The locality of their labours, their underground work in a heated and bad air, may conduce to these results as much as the free exposure to the sea breeze, under the light of heaven, with a diet into the composition of which iodine and bromine enter as ingredients, may contribute to ward them off and preserve health in the instance of the fishermen.

As there is a connexion between the diseases to which classes and races of men are subject and their habitual diet, so there is also a connexion between their habitual diet and their forms. Amongst those nations into whose food much oil or fat enters, fulness to corpulency prevails; witness the Esquimaux, the wealthy Turks, and the Chinese, and in a certain degree the English. We are told by an old and amusing writer, Père Lebat, that he was assured by the Caribs they could distinguish an Englishman when cooked from any other, by being more rich and succulent. On the contrary, a spare rather than a full habit will belong to those whose food, whether animal or vegetable, is destitute of, or contains little oleaginous or fatty matter; witness the Irish and French peasantry, the nomadic Arab, the North-American Indian. And we believe that fineness and coarseness of bone in like manner may be correctly referred to the quality of food, whether affording an ample or scant supply of the materials requisite to form bone. In relation to organic growth, the Lucretian principle is indisputable:

"Nullam rem è nihilo gigni unquam."

In the preceding remarks we have omitted mention of the fluids used as drinks in the examples brought forward. This omission has been intentional, drinks being foreign to our subject, and in relation to health and well-being, the fluids used to allay thirst being, we think, of less importance than the solids taken to afford support; and even in regard to the production of disease, we are disposed to believe, with one or two exceptions, that they are very subordinate; the exceptions which we hold to be of most importance are those diseases seated in the nervous system, such as delirium tremens and insanity, the one the product chiefly of intemperance in ardent spirits, the other most prevalent amongst races given to the same intemperance. It may be asked, is not gout also a disease to be excepted? We think not, it being almost unknown in the army and navy, in both which temperance is not the rule; and being chiefly witnessed, as already remarked, among the gentry; especially, we may add, of the last century; whose living was high, much animal food being used at the several meals, and the digestive function being disturbed by an excessive use of wine.

Before concluding this slight sketch, we beg briefly to advert to certain desiderata:

The first is, that our administrators should be indoctrinated in the physical sciences. A statesman may make a figure in the House of Commons or in the House of Lords, as an orator and debater, mentally

trained as Mr. Gladstone would wish, having thoroughly engrafted on him the literature of the Greeks and Romans,* and yet for practical purposes, as when presiding at the War-office, the Board of Control, or the Board of Trade, be totally unqualified for the duties assumed. Why was it that our troops were so nearly starved before Sebastopol? why was it that the Arctic exploring expeditions were so well and amply provided with appropriate and wholesome food, and enjoyed such excellent health? Was it not owing to ignorance in one instance presiding and making the arrangements, and intelligence and science making them in the other?—the Government officials directing the provisioning of the troops; the Royal Society, through some of its ablest members, directing the arrangements for the Arctic expeditions.† Had that gallant and unfortunate army (we speak of its disasters, not of its glorious feats of arms) been provided for in the same manner as the ships in any of the expeditions alluded to, how great would have been the preservation of strength and efficiency, the saving of life and exemption from suffering, and with economy of money-expenditure; indeed, if only pemmican and tea had been supplied in the place of salt beef, salt pork, and rum, much of the evil as to health might have been prevented. It would be well for the great laudators of classical literature to keep in mind that there are other excellences than a refined taste, and that there is a knowledge applicable to the ordinary purposes of life derivable from the study of the sciences—the experimental sciences—all of modern growth, which will be sought for in vain in the works of the ancients.

Secondly, we would point out as a desideratum the diffusion of agricultural and horticultural knowledge, so that the land might be made as productive as possible, the soil as enduring, and its culture as cheap and easy as possible. What waste there is even at present owing to weeds—what waste of manure! How often are inferior kinds of grasses, roots, and fruit-trees grown where superior ones might have been as easily and far more profitably cultivated! And yet agriculture and horticulture are said to be advanced in this country, as undoubtedly they are, and greatly so in comparison of what they were. It is this still imperfect state to which we would call attention, so well illustrated in the Essays of the late Mr. Andrew Knight, who did so much to improve them, especially his favourite, horticulture. We will take from his work only one example; it is that of feeding stock on potatoes. He states that potatoes eaten by hogs, cows, or sheep (all equally fond of the potato), will yield twenty times as much animal matter (butcher's meat) as the ground that grew the potatoes would have yielded if left in permanent pasture—that is, if the best kind of potato were grown, and with approved culture.

The next desideratum we would notice is that the people, especially the peasantry, should have instruction in the culinary art, so as to

* See Mr. Gladstone's reported speech on the recent occasion of the examinations at Trinity College, Glenalmond.

† Strictly, the first expedition, the arrangements for which were followed in all the others.

prepare their food well, with economy of means and least waste of substance and nutritive quality; and this instruction might advantageously be combined with some elementary information respecting the nutritive powers of different articles of diet, the best mode of preserving them, and the proportions in which certain of them may be best combined. The author whom we have just quoted expresses his confident opinion that four ounces of meat, with sufficiency of potatoes to satisfy the appetite, would afford more efficient nutriment than could be obtained from bread alone in any quantity, and at a less expense; and we have been assured by a distinguished Arctic traveller and man of science, that a quarter of a pound of suet with a pint of Indian corn (about two pounds), made into porridge, with the addition of a few wild plants, is a better and more supporting daily ration than eight pounds of meat, the ordinary allowance, when exclusive, of the huntsmen in the employ of the Hudson's Bay Company. The preservation of articles of food of a perishable kind is of almost vital importance, and yet how sadly, too, has it been neglected. Did our limits permit, we could have wished to have dwelt on this matter, and to have detailed some of the resources of science applicable to it; we must content ourselves with pointing out how even the perishable potato, whether cooked or uncooked, may be preserved by thorough drying without losing its nutritive qualities; if cooked, by being mashed and baked in the form of biscuits; if uncooked, by drying in slices, either by exposure to the sun and air, or with the aid of heat;* the desiccative process being one by which most articles of food, whether vegetable or animal, can be very easily and most economically preserved.

Another desideratum, we cannot but think, is that the prejudices of people should be removed on matters of diet, as well as their knowledge extended. An effort is now making in Paris to bring into use horse-flesh; and the report, as might be expected, is most favourable. Had it been used in the Crimea when our troops were suffering from the want of fresh meat, in the same manner as it was freely used at Kars, how much disease might have been prevented, and how many lives saved! There are other substances to which we are glad to see attention invited, such as sea-weeds, of which, growing on our shores, and for the most part neglected, there are several species known of an esculent kind, and possessed, in addition to their very nutritive qualities, of special virtues, owing to the important principles which enter into their composition, iodine and bromine.† What is remarkable in regard to the composition of these vegetables, besides the iodine and bromine which enter into their composition, is the large proportion of azote which they contain. According to the analysis of

* We have in our possession potatoes thus preserved—the common potato and the more perishable potato of the West Indies—and still in good usable condition, after having been kept more than three years without any precaution except the putting them in a dry place.

† A public-spirited and philanthropic man, Sir Walter C. Trevelyan, has offered prizes of 100*l.* and of 50*l.* for the best essays on these plants in relation to their qualities and virtues, dietetic and medicinal, as we mentioned some time back.

Dr. Apjohn, the distinguished professor of chemistry in Trinity College, Dublin, they contain in their dry state absolutely more azote than most articles of our ordinary vegetable food—more even than wheaten flour of the first quality—the mean of eight kinds examined being to that of flour as 2·407 to 1·317. The following table gives the results of the analysis referred to, and for comparison, we subjoin the proportion of azote determined by the same inquirer in a few articles in use as the food of man and cattle. We quote from the fourth article, in the heading of this paper:

"*Experiments made by Dr. Apjohn, in conjunction with Dr. Davy, in July*, 1854.

Specimens supplied by Dr. Davy, and dried at 212°.

	Nitrogen per cent.
Chondrus crispus, bleached	2·152
Fucus vesiculosus.	2·379
Rhodomania palmata (Dylisk)	3·776

Experiments made by Dr. Apjohn in August, 1854.

	Water.	Dry matter.	Per cent. of nitrogen in dry matter.	Protein contained in dry matter.
Chondrus crispus, bleached	17·92	82·08	1·534	9·587
„ „ unbleached	21·47	78·53	2·142	13·387
Gigantina mammillosa	21·55	78·45	2·198	13·737
Chondrus crispus, bleached, 2nd exp.	19·79	80·21	1·485	9·281
„ „ unbleached, 2nd exp.	19·96	80·04	2·510	15·687
Laminaria digitata, or dulse tangle	21·38	78·62	1·588	9·925
„ „ black tangle	31·05	68·95	1·396	8·725
Rhodomania palmata, or dylisk	16·56	83·44	3·465	21·656
Porphyra lanciniata, or levre	17·41	82·59	4·650	29·062
Iridæa edulis	19·61	80·39	3·088	19·300
Alaria esculenta, or murlins	17·91	82·09	2·424	15·150
Means	20·42	79·58	2·407	15·045

"N.B. The amount of *water* given in this table is considerably less than what belongs to the algæ when fresh from the sea, for they had all undergone a partial drying preparatory to being sent up from Ballycastle to Dublin for analysis."

"*Per-centage of Nitrogen in various Edible Substances dried at* 212°.—(J. A.)

Potatoes	·541
Flour of first quality	1·817
Beet-roots (mean of thirteen experiments)	1·848
Mangolds (mean of three experiments)	1·781
Sweedish turnips (mean of five experiments)	1·843
Means	1·567."

That the nutritive power of these seaweeds is considerable appears to be well proved by long experience, as also their wholesome qualities; and the manner in which the price of one of them, carrageen moss (*chondrus crispus*), has risen in the English market, becoming used for the purpose of feeding pigs and calves, shows the estimation in which it is beginning to be held. The avidity and manifest relish with which certain of these esculent algæ are eaten by cattle, probably first

suggested the idea that they might be fit for the use of man, and the pleasant taste of some of them and their grateful odour must have confirmed the idea when tried. Other substances might be mentioned, respecting which prejudices require to be removed or information afforded. We shall notice only one, the horse-chesnut, which we believe might be brought into use with great advantage, as part of the food of live stock, especially sheep and pigs. Now, we believe it is entirely neglected in this country, though possessed of highly nutritive qualities, and duly estimated for the same abroad, particularly in economical Switzerland.

The last subject we shall advert to as a desideratum is pisciculture, a new term for a neglected thing. There was a time, and that not very remote, when many of our English rivers abounded in salmon, and salmon was one of the commonest and cheapest of fish in the districts through which these rivers flowed—rivers, owing to want of due care and protection, now become almost if not entirely destitute of this valuable fish. The desideratum is that these rivers should be re-stocked, and that all our rivers and pieces of water capable of sustaining fish should be stocked with the kind best fitted for them. For this purpose, not only is it necessary that intelligent enterprise should be exerted, but also that laws for the prevention of poaching should be revised and rendered more stringent. Some years ago the fisheries of Great Britain, foreign and domestic, were calculated to produce not less than eight millions sterling a year. This is some criterion of their importance; but were they improved to the extent they might be, how vastly greater would be their value! Fortunately, so far as the propagation of fish is concerned, there is little practical difficulty. Take one of our most valuable fish as an example—the one we have spoken of as almost extinct in our English rivers, the salmon. Its impregnated ova are almost as manageable as the seeds of a plant; with moderate care to keep them moist and give them air, they may be sent hundreds of miles, even by post, without losing their vitality; they may be hatched in the most ordinary vessels, or in any pond or cistern supplied with running water of average purity, a daily change of water only being required; and after exclusion from the ova, during the space of six weeks or thereabouts, they require no food, having attached to them in the residual yolk-sack sufficient so long for their support; and further, after this time, when they need food from without, they may be allowed to leave their confinement, being capable of taking care of themselves; or if retained, they admit of being fed at no great cost or trouble. They appear to thrive pretty well on boiled liver broken fine, daily given them. These means of propagating and increasing the salmon, already in a few instances largely and successfully employed, are applicable to all the salmonidæ, its congeners, and, we believe, to all other migrating and fresh-water fish, many of which deserve to be better known and more widely distributed, such as the grayling, in this country, which is altogether unknown in Scotland and Ireland; such as the hucho (*salmo hucho*), of the Danube and its tributaries, an excellent fish, attaining a goodly size, of rapid growth when

well fed, and, it may be inferred, likely to flourish in many of our rivers, the temperature of which differs but little from that of its native streams. On the advantages of pisciculture we need hardly insist, they are so obvious—as, the rendering of rivers and lakes now running to waste sources of profit at a trifling cost; the multiplication and cheapening of nutritive and agreeable articles of food; and the promotion of a delightful recreation and wholesome exercise, which we hold angling to be—a recreation, an exercise which appears to advantage in many respects in comparison with field-sports, such as hunting and shooting; not, like them, entailing a loss, but eventually securing a gain: the deer-forest and the game-preserve, it must be confessed, being almost identical with a desert, and too often made, by great sacrifices; the preserve by throwing good land out of cultivation, with diminution and waste of agricultural produce ill borne in a densely-peopled country; the other, the deer-forest, too often made by the expulsion of the cotter and small farmer, so extensively witnessed of late years in the Scottish Highlands—the proprietors in these their acts regardless of Goldsmith's patriotic lines—lines which ought especially to be remembered now that a "*bold peasantry*" is becoming more and more our "country's strength," though unfortunately, in the mercantile spirit of the age, it has ceased to be "its pride."* Further, there are circumstances belonging to pisciculture of a remarkable kind, especially in the economy of Nature. We allude to the digestive and assimilating power of fish, and the smallness of their excretions and loss thereby, so that the greater part of the food they take passes into their substance, occasioning a rapid growth, little loss being sustained either of carbon in respiration to preserve their low temperature, or of azote (the lungs and kidneys having an adjusted action) in a scanty urinary secretion. We may refer those of our readers interested in the subject to a paper by Dr. John Davy, On the Urine of Fishes, published in the 'Transactions of the Royal Society of Edinburgh for 1857,' in which facts will be found bearing out the truth of our remarks. Moreover, as regards the fish we have taken as an example, the salmon, it should be kept in mind that its growth, and the growth of its migratory congeners, is chiefly effected in the sea, where, having abundance of food, its augmentation in size is wonderfully rapid; a young salmon in the course of six weeks returning to its native stream increased in weight from two or three ounces to five or six pounds. These fish may be likened to merchant vessels leaving our estuaries empty and returning heavily laden with rich cargoes from foreign parts. The economy of Nature we have alluded to is strongly shown, both in their manner of growth and in their scant urinary secretion connected with it, especially when compared with birds. The bird of high temperature consumes much carbon in respiration, excretes much

* The Roman Campagna is a somewhat analogous instance, being like so much of the Highlands made in great part a sheep-walk, giving rise to the lamentations of historians, such as Niebuhr and Sismondi, who looked back on Rome in its greatness, and the origin of that greatness. "Land," says the former, "which now supports thirty canons, cultivated as anciently by the hardy and frugal Romans, who with their virtues laid the foundation of the empire, would maintain 2200 families!"

azote in urine, and that in a solid form as lithate of ammonia, which, dropped on the land, helps to fertilize the soil, or, dropped on rocks and barren islands—barren from want of rain—forms by accumulation, in the course of ages, vast deposits of guano, as if, after the manner of coal, providentially intended to meet the wants of an increasing and crowded population on the most favoured and cultivated regions of the earth;—the one, the coal, to supply the place of exhausted forests; the other, the guano, to refresh our exhausted soil. The birds, too, to which we owe this admirable manure are sea-fowl, living in parts of the ocean far from the haunts of man, and unavailable as the food of man. Nor is that portion of their urine which is dropped into the ocean, nor the more scanty urine of fishes, without their use in the water, as they minister to the growth of the various aquatic plants, the prime organisms in their marine habitat, as land plants are acknowledged to be in theirs. In fact, in nature nothing is lost; strictly, there is no waste. Coal and guano are good examples; as the former is the result of the action of the elements of plants on each other at a certain temperature, so the latter may be viewed as a product of the urinary elements of birds under favouring circumstances.

We are glad to see, readverting to pisciculture, that the many appeals which have been made by individuals relative to the preservation of fish, are beginning to draw serious attention, and to excite that agitation without which nothing good, it would appear, can be effected in this country in the way of legislation. A "British Association," we learn, is formed "for the revision of the salmon fishery laws." It has already had a meeting, and put forth a manifesto, with an account of its proceedings.* We wish it all success, and have only to express regret that the attention of the society should be confined to one fish; why should it not be extended to all, and to pisciculture generally?

Other desiderata might be pointed out did our limits permit; but we must conclude. Though there is still so much undone, it is a pleasure to think of how much has been already accomplished; how, taking a large survey, the food of mankind has been increased, multiplied, and varied, especially vegetable nutritive products, and their elements ascertained, to the advancement of humanity and the glorification of science in its practical results, with the promise of further progress and greater benefits. This we say, keeping in mind how much health, how much vigour, bodily and intellectual, depend on the quality of a people's food and its just apportionment. But perhaps persons living at home in the midst of abundance can hardly understand the effects of diet. To appreciate them justly, if not taught by distressing experience, they should consult the narratives of our Arctic voyagers and travellers in Central Africa; the histories of voyages in the olden times; of sieges and campaigns; the accounts of famines; the details of savage life and manners; and then they can hardly fail of being

* The first meeting—a provisional one—was held on June 13th; an account of it was given in the 'Daily News' and other papers of June 15th, 1857.

impressed by the vast importance of the subject, or surprised that divine honours, such as were shown in the worship of Ceres, Bacchus, and Minerva, were paid to the benefactors of mankind for the early gifts of corn, wine, and oil.

Review IV.

1. *Lectures on the Principles and Practice of Physic.* By Thomas Watson, M.D. Fourth Edition.—*London*, 1857. 8vo, vol. i. pp. 871; vol. ii. pp. 984.
2. *Pathologische Physiologie. Grundzüge der gesammten Krankheitslehre im Zusammenhange dargestellt.* Von Dr. G. A. Spiess, Prakt. Aerzte in Frankfurt. Erste und Zweite Abtheilung.—*Frankfurt*, 1857.

Pathological Physiology. Elements of General Pathological Science, Systematically Arranged and Represented. By G. A. Spiess, Physician in Frankfort. First and Second Parts. pp. 709.

3. *A Manual of Medical Diagnosis: being an Analysis of the Signs and Symptoms of Disease.* By A. W. Barclay, M.D. Cantab. and Edin., Assistant-Physician to St. George's Hospital.—*London*, 1857. 12mo, pp. 612.
4. *Handbook of the Science and Practice of Medicine.* By William Aitken, M.D. Edin., late Pathologist attached to the Military Hospitals of the British Troops at Scutari, &c. &c.—*London* and *Glasgow*, 1857. 12mo, pp. 756.
5. *Contributions to the Physiology and Pathology of the Circulation of the Blood.* By George Robinson, M.D., Lecturer on the Practice of Medicine in the Newcastle College of Medicine in connexion with the University of Durham.—*London*, 1857. 12mo, pp. 273.

We propose in this article to direct our reader's attention to some of the recent and more important advances of practical medicine, in connexion with the works whose titles are enumerated above. That which stands first in the list is already familiarly known, and has deservedly taken the very highest rank among treatises on the general science of medicine. Having remained for three years "out of print," the present and fourth edition of Dr. Watson's Lectures is issued with such revision as the accomplished author—who, in extenuation of imperfections, may reasonably be allowed to plead the unceasing demands of a laborious profession—has been able to bestow upon it. The emendations and additions throughout both volumes we shall show to be considerable in number and importance.

The work of Dr. Spiess is, as its title denotes, on a very extensive scale, exhibiting exactly those prominent features of earnest labour and inquiry which we are so much accustomed to esteem and admire in our German brethren. The work is in three parts, two of which have already appeared, the third being speedily promised. The first part treats of the external manifestations of diseases (*Phänomenologie*);

the second, of the conditions and causes of diseases (*Aetiologie*); while the third is to comprehend the consideration of diseases according to appropriate classifications (*Nosologie*). When the work has been published in its entirety, it will merit a fuller notice at our hands than for the present we propose to undertake. Indeed, it will be evident from what has been already stated, that our intention is not now to deal with the subject of general pathological investigation; at the same time, we shall willingly avail ourselves of Dr. Spiess's admirable comments on certain diseased conditions in illustration of the remarks upon the advances in practical medicine to which, in connexion with the other works, we shall immediately proceed. Meanwhile, in order to exhibit the complete and satisfactory manner in which the various important topics included in his general division are discussed by Dr. Spiess, we shall here quote a portion of the table of contents of the second book—that on etiology:

OF THE CONDITIONS AND CAUSES OF DISEASES.

First Section—Of the changes of organic solids and fluids (*mischung*) as conditions of morbid vital action.

Chapter I.—Of the morbid changes in the blood and lymph.

I. Quantitative changes of the whole blood.

1. Morbid increase of the blood—polyhæmia. 2. Morbid decrease of the blood—anæmia.

II. Qualitative changes of the blood:

A. Morbid changes of the individual normal blood-constituents.

1. Changes of the watery portion. 2. Changes of the blood corpuscles; increase, diminution; qualitative changes. 3. Changes of the albuminous constituents (similarly considered). 4. Changes of the fibrinous substance (similarly considered). 5. Changes of saline contents. 6. Changes of fatty contents. 7. Changes of extractive matters. 8. Changes of gaseous contents of blood.

B. Of the foreign admixtures with the blood:

1. Of the retention of the essential bile constituents in the blood; bile dyscrasy; cholæmia. 2. Of the retention of the essential urinary constituents in the blood; urine dyscrasy; uræmia. 3. Of the uric-acid dyscrasy. 4. Of the passage of sugar in the urine; diabetes mellitus; melituria. 5. Of the absorption of pus into the blood; purulent dyscrasy; pyæmia. 6. Of the changes effected in the blood from miasmata, contagion, and other causes of fever.

III. Changes of the lymph.

RETROSPECTIVE VIEW.

Chapter II.—Of the morbid changes of the ligamentous textures.

1. Morbid changes in the textures themselves (hypertrophy, atrophy, loss of elasticity).

2. Morbid deposits in ligamentous textures.

a. Accumulation of blood—hæmorrhage. *b*. Accumulation of serous humour—dropsy. *c*. Gaseous accumulation—pneumatosis. *d* Accumulation of inflammatory products (changes of these); purulent and fœtid formations; tubercular deposit. *e*. Morbid growths—pseudoplasma.

Chapter III.—Of the morbid changes of the muscular structure.

Chapter IV.—Of the morbid changes of the nervous structure.

CHAPTER V.—Of the morbid changes of the vascular structure.
1. Of the enlargement of the bloodvessels (arterial, venous, and capillary). 2. Of the narrowing and occlusion of the bloodvessels. 3. Of the narrowing and occlusion of the lymphatics.
CHAPTER VI.—Of the morbid changes of the osseous structure—hypertrophy and atrophy.
CHAPTER VII.—Of the morbid changes of the teeth.
CHAPTER VIII.—Of the morbid changes of the cartilage.
CHAPTER IX.—Of the morbid changes of the epithelium.

A glance at this division of the subject will show how very thoroughly Dr. Spiess has entered into it, while the perusal of many of the chapters has satisfied us that the medical schools of Germany—and we understand there are several—which have made Dr. Spiess's work the text-book on pathology, have exercised a very prudent and altogether commendable choice.

The third work, that of Dr. Barclay, supplies a want which has been long felt both by students and by hospital physicians. The former, entering for the first time upon the study of diseases at the bedside, must, during a certain period at least, abandon themselves almost entirely to the cultivation of diagnosis; for it of necessity follows that, before the student can skilfully treat a disease, or entertain correct notions regarding its events, he must acquire a discriminating knowledge of the disease itself. It is in aiding clinical students to obtain this information, and so relieving to a certain extent—at all events greatly assisting—the hospital physician, that the work of Dr. Barclay promises to be most extensively useful; and regarding its merits we are glad to be able to express a very favourable opinion. In a few particulars we differ from Dr. W. Barclay, some of these we shall endeavour to signalize. We wish, too, that his work had been shorter than it is; we have no fault to find with the matter of his introductory chapter any more than with that of the two chapters immediately succeeding, the one, On the Method of Diagnosis, the other, On the Duration and Sequence of Phenomena; but we hold them to be alike unnecessary. In a work adapted for the clinical student, as Dr. Barclay's undoubtedly in an eminent degree is, we consider that the smaller and shorter—consistent with a due explanation of the subject discussed—it is, the better; and further, that the sooner the author enters *in medias res* the better. Chapter IV. should, in our opinion commence the work, and it will stand very well as Chapter I.

Dr. Aitken's work forms a part of the 'Encyclopædia Metropolitana,' and is intended to present a condensed view of the science and practice of medicine. It has been the author's object "to incorporate and connect the more recently established facts which illustrate the *Nature of Diseases* and their *Treatment*, with the time-honoured doctrines on which the science of medicine has been based." The work opens with an introduction extending to above a hundred pages, and treating, in a style at once easy and comprehensive, of the more important elements of general pathology; while the principles are shortly stated upon which the modern systems of nosology have been founded. The body of Dr. Aitken's work is divided into three parts. The *first* part,

embracing a few pages merely, is occupied by a brief account of nosology, or the classification of diseases. In it a tabular view of the classes and orders of disease, in the proposed nosology of Dr. Farr, is given. This classification Dr. Aitken employs in the succeeding division of his work, and he recommends its general adoption. The *second* part, extending from p. 9 to p. 726, inclusive, treats of the Nature of Diseases, Special Pathology, and Therapeutics. The general observations upon the work which we propose to make in this article, will relate to the second part exclusively. The *third* part, or last fifteen pages, under the head of Medical Geography, includes some interesting general observations on the geographical distribution of health and disease. This is a subject undoubtedly too much neglected, and we give Dr. Aitken considerable credit for having devoted some space to its consideration.* This part of his subject the author has further illustrated, by reproducing, in a greatly diminished but most satisfactory form, the admirable map of Mr. Keith Johnston.

The last work in the list does not embrace so large a field as any of the others, but it contains many suggestive and highly valuable remarks upon a class of the most important diseases—those affecting the circulation; and will not inaptly, therefore, be noticed in our short discussion of some of these.

"Among the questions examined" (writes Dr. Robinson, in his introductory remarks) "are the mechanism of absorption and effusion, the existence of an extra-vascular circulation, the nature and principles of treatment of inflammation and the allied disorders of the circulation, and the pathology of albuminuria, calcareous degeneration, epilepsy, apoplexy, and some forms of nervous disorder. The following is the order in which they have been investigated:—1. On the mode of production of albuminuria in Bright's disease of the kidney. 2. Researches into the connexion existing between an unnatural degree of compression of the blood contained in the renal vessels, and the presence of certain abnormal matters in the urine. 3. On the mechanism of absorption. 4. On certain points in the mechanism and physiology of the circulation of the blood. 5. On the nature and principles of treatment of inflammation, and the allied disorders of the circulation of the blood. 6. On the pathological changes occurring in certain devitalized tissues. 7. On the peculiarities of the cerebral circulation, and their connexion with the pathology of epilepsy and apoplexy. 8. On the influence exercised in health and disease upon the sensorial functions of the cerebro-spinal nerves, by the state of the circulation in the adjacent bloodvessels."

We can cordially recommend this volume to the notice of our readers, as the production in an especial manner of a very thoughtful and enlightened physician.

We do not intend to follow any definite order or arrangement in our consideration of these works, a brief statement of the purport and contents of which has now been given; but in directing attention to

* Dr. Aitken ought, however, perhaps to have known that the subject alluded to in the text is not "wholly untaught at our medical schools in this country." In Edinburgh, Dr. Pinkerton has lectured on Climatology, including the distribution of diseases, and an interesting Introductory Lecture on the subject from his pen has been published. Dr. Spiess, in the conclusion of the first chapter of the second section, and book second, has, under the title of 'Die Witterung and Das Klima,' some very interesting general observations on the same topic.

the recent progress of medical science, in regard to special diseases, we shall endeavour to notice the light reflected on them in the pages of all our authors, in as comprehensive and clear a manner as possible.

In Dr. Aitken's work, a very excellent account is given of the different forms of continued fever which are now recognised. He treats of typhus fever under the names—1. Typhus fever, or simply typhus; and, 2. Typhoid fever, "or as it has been proposed to be called by Dr. Wood, enteric fever." We agree with Dr. Aitken in considering the latter a preferable term for the disease, and probably the best that can be employed. Dr. Wood, however, has not the merit of having proposed the term, enteric fever; it was first named so by Dr. Christison, and this eminent authority on fever has always continued to use it. In referring to the literature of fever, Dr. Aitken (p. 132) confounds the present title of our Review with that of its distinguished predecessor, the 'British and Foreign Medical Review,' in the volumes of which, for 1841, there are the able articles on fever to which Dr. Aitken is evidently alluding. As 'The Science and Practice of Medicine' is precisely the kind of work for consultation, when search for authorities is made and references to periodical literature are required; and as its author has himself dealt largely, and generally most judiciously, in these consultations, we feel sure he will thank us for indicating such errors as those we have now adverted to. Before passing from this subject we may point out two others. It is not Walsh (p. 167), but Welch, the author of the work 'On the Efficacy of Bloodletting in Fever,' who wrote on the form of fever now styled relapsing, but which is identical with the synocha of Cullen, and the causus of older authors. While the Dr. Christian referred to is evidently Dr. Christison, who did not write in 1817–18 (though he *observed* then), as Dr. Wood, in his far from accurate bibliographical notice of this subject,* appears to intimate; but many years subsequently, in Dr. Tweedie's 'Library of Medicine,' has shown the probable identity of the fever he had carefully studied in the former years, and in 1826, with the fever described previously by Welch.

Again (p. 168), Dr. Aitken, writing of the same fever, states—"Still more recently it has been described by Dr. Mackellar, in 1847, who unfortunately fell a victim to the typhus epidemic at that time." The latter part of this statement is quite correct, but we can assure Dr. Aitken that Dr. Mackellar did not write a single word upon the subject of fever, though favourably known as the author of an interesting paper On Carbonaceous Infiltration of the Lungs of Coal Miners. Dr. Watson's account of fever, and more especially of the enteric fever, is considerably extended in the present edition, while he makes the important admission of his subscription to the views of "Dr. Jenner of our time," as he felicitously designates the able Professor of University College. "I told you," says Dr. Watson, in the commencement of the eighty-fourth Lecture, "that I had been converted from my former belief in the unity of species of continued fever, by the researches of Dr. William Jenner, who has demonstrated the separate

* Lectures, vol. i. p. 361.

identity of at least three species—namely, typhus fever, typhoid fever, and relapsing fever" (vol. ii. p. 800). We are glad to observe, that in a note to the previous Lecture (p. 795), Dr. Watson bears a willing testimony to the fact of Dr. Alexander P. Stewart, of the Middlesex Hospital, having, so early as the spring of 1840, in a paper read to the Parisian Medical Society,* clearly set forth the main distinctions between typhus and the enteric fever.

Dr. Barclay, like not a few distinguished physicians, is evidently not prepared to go the same length as Dr. Watson. We cannot, however, doubt that his description of fevers would have been more intelligible to the student if he had furnished an account of those symptoms which are considered to be characteristic of enteric fever on the one hand, and of typhus on the other; instead of pleading in an apologetic manner, that "in a treatise on diagnosis it is obviously impossible to discuss disputed points in semeiology, and all that can be done in this place is to point out the different characters the spots are liable to present." (p. 55.) We further think that a *résumé*—it need not have been otherwise than brief—of Dr. Jenner's observations would have been more valuable—followed by a caution from Dr. Barclay regarding a too exclusive acceptance of them, if he deemed such required—than the following sentence, where again referring to spots, he observes, "But the student must work out for himself the question whether there be any specific virus that produces one appearance or the other; as, in fact, their cause is yet quite undetermined." (p. 56.)

We have thus very briefly alluded to the subject of fever, in connexion more especially with that particular point which has of late years peculiarly occupied the attention of physicians. Nothing could be clearer, and according to our own view nothing more satisfactory, than the statements of Dr. Watson upon this topic; but as in the pages of this Review the matter has very recently been frequently discussed, we need not now enlarge upon it. In noticing, even in a manner the most cursory, some of the recent advances in practical medicine, it were unpardonable not to allude, even were an entire adherence to their correctness kept back, to the patient and sagacious labours of Dr. William Jenner.†

Fevers form the subject of Dr. Barclay's fourth chapter; the fifth is devoted to Rheumatism and Gout. The particular symptoms of the former disease, and the means of forming a correct diagnosis, are, with one exception, sufficiently clearly stated; there is no reference to the characters usually presented, nor to the changes undergone by the urinary secretion. Yet the careful examination of the urine is most important, and at all times required. Dr. Aitken in his brief definition of rheumatism very properly alludes to the condition of this secretion; and Dr. Watson, in his usual perspicuous and comprehensive manner, when detailing the chief symptoms of acute rheu-

* Afterwards published in the Edinburgh Medical and Surgical Journal for 1840.

† Care must be taken that, in the present discussion of this subject, the important observations of Dr. Peacock, as well as of other physicians, are not lost sight of.

matism (vol. ii. p. 739), refers to the "turbid and acid urine." Dr. Barclay notices the acid odour of the perspiration; the acidity of the urine surely deserves as much attention. Notwithstanding the remarkable ability with which of late years the disease now under consideration has been studied, we are still forced to acknowledge that our notions in regard to it are vague and unsatisfactory. We have, it is true, by clinical observation and by chemical research, arrived at the knowledge of many most important and valuable truths, but we are still ignorant of the real essence of rheumatism. Before we shall succeed in obtaining correct views upon this all-important point, it appears necessary that a more precise acquaintance should be formed with the different varieties of rheumatism, as varieties; there must be a juster discrimination of these. When this shall have been effected, we may then perhaps be able—and all must assent to the extreme importance of such advance in our knowledge—to determine the cases of rheumatism in which the cardiac affections, for example, are most apt to supervene, and those in which the arthritic changes will most probably prove serious. As yet any opinion formed on these points is merely conjectural. It really appears as if, under the title rheumatism, there were included several diseases, resembling one another in some particulars, but widely different in others. The physician who shall succeed in unravelling this complex subject will deserve well of his profession. The variable degrees of success which attend the different modes of treatment of the so-called acute rheumatism, appear to attest the truth of our former observation. This is especially the case in regard to colchicum, still a favourite remedy with many; its preparations have sometimes, to use Dr. Watson's words, "an almost magical effect in subduing the disease;" while at others, without the occurrence of any of those specific effects of this drug, and which indicate the propriety of its being discontinued, no good results, the course of the disease is unaltered—the secret essence of it is evidently unreached. Dr. Watson very probably has assigned the correct reason—practically it does appear to be so—when he observes:—"I believe that, in proportion as the synovial symptoms predominate or mix themselves distinctly with the fibrous—in proportion as the disease approaches in its characters to gout—you may expect to be successful with colchicum." (vol. ii. p. 746.)

We are glad to find that Dr. Watson has assigned a prominent position to the remarks of the late lamented Dr. Golding Bird on the treatment of acute rheumatism by the salts of potash, and more especially by the acetate—his "favourite drug." Dr. Bird's views on blood depuration, and on the existence of a peculiar class of remedies, the renal depurants, are in our opinion eminently sound, and in the highest degree worthy the attention of all physicians. We can confirm, after an extensive trial of the acetate of potash in cases of acute rheumatism, Dr. Bird's observation as to the pain becoming remarkably and suddenly less as soon as the urine becomes alkaline and rises in specific gravity. We regard Dr. Bird's observations as among not the least important of the recent advances of practical medicine; and

we must express our regret that Dr. Aitken, while referring to the interesting experiments with liquor potassæ made by Dr. Parkes, and to Dr. Fuller's important experience of alkaline remedies in the treatment of rheumatism (p. 395), has not had his attention directed to the still more valuable results of Dr. Bird's inquiry.

Gout receives from Dr. Barclay not more than a definition; the pathology of the disease is not touched upon, further than to state that "the researches of recent times have gradually led to the discovery of an important element in gout—the presence of an excess of uric acid in the blood." Dr. Aitken might, with advantage to his readers, have quoted more fully the opinions and observations of Dr. William Budd and Dr. Garrod, and less those of certain authors, who, however respectable, cannot be regarded in the same light as the physicians now mentioned. As the most recent champion of those views in regard to the pathology of gout which are opposed to the theory of the presence of a morbific matter (materies morbi) in the blood, and as the author of an excellent practical treatise on gout, we should have been glad to have found reference to the opinions and experiments of Dr. Gairdner.*

In Dr. Watson's account of gout, the views of Dr. Budd and Dr. Garrod receive that prominence to which they are justly entitled. No recent observations have had a more important influence, as well from the careful nature of the experiments upon which those of the latter were founded, as from the ingenuity of the former's speculations, in upholding the humoral pathology of gout. Dr. Watson is satisfied that this, the ancient doctrine of the origin of gout, is the true one. Gout is very properly described by Dr. Aitken under the head of Constitutional Diseases, and under the first order of that class—namely, Diathetic Diseases—Diathetici. Our only regret is, that Dr. Aitken has rested satisfied with the account of this disease viewed *per se*, and has not offered illustrations of the manner in which many affections—inflammatory and otherwise—are modified by the gouty diathesis. That the treatment of pneumonia and bronchitis, for example, occurring in a gouty subject, frequently require the employment of such remedial means as tend to eliminate the blood poison, as well as of such as are efficaciously used in the more ordinary cases of the inflammatory affections just named, is undoubted. This indeed is borne testimony to by the recent observations of many practical physicians; and it is probably not going too far to say, that were an increased attention bestowed upon such modifying circumstances in particular cases of various diseases, a corresponding improvement in their treatment would be the gratifying result.

Dr. Aitken passes from gout to the consideration of anæmia, of which condition both he and Dr. Barclay offer brief but comprehensive accounts. Neither author notices, however, that peculiar association of phenomena in connexion with anæmia, which, first alluded to by Dr. Parry, was afterwards described by Dr. Graves and Sir Henry

* On Gout: its History, its Causes, and its Cure. By William Gairdner, M.D. London. Third edition. 1854.

Marsh, and more recently by Drs. Macdonnell, Begbie, Stokes, Banks, Bellingham, and Messrs. White Cooper and Taylor, in this country; by Romberg and Henoch in Germany; and Hervieux in France. Dr. Watson thus refers to the affection in question:

"Among anæmic young women, more or less fulness of the fore part of the neck, from slight enlargement of the thyroid gland, is of very frequent occurrence. Sometimes the eyeballs are at the same time unduly prominent."

That this peculiar disorder, represented specially by enlarged thyroid gland, often greatly enlarged and prominent eyeballs, has its dependence on some specific change in the blood, at least analogous to anæmia, was first pointed out by Dr. Begbie, and seems now to be the generally received opinion. The morbid condition just described is almost invariably preceded by such circumstances as are met with in the cases of most marked anæmia, while the sufferers from it, as a general rule, present unmistakeable evidences of blood impoverishment, and except in very rare cases, are benefited by such treatment as is likely to overcome the general disorder. We regret that Dr. Watson has not more fully alluded to the subject; he has only done so, incidentally almost, when treating, in Lecture forty-third (vol. i. p. 810), of bronchocele; cases of the disease are by no means uncommon, while the prevalence of correct views in respect to its nature is still to be desired. Though most frequent among women, just as anæmia itself is, the affection has been observed in men. Perhaps the most correct and complete account of the subject is contained in the twenty-first chapter of the lamented Dr. Bellingham's treatise 'On Diseases of the Heart.'

There is another peculiar condition depending on alteration of the blood, which, as possessing "more of physiological than of practical interest," Dr. Watson merely treats with a passing notice—namely, leukæmia or leucocythemia. Dr. Aitken's account of this constitutional disease is extended and complete. He has very judiciously ascribed the exact amount of credit due to the erudite and distinguished German pathologist, Virchow, and to the able professor at Edinburgh, Dr. Hughes Bennett, in connexion with this subject. The essential and prominent character of this affection, that from which its name is derived, is the increase of the colourless or white cells of the blood. In close connexion with this, however, there are certain other morbid conditions, and two of these of very great importance—namely, enlargement of the spleen, and affection of the lymphatic glands. Dr. Spiess' observations on this disease, under the head of morbid changes in the blood and lymph (page 321), are very instructive. He remarks:—

"According to recent researches, especially by Virchow, it appears that the morbid increase of the colourless blood corpuscles upon which the condition of leukæmia or leucocythæmia chiefly depends, is an immediate consequence of the enlargement of the spleen and lymphatic glands, with which the former is so intimately connected. The morbid increase of the colourless corpuscles may attain so great a degree as to constitute a fourth part or even a half of the whole blood globules."

The various important questions, both of physiological and patho-

logical interest, which suggest themselves in connexion with this remarkable condition of the system, are also alluded to by Dr. Spiess. Among others, whether, in relation to the origin and purpose of the white cells, these represent an earlier development of the normal blood globules. Whether, also, the spleen and lymphatic glands are the organs in which these embryo blood globules—if they be such—are formed. Then, in more direct relation to leucocythæmia, Dr. Spiess inquires whether the morbid enlargements of the spleen and the lymphatic glands are to be regarded as the consequences of an increased formation of the colourless blood globules, or as—and this is the more probable view—due in some way or other to the diseased condition of the newly formed blood globules. It will be evident that these are inquiries of very great importance as well as interest, and while for the present no very satisfactory conclusions can be drawn, still, from the amount of knowledge that has recently been accumulated, it is reasonable to suppose that this is one of the directions in which the science of medicine is likely to make further and early advances. It is not likely that the removal of the spleen, which has been proposed and *practised* in Germany as a means of treatment in this and allied splenic affections, will ever be generally adopted. From the consideration of the affection with which the names of Virchow and Dr. Bennett are intimately connected, Dr. Aitken passes by an easy transition to that of "bronzed skin," in connexion with disease of the supra-renal capsules, associated as closely with the name of another highly distinguished physician, Dr. Addison. Of this most interesting condition in its various relations, Dr. Aitken has afforded a remarkably interesting and instructive summary. Dr. Watson's remarks upon this affection are well entitled to attention, and as they appear for the first time in the present edition of his Lectures, we shall transfer some of them to our pages:

"In former courses of these lectures, I have said nothing, for I knew nothing, of the morbid conditions of the supra-renal capsules. I was ignorant alike of their physiology and of their pathology, of their uses and of their diseases. A pathology, however, they have, which vindicates the importance of these little organs in the bodily economy, although it does not disclose their purpose. The suspension of their function (the necessary result of the destruction or the extensive impairment of their structure) is fatal to life.* And this mortal inward change is revealed before death by one very conspicuous signal —unregarded, indeed, till our own day, and reserved for the sagacity of our distinguished countryman, Dr. Addison, to discern and to interpret. It is very remarkable that two physicians living at the same time, in the same town, and attached to the same hospital, should have brought to light two such serious forms of disease, utterly unknown before, in parts of the body so contiguous, and have thereby won for themselves an enduring place in the records of medical science. Henceforward the names of Bright and of Addison must be held in honourable remembrance whenever mention is made of renal and of supra-renal pathology."

The reader previously familiar with Dr. Watson's elegant and graphic style will recognise it in the sentences quoted. What is

* The experiments of M. Philipeaux and Dr. Harley on animals (rats) show that the supra-renal capsules are not absolutely essential to life.

new in the volumes now for the fourth time issued, is in this respect as remarkable as are the most admired of the well-known passages in the former editions. No medical author of our day approaches Dr. Watson's felicitous manner in rendering honour to whom honour is due.

"It is not uncommon," he continues, "to meet with a sick person whose history is of this kind. He knows not how, nor precisely when, his illness commenced, but he has gradually fallen from his usual state of health; has become weak, pale, thinner, but generally not emaciated, languid, spiritless, unequal to bodily or to mental exertion, with flabby muscles, and a soft pulse, which commonly is very feeble also. For this deteriorated and anæmic condition you can trace no intelligible cause. There has been no exhausting profluvium, no loss of blood, morbid or artificial; no diarrhœa, diuresis, or other drain upon the strength; no wasting excess or indulgence; no mental shock or anxiety. No fault is discoverable in the lungs, in the heart, in the kidneys, in the digestive organs; no direct evidence of malignant disease in any part. Yet the diminution of strength is progressive—faintness and vomiting sometimes supervene—the mind becomes confused, the pulse grows weaker and weaker, until at length the flickering flame of life goes out, or is extinguished in a sudden convulsion. This unexplained train of symptoms should suggest the suspicion of disease in the supra-renal capsules. And the suspicion will be turned into something like certainty if, during the progress of the case, a change of colour begins to be perceptible in the patient's skin, first and chiefly in parts that are uncovered by the dress, as the face and hands, but elsewhere also. The colour which thus takes the place of the natural hue is a brownish yellow. . . . The hue is often partial, and occupies principally the front of the body and of the limbs, especially of the thighs. When it is spread over the whole surface, it varies in intensity, being darkest in the flexures of the body, in the armpits and groins, round the navel, and upon the scrotum. Sometimes it is deeper here and there, and gives a mottled appearance to the surface; and sometimes it is diversified with white patches, in which the skin is blanched, is more white and colourless than healthy skin, like the nails and conjunctivæ; and the hair on the head and on the pubes corresponding to these spots becomes perfectly white." (vol. ii. p. 729-30.)

Such is Dr. Watson's description of a class of phenomena which, first noticed and described by Dr. Addison, has since been found, in numerous instances, by other observers, both in this country and abroad, to be in a remarkable manner related to disease of the supra-renal capsules. The condition of disease in which these bodies have been found has differed considerably; sometimes they have been the seat of cancerous, sometimes of tubercular deposit; in some cases atrophied, in others hypertrophied. "Most frequently of all they have undergone a calcareous degeneration, or have been converted into a softer putty-like substance." The "extent and completeness," rather than the "character," of the change, seems to be the important fact. The careful reader of Dr. Addison's observations will remember that it is to the peculiar constitutional cachexia now described that he especially directed attention. In connexion with it the clinical fact was announced, of the frequent occurrence as a symptom of bronzed skin. Now, and as Dr. Aitken very properly observes, there can be little doubt that, since Dr. Addison's observations appeared, too much attention has been given to this peculiar symptom—at least in the

sense of too little attention having been directed to the investigation of the constitutional state. It is plain, however, that the latter is the direction in which labours must be prosecuted before we shall arrive at a satisfactory knowledge of this peculiar condition. We have not the opportunity at present for entering upon an examination or discussion of the recent observations on this subject which have been made. There are, however, many discrepancies which require reconcilement. Disease of the supra-renal capsules does occur without a trace of the characteristic bronzing. Still more frequently it will, we believe, be found that bronzing occurs without disease of the capsules. Dr. Watson says once only, so far as known to him, has this occurred. The most marked instance of bronzed skin we have seen, in which the trunk of the body anteriorly and posteriorly, the limbs upper and lower, hands and feet, with the neck and face, were variously but symmetrically marked; there existed cancerous disease of the stomach, but the supra-renal bodies were perfectly healthy. As far as our own experience goes, we feel inclined to agree with those who associate the bronzing of the skin with the constitutional malady, and not with the affection of any particular organ.

The following remark of Dr. Aitken is of great significance:

"One very important point is remarkably deficient of illustration—namely, the *early symptoms of this cachexia*, independent of *bronzing of the skin*. From what has been shown relative to the disease, it would appear that when *bronzing* of the skin has been established, a sign of disease has been discovered when it is too late to be of any service, for all the cases appear to terminate fatally in which this state has been unequivocally established." (p. 421.)

Previous to the valuable observations of Dr. Addison, the connexion of a peculiar darkening of the skin with a very marked constitutional affection, had been noticed in the disease, not known in this country, but endemic in certain parts of Italy, "Pellagra." The pathology of this remarkable disease remains most obscure. Certain it is, that in many fatal cases of pellagra, no lesion is found capable of satisfactorily accounting for death. Again, recent observers are unanimous in considering the characteristic dark appearance of the skin, which first appears on the hands and face and then extends over the body, to be nothing more than one of the manifestations of a peculiar constitutional condition under which the victims of pellagra labour. We are not aware of any observations which have been made in regard to the state of the supra-renal capsules in pellagra, but the inquiry will, we trust, be instituted. We ought not to omit mentioning that, by a recent Italian author, Dr. Labus, of Milan,* it has been attempted to prove that there does exist a distinguishing pathological condition in this disease—namely, an atrophy, or extreme attenuation of the ileum (*assottigliamento dell' ileo*), specially characterized by complete absence (*mancanza*) of the *valvulæ conniventes*. In this condition all the coats of the intestine participate—chiefly, however, the mucous and muscular. We regard this condition as of very great interest in connexion with the experience of Dr. Aitken, as recorded in the following passage:

* La Pellagra investigata sopra quasi duccento Cadaveri di Pellagrosi. Milano, 1847.

"During the past nine years, in which I have been in the daily habit of making post-mortem examinations, or of seeing them made by others, I have been familiar with a class of cases which, after a prolonged period of ill-health and gradual loss of strength, at length died, and post-mortem examination failed to detect any obvious lesion in visceral organs. As to the state of the supra-renal capsules, however, I cannot write with certainty. One thing I have constantly noticed in such cases (to which my attention was first directed by Professor Simpson, of Edinburgh), namely, a uniformly wasted anæmic condition of the whole mucous membrane of the alimentary canal from lips to anus. This was obvious in the thin transparent state of the intestines, and an alteration of the specific gravity of the mucous membrane. The mucous membrane of the mouth during life was thin and pale, the submucous glands of the labial and buccal membranes shining prominently through the thin and anæmic tissue, and fine bloodvessels, with dark blood sometimes irregularly ramifying around them, conditions which might be considered as symptomatic of the state of parts in the rest of the alimentary canal. Such observations undoubtedly indicate a constitutional cachexia, whose pathology has still to be worked out." (p. 425.)

In the disease we have been considering, there have been but few microscopical examinations of the blood made; in such, however, there have been found decided increase of the white corpuscles.

"It would be idle," writes Dr. Watson, "to speak to you of any cure for this disorder. A strengthening plan, both of diet and of drugs, would naturally suggest itself. Mr. S—— (a patient) believed that he improved greatly for a time under quinine, and Margate ale. The pallor and the feebleness invite to the use of steel; but the malady, which has perhaps yielded to time, and to the restorative force of nature, has been obedient to no special effort of our art that I am aware of." (vol. ii. p. 733.)

In what remains of this article, we propose to follow Dr. Barclay in his arrangement and treatment of the subjects which occupy his chapters from the twelfth to the thirty-fifth inclusive. In doing so our observations must necessarily now be brief. In his previous chapters, Dr. Barclay has discussed the important class of diseases included under the title of febrile, also rheumatism and gout, diseases of adventitious origin (poisoning and entozoa), diseases of uncertain or variable seat (dropsies and hæmorrhages), chronic blood ailments, depraved constitutional states, and the quasi-nervous diseases, as he calls them, comprehending hysteria, chorea, tetanus, and delirium tremens. And before passing to the description of diseases affecting particular organs, he offers in Chapter XI. some general observations on the examination of regions and organs. Those of the head, chest, and abdomen, are considered in their order. Referring to Chapter X., we cannot congratulate Dr. Barclay upon the description which he has given of "chorea and tetanus," and we doubt the possibility of the student being able to form any accurate notions of these diseases from it. Why are they so inseparably yoked together by Dr. Barclay? They are diseases of the nervous system, differing remarkably in the phenomena by which they are characterized, and affording therefore a good opportunity for contrast, but both are important diseases, and surely deserve a separate, as well as a fuller, consideration than Dr. Barclay has given them.

We cannot agree with this author in considering the element of the former disease (chorea) as "quite unknown to us." At least, in certain cases it is now as clearly demonstrated to depend upon a morbid condition of the blood, as are many diseases the essential element of which, though still obscure, is certainly not "quite unknown" when we have advanced so far as to assign them a humoral pathology.* Dr. Barclay, however, does allude to the association of chorea "with that peculiar condition of the blood that manifests itself in rheumatism." The observations of Dr. Begbie and Dr. Todd, besides many others in this country, and of M. See in France, appear to have conclusively settled the question, first as to the association of rheumatism and chorea, originally pointed out by Drs. Copland and Bright, and secondly, as to the correctness of the view which is expressed by Dr. Watson as follows:

"Acute rheumatism is a blood disease, and it is most likely that the unhealthy blood, circulating through the several organs, is the common source and cause of the articular, the cardiac, and the spinal symptoms, and the bond of connexion between them. This theory explains also the occurrence, which Dr. Begbie has noticed and recorded, of chorea in some and of acute rheumatism in others of the same family."†

While British and French physicians have so frequently noticed and described this association of the two diseases, it is worthy of remark that Romberg, the author of the well-known work on nervous diseases, writing for Germany, says:

"Bright has found that rheumatic affections, articular rheumatism, and rheumatic pericarditis, are frequent complications and causes of the complaint. My own experience and that of other writers does not coincide with this opinion. I have never omitted examining the heart in chorea patients, even when there were no functional disturbances to draw attention to the organ; but with the exception of two cases, in which the complication was accidental, and in one of which it was only observed after the fifth relapse, brought on by a rheumatic attack, I have not found chorea accompanied by disease of the heart."

The relation of chorea to disease of the heart is not brought fully out by either Dr. Barclay or Dr. Aitken. No doubt further observation is required, and the subject being one of great interest and importance, will certainly receive it. It is already evident, however, that the cardiac affections apt to supervene in chorea are different in kind as well as in degree. They are, we believe, three in number—firstly, rheumatic endocarditis or pericarditis, revealed by the ordinary general and physical signs of these affections, and resulting in organic change;

* "Surely this view of the pathology of chorea receives a strong amount of probability from the circumstance that, after death in uncomplicated cases, the most experienced observers have failed to detect any lesions by which the occurrence of the symptoms could be explained." (See Dr. Aitken's work, p. 527.)

† Dr. Todd strongly upholds the rheumatic alliance of chorea. After describing the characters of the urine—its high specific gravity, the presence of lithates, and often of lithic acid, also of oxalate of lime—this eminent physician observes: "You cannot fail to notice that the general character of the urine in chorea bears a marked resemblance to that in rheumatism. This and other circumstances have led me to associate the state of constitution in which chorea is apt to occur with the rheumatic diathesis."—Clinical Lectures: On Affections of the Nervous System, p. 441.

secondly, functional derangement due to an impoverished condition of the blood, characterized by the ordinary symptoms of anæmia, and by inorganic cardiac murmurs (Romberg); lastly, choreic affection of the heart, disappearing as the general chorea disappears, evidenced by the existence of a systolic murmur at the left apex, "which," to use the words of Dr. Walshe in his admirable and complete treatise 'On Diseases of the Heart and Lungs' (p. 244), "cannot be referred to inflammation or organic change of the mitral valve, which has not the usual accompaniments of a hæmic murmur, but which does seem plausibly ascribable to disordered action of the muscular apparatus connected with the valve." We are satisfied that in our own experience we have seen in different cases of chorea each of these three distinct forms of cardiac affection.

The account given by Dr. Barclay of diseases of the brain and spinal cord is full and complete. In treating of apoplexy, he refers to the cerebral lesion arising from the arrestment of fibrinous vegetations in the vessels, to which the attention of the profession has been specially directed by Dr. Kirkes:

"To another condition attention has been drawn of late years—viz., the washing down in the current of the blood of some vegetation which has been gradually growing on the valves of the heart. This is suddenly arrested in some of the smaller arteries of the brain, stopping the supply of blood to the parts beyond, and interfering with their nutrition. In consequence of such an accident, paralysis may either supervene rapidly when deficient supply is sufficient to produce it, or may come on gradually when imperfect nutrition has led to disorganization of part of the brain-structure. In either case the mental phenomena of unconsciousness, &c., are generally wanting; and this may serve, along with the physical evidence of valvular lesion, to lead to a pretty correct guess at its cause." (p. 219.)

This interesting subject is shortly considered by Dr. Watson in his twenty-ninth lecture. The readers of Dr. Watson's lectures must be familiar with his views in regard to the relations between cerebral and cardiac disease. Ably, and as we think convincingly, he has shown that the hitherto received doctrine, of apoplexy being closely connected with, and in a great many cases consequent upon, hypertrophy of the left ventricle, is erroneous; and that in disease of the right side of the heart, obstructing "the ready and regular descent of the blood from the head through the veins," together with fatty or calcareous deposit in the cerebral vessels, as well as in the arterial system generally, are we to look for the bond of union. Dr. Aitken will permit us to remark that, in overlooking any allusion to these views of Dr. Watson, which are now very generally accepted, he has omitted the enunciation of one of the most important of the more recently established facts in disease. Dr. Aitken speaks of the heart having acquired "an abnormal power, driving the blood with great violence towards the brain." He has neglected to mention that the hypertrophy of the left ventricle here alluded to is, in the great majority of instances, a compensatory lesion, to obviate the mechanical obstruction which exists to the circulation from aortic disease:

"It is in consequence of these mechanical obstacles to the free exit of the blood from the left ventricle, that the walls of that chamber, urged to more vigorous contraction, become thicker and more powerful. . . . . The strength of the left ventricle, therefore, in such cases, is not a true measure of the force with which the blood is driven into the distant arteries. Quite the contrary. It is a measure of the *difficulty* with which the blood is circulated through the *primary branches*, and therefore through the entire system of the arteries."*

We cannot at present do more than quote a few sentences from Dr. Robinson's seventh essay, in regard to the cerebral capillary circulation as explanatory of the occurrence of apoplexy. A physiological law, regulating the action of the nervous structures generally, upon which this accomplished author strongly insists, and to which frequent references are made in different parts of the volume now before us, is

"That the incessant motion of the blood through the capillaries of the brain is essential to the performance of its functions. Whenever, therefore, from any cause, external or internal, that capillary circulation is arrested, the functions of the brain immediately cease. If, then, it can be shown that, during an augmented flow of blood to, or its impeded return from, the brain, the physical conditions under which that organ is placed tend to arrest and obstruct the free passage of the blood through its capillaries, an explanation will, I conceive, be afforded of that sudden suspension of the cerebral functions which forms the most striking phenomenon in epileptic and apoplectic seizures." (p. 264.)

We must refer the reader to the work itself for proof of the able and earnest manner in which this proposition is supported by Dr. Robinson. Chapters seventeen to twenty-two, inclusive, of Dr. Barclay's work are devoted to diseases of the chest, lungs, and heart. The remarks on the so-called physical diagnosis of this important division of diseases are very well put, and their perusal cannot fail to be of use to the clinical student. Dr. Aitken's account of the different forms of thoracic diseases is also very full. We must however limit ourselves to noticing a few points of importance merely; selecting firstly, Pulmonary Emphysema, and this because an increased degree of attention has of late been directed to the subject of its production or determining causes, and because Dr. Watson enters fully upon the consideration of the views of Dr. William Gairdner, which have recently been brought before the notice of the profession. We cannot do better than quote some of Dr. Watson's interesting remarks. After having described the morbid state of emphysema, the appearance and other characters of the affected portions of the lungs, Dr. Watson proceeds:

"I must confess that, on previous occasions, I have given you what I now know to have been an erroneous account of the generation and relations of vesicular emphysema. The permanent dilatation of the pulmonary vesicles is not attributable, as, in common with others, I formerly supposed, to the accumulation, imprisonment, and distending force of air shut up within the vesicles by obstructions in the air-tubes; obstructions in the air-tubes caused an emptying of the portions of lung to which they lead—a collapse, and not a distension, of the vesicles. It is to Dr. William Gairdner that we are indebted

* Watson's Lectures, vol. i. p. 524.

for more correct views of these structural changes so opposite in character, and of their mutual relations. He has shown that, in a vast majority of instances, pulmonary emphysema is a consequence of pulmonary collapse. . . . It is found, too, that the tubes which belong to the emphysematous parts are rarely obstructed. Air blown into them reaches with facility the dilated vesicles. The tubes of the collapsed portion are filled with viscid mucus. . . . Collapse implies a diminution in the *bulk* of the lung, and other modes of condensation (hepatization—tubercular) are not necessarily attended with such diminution. For every portion of lung closed by collapse there must be a proportionate loss of bulk. But since there can be no vacuum, it follows that, as the thorax expands, more than the standard quantity of air must enter those tubes and vesicles which are not involved in the collapse. And if the quantity exceeds a certain amount, dilatation of the vesicles (emphysema) must needs ensue. . . . The old explanation was founded upon the supposed effect of pressure, made during strong expiratory efforts upon air shut up in certain vesicles. But in truth no partial pressure can be thus applied. It is when a portion of the lungs has lost its expansile power, and those organs rise in pursuit of the widening parietes of the chest in inspiration, that some of the permeable portions are apt to be stretched beyond their healthy dimensions by the force of the entering air. Remember, then, that vesicular emphysema is a mechanical and complementary (though by no means a compensatory) effect of the condensation and diminished bulk of other portions of the lungs, and not a substantial affection in itself." (vol. ii. pp. 167–8.)

It must be very gratifying to Dr. Gairdner to find his views of emphysema, founded upon a careful and complete investigation of the whole subject, endorsed with the authority of so able and competent a judge as Dr. Watson. These volumes of Dr. Watson contain no stereotyped lectures, as even our readers will by this time have discovered; but from their perusal we are irresistibly impressed with the belief that, when their author does change his views, and is led to express opinions different from those he formerly entertained on the same subject, it has only been after the most conscientious, as well as the most diligent, study and observation. After such, and such only, we are persuaded, did Dr. Watson write his remarks on fevers already referred to, and his opinion on emphysema now quoted. Other and very high authorities have given in their adherence to the inspiratory theory of emphysema, as "moulded into an exact form" by Dr. Gairdner; among these, Lebert, Rilliet, Barthez, Valleix, and Dr. Sieveking. The opposite view, or the expiratory theory, still retains many among its old adherents, and has recently found a very able advocate in Dr. William Jenner.* We cannot at present discuss Dr. Jenner's most important observations, nor can we allow that they have re-converted us; but we recommend their perusal to our readers. We feel much disposed to adhere to the opinion we formed when our attention was for the first time directed to the subject, that probably emphysema may result from a forced expiratory, as well as from an increased inspiratory, act. Good service has been done by Dr. Jenner in pointing out more clearly what was the real nature of the views entertained by Laennec and others.

Of fatty degeneration of the heart there is a notice in the work

* See his paper in vol. xl. of the 'Medico-Chirurgical Transactions of London.'

of Dr. Barclay. Both he and Dr. Watson describe the presence of the "arcus senilis" as affording a strong presumption that the fatty change visible in the cornea may be making progress in other organs. In regard to the other diagnostic signs of this formidable disease, Dr. Watson observes:

"I say that the evidence of fatty degeneration of the heart can never amount to more than presumptive evidence. But if there be tokens of feebleness of the heart, yet no tokens of any valvular or mechanical flaw—if the patient have attained his 'grand climacteric'—if he have shown of late a tendency to grow somewhat fatter, and somewhat paler also, and sicklier in complexion—and if there be withal a marked arcus senilis—then you may reasonably conjecture that (to use Dr. Begbie's words) 'the great organ of life is yielding, through the progress of time, to those organic changes which mark the decay of its structure, and foretell the not distant cessation of its long-continued functions.'" (vol. ii. p. 272.)

In one of the earliest lectures (the third), there are some interesting remarks, under the head of transformations of the tissues, on fatty degeneration in general. Dr. Spiess's observations on this subject are full and most instructive; we have room only for a sentence or two:

"The fatty degeneration appears almost exclusively in the striped voluntary muscles, but especially also in the heart, and that in an eminent degree. The microscopic examination shows in part free fat globules (*Fett-tropfen*) and fine fat granules, in larger or smaller masses, between the primitive bundles and primitive fibres of the muscle; but these primitive fibres themselves are found more or less occupied with large or small fat granules, and consequently their own peculiar nature is destroyed. The manner and origin of the fatty degeneration of muscles are still involved in obscurity." (pp. 440–1.)

Dr. Spiess, like Mr. Paget, regards fatty degeneration as a kind of atrophy:

"Fatty degeneration," observes Dr. Robinson, in his sixth essay, "offers one of the most interesting examples which pathological science has yet revealed, of the wonderful extent to which obscure and deep-seated morbid changes may result from simple physico-chemical laws, brought into operation by the diminished vitality of the tissues of the living body. For in all essential particulars it corresponds to the change into adipocere of muscle and other animal structures, occasionally noticed when bodies have been for a length of time exposed to certain physical conditions." (p. 249.)

In Chapters XXV. to XXXIII. inclusive, of Dr. Barclay's work, consideration is given to diseases affecting the abdominal organs. All the parts under this head merit commendation, but more especially the full and very clear account of the examination of the urine, as given in Chapter XXX. We do not agree with Dr. Barclay, in considering that "oxalate of lime, as a discovery of late years, received at one time a greater share of attention than it deserved;" on the contrary, from every day's experience, we are led to believe that the views inculcated by Dr. Bird on this subject were correct, and we have a strong feeling that in the long run they will become generally adopted. Chapter XXXIII. is devoted to diseases of the uterus; the last two chapters of the work to diseases of the bones, joints, and muscles, and to those of the skin and cellular tissue. Upon

these we have only one critical remark to offer, and it relates to the first mentioned. At p. 557, we find Dr. Barclay stating, under the special head of Congestion and Ulceration of the Uterus, that "true pathology and useful practice have been neither advanced nor benefited by those who have made them their study, and posterity will regard very differently the inventor of the stethoscope and the speculum." Who invented the speculum? It is a very old story, stretching back to Roman if not to Grecian physic; posterity, we venture to affirm, will never trouble itself by an inquiry into the relative merits of Laennec and Recamier (the modern inventor of the speculum), and for the very good reason, that the greater is not to be compared with the less. Dr. Barclay, in his laudable zeal against the improper and indiscriminate employment of the speculum and other instruments, has, in the sentence we have quoted, and in certain other sentences, though perhaps in these to a less degree, been guilty of indiscretion. We are as opposed as he is to the practices he denounces, but his statements are altogether too wide and too condemning in their character. We hope to find his future editions expurgated from them.

Had our space permitted it, we had intended to bring this article to a close by noticing in a more extended manner some of the interesting subjects discussed by Dr. Robinson; for this purpose, however, we must wait for another suitable occasion.

Review V.

The Cause of the Coagulation of the Blood; being the Astley Cooper Prize Essay for 1856, *with Additional Observations and Experiments; and with an Appendix showing the bearings of the subject on Practical Medicine and Pathology.* By Benjamin Ward Richardson, M.D., Physician to the Royal Infirmary for Diseases of the Chest, Lecturer on Physiology and on Hygienic Medicine at the Grosvenor-place School of Medicine, &c. &c.—*London*, 1858. 8vo, pp. 466. With Three Coloured Lithographs and numerous Wood-Engravings.

Among the admirable memoirs which have been honoured by the award of the Astley Cooper Prize,—and we doubt whether the offer of any Prize has ever given the incitement to a more valuable series of investigations,—a very high place must certainly be assigned to Dr. Richardson's Essay, looking simply to the amount and excellence of the work which it represents. But when we take into account that this work has had far more than an ordinary measure of success, having issued in a discovery of a most important character, about which it does not seem to us that any reasonable doubt can be entertained among those who carefully examine the evidence (the good faith of the author's statements as to facts being of course presupposed), we may congratulate Dr. Richardson on having produced that one among the Essays yet rewarded by the Astley Cooper Prize, which will be

longest referred to as making a definite advance in Physiology, and therefore as possessing the highest scientific merit.

In saying this, however, we would not be understood as endorsing the affirmation of the author, that he has discovered "the Cause of the Coagulation of the Blood." That cause, as we shall show in the sequel, is far more recondite than he seems to suppose. But we believe him to have succeeded in bringing to light an immediate and necessary antecedent of this phenomenon, which was previously unsuspected; and to have made clear the physical and chemical conditions under which occurs that spontaneous passage of fibrin from the liquid to the solid condition, which, as our readers well know, constitutes the essential feature in the coagulation of the blood.

In presenting our readers with a critical summary of Dr. Richardson's inquiries, we think it better to follow pretty closely the path which he has himself marked out; passing by, however, for obvious reasons, the *résumé* of the inquiries and opinions of others which he gives at the commencement of his volume. Every point of importance which had been clearly established by Dr. Richardson's predecessors, will present itself in its proper place in our account of his researches. Several of them had been led to the idea that the immediate condition of the coagulation of blood consists in the evolution of some volatile principle; in support of which idea there may be urged a variety of considerations well known to physiologists, the chief of which are the following:—

It is a well-established fact that coagulation is retarded by a reduction of temperature, and that it may be altogether prevented for a time (as John Hunter, we believe, was the first to show) by freezing the blood, which yet coagulates when thawed and exposed to an atmosphere of ordinary warmth. Conversely, the process is quickened by an elevation of temperature, above the natural standard, towards the point at which albumen coagulates. And thus if, of two samples of blood taken at the same time from the same animal, one be warmed and the other cooled, there will be a marked difference in their periods of coagulation, the interval depending upon the amount of difference of their respective temperatures.

Again, it has been conclusively shown that the more freely blood is exposed to the air, the more rapidly does it coagulate; so that the clot is formed more speedily when the blood is drawn into a wide shallow vessel, than when it is received into a narrow deep one. Coagulation is also quickened by diminution of the pressure of the air, taking place most rapidly in a vacuum. Conversely, coagulation is retarded, sometimes for a lengthened period, by exclusion of the blood from the air, whether in the blood-vessels of the animal body, or in an inorganic receptacle; and, as Dr. Richardson has shown for the first time, the retardation may be sustained *ad libitum* by adding pressure to exclusion. Thus, having laid bare the external jugular of a cat, and placed an inch of it between two ligatures, he removed the ligatured part, and immersed it in mercury beneath the pressure of a column about thirty inches high. After having been left there for

twenty hours, the piece of vein was withdrawn, placed in a spoon, and punctured; its contained blood flowed out entirely fluid, and on its exposure to the air coagulated firmly in four minutes. Some blood of this same animal, derived from the jugular vein at the time of operation, coagulated on exposure to the air in two minutes and a half.

Further, coagulation is materially retarded by the admixture of blood with fluids at or above its own density, and also by addition of water in excess; the addition of water to blood, however, in the proportion of equal parts of each liquid, does not materially influence the period of coagulation, unless the temperature of the water added be raised to that of the blood, in which case the coagulation is accelerated.

Lastly, agitation of blood that is freely exposed to the air, has a very decided influence in quickening coagulation. Yet motion *per se* has no such influence; for if the blood be shut up in a closed vessel, motion rather retards than accelerates its coagulation. Dr. Richardson has devised an ingenious apparatus for imitating the natural circulation in some degree, by keeping the blood in movement within a circuit of tubes composed of inorganic materials, and closed as completely as possible; and he has found that blood may be kept in motion in such a circuit for a brief period without coagulating, but that ultimately the fibrin is deposited on the expanded parts of the circuit.

The foregoing conditions, as our author justly observes, are all in perfect accord with the hypothesis that coagulation is immediately dependent on the escape of a volatile agent from the blood; whilst, in the presence of any other view, these phenomena are not merely disjointed, but even appear contradictory. With a view of putting this hypothesis to the test, Dr. Richardson devised the following ingenious experiment. A large Woolf's bottle was connected by bent tubes with two small bottles; the apparatus being so arranged that a stream of air from a small pair of bellows should first pass through an ounce of blood in one of the small bottles, then through two pounds of blood in the large bottle, and lastly through another ounce of blood in the second small bottle. The three bottles having been charged with blood obtained at one gush from the throat of an ox, the difference in the rate of coagulation of their contents was very striking. The blood through which air was first passed coagulated in two minutes; that in the large bottle coagulated in three minutes; whilst the blood in the third bottle, which received the vapour from the whole mass of the contents of the second, retained its colour and its full fluidity for eight minutes and a half. In another experiment of the same kind, it was observed that the lower stratum of blood in the third bottle coagulated rapidly and became dark, while all above remained of a bright colour and fluid; and it was found on examination that the tube which conducted the blood-vapour from the large bottle was not pushed down far enough, so that the lower stratum of blood was not subjected to its influence.

That the volatile agent on the escape of which the coagulation of the fibrin immediately depends, is Ammonia, seems to have been suggested

to Dr. Richardson by the well-known power which the fixed alkalies possess of keeping fibrin in solution. On this point he has made a large number of experiments; and he has shown that the discrepancy of the results obtained by others is probably to be attributed to a want of purity in the reagents employed, as in the following striking example:—

"Being anxious to ascertain the effect of nitrate of potass on freshly extracted fibrin, I obtained two specimens of this salt from two different chemical establishments. Both specimens were warranted as carefully prepared. With each of these specimens I made a solution in the proportion of ten grains of the salt to one ounce of distilled water. Into each solution I dropped two grains of moist fibrin, derived from the same blood, and left them standing side by side under the same conditions. In three days, I found that the fibrin in one of the solutions was in a great measure dissolved; in the other it remained unchanged. This difference arose from the fact that the salt which had dissolved the fibrin contained free potassa; the other salt did not." (p. 50.)

Dr. Richardson states, as the result of his experiments on this point, that for holding blood permanently fluid, the proportion of either of the fixed alkalies must not be less than one grain to the fluid ounce, or one part in five hundred and five by weight; and that no normal blood-salt has the power of effecting the solution of fibrin, nor yet of holding blood fluid, except when added in proportions which would be incompatible with life if introduced into the circulation. Hence it becomes obvious that as the fluidity of the blood in the vessels cannot be attributed to the presence of the fixed alkalies, since these are equally present in blood which is undergoing coagulation, if the presence of an alkali has any concern with it, this must be the volatile alkali, ammonia: and under the guidance of this clue, Dr. Richardson has made a large number of experiments, to determine, in the first place, whether ammonia is given off from blood during its coagulation; secondly, whether by the retention of ammonia in the blood its fluidity may be preserved; and thirdly, whether a coagulum of fibrin already formed can be redissolved by the addition of ammonia, to be re-formed when the ammonia is allowed to escape.

That ammonia is evolved from blood on its being withdrawn from the vessels and exposed to the air, has been proved most satisfactorily by Dr. Richardson's experiments, which have been so multiplied and varied as to exclude all sources of fallacy. The test is a very simple one; being the formation of minute crystals of chloride of ammonium on a slip of glass previously moistened with hydrochloric acid and exposed to blood-vapour. For the precautions which must be taken in order that this test may be applied with a satisfactory result, we must refer to Dr. Richardson's own pages. It is worthy of being specially noticed, that the evolution of ammonia does not cease with the act of coagulation, unless the clot be exposed to a temperature at or below 32° Fahr., when all gaseous evolution ceases, and also the further act of separation into clot and serum. If, on the other hand, a recently formed clot be divided into small pieces, and be placed in a vessel surrounded with water at 100° Fahr., there is a rapid separation of serum and a continued evolution of vapour; the subsequent contrac-

tion of the clot being but an extension of the first act of coagulation. Thus, then, the fact of the evolution of ammonia from freshly-drawn blood is incontestably established; but the quantity evolved is extremely minute; and this phenomenon, like the evolution of carbonic acid, might reasonably be regarded as a mere coincidence, if its special relation to the act of coagulation could not be proved in any other way.

It appears, however, from Dr. Richardson's next series of experiments, that the introduction of a small quantity of ammonia into blood whilst yet liquid, has the effect of retarding its coagulation for a period proportional to the amount; and that the like effect is produced by the transmission of air charged with ammonia-vapour through blood. Thus, when four one-thousand-grain bottles, into which respectively one grain, one-half grain, one-third grain, and one-fourth grain of ammonia had been previously introduced, were filled with blood drawn from the same animal, the blood in the first was fluid three hours afterwards, that in the second coagulated feebly at the end of thirty-five minutes, that in the third remained fluid for twelve minutes and then coagulated, whilst that in the fourth coagulated in three minutes; another sample of the same blood coagulated firmly in one minute on simple exposure to air at sixty degrees Fahr. By means of an apparatus specially contrived for the purpose, Dr. Richardson drove air charged with three grains of ammonia-vapour through five hundred grains of blood; the blood remained fluid during the twelve minutes that this process lasted. Half the blood being then set aside in an evaporating dish at rest, half an hour was required for its coagulation; through the other half a current of air was driven, and it coagulated in three minutes. The blood of the animal used in this experiment coagulated naturally in four minutes at a temperature of fifty degrees. The solvent effect of ammonia upon fibrin had been observed by Dr. Richardson some years previously, in the course of some inquiries which he made with a view to determine the most likely means of preventing the formation of fibrinous concretions in the heart during life. Ten-grain masses of moist fibrin being placed in solutions of ammonia of the respective strengths of five grains, two and a-half grains, and two grains, to one thousand grains of water, their solution was accomplished in fifteen, seventeen, and twenty-one days respectively. In another experiment, a similar quantity of fibrin was divided into small pieces, which were placed in a mixture of twenty minims of strong liquor ammoniæ with two ounces of water: the fibrin soon began to gelatinize, and in five days had entirely disappeared, forming a sticky solution, which evolved ammonia freely. This solution was kept by Dr. Richardson nearly four years, closed up in its bottle, during which time it seemed to undergo no change. It does not seem to have occurred to him, however, to try whether, on freely exposing it to the air so as to allow the ammonia to escape, its redissolved fibrin would again coagulate.

That after the solvent action of ammonia has been exerted on the coagulum of blood, so that fluidity has been completely restored, a re-coagulation will take place when the escape of ammonia is permitted,

is the result of another series of Dr. Richardson's experiments; the conditions of which were so varied as to justify his statement that "in blood held temporarily fluid by excess of ammonia, coagulation is favoured or retarded by the same physical agents as those which favour the normal coagulation of blood."

The conclusion drawn by Dr. Richardson from the series of facts of which the foregoing are samples,—namely, that the fibrin of the circulating blood is held in solution by ammonia, and that on the escape of this volatile solvent from newly-drawn and liberated blood the passage of the fibrin from the fluid to the solid condition immediately depends,—seems to us to be entitled to take rank as a well-supported theory, if not to be at once admitted as a demonstrated doctrine. We are not inclined to attach much weight to the objection which we have heard raised against it, that the liberation of ammonia from the whole mass of coagulating blood could scarcely take place in so brief a period; for it is well known that ammonia may be transmitted through aqueous liquids with extraordinary rapidity; and (as already shown) although the escape of ammonia commences almost immediately that the blood is drawn, it really continues for some time afterwards. The objection seems early to have occurred to Dr. Richardson himself; and he remarks that his doubt was dispelled by the constancy of the relation between the rapidity of the evolution of ammonia, and the rapidity of coagulation. Thus in sheep's blood which coagulates rapidly, the indication of its liberation afforded by the formation of crystals is as marked in one minute, as it is in the case of the blood of the ox, which coagulates slowly, after three or four minutes. And the chemical condition of rapid evolution seems to be supplied by the fact, that the blood is rendered feebly alkaline by the presence of a quantity of fixed alkali or alkaline carbonate, which is not sufficient to hold the fibrin in solution, but serves to expel the ammonia (especially with the aid of gentle warmth) when the blood is exposed to air or to a vacuum. Dr. Richardson has endeavoured to ascertain by experiment the proportion of ammonia which will be required to maintain the fluidity of circulating blood; and he has come to the approximate conclusion that the maximum would be one part of the alkali to sixteen of fibrin, or one part of the alkali to eight thousand of blood containing two per cent. of fibrin. The proportion, he remarks, may be less, but need not be more. Considering how very imperfectly the natural condition of the circulating blood can be imitated by any artificial arrangements, we are inclined to think that the actual proportion of ammonia in the blood of the living body is considerably below this estimate.

It is an important corroborative fact, as indicating the normal presence of free ammonia in the circulating blood, that the volatile alkali is ordinarily given off in the act of expiration. This was first noticed about fifteen years ago, by the Rev. J. B. Reade; who detected microscopic crystals of chloride of ammonium on a slip of glass, previously moistened with hydrochloric acid, which had been exposed to the expiratory blast. MM. Viale and Latini, in 1854, arrived at the same conclusion, by a series of independent labours; and Dr. Reuling

published a paper in the same year, in which he affirms that ammonia is contained in the expired breath of every one, though ordinarily to no greater amount than in the atmosphere, but that it is in large excess in the breath of patients suffering under typhus, pyæmia, and uræmia. Dr. Richardson states that he has made upwards of a thousand experiments on this point, on animals of various kinds, as well as on human beings of both sexes, and under varying conditions as regards temperature of body, time of day, repose and fatigue, health and disease. He has found the test of the formation of microscopic crystals more satisfactory than any other; and he applies it by means of the following simple apparatus:—A slip of glass is secured by india-rubber bands



across the trumpet-shaped orifice (A) of a straight breast pump, into the bulb of which (B) a drop or two of pure hydrochloric acid has been introduced; the subject of the experiment being made to expire through the small end of the tube, the air passes through the acid vapour, and whilst part of the alkali is fixed in the bulb, another portion goes on to be condensed on the slip of glass. Dr. Richardson has found the exhalation of ammonia to be the general fact, though not without exception; and he has proved that the alkali is really thrown off from the body, and not merely returned from the air inspired, by testing the air which was being breathed,—driving it through the same apparatus for a time equal to that consumed in the respiratory acts, without the formation of any crystals of chloride of ammonium. The most marked and constant exception presented itself in the case of a gentleman who lived entirely on vegetable food and abstained from alcoholic drinks; no trace of ammonia being detectable under any circumstances in his breath. But Dr. Richardson states that he sometimes found his own breath free from it on rising in the morning after a sound night's rest, and that the quantity was very much reduced in cold weather; whilst, conversely, a state of fatigue and a high temperature are accompanied by an augmented evolution. In a case of simple anæmia, he could not obtain the slightest evidence of the presence of ammonia; whilst an excess presented itself in a considerable variety of other diseases. It has been found by Mr. Blake that when ammonia is artificially introduced into the blood, it is so rapidly removed by pulmonary exhalation, that a rod dipped in hydrochloric acid gives white fumes when held in the respiratory current a few seconds afterwards.—The evolution of ammonia in the cutaneous exhalation has long been known; and the suddenness with which it sometimes takes place, especially in females, under mental emotion, is an evidence both of the large amount which must be present in the blood, and of the

rapidity with which, under conditions not yet known, it may be set free. Dr. Richardson states that the cutaneous evolution is so marked during and after the colliquative sweats of consumptive patients, that he has found a rod dipped in hydrochloric acid give white fumes when held near the cutaneous surface.

Thus it would seem that while the presence of a certain amount of ammonia in the blood is not only a normal condition, but is absolutely essential to the continuance of its fluidity, there is a provision in the pulmonary and cutaneous exhalation for the removal of any excess imparted to it by an acceleration of the ordinary rate of decomposition, as a consequence either of excessive exertion or of disintegrating disease. In the Appendix to his Essay, Dr. Richardson enters upon various considerations, which seem to connect an excess or deficiency of ammonia in the blood with particular groups of morbid phenomena, and which seem to afford indications for the rational employment of acids and alkalies in the cure of disease. Into these inquiries, however, we shall not at present follow him; since the subject has been hitherto far too little elucidated to admit the establishment of any satisfactory conclusions. Thus for example, in regard to typhoid fever, it is by no means so clear as Dr. Richardson seems to suppose, that the excess of ammonia in the blood is to be regarded as the essential phenomenon of the disease; for to us it seems more likely that it is only a result of that disintegrating change in the fluids and solids of the body which is the consequence of the action of a zymotic poison upon them. Still this portion of the work is eminently suggestive; and we trust that not only by the author himself, but by many others, will observations and experiments be systematically carried on, with the view of accumulating a body of information upon these important questions, on which valid pathological principles and sound rules of practice may be securely founded.

Although we have expressed our readiness to accept the facts which Dr. Richardson appears to have conclusively established, yet we are compelled to record our dissent from some of his inferences. He seems to conceive that in showing the coagulation of fibrin to be immediately dependent upon the liberation of ammonia, he has demonstrated that this coagulation is a purely physical phenomenon; and he does not hesitate to liken it to the coagulation of albumen by heat, to the gelatinization of a solution of gelatin on cooling, and even to the formation of a film of collodion by the evaporation of its ethereal solvent. Now to us it appears that the coagulation of fibrin cannot be rightly compared with that of either of the foregoing substances; inasmuch as the coagulum which they form is homogeneous, whilst the coagulum of fibrin presents an approximation (which becomes very distinct in particular cases) to an organic structure. Dr. Richardson has, moreover, committed that very common error, of imputing causation to the last antecedent alone, instead of looking for it in the sum of all the antecedents. We may, perhaps, best illustrate our meaning by referring to the phenomena of germination. It is well known that the extrication of carbonic acid is so necessary a condition of this process, that, if it be prevented, germination will not take

place; it is further well known that a certain measure of heat is requisite for germination, which may be indefinitely retarded if this heat be withheld. Yet would Dr. Richardson or any other thoughtful physiologist assert, on these accounts, that germination is a purely physical process—ignoring the fact that it essentially consists in the growth of an organized structure, and that only a seed which retains its vitality can exhibit these phenomena? The *cause* of germination surely does not lie simply in the application of warmth to the seed, or in the extrication of carbonic acid; these are necessary conditions of the act, as is also the presence of nutrient material stored up for the use of the germ; but a condition not less necessary is the presence of the germ, endowed with properties, which, as being peculiar to a living being, we distinguish by the epithet "vital."

In like manner, the cause of the coagulation of the blood does not rest alone in the liberation of ammonia; the presence of a substance endowed with the property of passing, in the act of solidification, into a rude form of fibrous tissue, is no less essential a condition; and so far as we at present know, that substance can only be generated by the agency of a living body. Chemists are sanguine of being able, some day or other, to manufacture albumen; and we see no reason why they may not succeed, since albumen displays no properties which can be regarded as putting it beyond the pale of pure chemistry. But until the chemist shall have effected in his laboratory the transformation of Albumen into a substance possessing the distinctive properties of Fibrin, as evidenced not merely in the spontaneous formation of a coagulum on the escape of ammonia, but in the peculiar structure of that coagulum, we must retain our conviction that this transformation can only be effected by the agency of a living organism, that it must therefore be distinguished as "vital," and that the act of coagulation is essentially a "vital" phenomenon, dependent (like every other of the same class) upon certain physical conditions. And it is no valid reply to this doctrine, to point to the length of time during which a solution of fibrin may retain its coagulating power; for the like is true in numerous other cases, in which no physiologist (so far as we are aware) hesitates to admit that vital properties may be retained for an unlimited period. How can we explain the revivification of a dried-up Rotifer on the application of moisture, or the germination of a seed buried for thousands or tens of thousands of years deep in the earth, under exposure to warmth and air, if we do not admit that vital properties may be retained in a dormant or inactive condition for indefinite periods?

There is a great tendency at the present time, amongst those who make a special study of the Physics and Chemistry of living bodies, altogether to ignore all Vital agencies; yet nothing, as it seems to us, can be more false than to attempt to resolve the latter into the former. We are willing to go as far as any one in the prosecution of the inquiry how far Physical and Chemical forces can be held to account for the phenomena presented by living bodies; but we are of opinion that no really philosophic Physiologist will seek to refer the phenomena of Assimilation and Nutrition, Growth and Development, to any other

than Vital agencies acting under determinate physical conditions. As well might we say that Heat is Electricity, or that Light is Magnetism, as affirm that the duplicative subdivision of a cell is simply a physical phenomenon, or that the action of the spermatic fluid on the germ-cell is a mere chemical change. There is nothing more hypothetical in the assumption of Vital force, than in that of Electric or Magnetic force. We know no more of the intimate nature of the latter, than we do of that of the former; each can be studied only in its phenomena; and it is the first object of all inductive inquiry, to determine what are analogous or related phenomena. For the reasons we have specified, and for many more that we might adduce, we still hold to the opinion of John Hunter, that the coagulation of fibrin is essentially a vital act, being dependent on the properties which the fibrin has acquired as a constituent of a living organism; whilst we are ready to accord the highest credit to Dr. Richardson, for the important advance he has made towards the exact determination of the physical conditions under which this change occurs.

REVIEW VI.

Traité de Géographie et de Statistique Médicales et des Maladies Endémiques; comprenant la Météorologie et la Géologie Médicales, les Lois Statistiques de la Population et de la Mortalité, la Distribution Géographique des Maladies, et la Pathologie Comparée des Races Humaines. Par J. CH. M. BOUDIN, Médecin en Chef de l'Hôpital Militaire du Roule, Officier de la Légion d'Honneur.—*Paris*, 1857. Two vols. 8vo, pp. 576 and 764.

Treatise on the Medical Geography and Statistics of Endemic Diseases; comprising Medical Meteorology and Geology, the Statistical Laws regulating Population and Mortality, the Geographical Distribution of Diseases, and the Comparative Pathology of the Races of Man. By J. C. M. BOUDIN, Chief Medical Officer of the Military Hospital du Roule, &c.

THE author of this comprehensive work, who holds a distinguished position among the military surgeons of France, has been long known as an indefatigable and ingenious statistician. His labours in this field have been made public in a succession of memoirs, some of the most interesting of which appeared in the 'Annales d'Hygiène,' of which he is one of the editors.

Since the publication of his 'Essai de Géographie Médicale,' in 1843, M. Boudin appears always to have intended at some future day to resume the subject, and in the interval he has spent much time in amassing materials bearing on the various questions involved. He was thus in a favourable position to accede to a request to write such a work as the present, made by his publisher, M. Baillière, who deemed it a desideratum in professional literature.

Such appears to be the history of the development of the work, for which, therefore, we have alike to thank the industry and talent of its writer, and the enterprise of its publisher. It professes to bring

together all the facts which science now possesses, bearing directly or correlatively on medical geography, and to produce a framework in which future observations may be classed and arranged.

Towards the end of the last, and in the earlier part of the present century, there were several German works on the subject, and at a later date, Van der Hoeven, Fuchs, and Mühry, have been valuable contributors. In our own language, we have many well-known works on medical and vital statistics, and on climate; and our serial literature is rich in disjointed and fragmentary contributions to the study of the geography of disease, the influence of race, and the history of endemic and epidemic ailments. Indeed, it is not without a certain degree of pleasure and pride that we observe, throughout the whole of M. Boudin's work, how frequently he has to acknowledge the labours of Englishmen, and of what value to him have been the statistical reports on the army, the annual reports of the Registrar-General, and the various memoirs published by the Statistical Society of London. Scattered over the face of the earth as are the British possessions—in polar, temperate, and tropical regions, in the north hemisphere and south, including such varied races of men, some in their place of birth, others moved northwards, and others southward from it—considering these things, it is not wonderful that Great Britain should possess the richest sources of information on questions of medical geography. She holds, in fact, within herself, elements for the solution of many of the most interesting points in such researches, and it is gratifying to find, from the prominent place they occupy in M. Boudin's work, that they have not been passed by unnoted by our army, navy, and colonial surgeons. To everything accessible in French, German, and British works, he has added that contained in Italian, Swedish, American (U. S.), Swiss, and Danish literature. The last becomes particularly interesting, as it embodies the recent researches of M. Schleisner, who was charged officially to inquire into the sanitary condition of Iceland. The author has thus really made an effort to accomplish the task of giving a *résumé* of all the facts which science now possesses, and has produced a work which is almost encyclopedic in its character, discussing with more or less fulness everything which he thought could influence results in such researches. It will be no easy matter, therefore, within the scope of a limited review, to present a complete analysis of so comprehensive a work.

The author divides the work into two parts. The first treats of the Physics of the Globe, and of Medical Meteorology; and to this, the whole of the first volume and a few pages of the second are devoted. The other division considers "man in a geographical point of view," and occupies a rather greater space. Much that is in the first division may appear to have no other than a purely scientific interest; and M. Boudin in many cases does not attempt to show the way in which the phenomena discussed affect the laws of health or disease, nor indeed to establish between them any positive connexion. But, where all is so complex and so much is obscure, he appears to have thought it wise to omit nothing which might in any way throw light upon such questions.

His first chapter he devotes to the Solar System; and he is led, from discussing the annual and diurnal movements of the earth, to a consideration of periodical phenomena in man, making out a striking parallelism with this double movement, and adducing numerous illustrations. "Sicut in anno continentur periodi ægritudinum, eodem modo, una die."

According to the statistics of Quetelet, Buck, Ranken, Casper, Guiette, and Virey, which he has drawn together, the maximum of births takes place between midnight and six o'clock in the morning, and the minimum between midday and six P.M.; while, from the statistics of the same author, the minimum of deaths takes place invariably between six P.M. and midnight. The maximum of deaths occurs less definitely at one period. The point of real value—the strong point—in these statistics, is the uniformity and steadiness of the results in the hands of different observers. But the whole number of observations is too small to permit the deduction of any trustworthy inference. The sum of all the observations of the period of the day at which births take place only amounts to 5841, and of deaths only to 5591. The influence of seasons, however, on the same occurrences, rests on a much wider basis. For instance, by combining the statistics of Lastri and Ferrario, he presents a document embracing 1,186,515 births, spread over four hundred years, from 1451 to 1845. The analysis of this shows:

"1st. That the months of April, May, and June—which were, towards the middle of the fifteenth century, the most fruitful, are still in our period the richest in conceptions; 2nd. That since the end of the fifteenth century the month of September has not ceased to be one of the months having the lowest share of conceptions; and 3rd. That the proportion of births of the one sex to the other has never varied during the course of four centuries."

With regard to the first two of these conclusions, he shows that they hold good over long periods for other towns in Italy; the minima, however, more constantly than the maxima. France appeared at first to present an exception to this rule, but by applying certain corrections, which he thinks required and just, M. Boudin elicits from them also substantially the same inference.

The influence of the seasons on the distribution of deaths he holds to be very evident. In France, the maximum of mortality corresponds to the cold period of the year—the spring months being highest of all. The same is true of Great Britain, of Denmark and Schleswig, and of Europe generally, with the exception of such places as are devastated by marsh fever. Iceland, however, appears to be an exception, the maximum there occurring in July and August (1838 to 1844). If it be true, as M. Bunsen affirms, that Iceland is a malarious country, this seeming exception vanishes. There are several conditions which may perturb the influence of season on the rate of mortality, such as race, the special features of reigning diseases, &c. Thus, he tells us that in hot countries the highest mortality among Europeans corresponds to the hot months, while it is not always or usually so with the indigenous population. And he adduces in proof the statistics of Calcutta by Colonel Sykes. Of the other source of

perturbation he draws his illustration from the influence of the plague in London. During its five visits to the metropolis (1593, 1625, 1603, 1636, and 1665), the greatest mortality fell in July, August, and September; and the same occurred during intervening normal years (1606 to 1610), which were remarkable as being free from plague; while now, at a period far removed from its direct or indirect influence, the maximum has become minimum, and *vice versa*.

Still further, on the subject of periodicity, he shows:

"1st. That the admissions of the insane into Charenton, from 1826 to 1832, followed a progression parallel with the monthly increase of temperature, making the admissions in June and July fifty per cent. higher than in January; 2nd. That from 1835 to 1846 the number of suicides in France rose and fell with the rise and fall of the thermometer, so as to acquire in June and July a daily number double of that in January and December."

In the familiar phenomena of menstruation, the rut, hybernation, &c., we have striking instances of periodicity in animals; and there are many reasons for believing that it enters more extensively than we think into the laws of animal life, although it may be in a manner which is obscure. In vegetable life its operations are more general and more manifest, but perhaps not more real or important, than in animal. Every contribution, therefore, to the study of the question is of importance.

The second book treats of Medical Geology. After describing the different kinds of soil and their physical properties under a medical aspect, the author proceeds to show "that man is in more respects than one the mere expression of the soil on which he lives;" and then shortly notices some of the ailments which are more directly connected with soil—such as ague, vesical calculus, goître, cretinism, and cholera.

Medical Hydrology is the title of the third book. After giving a full account of the physical and chemical properties of rain, spring, sea, and river waters, he details the researches of Chatin on the presence of iodine in fresh water, and its connexion with goître, which he seems to consider doubtful. Then follows a notice of the composition and other properties of mineral waters. Marshes, drainage, and water considered as a source of disease, terminate this book, and introduce us to a similar analysis of the atmosphere, to which three books are devoted. This is, in fact, a short treatise on medical meteorology. After an account of the physical and chemical properties of the atmosphere, and the phenomena of respiration, he is led to ask, "Whether carbonic acid possesses in itself a toxic action, or simply a negative one, due to the absence of atmospheric air." It appears that Orfila, Seguin, D'Arcat, and some others, accord to it a positively deleterious action; while Bichat, Nysten, Malgaigne, Berard, and perhaps Regnault, pronounce for the negative. The last observer, he tells us, has kept animals for several hours in air containing seven per cent. of carbonic acid; and he adds, that in order to kill a dog the air must contain thirty to forty per cent. But "it is said," he adds, "that three to four per cent. would suffice *under the influence of combustion*, in consequence of the presence of the oxide of carbon, which, in the

proportion of one per cent., destroys life." The subject of winds he discusses briefly; although he admits that, in a medical point of view, they play an important part, "sometimes in dissipating miasmata, sometimes in serving as a vehicle for them." On the last of these points, he says that science as yet wants well-observed facts. But winds do more than this. They are in a great measure the controllers of temperature and humidity, and the promoters of evaporation; and they possess individual unexplained peculiarities which affect both vegetable and animal life.

On the influence of diminished and increased atmospheric pressure, M. Boudin has brought together the different experiences of those who have ascended to great elevations on mountains, or in balloons, and of those who have subjected themselves to the reverse condition in diving-bells, mines, &c. From these it would appear that man can tolerate both states with less disturbance or injury than is generally supposed. Sudden transitions are felt, and have palpable and important influences; but they pass away or subside, so that at any rate discomfort is not felt. There is, however, in all probability, a permanent influence, the nature of which observation has not yet indicated.

Hygrometric phenomena are also briefly discussed—more briefly, perhaps, than they merit. Although *wet and dry bulbs* are in the hands of almost every observer, there is a poverty of materials on this subject. Whether it is the fault of the instrument, or of the troublesome and unsatisfactory character of the deductions and corrections to be made, this is certain—that there is a tendency not to give this element of meteorological science the consideration it deserves. For, although temperature may be first in importance in its influence on living beings, yet, for evident reasons, the approach to or removal from the point of saturation in the atmosphere must exert great power for good or evil.

On the distribution of temperature at the surface of the earth, a mass of most valuable facts are offered to the reader—concluding with a table giving the mean annual temperature, the mean for the seasons, and that of the coldest and hottest months for 524 localities. Here, as everywhere indeed, the encyclopedic character of the book is shown, which will bring it often into the hand as a work of reference.

The influence of temperature on mortality, and on the proportional number of some diseases, the power of resistance possessed by the organism to excesses of heat and cold, the phenomena of congelation, &c., are briefly discussed in the ninth book, but more fully under various heads throughout the second volume. With reference to congelation, he holds it shown that race and nationality have predisposing or protecting influences; that inaction or sleep are predisposing causes; and that its occurrence "depends less on the intensity than on the quality of the cold." He supports this by the experiences of the French during the retreat from Constantine in 1836, and the Bou-Thaleb expedition in 1843—of which Mr. Shrimpton has written an account, detailing 355 cases. On neither occasion did the thermometer fall much below 32° Fahr.; but M. Boudin is of opinion that the

combination of wet and cold in both of these instances developed the results.

The seventh and eighth books are occupied with botanical and zoological geography. The last contains some valuable remarks on parasitic animals, more particularly in reference to the endemicity of some of them.

"Such are the hydatids of the liver in Iceland, which attack a seventh of the population of that island; the distoma hæmatobium in Egypt, which is perhaps the veritable cause of the endemicity of vesical catarrh and calculous affections in that country. Such, also, is the tænia, which reigns almost over the whole extent of the African continent, from the shores of the Mediterranean to the Cape of Good Hope. At Geneva, a fourth of the inhabitants have had, or will have, the bothriocephalus; whilst at Zurich only the tænia solium is seen. In the east of Europe the Vistula separates the two varieties; on the right bank reigns the bothriocephalus, on the left the tænia solium."

The tenth book deals with the electrical phenomena of meteorology. To the consideration of these nearly 140 pages are given. The medical history of lightning, which occupies the greater part, is, so far as we know, the only one in existence. It develops a succession of the most peculiar and unexpected announcements. For the collection of these, indeed, M. Boudin appears to possess a singular gift. He frequently contents himself with the mere narration of occurrences or opinions, giving the source, and abstaining from all comment. The reader is left, therefore, to receive or not as he may think fit. Many without doubt will hesitate. The witnesses will often be considered incompetent, or at any rate insufficient, when speaking to such unlooked for and often astounding events. But the whole does not rest on such evidence; and the book will be found to contain a great deal that is trustworthy and valuable; and even things that at first sight may appear merely to possess a fanciful interest, probably are of some practical value, and may prove suggestive in future inquiries.

As briefly as possible we shall sketch the contents of this book. As compared with the country, towns, and especially the larger and more populous ones, appear to possess an immunity from accident to life by lightning. Thus between 1800 and 1851, not a single death by lightning was recorded in Paris; and in 1786 it was calculated that out of 750,000 deaths in London during thirty years, two only had been produced by this agency. During a century, only three persons were killed by lightning in Göttingen, and two in Halle. Comparing these numbers with the total deaths from this cause, and with the fact that 25 per cent. of all happen under trees, he holds it reasonable to conclude "that lightning finds more victims in open country than in cities." Another "most curious phenomenon, beyond contradiction, is the tendency it has to strike the same places and the same edifices at different epochs." Of this Dr. Boudin produces several illustrations, and quotes M. Pouillet in support and explanation.

"An arid soil, composed of a thin layer of vegetable earth, under which thick formations of dry sand, of lime, or granite are found, does not attract the lightning, because it is not a conductor of electricity. . . . And the houses participate to a certain point in the privilege of the soil. . . . But if under this arid and dry soil, at the depths of many tens of metres, there be great

metallic beds, vast caverns, sheets of water, or only abundant springs, the thunder clouds exercise their action on the conducting matters, the lightning is attracted, and it explodes in leaping over the interval;—the dry crust is not an insurmountable obstacle; . . . then ruin to the constructions which occur on its passage, be they of wood or stone."

With regard to the frequency of accidents by lightning, fatal to human life in France, he tells us that from 1835 to 1852 inclusive, 1308 persons were killed.

"This number," he says, "which is clearly the minimum, and which does not, moreover, include the individuals injured, rendered infirm, or crippled, gives a yearly average of 72 persons killed by lightning. In no case does the number fall below 48, while it rises in 1847 to 108, and in 1835 to 111."

M. Boudin thinks that the persons injured are at least twice as numerous as those killed. Some United States statistics show the injured to be to the killed as 5 to 1. Many more men than women are killed, and not in France only, but also, though not in so marked a proportion, in Sweden (1815 to 1840) and in England (1838 and 1839). He seems to think that this is not explained by the greater exposure of men in the fields, but still he does not think we are warranted in concluding "that, all things being equal, woman runs less danger than man;" but he considers the question as "worthy of being submitted to the test of observation." And he quotes the following peculiar passage from Arago, declining, however, to "maintain its rigorous exactitude:"

"In two conditions altogether alike," says Arago, "one man, by the nature of his constitution, runs more danger than another. There exist persons who arrest abruptly the communication of electricity, and do not feel the shock, even when they occupy the second place in the file. These persons, by exception, are not conductors of the electric fluid. Exceptionally, then, we must rank them amongst non-conducting bodies, which lightning respects, or which, at least, it strikes rarely. Differences so marked cannot exist without there being also shades of difference; but every degree of conductibility corresponds, during the storm, to a certain measure of danger. The man who is as conducting as metal will be as often struck as metal; the man who interrupts the communication in the chain will scarce have more to fear than if he were glass or resin. Between these limits there will be found individuals whom the lightning will strike as readily as wood or stone. Thus, in the phenomena of thunder, all does not depend on the place which a man occupies; *the physical constitution of the man plays also a certain part.*"

This may possibly be true. The authority is a high one. But the distribution of the sexes in the list of deaths from this cause appears to us not only not necessarily connected with it, but also quite explicable on the ground of greater exposure. For England, at any rate, we should feel inclined to deem this sufficient.* The statistics for Sweden show in their total no very striking difference (5·7 to 3·8), and for five years (1846–50) the sexes are exactly in the same proportion. His English figures, again, apply only to two years, and have therefore not much value. As one would expect, "the configuration of the soil and its mountainous character" exercise an influence on the frequency of

* [It is to be remembered that women do more field-labour in France than in England.—ED.]

accidents, which, for instance, in proportion to the population, are much rarer in the departments of the Eure and Seine than in those of the Dordogne, Lozere, High Loire, and Low and High Alps. Less danger is run in the house than in the fields, and in towns than in the country:

"Out of 53 killed in France in 1853–4, whose position at the moment of the accident is precisely noted, only 10 (that is less than one-fifth) were struck in the inside of a house or barn, and 43 (or more than four-fifths) in the fields or on the road. And it is scarcely to be admitted that, at the time of the storms, only one-fifth of the population were housed and the other four-fifths in the fields."

Not one of these was killed in the capital town of a department, and only one in that of an arrondissement—viz., in Nantua, with a population of 3750. 15 of the 34 killed in the fields in 1853 were struck below trees. The statistics of nine years in France show not one accident in the months of November, December, January, and February, and the maximum occurs pretty steadily in June, July, and August. In 1853 and 1854 by far the greater number of deaths occurs between nine A.M. and nine P.M., being almost seven times as frequent during day as during night. "The minimum, represented by zero, corresponds to the period from eleven P.M. to three A.M., and the maximum to that between three and seven P.M." On the same day, and sometimes even at the same hour, accidents happen at points far distant from each other, showing, as he says, that storms are often spread over a larger surface than is thought. Of this, as usual, M. Boudin is rich in illustrations, drawn from the most varied sources. Fires, too, he believes, have this origin more frequently than is supposed. "In the little kingdom of Wurtemberg, between 1841 and 1850 inclusive, 117 fires were reckoned to have lightning for a cause." "In France, in the year 1852, the archives of the Minister of Commerce show 105 fires" so produced.

The effects of lightning on man he makes either curative of pre-existing affections, productive of wounds or injuries, or productive of death. The injuries it may produce seem to be very varied:

"Burns more or less extensive; exanthems; partial or complete epilation; bleeding from the nose, mouth, or ears; temporary or persistent paralysis, especially of the lower extremities; amaurosis; deafness, with or without perforation of the tympanum; dumbness; imbecility; and abortion"

are those he names. To the peculiar images, said to have been observed on the bodies of some persons killed by lightning, he gives the name of keraunographic images, and he relates some of the most singular instances of it on record, giving the sources, which are not always the most reliable. The study of its effects on the dead body may on some occasions prove of importance in medico-legal inquiries, and we recommend our readers, so interested, to peruse all M. Boudin says on the subject, and judge of its value for themselves:

"That which particularly characterizes," he says, "the effects of lightning, is the *unexpected*, the *proteiform*, the *contrast*, and the *opposition*. At one time the individual is killed at once on the spot—the dead body remaining seated on horseback, or erect; at another time, on the contrary, we see the man killed thrown to a distance. Sometimes the lightning undresses its victims, destroys

their clothes, and respects the body; sometimes, on the contrary, it burns the body and leaves the clothes untouched. *Here* the destruction goes to a frightful extent, with rupture of the heart and crushing of the bones; *there* the most careful examination results in a negative autopsy. *Here* you have flaccidity of the members, softening of the bones, collapse of the lungs, and fluidity of the blood; *there* distension of the lungs, coagulated blood, and rigidity of the members, with lock-jaw. Sometimes the body appears to brave the laws of decomposition; sometimes, again, the most rapid and the most horrible putrefaction immediately seizes the corpse. In fine, lightning, which crushes a tree, and even stonework, appears to produce with difficulty mutilations in man, with separation of parts of the body."

The second volume begins with the consideration of "light and its influence," and this terminates both the subject of medical meteorology and the first part of the work. "La lumière exerce une influence aussi puissante que variée sur l'ensemble de la nature," is the remark with which the author begins his observations on this part of the subject; and he is undoubtedly right. It would be a good thing if we knew more of the nature and laws of this influence. As one of the elements of meteorology, it stands high in importance. We lack observations, however, but all that is known M. Boudin has carefully brought together.

The second part of the work, which has to do with the medical geography of man, will be that most generally interesting, and will find most readers.

The statistics of the soil and of population occupy the first book, and are discussed at some length. France receives naturally most attention, but all the countries of Europe, the United States, and the British and other colonies, obtain a fair share, and are brought together in an interesting and instructive comparison. He looks at and compares the population of all these countries under the usual aspects of density, age, sex, marriage, fecundity, increase, average life, mortality, religion, and crime. The authorities from which he has drawn his information are formidably numerous, and of the most trustworthy character. The ethnography of Europe is the title of the next book.

Whenever M. Boudin falls upon a *curious* question, he seems to have aptitudes quite peculiar for bringing out and developing the *curious* in it. The Jewish race, looked at medically, is such a question, and we find it so handled. In this light, as indeed in most others, we are, as it were, prepared to admit them an exceptional people. Of peculiarities of all kinds in them we are accustomed to hear. But so exceptional do they become under M. Boudin's figure- and opinion-statistics, that we are inclined to ask with him, is not all this "rather the index of a providential mission than mere chance?"

"The Jew," he says, "dwells now in all parts of the world. We find him in Europe, from Gibraltar to Norway; in Africa, from Algiers to the Cape of Good Hope; in Asia, from Cochin to the Caucasus, and from Jaffa to Pekin; in America, we encounter him from Monte Video even to Quebec; for fifty years he has been in Australia, and already he has given proof of acclimatization under the tropics, where people of European origin have always failed to perpetuate themselves. With respect to altitude, although the Jew dwells little on the mountain, there is, nevertheless, nothing to make us presume in him

any physical incompatibility for elevated localities. On the other hand, the Jew has lived for a long series of ages, and lives still to-day, on the only point of the globe situated at more than four hundred metres below the level of the sea—a country in which it is doubtful if the European will ever succeed in propagating his race. In fine, wherever the Jewish race has been studied up to the present time, it has shown itself subject to statistical laws of births, deaths, and proportionality of sexes, completely different from those which affect the other nations in the midst of whom they live.

"Of all known people," he further says, "there is, perhaps, none more curious than the Jew, dispersed among all nations, and over all points of the earth, unmixed and unmixable, having its own diseases and its own pathological immunities, everywhere acclimatized, the only truly cosmopolite people, representing in time and space, physically and morally, the most surprising historical and ethnographical phenomenon. Everywhere it has remained itself, guarding its traditions, its rites, its nationality, and its type, as the Rhone, which traverses the lake of Geneva, preserves always its trace and the original quality of its waters."

The important question of acclimatization now comes under discussion. "It governs the grave problem of colonizing," says M. Boudin, "and that of the choice of troops destined to serve in countries more or less distant from the motherland; it affects public hygiène and political and social economy."

"Because man possesses the faculty of adapting himself, *to a certain extent*, to a climate different from that in which he was born, it by no means follows that this power is without limit—in other words, that man is cosmopolite, as was long believed, and as indeed is still generally believed." M. Boudin is, in fact, one of the strongest opponents of Malte Brun. On this point he has very decided opinions, and he defends them resolutely. He believes that this faculty is distinctly limited—we may almost say very limited. At the root of the question, however, lies the definition of the word. It is capable of many shades of meaning, and we think M. Boudin has given to it its widest and not the most usual. It is not with him the simple conservation of existence, but the *entire* preservation of the physical, intellectual, and moral faculties. Now, it certainly is very doubtful if man can remove from northern to southern latitudes, or the reverse, and preserve, either in himself or his descendants, a perfect and an absolute copy of what he was originally. Positive changes in all these respects do and must occur. They are often seen as the result of slight removals, and more surely as the result of great. He ought not, in fact, to be the *same* man in the tropics and in the arctics. He must be *adapted* to the new climate and conditions; and the very word involves changes. He must change his habits, food, and occupations, and these changes necessarily entail physical ones; his face, and skin, and figure, and strength become different, and naturally, also, follow intellectual changes. This, in short, is being acclimatized, and it is complete if he can perpetuate his race and enjoy an average measure of health and life. The experiences of the French in Algiers seem to have first directed M. Boudin's attention to this subject, and his analysis of the official and other documents leads him to the inference that the acclimatization of the French there is an impossibility. How far any man

is entitled to draw such a conclusion from the early history of any new settlement, and especially of one till within the last ten years under all the disturbing influences of war, and not yet wholly free from them, seems doubtful. The monks of Staouelli were in their first year decimated by death, but the first difficulties over (and these belong to *every* new settlement, and are fatal in all climates), the mortality among them has not been above, but under the average. May it not prove so with the Algerian settlement as a whole, giving it a proportional length of time?

With plants, the author admits that the question may be reduced to one simply of conservation of species; but even here he holds they must be capable of reproducing and continuing themselves spontaneously without any artificial means, before they can rank with indigenous plants, and be in strict language acclimatized or naturalized. Our domestic animals—the dog, horse, ox, sheep, goat, and ass—are all of Eastern origin, and came originally from hot countries. This is admitted. We are accustomed to look on them as fully acclimatized. Would they, then, if left without the interference of man, propagate themselves and continue in our island? The experiences of Australia and America incline us to say *yes*. As regards man, the author looks at the question of acclimatization from two points of view. Firstly, as concerns race, nationality, and the peculiarities of the place from which he comes; and secondly, as concerns the latitude, altitude, soil, climatology, and pathogenic features of the place to which he goes. No part of the work is more interesting than this, and none more important or useful.

The Jew and Gipsy he makes the most cosmopolite. The Spaniard, Italian, and Southern Frenchman he considers more apt than the northern people of Europe. M. Boudin endeavours to show that transitions from south to north can be made more easily than from north to south. He does not believe that crossing with the indigenous population betters the prospect. Nor does he think that the mortality of European armies in hot countries will be lessened by lengthened residence, but the reverse. On all these points he produces copious statistical support. In reference to the acclimatization of individuals —the simple *vivere et valere*—he discusses at great length the sanitary condition of armies serving out of their own country. And he holds that humane as well as financial considerations dictate the raising of troops from the indigenous population of tropical colonies.

The argument for cosmopolitism derived from the unique primitive origin of man—that is, his origin from one couple—he refutes thus. In the first place, because, however plausible it may be, it cannot invalidate facts, and in the second place, because we are discussing the powers of the man of this age, and not of ages long gone by. And he accepts the theory of De Maistre, that the savage in no respect represents the primitive man, but is degenerate, and that the most civilized man of to-day is to the primitive what the untutored savage is to him. He holds, in fine, that man, as we know him, is proved, by the facts we possess, not to be cosmopolite, but to possess faculties

of acclimatization "essentially limited, though varying notably according to race."

Substantially we accept these opinions of M. Boudin. We differ, perhaps, as to the degree of the faculty. He has shown, that at any rate it is with great difficulty, and at great cost, that man adapts, or attempts to adapt himself to the climatic and other influences of countries removed from the one of his birth. And *this*, after all, is the point of practical value, both to the physician and the political economist, and to both a question demanding the most serious consideration.

The geography and statistics of disease form the subject of the fourth book, and the natural introduction to the next, which occupies 400 pages, finishes the work, and contains the geographical and statistical history of fifty-six endemic and other diseases and infirmities, interesting in this point of view. These the author has arranged alphabetically, in order to increase the facility of reference.

"Man is not born," says M. Boudin, "does not live, does not suffer, does not die in the same manner on all points of the earth. Birth, life, disease, and death, all change with the climate and soil, all are modified by race and nationality. These varied manifestations of life and death, of health and disease, constitute the special object of medical geography."

The comparative pathology of races, and the geographical distribution and migrations of disease, occupy the rest of M. Boudin's work.

"Like plants, of which some are found in almost all countries of the globe, while others are in a manner endemic in certain localities more or less circumscribed, so also the diseases of man are either spread over the whole surface of the earth, or are fixed to certain zones—to certain localities."

While the study of this distribution of disease throws light on the influence of climate, soil, nationality, and race in the production of disease, it at the same time

"Guides the physician in the choice of localities best adapted for the sojourn of the invalid; it indicates to the hygienist the places he ought to seek or avoid; it furnishes an experimental base to the laws of quarantine; and it makes known to governments the military aptitudes of a country."

In the investigation of this question, he compares the diseases which produce death in one country with those which do so in others; and he does this for most of the countries of Europe, and for some hot countries. He then compares the causes of death in the armies of France, Sardinia, Belgium, Prussia, Sweden, Russia, the United States, and England at home and in her numerous foreign possessions. Such a document cannot fail to be interesting and instructive. In doing this, he finds many diseases to be endemic (or at any rate to be exceptionally frequent) in certain localities, which are not usually regarded as being so. Among these, hernia, epilepsy, myopia, &c., occur. On the other hand, he appears to establish the non-endemicity of other affections hitherto regarded as endemic.

According to the researches of M. Boudin, cholera has not yet passed 64° N. and 21° S. latitude; marsh fevers in the North hemisphere have kept within the isothermal line of 5° C., and in the South

they do not reach the line of 15° C.; yellow fever has never passed 48° N., nor 27° S. latitude, and the longitudes of Acapulco and Leghorn have been its other limits. Cretinism and goître belong to mountainous localities. In elevated regions marsh fevers change their type, going further and further from continuity as you ascend. Verngas may be said to belong to Peru; the pinta to Mexico; the plica to Poland; hydatids of the liver to Iceland; the tænia to Abyssinia; croup to some parts of Sweden, &c. Cancer is very rare in Egypt; gout almost unknown in Brazil, Peru, or Nubia; phthisis is little known in Iceland, in the Feroë Islands, and in the steppes of Kirghis, and in the same places the same is true also of scrofula. Goître belongs to calcareous districts; and cholera

"Shows a marked preference for tertiary and alluvial soils, and rapidly deserts the ancient formations. In several parts of the United States and of Switzerland, the disappearance of marsh fever (resulting from drainage of the soil) appears to have been followed very soon by the appearance or increase of pulmonary phthisis."

The sea ("the soil of the sailor") has also a modifying influence on the character and fatality of disease. M. Boudin holds it to be *preventive* of phthisis; *curative* he does not assert it to be. And he founds this chiefly on these figures:

The English army counts annually in the United Kingdom 8·9 deaths by phthisis to 1000 men of the Line, and 12·5 to 1000 of the Guards; while the losses by phthisis in the navy, from 1830 to 1836 inclusive, amount to 1·7 to 1000 men, this being an average calculated from all the stations.

On the *therapeutic* action of climates in phthisis, he holds our information to be much less complete than on the *preventive*. And he does not indicate any locality as *peculiarly* beneficial to those labouring under chest affections. The author analyses the statistics of the English Government on the sanitary condition of the troops, and finds that the maximum of deaths by phthisis occurs at home, and that the loss becomes less, not only in warm climates, but also (and in a manner even more marked) in cold countries—as in Nova Scotia, New Brunswick, Newfoundland, and Canada. This is an unexpected result, and still more so is it to learn that statistics show this disease to be nearly unknown in Iceland, the north of Norway, and in the Feroë Islands. As compared with the English, Negroes (out of their own country) are proof against the action of the marsh miasm, but phthisis commits fearful ravages amongst them. So also is the Negro less subject than the English to dysentery and hepatic affections (which ailments M. Boudin regards as closely allied by geographical ties), and the Sepoy less liable than both. M. Boudin still adheres to his theory of an antipathy between marsh fever and phthisis, but we think not so strongly as when he first advanced it.

One would hardly expect to find such a disease as hernia endemic, yet such results from the analysis of the causes of exemption from military service in France. The aggregate for a long series of years shows this cause to be very frequent among conscripts from some

localities as compared with those from others. But not only does the aggregate show it, but each year of the series shows it steadily affecting the same localities, in a manner that cannot be accidental. It is probable, however, as M. Boudin supposes, that this has to do with hereditary influences rather than with climate or soil. Epilepsy, myopia, and a number of other causes of exemption, present similar results of endemicity. Such are a few of the more interesting and peculiar facts picked out of this part of the work. A large number of the diseases thus described are little known to readers of ordinary medical literature, and they scarcely concern them. But a knowledge of them may be valuable to those practising in large centres of population, having frequent intercourse with all parts of the world, but more especially to army and navy surgeons, who are called on to change their field of practice continually, and must be prepared to recognise and treat diseases which the ordinary home practitioner has no chance of ever meeting. The work, however, addresses itself not to the medical profession only, but also to the hygienist, to political economists, and to public administrators. It is almost needless to say of such a work, that it rests on a statistical basis. It could not indeed be otherwise. It is clear, as M. Boudin says, "that the endemicity, the frequence, the gravity of a disease, the salubrity of a country—in fine, the great question of the acclimatization of man, are so many problems which imperiously demand a statistical basis, without which, indeed, the very expressions, *frequent, endemic, salubrious,* are words without sense."

To all the errors to which statistics are usually liable, these of course are subject; and perhaps to more than the usual amount, since they are employed in the elucidation of such complex questions. That they can, however, in such matters, be made to yield sound results, is proved by the best of all tests—a commercial one—that derived from the history of life assurance companies. Nor is it any real objection to their use that in reasoning on them we are liable to error. We learn from this simply the need of a greater caution. In all the affairs of life we unconsciously tabulate our experiences and calculate means. Our *oftens, nevers,* and *seldoms,* are nothing but these averages; and in the fact that each of these words may apply to the same set of conditions, according to the temperament of the man who uses it, we have proof of our liability to reason unsoundly. They cannot all be correct. The reasoning is at fault, however, and not the data; and in the ordinary affairs of life, he is styled the sound practical man whose judgment discerns the right interpretation, appreciating the points of value, and separating and dismissing those that are worthless.

But in all cases the precise statement of a per-centage preceding the *adjective* (or, in other words, the *inference*), whatever it may be, becomes definite and valuable information, which we can use separately; while the *adjective* or *inference* becomes also a thing to be tabulated in the statistics of opinion, and though of less, yet not without value.

Some of the points of greatest novelty are—the medical history of lightning; the influence of race, more particularly as observed in the

Jew; the limited faculty of acclimatization possessed by man; the rarity of phthisis in some cold countries; the endemicity of certain affections not hitherto regarded as such, and *vice versâ;* the probable disappearance of the plague; and the history of a large number of unusual diseases or infirmities not elsewhere to be found, brought together and fully described.

The work is got up with care, and, considering the masses of figures involved, with average accuracy. It is ably written, and in all respects a most valuable addition to medical literature. The "*frame-work*" is already well filled, but many gaps still exist. These M. Boudin has pointed out. We feel sure that he will himself, if spared, contribute further to the filling up; and when old inferences are affected by new observations, or fresh materials give birth to changed conclusions, he will be the first to point these out.

Review VII.

Medico-Chirurgical Transactions. Published by the Royal Medical and Chirurgical Society of London. Vol. XL. 1857. 8vo, pp. 238.

The volume before us is smaller than those which we have lately been in the habit of receiving, a fact which is attributable not to any shortcoming on the part of the members, but to many of the papers read before the Society having already been published in a new form, under the title of 'Proceedings.' In the 'Transactions,' only such papers have hitherto appeared as have been deemed by the Council most fit for publication; but in the 'Proceedings,' which it is intended to publish every two months, an abstract of all the papers is given; those papers are published in full which the authors do not wish to expose to the risk of being rejected by the Council for publication in the 'Transactions.' Our own opinion is, that this innovation is one by no means necessary or desirable. We cannot but regard it as an instance of that tendency to the production of an ephemeral literature so characteristic of the present age, and to which we have lately more than once had occasion to advert in terms of disapprobation.

The present volume of 'Transactions' contains fifteen original papers, and is illustrated by five lithographic plates. The first paper is entitled—

I. *On the Treatment of Aneurism by Manipulation.* By William Fergusson, Esq., F.R.S.—By the term given in the title, the author means—"a particular manipulation of an aneurism, whereby the fibrin within may possibly be so displaced as, either in part or in whole, to block up the main artery on the distal side of the disease."

This plan of treatment appears to have been first suggested to Mr. Fergusson's mind by a case, which Professor Turner of Edinburgh, was wont to cite in his lectures, of a gentleman with a formidable

aneurism of the subclavian artery, who, in spite of all remonstrances to the contrary, would persist in his favourite amusement of swimming in the open sea. On one occasion, soon after his usual indulgence, a change was noticed in the tumour, which continued to decrease in size until it underwent a spontaneous cure. In this case the cure was attributed by Professor Turner to the artery on the distal side of the aneurism becoming blocked up by a layer of lymph displaced by accident from the interior of the sac. Acting on this suggestion, Mr. Fergusson has attempted to imitate this supposed natural process of cure, by squeezing the blood out of the aneurismal sac, and then rubbing the opposed surfaces of this together. Two cases, both aneurisms of the right subclavian, are given, in which this experiment was practised. In the first patient, a man aged forty, no pulsation could be felt in the arteries of the corresponding arm for several days after the operation; the tumour diminished somewhat in size, but death followed seven or eight months after the experiment, from bursting of the aneurismal sac. In the second patient, a man aged forty-four, the operation was certainly followed by a complete disappearance of the tumour; but the manipulation was practised on the 4th of August, and repeated on the 6th of October, 1853; in October, 1854, no perceptible change had taken place in the tumour, and it was only in August, 1855, that it was observed to have disappeared. From the lengthened period which intervened between the experiments and the disappearance of the aneurism, it may fairly be doubted how far the two are to be regarded as cause and effect. Aneurisms, it is to be remembered, may undergo a spontaneous cure.

At the same time, in both of the cases the experiments were followed by symptoms of a most alarming nature, which might have well led the author to hesitate before recommending the practice to his professional brethren. The first patient became temporarily confused, exclaimed that he had been made tipsy, and staggered as if about to fall. The experiment in the second case was instantly followed by left hemiplegia, which lasted for many months. These results, especially in the latter case, were no doubt owing to portions of fibrin becoming detached from the aneurism and plugging up some of the cerebral arteries, in the manner explained by Virchow and Kirkes. The dangerous consequences of this nature, which may result from the mode of treatment recommended by Mr. Fergusson, have been urged with much force in a paper recently published by Dr. Esmarch, Professor of Surgery at Kiel, in which he gives an account of a case of embolic apoplexy resulting from the detachment of fibrinous coagula in an aneurism of the carotid.*

We certainly are somewhat surprised that Mr. Fergusson should consider the two cases which he has adduced as illustrating in many respects practically and satisfactorily the views broached in the early part of his paper. On this head we would fain believe that the author has expressed himself more strongly than he really feels; which conclusion seems the more probable, when we take into account the hasty

* See British and Foreign Medico-Chirurgical Review, Oct. 1857, p. 532.

composition of which the paper bears internal evidence. The style is in many places, to say the least, far from elegant; while the word "consultants" has no place in English literature.

II. *Case in which a Large Cyst was Successfully Removed from the Upper Part of the Neck of a Young Woman, aged Twenty-seven, who was considered at Birth to have been the subject of Spina Bifida.* By Samuel Solly, F.R.S.—This case is one of great interest, whether the tumour be regarded as having been of spinal, or, as is more probable, of intra-cranial, origin. The paper is accompanied by three drawings, showing the size of the tumour at the age of eighteen months, and immediately before the operation, as well as its appearance after removal.

III. *On the Determining Causes of Vesicular Emphysema of the Lung.* By William Jenner, M.D.—The author advocates an expiratory theory of emphysema, in opposition to the inspiratory one maintained by Williams, Nasse, Rokitansky, and others, and which in late years has been so philosophically elaborated by Dr. W. T. Gairdner. Dr. Gairdner urged as the most serious objection to the expiratory theory, that in consequence of the uniform pressure of the external parietes of the thorax upon the whole pulmonary surface, the expiratory act is mechanically incapable of producing distension of the lung or of any part of it. Dr. Jenner, on the other hand, denies that the lungs are uniformly compressed during expiration, and endeavours to make out that at some parts, especially the apices and anterior margins, there is a want of local compression, or that the walls of the chest are more yielding than at others, and that it is these very parts which are most frequently the seat of vesicular emphysema. During violent expiration, he considers that the air is forced from the parts which are most compressed into those which are less so. Our space will not allow us to enter into the arguments which might be brought forward on both sides of this question. We cannot, however, consider that the facts which have been adduced by Dr. Jenner, and handled by him with considerable ingenuity, are by any means conclusive, and still believe that the now generally-adopted inspiratory theory of emphysema is the one which affords the most rational explanation of its production in the majority of cases.

IV. *Five Cases of Tracheotomy in Croup, with Remarks on certain Points connected with the Operation, with a Postscript containing Two Additional Cases.* By Henry William Fuller, M.D. Cantab.—This is a paper of much practical importance. Of the 7 cases recorded by Dr. Fuller, 3 recovered and 4 died. The first case was a girl, aged eight, in whom the operation was performed on the fourth day of the disease, and was followed by recovery; in the second case, a girl aged five, the operation was performed on the ninth day, and was also followed by recovery; the seventh case, a boy, aged five, recovered, tracheotomy having been performed on the ninth day of the

disease. In the other four cases, which terminated fatally, the patients were a girl aged sixteen, a boy aged five, another boy aged four and a half, and a girl aged five; and the operation was resorted to on the sixth, fourth, second, and third days respectively. In none of the cases was the operation had recourse to until collapse had supervened, and the patients were *in extremis*. With one exception, the operation was followed by great and immediate relief in all the seven cases, and in several, the patients were observed to cough up plugs of white false membrane through the wound.

Appended to the histories of these cases are some valuable remarks on the following important questions:

1st. Whether the operation of tracheotomy is justifiable in any case of croup? Under this head the author considers first the results of the operation as performed in France, where the diphtheritic form of the disease is so common, and by collecting several published statistical statements obtains a total of 168 operations and 56 recoveries, or a proportion of one recovery to every three operations. Hence he concludes that in France the operation is eminently successful, and productive of an enormous saving of life, for before the operation was introduced into practice, the disease was almost invariably fatal when it reached its second stage. He then goes on to show, from numerous statistical facts, that when diphtheritis attacks the air-passages, the extent of mischief is nearly if not quite as great as that which is met with in the idiopathic inflammatory croup of this country; and hence, that there is as great prospect of success in the latter case as in the former. That this prospect is not a visionary one Dr. Fuller proves by collecting all the recorded cases in which tracheotomy has been performed in croup in this country. These amount to 23, including the author's own 7 cases; and although in all of them, without exception, the operation was deferred until the last stage of the disease, when suffocation was imminent, yet in 9 cases it was successful. (Since the appearance of Dr. Fuller's paper, 5 additional cases have been published by Mr. Spence, of Edinburgh, 3 of which were successful.)* Dr. Fuller concludes, that, (even allowing that death was an unavoidable result in all the fatal cases,) no practitioner would be justified, except under some peculiar circumstances, in withholding the chance afforded by the operation.

2. At what stage of the disease ought the operation to be undertaken? The author does not approve of the early performance of the operation recommended by Messrs. Bretonneau and Trousseau, and observes:

"As long as it appears possible to save life without operative interference, so long I maintain we ought to withhold the knife; but directly it becomes obvious that our remedies have failed, and that life will be sacrificed if the trachea be not opened, that moment we ought to urge upon the parents the immediate performance of tracheotomy. Each minute that is lost afterwards is so much against the patient's recovery."

3. Whether the existence of certain symptoms or other circum-

* Edinburgh Medical Journal, Feb. 1858.

stances ought not to cause us to hesitate in recommending its performance? Pulmonary complications, Dr. Fuller admits, would counter-indicate the operation; but he insists that, from the peculiar circumstances of the case, neither by a physical examination of the chest, nor by the most careful observation of the general symptoms, can we arrive at a certain conclusion as to the condition of the lungs. He adds:

"If the patient has been out of health prior to his attack of croup, if his illness has been preceded by pneumonia or severe bronchitis, if he is suffering from any exanthematous or other disorder; and further, if he is in such a position of life that his parents are unable to secure for him proper skilful attendance day and night, the operation ought not to be recommended, however promising it might otherwise appear. For under such conditions it must almost certainly prove fatal."

4. Whether any, and what, medical treatment is necessary after an opening has been made into the trachea? The author observes that the operation merely admits air, and does not arrest the disease; and hence that those remedies, calomel and antimony, which are usually considered useful before the operation, must be steadily persevered in afterwards. Except in the diphtheritic form of the disease, and in those varieties which follow measles, scarlatina, or other of the exanthemata, he objects, both on theoretical and practical grounds, to the administration of stimulants, as being not only unnecessary, but positively injurious.

The paper concludes with some useful suggestions as to the construction of the trachea tubes. These are recommended to be larger, shorter, and less curved than those in common use, of uniform diameter throughout, and with the outer tube constructed of two flattened blades, which can be immediately expanded by merely pushing in the inner tube between them.

V. *Some Observations on the Anatomy and Pathology of the Adult Prostate, founded upon Fifty Preparations of the Organ Dissected by the Author.* By Henry Thompson, F.R.C.S.—These observations have reference to the following heads:

1. The frequency with which enlargement of the prostate occurs in advanced age. Contrary to the current opinion, which has been advocated by Sir Everard Home, Sir Benjamin Brodie, and others, Mr. Thompson, from an examination of his fifty cases, arrives at the following conclusion:

"It may, then, be regarded as established by the facts before the Society, that enlargement of the prostate, so far from being a change natural to old age, is an exceptional condition. And it may further be regarded as highly probable, that a slight tendency thereto, almost if not quite unrecognisable during life, may occur in about one out of three individuals after fifty years; and that a marked enlargement may be found in one out of eight, rarely, however, before sixty years of age."

2. On the weight and size of the healthy adult prostate. The various measurements of the organ are given with considerable minute-

ness; and the *prevailing* and *average* weight are stated to have been found the same, the latter being 4 drachms and 38 grains.

3. On the nature of a part called "the third" or "middle lobe." The author confesses that he has not been able to detect a portion of the healthy organ in this situation, sufficiently defined by form or position to entitle it to the distinctive appellation conferred upon it by Sir Everard Home, and thinks that the term "posterior median portion" is more applicable to the thin laminæ of the organ lying above and below the urethra. The occasional appearance of a distinct lobe in this situation he considers to be a result of disease, as was long ago asserted by Morgagni.

4. On the existence of distinct tumours in the prostate. The existence of distinct tumours, either embedded in, or projecting from, the substance of the prostate, Mr. Thompson thinks is more common than is generally believed. From an analysis of his own cases, and of seventy specimens of enlarged prostate in the Museum of the College of Surgeons, he shows that these tumours are more frequently than otherwise the essential element of the pathological condition generally known as hypertrophy of the prostate. Three sorts of these tumours are described—viz., 1. A simple fibrous tumour; 2. A tumour containing in addition to fibres some of the glandular structure of the prostate imperfectly developed; and 3. A tumour composed of the ordinary structures of the prostate fully developed. The latter springs from the posterior median portion, and has its own special ducts. A striking analogy is stated to exist between these tumours and those of the uterus.

5. On the existence of minute "concretions," their mode of production, and their relation to the formation of prostatic calculi. Minute concretions, the largest about the size of poppy seeds, were detected in the prostatic ducts of every one of the 50 cases; and the author concludes, that their existence is a necessary result of the performance of natural functions on the part of the prostate. The structure of these concretions, and the manner in which they may become converted into prostatic calculi, are carefully described.

VI. *On Disarticulation of the Scapula from the Shoulder-joint.* By James Syme, F.R.S.E.—The operation described in this paper adds another to the many new operations, with which the distinguished Edinburgh Professor has already enriched the practice of surgery. The operation was performed on a female, nearly seventy years of age, on account of a large cerebriform tumour of the "scapula." The wound healed favourably, and in no long time the woman could use her arm so freely, that she declared it was in no respect inferior to the sound one. The patient herself, however, gradually became weaker and more emaciated, and ultimately died, fully two months after the operation. The author appends to the history of the case some valuable practical remarks; and observes, that although it would have been more satisfactory if the patient had lived longer, yet the progress of the case was sufficiently advanced to show—1stly. That the entire scapula may

be disarticulated from the shoulder-joint, without a loss of blood to any great extent. 2ndly. That the wound resulting from this operation does not necessarily occasion an excessive amount of discharge. And 3rdly. That the arm which remains is not a useless appendage, but a serviceable limb.

VII. *On a New Method of Operating for Impermeable Urethra.* By JAMES SYME, F.R.S.E.—Mr. Syme commences by pointing out the error of those who have attributed to him the opinion, that in no case is the urethra impermeable to instruments. All that he has asserted is, that the nature of a *stricture* is inconsistent with impermeability; and he has never denied, that in consequence of wounds or sloughing, the urethra may become completely *obstructed* beyond the fistulous opening, so as to be impermeable both by urine and instruments. Every practical surgeon knows full well the distressing nature of these cases, and how embarrassing and uncertain in its results is the ordinary operation of cutting upon the point of a catheter passed down to the seat of obstruction. Mr. Syme proposes the following mode of procedure in such cases:—To introduce into the bladder through the fistulous opening—which, if necessary, might be dilated—a staff like that used in lithotomy, but with the groove on its concave instead of its convex side; then to insinuate through the urethra, as far as possible, the guide director employed for dividing strictures by external incision; and while the staff, confided to an assistant, is supported by a finger of the operator on the perinæum or in the rectum, to push the director onwards in the direction it ought to take, if the canal were free, so as to pass through the obstructing texture, enter the groove, and proceed into the bladder. The case might then be treated as in the ordinary operation for stricture by external incision. We shall be glad to hear the results of some of Mr. Syme's cases treated in the above manner.

VIII. *Case of Double Talipes Varus, in which the Cuboid Bone was partially Removed from the Left Foot.* By SAMUEL SOLLY, F.R.S.—For an account of this case we must refer our readers to the original paper.

IX. *On Forcible Extension and Rupture of the Uniting Medium of Partially Anchylosed Surfaces.* By BERNARD E. BRODHURST.—In cases of partially anchylosed joints, the author proposes this plan of treatment as less hazardous and more efficient than resection of the joint. He commences by first dividing subcutaneously the tendons likely to interfere with the extending process; seven or eight days after, the patient being under the influence of chloroform, he proceeds to rupture the adhesions by forcible flexion and extension of the limb. Afterwards, passive motion is practised daily on every second day. The history of eight cases in which this practice was pursued, is given: three, of partial anchylosis of the knee; four, of the hip; and one, of the elbow. In all of these cases the result appears to have been favourable.

X. *On Scirrhus of the Male Breast* By J. L. Milton, Esq.—The paper contains an account of a case observed by the author. The patient was a man, aged fifty-eight, and the case terminated fatally, about a year after the first appearance of the tumour, no operation having been attempted. It is to be regretted that no account is given of the structure of the tumour, except that it was pronounced by Mr. Quekett to be scirrhus. The author has also collected, apparently with much industry, betwen 50 and 60 other cases of scirrhus of the male breast.

XI. *Analysis of Fifty-two Cases of Epilepsy observed by the Author.* By Edward H. Sieveking, M.D.—The author, without any attempt to theorize upon the disease, records a few valuable facts, the result of his own observation. These are classified under the following heads:

1. *Sex.*—Of the 52 cases, 24 were females and 28 males. From this it would appear that the male sex is somewhat more liable to epilepsy than the female. The same result is arrived at by referring to the number of deaths from epilepsy in England and Wales, as recorded in the Registrar-General's Reports for seven years (1848 to 1854 inclusive). Out of 12,878 cases, 6729, or 52·26 per cent., were males.

2. *Age.*—Of the 52 cases, 36 were from infancy to the age of twenty years; 8, from twenty-one to forty; and 8, from forty-one to fifty-five years. Dr. Sieveking also confirms the observations of Tissot and Esquirol, that it is only at the period of puberty that the male exhibits a greater tendency to epilepsy than the female.

3. *Causes.*—In 16 cases a definite cause was assigned; in 6 only could an hereditary taint be traced.

4. *Premonitory Symptoms.*—These existed in 27 out of the 52 cases. The various characters of these symptoms are enumerated; but they were never described by the patient as a puff of wind, or an "aura" in its verbal sense.

5. *Individual Symptoms.*—Headache was observed in 33 of the 52 cases. Of these, it was constant in 19; before the fits only, in 4 cases; and after the fits only, in 10 cases. Biting the tongue occurred in only 17 cases. In 19 cases the urine was tested for albumen, which was found temporarily present in one, and permanently in a second; in 14 cases it was examined for sugar, which was not found once.

6. *Results of Treatment.*—The number of apparent cures was fifteen; and it was observed that the curability of the disease bore an inverse ratio to its duration before treatment. Some remarks follow on the various plans of treatment pursued in the different cases. The author expresses his disbelief in any specific for epilepsy, but thinks we must be guided by the following indications: to remove local congestion by counter-irritants, to promote the healthy action of the secernent organs, and to give tone to the constitution by vegetable and metallic roborants, as well as by suitable regiminal measures.

XII. *A Case of Disease of the Heart, with great Dilatation of the Auricles.* By W. O. MARKHAM, M.D.—In this very interesting case, the following signs were observed during life:

"At a point about an inch and a half or two inches from the right edge of the sternum, and in the fifth intercostal space, a *pulsation* was observed, synchronous with the ventricular systole. This pulsation was visible along a space of about three-quarters of an inch: it communicated a strong thrill to the finger placed upon it, and likewise forcibly raised the finger. The stethoscope placed over it, transmitted to the ear of the observer a loud, prolonged, rough murmur."

The pulsation was quite distinct from that of the apex, which could be felt quite in the left lateral region of the thorax. During life an opinion was expressed that the pulsation was aneurismal; but after death it became manifest that the pulsation, the thrill, and the long prolonged bruit, took their origin from the right auricle, which reached away far to the right of the sternum. That tricuspid regurgitation did not cause the pulsation, seemed indicated by the perfect condition of the tricuspid valves and other circumstances. "Hence, then, it would seem that we must place the pulsation, the thrill, and the murmur, to the account of the blood rushing into the auricle from the venæ cavæ during its diastole."

The patient was a man, aged sixty-nine. Twenty-four years before his death, his life had been despaired of on account of extensive dropsical effusions; and fifteen years before, he had been told that the dropsy and painful symptoms from which he suffered were the consequences of disease of the heart. The case then demonstrates the extraordinary degree of deviation from the healthy state of the heart with which a long life may be compatible.

XIII. *Case of Intense and Long-continued Photophobia and Blepharospasm, Relieved by the Inhalation of Chloroform.* By WILLIAM MACKENZIE, M.D. Glasgow.—The patient, a female, aged twenty-two, had suffered from photophobia and rigid compression of the eyelids for sixteen years without intermission, and had been admitted into the Asylum for the Blind as one hopelessly deprived of sight. At intervals of three or four days she was put seven times under the influence of chloroform, but never to the extent of complete insensibility. Each administration produced a decided diminution of the symptoms; and after the seventh application she opened her eyes fully, and saw every object around her. The improvement was permanent.

XIV. *On the Effects of Twelve Weeks' Residence in Bulgaria, during the Months of June, July, and August,* 1854, *on the subsequent Health of the British Army in the Crimea.* By WILLIAM AITKEN, M.D.—It will be recollected that at the commencement of the late Russian war a large body of our troops was stationed in Bulgaria for many months, previous to their embarkation for the Crimea. Cholera there raged among them to a fearful extent; and those who escaped its deadly ravages were subjected to influences of the most debilitating nature.

Depression of spirits from inaction and from the suddenness and fatality of the attacks of disease among their comrades, long drills, bad tents, great alterations of temperature, malaria, and bad food—all contributed to "use up" the strength of the men. It became, then, an interesting question to determine the amount of disease and mortality among these troops after their arrival in the Crimea, as compared with that among the troops which had been transported direct from England. Dr. Aitken has, with this object, divided the Crimean army into two bodies: the troops which had previously served in Bulgaria he designates *ex-Bulgarian;* the others simply *Crimean.* He then proceeds, by a series of elaborate analyses of the Army Returns, to investigate the prevalence of, and mortality from, different classes of disease in these two divisions. As regards the prevalence of different diseases, he shows that the admissions for fevers, scurvy, pulmonary affections, and many other diseases among the *ex-Bulgarian* troops, greatly exceeded those among the *Crimean.* To these, cholera and other enteric diseases presented a remarkable antithesis, the admissions for these affections being greater among the Crimean troops. This apparent anomaly, the author observes, is in a measure explained by what we know of these diseases. During the Bulgarian campaign, the more susceptible among the troops had already suffered from them; and fewer, therefore, were in a condition to become affected by them in the Crimea. The mortality from the different diseases, as the percentage of deaths calculated in the number of admissions, Dr. Aitken has ascertained to have been invariably greater amongst the *ex-Bulgarian* forces: in this respect cholera and dysentery proved no exceptions.

The residence in Bulgaria appears also to have exercised a prejudicial influence over the results of the surgery of the war. Thus—

> "The ratios of the deaths per cent., in the Crimea and at Scutari, on the total admissions for wounds and injuries, were 19·1 per cent. amongst the *ex-Bulgarian* troops, and 13·7 per cent. amongst those who had served in the Crimea only."

XV. *Further Observations on the Use of the Speculum in the Diagnosis and Treatment of Uterine Diseases.* By Robert Lee, M.D., F.R.S.—Dr. Lee is well known as a most determined opponent of the use of the speculum-vaginæ for any affection whatsoever. In a previous volume of the 'Medico-Chirurgical Transactions,' he has published a collection of 220 cases in which the speculum and caustic had been employed by other practitioners; and we have now the details of 80 more, making in all the overwhelming number of 300. Dr. Lee argues with his usual energy, that in all these cases the introduction of the speculum was unnecessary, injurious, or immoral. "The speculum" (he says) "emanated from the syphilitic wards of the hospitals of Paris, and it would have been better for the women of England had its use been confined to those institutions!" We quite agree with Dr. Lee, that the speculum is an instrument which has been, and is, much abused; but we cannot go along with him in his sweeping

denunciations against it, or believe that it is not at times of great service. Cases may be collected and used in such a way, as to prove or disprove any method of practice either in medicine or surgery; and when we examine Dr. Lee's great array of cases, we cannot avoid the impression that he must have been led away, however unwittingly, by a foregone conclusion. He would have conferred a much greater boon upon the profession by accurately defining these classes of symptoms, which alone might justify the practitioner in employing the speculum.

REVIEW VIII.

On Dislocations and Fractures. By JOSEPH MACLISE, F.R.C.S. *London*, 1858. Imperial folio, Fasciculi I. to IV.

MR. MACLISE is favourably known to the profession by his work on 'Surgical Anatomy.' His object, he says in the preface to the present work, is to give a *demonstration* of those facts which have been already *described* by Sir A. Cooper and others, and of those also which have not. To do this he has been urged by many correspondents, and amongst them by the editor of Sir A. Cooper's work himself. "The practitioner, observed the late Mr. Bransby Cooper to me, has already sufficiently heard of fractures and dislocations, and what he now desires is to *see* them. This requirement, next to an examination of the actual objects themselves, may be best fulfilled by an actual delineation of them."

The author has the merit of being an accomplished draughtsman. We wish he would cultivate simplicity in composition. Many passages, in the preface especially, are characterized by quaintness and intricacy of expression, which tend much to obscure their meaning. In works of this description the reader has a right to expect perspicuity, strict accuracy, and precision of style. We make the above remarks in a kindly spirit, believing the author capable of producing a work which shall be useful to the profession and creditable to himself.

The author commences with a description of the form and mechanism of the cranio-spinal axis, fractures and dislocations, concussions, compressions, and lesions of the nervous centre. In the accompanying plate are delineated the cranio-vertebral form, with the cerebro-spinal axis seen posteriorly; and also representations of dislocations and fractures of several of the vertebræ. We concur in his statement, that "the diagnostic symptoms of concussion and of compression are obscure, and not unaccountably so; for, like the forces which caused both states, the effects of these on the body generally are the same." On another point, however, we must join issue. Mr. Maclise says:

"As the brain and its enveloping membranes occupy the cranial chamber so completely as not to leave the smallest encephalic space in the state of vacuum, it is clear that all matter suddenly encroaching upon that space must cause compression of the brain."

Surely the author overlooks the existence of the subarachnoid fluid of the brain and spinal cord, which oscillates, so to speak, between the one cavity and the other, and thus allows a certain amount of blood to be extravasated without notable injury to the sensorium, and which could not be the case were the cranial cavity completely filled by the brain. Indeed the experiments of Serres and Malgaigne go to prove that the brain can sustain a considerable degree of compression without material disturbance of function. The above circumstance affords one of those beautiful instances of compensation which prevail throughout nature.

The author makes some good practical remarks on lesions of the cord, and the resulting forms of paralysis. He next treats of the form and mechanism of the facial apparatus, fractures, and dislocations of the maxillæ. In reference to the lower jaw, he observes:

"The only direction in which a complete dislocation—that is, in respect to both temporo-maxillary joints—has been noticed to occur to the lower jaw-bone, is *forwards* into the temporo-maxillary fossa. The extreme rarity of its occurrence—indeed, the fact that it never happens in any other direction, may be accounted for anatomically. The lower jaw is a duplex or symmetrical bone, and the temporo-maxillary articulations are formed symmetrically also; so that the same guards which prevent displacement of the one condyle in an internal or backward direction, act in that capacity for both sides of the jaw as long as no fracture has occurred."

This is all true; there are one or two instances on record of dislocation backwards, but with simultaneous fracture of the bony meatus auditorius.

The chief part of the second fasciculus is occupied with a digression on the circulation of the blood, which we consider quite out of place, the object being to prove that the heart's action, as impulsive of the blood, is limited to the arterial system of vessels, and that the thorax is the only motor power whereby the venous side of the circulation is effected. The first proposition which the author lays down is, that the heart affects the body by a motion of the blood in arterial direct lines. According to his second proposition, "the heart has no action but systole," the ventricular systole succeeding the auricular; but the one is not to be understood as the effect of the other. The opposite state is that of diastole, or relaxation, which is a merely passive state of the muscular organ.

"Systole, therefore, is the only *action* which the heart exercises for giving the blood its motion. The auricles by systole propel their contents into the ventricles while these are passive. The ventricles then, by systole, propel their contents into the system at large while this is passive, as it always is in respect to the circulation. The motion of the artery is caused by, and synchronous with, the systole of the ventricle only."

The next proposition is, that "the arterial system is the measure of the heart's systolic impulse to the blood." The arterial blood being put in motion solely by the systolic jet as *vis a tergo*, it follows that the circulating force of the heart's impulse having to pass through the whole arterial mass of the blood, must be at zero in the capillaries;

accordingly, the capillary circulation is slow, indicating the heart's systole to be spent.

In the two following propositions the question of capillary action is further discussed. In the one it is asserted that "capillary power as a motor of the blood does not exist;" in the other, that "the capillary field is the limit of the heart's action on the blood."

The author next proceeds to show that "the capillary circulation is midway between the place where the heart's force ceases and that where another force commences." He asserts that "the fœtal thorax is a blood-ingestive apparatus."

"The thorax exhibits that mechanism at all periods of its development. The heart and the thorax have a synchronous development. As soon as the heart appears, having a definite form and function, the thorax encases it. The thorax, from its earliest phase of development to the period when, as in manhood, it assumes the full measure of its diameters, must, by reason of its structural form and its increase of capacity, be a self-exhausting apparatus; that is, it must of necessity cause a tendency to vacuum in its interior: it could not else be developed as a resisting recipient. And though this vacuum is never realized in it, this only proves the ever-existing *nisus* for effecting it, and the ever-existing origin of motion to possess the blood of vacuum. This we have now to learn is also the prime-mover of the venous current."

To prove the "blood-ingestive action of the thorax," the author adduces some ingenious pneumato-hydraulic experiments, to which the reader is referred. Mr. Maclise contends that it is independent of the act of inspiration, and "therefore the fact of the fœtal thorax not being as yet a respiratory apparatus cannot be held as a valid objection to the present doctrine—viz., that the thorax, on pneumato-hydraulic principle, is the sole prime-mover of the venous blood by induction, by ingestion." The anatomy of the fœtal thoracic organs, he says, agrees with the last proposition. It is next maintained that

"The blood-ingestive and the inspiratory acts of the thorax are parts of the same act. The thorax, being from the first an agent ingestive of the venous blood, and giving this fluid its returning course to the heart, the apparatus, as such, undergoing no change (excepting that of development) either in form or structure after birth, must be accounted as performing in this act throughout life."

According to the next proposition, "the blood-ingestive act of the thorax can be independent of the respiratory act." The conclusion of the whole is, that the thorax and heart are the two actors for circulation.

Fracture of the ribs—an accident of common occurrence—is briefly discussed. The author properly recommends sustaining the fragments by a bandage, and enjoining rest, so as not to excite the necessity for forced inspiration, which would give motion to the ribs. We consider the addition of shoulder-straps to the bandage to be advantageous. In the instance of fracture of the floating ribs, a bandage does harm.

As a bone naturally related to the thorax, the clavicle claims attention next in order. After describing the ordinary forms of displacement, the author objects to the plan of treatment which enjoins the retention of a pad in the axilla, and the continual exertion of an ex-

tending power, in order to prevent recurrence of the accident. "The pad, against which the head of the humerus forcibly bears as a lever for the outward traction and sustentation of the clavicle, compresses the axillary vessels and nerves against the humerus, and of this we know the consequences." He therefore suggests the employment of a crutch-shaped splint, of convenient length and form, on which the arm might be laid and bound by a bandage, which could, after the necessary traction was made in regard to the clavicle, be so placed as to bear against the side of the thorax, and be maintained in that position to exercise continued extension for the time required. We have found it a most difficult matter in practice to maintain the displaced bone in its proper position after reduction, and coincide in opinion with the late Mr. Vincent, that it is better to avoid all bandages, which, by constraining muscles, only excite them to undue action. All that is necessary is to place the patient on a flat bed, and let the arms be kept as quiet as possible. In reference to fracture of the clavicle, the author asks, "Why it occurs that a nice coaptation of the fragments of a clavicle is so seldom accomplished? It cannot surely nowadays be attributable to an ignorance of anatomy. It must, therefore, be owing to the inefficiency of that apparatus which is in general use." On this point we agree. In fracture of this bone the outer fragment is pulled downwards by the action of the subclavius and deltoid muscles, while the inner fragment is drawn upwards, sometimes almost perpendicularly, by the action of the cleido-mastoid muscle. Now it is not easy to devise an apparatus which shall control these opposing forces. We believe the best practice is to eschew the use of bandages, to keep the patient on a flat bed, and in about three weeks all will be well. Here the weight of the shoulder supplies an abiding means of extension. Mr. Maclise, however, proposes the use of the crutch-shaped splint above mentioned for adjusting fractures of the clavicle. "By extending the arm on it at a right angle with the body, and by bracing the arm to it while it is forcibly bearing against the side, the fragments can be drawn into apposition at the same time that the deltoid and trapezius muscles are relaxed, the subclavius muscle sufficiently stretched, and the outer part of the bone raised to the level of the inner." Mr. Maclise allows that the use of such an apparatus would look awkward, and would necessitate the horizontal position. Of course, this appliance remains to be put to the test of trial. We question much, however, whether it will supersede the simpler method above described. An individual in good health, who happens to meet with an accident of this kind, finds confinement to bed irksome enough, without the additional tax of having an arm bound up and kept on the stretch, at a right angle with the body, for a period of weeks.

The whole of the third, and the chief part of the fourth, fasciculus, are devoted to the consideration of the form and mechanism of the shoulder apparatus, of the scapulo-humeral joint, of dislocations and fractures of the scapula and of the humerus. After pointing attention to the analogues of the clavicle and scapula in the human skeleton, the author gives a clear anatomical description of these bones. He

observes, that the shoulder-apparatus moving on the thorax, is to be regarded as a muscular joint, for (with the single exception of the small sternal connexion) the opposed motor surfaces are muscular, and the muscles are the ligaments, which at the same time connect and act upon it.

"While we notice that the shoulder-apparatus is so weakly supported by osseo-fibrous connexion with the thorax, we find, however, that there is no other part of the skeletal fabric so powerfully bound to the body in all directions, as this apparatus is by muscles. Active connexion is here substituted for merely passive union, and whereas this latter condition could not exist compatibly with free and untrammelled motion, the former conduces to both requirements."

Dislocation of the scapula from the clavicle at the acromio-clavicular joint, is fully described. The nature of the accident is easily recognised, for the end of the clavicle projects subcutaneously above the acromion. The reduction may be readily effected, according to the author, by first making extension in the axis of the abducted arm of the patient, and then lifting the acromion on the head of the adducted humerus, at the same time that the end of the clavicle is being forcibly depressed. To effect the permanent retention of the parts in coaptation, which is attended with difficulty, the author gives the following directions—

"If, after the reduction, the arm be adducted to the side, and a bandage be passed over the end of the clavicle and under the elbow, it will, according to the degree of tightness exerted, keep the clavicle depressed; at the same time, that the head of the humerus is made to bear the acromion upwards and outwards. In this manner the force exerted will not be at the expense of pressure on any parts but those which can well afford to bear it."

Allusion is made to that peculiar displacement, in which the inferior angle of the scapula slips outside the upper margin of the latissimus dorsi muscle, and projects subcutaneously. This occasions deformity, and impaired action of the shoulder-apparatus. To effect reduction, the arm is to be drawn backwards, whereby the angle of the scapula rises on the back of the thorax, and by a little compression may be passed into its natural position, and there kept by a pad and bandage. Fracture of the scapula is next discussed. It may occur in any part of the bone. Thus the acromial process may be broken, the glenoid fossa has been starred with fracture, the neck may give way in two situations, one in front of the coracoid process, and one behind it, and the coracoid process itself may be fractured near its apex, at its middle or at its root. Fabricius Hildanus has placed on record cases in which he found both scapulæ fractured, through a fearful mode of torture practised in his time in Italy, and called *Tratto di corda.* The wretched victims were drawn up by the hands pinioned backwards, and heavy weights attached to the feet in order to enhance the torment.

The author gives a very full account of the scapulo-humeral articulation, which he views as a joint received within a joint, the one being as a socket to the other. The capsular ligament of this, as of all other joints, he regards merely as the periosteum separated from

structural connexion with the articular facettes of the bones, the interior being lined by a synovial membrane, a short sac corresponding always to the form of the articulation. "The *interior* of the capsular ligament is a state of *sides in contact.* There is no space in the interior unoccupied." The purpose of a capsule is not so much to act as a ligament, or mechanical bond of union to the joint, as to serve for knitting the joint on pneumatic principles. In the joint under notice, the capsule is remarkably loose, in order to allow of free motion. It is surrounded by bursæ, and perforated in various situations, each perforation corresponding to an overlying bursa. These bursæ, therefore, communicate with the interior of the synovial sac. From trials instituted on the dead body, the author is led to believe that in no case of complete direct luxation is the capsule to be found otherwise than lacerated on opposite sides. If this be not the result, then the dislocation has been either partial, or it has happened by a twisting of the head of the humerus out of its place. May not the head of the humerus be dislocated inwards and upwards in the direction of the coracoid process without the capsule being torn? We admit, indeed, that unless the capsule is completely torn through, dislocation of the head of the bone, in either of the three leading directions, cannot take place. A very ample account is given of the muscles of this region, and of their actions. The great pectoral and the deltoid are viewed as one, the deltoid portion of the muscle being abductive by its middle, and adductive, &c., by its anterior and posterior sides, while the pectoral part is altogether powerfully adductive, &c.

The three principal varieties of dislocation of the shoulder are fully described, together with the condition of the adjunct muscles, and the relative position of the vessels and nerves. An interesting remark is made under the head of dislocation of the humerus backward, in regard to an important nervous branch—to wit, the circumflexus humeri. It is in this dislocation that the nerve mentioned is most liable to be ruptured; and the occurrence seems to be owing to the circumstance that the nerve, in following the neck of the dislocated humerus, has to be tensely bent over the origin of the long head of the triceps at the inferior border of the glenoid fossa. What is true of the circumflex nerve must be true of its accompanying vessels.

In reference to the procedure to be adopted for reinstating the bone in its proper position, in the instance of dislocation downwards in the axilla, the author deprecates all measures of extension. He contends that such a position cannot exist unless by having already caused *extension.* The head of the humerus is below the level of the glenoid fossa, and the object of the surgeon is to raise it to the level of the fossa. He shows, from anatomical reasons, that extension of the limb can effect no good purpose in the existing state of the parts, but, on the contrary, may do much harm. The only effort which need be exerted is that by leverage:

"The head of the humerus requires only a motion upwards and outwards from the axilla to the glenoid fossa, and this is the direction of the rent which the bone, in leaving the fossa, has made. Through this torn passage alone

can the humerus be led back to its proper place. For this purpose a fulcrum is evidently required at the inner side of the neck of the humerus, so as to make this bone bear as a lever upon it. The proper motion of that lever on that fulcrum is adduction of the limb, for this implies abduction of the head of the lever, and, as good fortune would have it, this is the very motion of the head which can best relieve the axillary vessels and nerves from pressure. Of all other means for effecting this object, there is none so easeful, so effectual, or so natural, as by the operator's knee or naked heel in the axilla."

In the forward or backward luxation, extension requires to be exerted on the limb, and in either case it may be exerted without detriment to the soft parts. In reduction of the backward luxation, extension is almost the only motion that need be made; but in that of the forward luxation some leverage is necessary, after the head of the bone has arrived at the level of the glenoid fossa.

When the dislocation of the humerus is reduced, we would call the author's attention to the circumstance that sometimes a lengthening of the arm remains, which might mislead inexperienced practitioners into the belief that reduction had not been accomplished. According to Hyrtl, the highest point of the head of the humerus is, in the natural condition, some three or four lines under the acromion; the interval being occupied by the tendon of the supra-spinatus muscle, its adipo-cellular envelope, and a *bursa mucosa*. Where the dislocation has lasted some little time, these parts become swollen, and it follows that the head of the bone, after reduction is effected, cannot approach so near the acromion as in the natural state.

Mr. Maclise properly remarks that the most suitable time for the reduction of all dislocations is as soon as possible after the accident has occurred. For this he assigns various reasons besides that of the immediate ease of the sufferer. There is a circumstance connected with this subject which deserves a passing notice—to wit, whether the rent in the capsule can become so contracted as to offer a hindrance to reduction. This is a moot point. Pott, Sir A. Cooper, Boyer, and Malgaigne, deny the possibility of such an occurrence; while Bichat and Desault admit it. Monteggia has recorded a case which seems indisputable. He found, in the body of a man who died a few days after several fruitless attempts had been made at reduction, the opening in the capsular ligament so narrow that the head of the bone could only be restored to its normal position after dilatation of the opening by incision Pétrequin has adduced another case in corroboration.*

We may observe, in conclusion, that each fasciculus contains eight folio pages of letterpress and four plates in illustration, which are well and tastefully executed in lithography. The usefulness of the latter might, we think, be still further enhanced if the more complicated drawings were accompanied by outline sketches, as the introduction of tinting would probably render a higher price necessary. We augur favourably of the progress of the work, from what has appeared. We consider it well-timed, and likely to be acceptable to the profession, especially if the author will avoid the mannerism of style above adverted to.

* Gazette Médicale, p. 20. 1837.

Review IX.

Investigations, Chemical and Physiological, relative to certain American Vertebrata. By Joseph Jones, M.D., Professor of Chemistry in the Savannah Medical College. ('Smithsonian Contributions to Knowledge.' Published by the Smithsonian Institution, Washington, July, 1856. 4to, pp. 137. With 27 Woodcuts.)

There are few departments of physiological research that promise such interesting results as those in which the resources of modern chemistry are brought to bear upon the diverse circumstances under which life is carried on in the various animals. Merely to investigate the structure and organization of a number of unknown species, gains much for physiology. To learn their natural history—their habits, their climate, their food—adds still more, by vivifying (as it were) that anatomical knowledge which is, perhaps, always more living than its subject-matter would suggest. But to trace by chemical research the nature and amount of those changes that constitute the physical life of the animal, gives a third and complementary means of inquiry, which may almost be said to exhaust our methods of research, and to restrict all future inquiry to the inexhaustible (but narrower) objects of amplifying and correcting the information derived from these three sources.

Among the many countries which the next few years may fairly be expected to bring forward as contributors of such information, the *fauna* of America renders it one of the most promising. And whatever may be the deficiencies of the book we now introduce to our readers, it is impossible to avoid expressing the hope—or rather the belief—that it will have the merit of inaugurating a series of researches of this kind, such as this great country has unusual facilities for undertaking. Vast as are the strides made by American physic and physiology during the last few years, they have scarcely been commensurate with the advances it has made during the same period in other sciences. And this fact—which we suspect depends chiefly on the very indirect visible relation borne by physiology to the material prosperity of a nation—perhaps entitles us to assert, that nothing short of the careful cultivation of the higher branches of biological science by men especially affected thereto, can give America her proper position in this, the greatest (because the most useful) of modern sciences.

The chemical character of Dr. Joseph Jones's investigations is illustrated by a vast number of analyses, which indeed constitute the greater (just as they are scientifically the most accurate) part of the contents of his memoir. With laudable candour, he begins by detailing the method of analysis he has adopted in his numerous analyses of the blood; a proceeding which, always of extreme importance as a necessary condition for comparing the results of any two different chemists, is, if possible, even more essential in the chemistry of the blood. We are sorry to add that the practical usefulness of such

an explicit statement is only too well shown in the book before us; in which, with every wish to find our critical functions limited to a laudatory *résumé* of a very elaborate and interesting treatise, we have been obliged to come to the conclusion that the analyses which form its most important ingredients are so rough and inexact—not to say faulty—as scarcely to deserve the name "analysis" at all. It seems to us, in short, that the author's process of analysing the blood is one which is sure to present, in its results, errors the amount of which can scarcely be estimated, but which are sufficient to destroy almost all their usefulness.

The solids of the blood are determined by evaporation of this fluid in a chloride of calcium bath, at a temperature which seems rather a high one—namely, 220° to 230° Fahr. Such a temperature at least would entail a loss of substances which, if not ranking as solids, are perhaps too important to be altogether overlooked, such as ammonia and fatty acids. In any case, however, this would hardly be a material error. But this process is repeated for the serum; the solids of which per 1000 parts of blood are next calculated on the extraordinary assumption, that the water of the blood "exists wholly in the form of serum." Further, "the amount of albumen and extractive matters in 1000 parts of blood may be determined by subtracting the saline matter of the serum from the solid residue of the same, as determined by the above calculation." The dried corpuscles are also determined by calculation, by subtracting the fibrin and the solids of the serum from the entire blood-solids. To obtain the moist corpuscles, the author merely multiplies the dry by four, a formula which (while we quite share his respect for Schmidt, its inventor) we must say is, on the very face of it, very doubtful, when perverted from a convenient summing up of some observations in the higher Mammalia to a means of inquiry in the opposite extremity of the Vertebrate series. We presume the salts of the corpuscles are obtained by subtraction also, as are confessedly the dried organic constituents of the serum.

It is surely unnecessary to comment upon such a method of research, or to point out how the unavoidable errors of its first steps, instead of being checked and exposed by its next, are added to, and every now and then multiplied, by those that follow, until the figures expressing the results become almost valueless—utterly valueless, at any rate, as regards those smooth and perfidious decimals that stand at the right hand of the dominant whole numbers, like the fawning parasites of an Eastern court. No wonder that our author's tables are numerous, and their totals as neat and perfect as those in an audit of a Royal British Bank! All that we can say is, if this be analysis, every man who separates the coins accidentally mixed in his pocket, into gold, silver, and copper, has conducted a process possessing nearly equal claims to a scientific character, and (like M. Jourdain talking prose so many years without knowing it) is an unconscious analytical chemist.

Another prominent and even systematic fault of the book is, that it mixes with valuable original observations a large quantity of physiological doctrine, such as would be only excusable in a loquacious

member of a small medical society, or a student newly "ground for the College." Here and there may be found statements objectionable chiefly because out of place: solids (if we may use the comparison) derived from well known text-books, and imperfectly suspended in much water. Much allowance ought of course to be made for that fatal wish to be striking and eloquent, which is said to be epidemic across the Atlantic. But authors other than American sometimes require to be reminded that dilution and admixture, which are bad taste in poetry and narrative, are something much worse in strict scientific inquiries. Especially, the details of a series of observations are positively injured by the interpolation of doctrines, which may be useful without being probable, and probable without being true. Unconsciously perhaps to the reader, they impair the exactness of his comprehension, if indeed they do not lower the credibility he attaches to the whole. And hence, when we find such phrases as "unstable arrangement of atoms," and "feeble states of vital force," side by side with per-centages, and other details elicited by careful research, we cannot help wishing that the author had remembered the advice given to the lazy waiter in the jest-book by the angry *gourmet*—had put the flies and the soup into two separate dishes, leaving those of his intellectual guests who liked them mingled to mix for themselves.

But it is hard to see so much ability and research thrown away, without making an effort to save every possible fragment. And before passing on to notice the valuable and suggestive facts which lie scattered through these Essays, it is impossible to avoid inquiring whether the above general inaccuracies and oversights in our author's method of analysis can be tracked in such a way as to receive any correction, however vague or conjectural. Without answering such a question with a direct affirmative, we may point out two of his most important numerical estimates, the errors of which are not only marked, but almost measurable. As already stated, our author assumes that the water of the blood exists wholly in the form of serum. Taking the latter words in their strict sense, they would imply that the moist blood-corpuscles were anhydrous. But as this cannot have been intended, we infer that what Dr. Jones really wished to express was, that this water was present in *a* serum, which soaks the contents of the corpuscles. It is hardly necessary to call attention to the more than equivocal title of such a fluid to the term "serum;" or to say that, though of course it has never been isolated in sufficient quantity for any real analysis, we are fully entitled to assert that its composition is utterly different from that of the true serum—from that of the intercellular fluid of the blood deprived of its fibrin. Indeed, there can be no reasonable doubt that the cavity of the blood-corpuscle is occupied by a plasma of great richness and density—one which probably, in both these respects, as far transcends the *liquor sanguinis* (*à fortiori* the serum), as this latter does the average nutritional fluid of the tissues, if we may use so vague a term. Calculating in this way the solids of the serum in 1000 parts of blood, our author therefore adds to the true serum a fluid quite distinct from it. He thus

estimates the solids of the serum at far too high an amount. While, on the other hand, since he subtracts the mixed solids of this compound "serum" from the total blood-solids, to give the solids of the corpuscles ("dried corpuscles"), it is evident that he estimates these at as much too low an amount.

To determine the precise amounts of excess and deficiency respectively, is of course impossible. Indirectly, however, they may be guessed at. For it is evident that what one gains the other loses; that the nominal albumen and extractive of the so-called serum contains what is really part of the deficient blood-corpuscles. So that, for example, if we could assume any maintenance of the ordinary proportion between the two in healthy human blood (about one part of solids of plasma to three of dried corpuscles), it would be easy to give a formula for reducing our author's numerous results to the analyses of other chemists. But this is precisely what we cannot do. It is probable that the proportion of solids of the corpuscles to solids of the plasma, varies extremely throughout the Vertebrate series. And though there seem evident indications that this variation is mainly at the expense of the plasma—in other words, that the materials of the corpuscles, perhaps even the total bulk of these bodies themselves, is a far less fluctuating quantity than the solid residuum of their liquor sanguinis, which is disproportionately poor in albuminous materials throughout the lower orders of the Vertebrate class,—still this fact little assists us. And even in the Mammalia, these proportions seem to have wide variations. Thus the blood of few of the higher Mammalia approaches more nearly to that of man than the blood of the dog. And yet, while some of the best chemists of modern times (especially Schmidt and Lehmann) assign to the solids of the corpuscles and plasma in man an amount of 156 and 48·5 respectively—a proportion nearly that of three to one above alluded to—these numbers in the blood of the dog appear to be about 151 and 75, or two to one respectively;—a contrast in favour of the plasma of the lower animal, such as may perhaps be referred to its carnivorous diet, but assuredly not without some hesitation and reserve.

Hence, with no definite proportion between either the quantity or the residuum of the serum and corpuscles, such as might be supposed approximatively constant throughout the classes of animals whose blood has been examined by our author, it is impossible to offer more than the general statement, that the albumen and extractive of the *liquor sanguinis* are systematically represented above, and the dried corpuscles below, their true quantity. It may, however, be conjectured that the amount of these two errors is diminished in the Fishes and Reptiles, in whom the probably greater difference in the quantity of these two residua renders the similar errors less influential; or, in other words, the poorer plasma deducts less from the comparatively less altered corpuscles.

These remarks appear to be fully confirmed by comparing two estimates given by our author, of the blood of a dog in the ill-fed and well-fed states, with the results of other observers. Careful analyses by

a variety of chemists, especially Andral, Berthold, Delafond, Denis, Dumas, Gavarret, Nasse, and Prévost, afford an average (deduced from about thirty instances) of the amount of the dried corpuscles in this animal; and a similar, but smaller, series of observations gives us the quantity of solids of the serum.

Analyses of Blood of the Dog.

	Solids of corpuscles.	Solids of serum.	Water.
Other observers' average	151·13 (124 – 182)	... 75·(65 – 76)	... 774·87
Dr. J. Jones's average .	72·	... 111·26	... 816·74

Contrasting the former with the estimates calculated by Dr. Jones, we are at once struck by their complete reversal of the proportions obtained by this gentleman. And while we thus find the calculations of Dr. Jones exhibiting precisely the discrepancies above deduced from a consideration of his method, it is interesting to notice that the very amount of these discrepancies seems to be also explained. Allowing for the rather unusual quantity of water present in the blood of the dog he examined—a circumstance which might easily be due to the idiosyncrasy or habits of the animal—the discrepancy of the two series is exactly removed, by supposing that the serum-solids of the latter include so much of the solids of the corpuscles as would correspond to a nearly equal quantity of these cells; in other words, to anything approaching such a quantitative mixture of corpuscles and liquor sanguinis as obtains in the human subject. Thus, $72\cdot + 111\cdot26 = 183\cdot26$: and $\frac{2}{3}(183\cdot26) + \frac{1}{3}(183\cdot26) = 122\cdot17 + 61\cdot1$, numbers which have nearly the same proportions to each other as the above 151 and 75.

But though Dr. Jones's analyses seem to us quite wanting in that accuracy* necessary for throwing much new light on the important subject of the comparative chemistry of the blood, and though for a similar reason they cannot even be connected and compared with those of preceding chemists, it would be wrong to overlook the information they bring on many points. Of these, some of the most interesting seem to be the following:

As regards the *quantity* of the blood, he shows how inapplicable are the methods suggested by Valentin and Lehmann; the first being the results of injecting a given quantity of water on the specific gravity of the blood; the second, being the quantity of grape-sugar requisite for its escaping decomposition in the blood, so as to be eliminated by the kidneys. He adopts the simple expedient of holding the animal's body perpendicularly downwards, with extended neck, after having divided both jugulars and carotids; and sagaciously adds, that this method is peculiarly adapted to cold-blooded animals, from the fact that their hearts continue beating for many hours after almost all blood has been abstracted. In this way, he found that the quantity

* We pass over many lesser inaccuracies which may be charitably presumed to be errors chiefly of expression. For example, "If we multiply the *number* of dry blood-corpuscles by four, we obtain the *quantity* of fresh blood-cells." The errors of orthography are probably the printer's. But in a rather sumptuous essay in *quarto*, such phrases as "liquor potassa," "aqua ammonia," have a strange appearance.

of blood contained in the Chelonian and Ophidian reptiles he examined, amounted to a fraction of one-tenth to one-seventeenth of the weight of the body; or, approximatively, to a proportion of the total bodily mass scarcely half that found in the higher Mammalia.

In respect to the fibrin, it is curious that, while our author's facts tend decidedly to confirm the views of Zimmerman respecting its subordinate (or rather excretory) import, his opinions appear to be those still retained in most of the physiological text-books of the day. The strong arguments brought forward by Simon in this country,* seem scarcely to have found any favour in his eyes, even while his observations repeat some of their most important statements in a newer and weightier form than they have ever hitherto been brought forward.

Thus, on inspecting the mere amounts of fibrin in one thousand parts of blood, as determined by Dr. Jones in some sixteen Birds, Reptiles, and Fishes, we are struck by the wide range they offer. Excluding three instances in which its quantity and solubility prevented any definite estimate being made, the minimum and maximum of fibrin vary as widely as ·35 and 5·73 respectively, and correspond with two Chelonian animals, the Snapping turtle (*Chelonuria serpentina*), and the Gopher (*Testudo polyphemus*), in whom the different diet (carnivorous and vegetivorous respectively) appears to be the only great nutritional fact of correlative importance; while their structure seems to differ in scarcely any other respects than in those peculiarities of the digestive apparatus which we should expect to find varying consonantly with this difference of alimentation. Without assuming that the blood of the vegetivorous reptile really contains sixteen times the quantity of fibrin of the carnivorous one, or that the difference, if in no degree accidental, may not be somewhat compensated by other peculiarities of the blood—it is strange to find the fibrin-eating animal suddenly degrading (if not disposing of) the vast surplus of this principle it habitually eats, while the creature by whom this and the other protein-compounds are obtained in scanty (and probably but necessary) amounts, exhibits a large proportion of what is supposed to be the highest result of the ingestion and elaboration of nutriment.

In the Fishes and Batrachians examined, the quantity of fibrin was scarcely to be determined from its unstable character, it being redissolved in the blood with great rapidity after its first deposition. This and facts of the same kind, which have also been noticed by other observers, confirm the opinion of Mulder and others, that fibrin has little claim to be regarded as a specific compound, identical in composition as it exists in different species or individuals. They show, indeed, that it merges into albumen by what are probably innumerable gradations; and that it is often so utterly incapable of isolation from albumen, that not only is any quantitative estimate inaccurate, but that a scanty deposit of fibrin is likely to be sometimes overlooked altogether.

These, however, are by no means the deductions which the same facts suggest to Dr. Jones, who impresses upon his reader, with obtru-

* Lectures on Pathology, p. 49. London, 1850.

sive italics, that "from this table we learn that the fibrin constitutes a remarkable index of the vital, organic, and intellectual endowments of animals."

With respect to this opinion it need only be said, that, whatever view we take of fibrin, to whichever extreme of nutritional life we regard it as pertaining; whether it be the very pinnacle and climax of blood-life (as is suggested by Carpenter and Paget) or its decay and obsolescence (as is taught by Zimmerman and Simon) there is little difficulty in understanding why it should generally vary with that intensity of nutrition which constitutes completeness of organization. But in proof that its larger variations in the various species are based upon some other law, some more specifically nutritive phenomena, we could hardly find any better evidence than this very table. Is the Gopher (*Testudo polyphemus*), whose blood contains one-seventh more fibrin than any other animal mentioned here, and nearly twice as much as the blood of man, the physical and intellectual "Lord of the creation?" Or, if so, is it judicious in Dr. Jones to publish such a statement in a country where Gophers abound; and, considering their vital and intellectual endowments, no doubt read all that passes through the press? Or does our author, like Buckland in the geological ballad, rely on their herbivorous tendencies? But alas, on reconsidering the matter, this view of their superiority, humiliating as it must be to a member of the hitherto dominant race, is corroborated by the traditions of man (their usurper) in many countries. The *Unamis* of the American Indian; the tortoise of the Hindoo legend, sustaining the world; the "love of the turtle," in ancient Greece; as well as in modern London, where its entombment in a kind of sarcophagus called an alderman is still one of the most solemn duties of our admirable system of municipal government—all these, and many more, suggestive facts start into light at the electric flash of our author's inspiration. Nor do we think anything can protect us from a *Gopherocracy*, unless, in the struggle for power, the human race should acquire a fibrinous supremacy by their sufferings, —by the bloodshed and inflammation consequent upon the horrors of such a servile war as this indiscreet book is calculated to provoke.

The odour of the blood is alluded to as depending chiefly on the serum, and as producible with great distinctness by applying to this fluid Barruel's test of heat and sulphuric acid. We may remark, however, that, whether dependent or no on the fatty acid usually assumed to be its chief constituent, there can be little doubt that the odorous principle peculiar to this or that species pervades the whole body of the animal, and may be found in most of its secretions. It may certainly be detected in the fæces, in the pulmonary and cutaneous transpirations, and in the special stink-glands which in some creatures are developed as offshoots of the cutaneous or intestinal surface; and it is pretty distinctly to be recognised in the milk, the bile, and the urine. Furthermore, it appears to exist in the muscles—so far as can be judged, quite independently of the blood generally retained in their mass — thus forming the peculiar flavour of the different meats,

however modified by cookery. That it should be present in the serum is therefore by no means surprising; while, at the same time, the fact does not suffice to refute the opinion entertained by some anatomists, that, on general as well as special grounds, we must seek for its source in the structures, which are, κατ' ἐξοχήν, the blood—namely, in the blood-corpuscles. The curious distinctness with which the odour may be developed in dried and comparatively stale blood, seems to confirm this opinion.

On the specific gravity of blood and of the serum in different animals, the author gives us two tables, which lead him to a conclusion which we are scarcely inclined to contradict; though we must really doubt whether it is deducible from the facts he offers. "From this table we learn that the blood becomes more concentrated as the organs and apparatus and intelligence of animals are developed." Considering that we find in it a precisely identical specific gravity for the blood of an alligator, a cur-dog, and a pregnant woman, we really think the proposition can scarcely be enunciated, except on materials somewhat different from those Dr. Jones affords us; and are entitled to hope that a deputation of some society for promoting the "Rights of Women" will join us in begging their compatriot hereafter to base this proposition on less invidious or more indefeasible evidence. Perhaps, however, mere philoprogenitiveness is regarded by them as so incompatible with "woman's mission," that they would only consider the above statement as an awful warning against stooping to domestic life.

In point of fact, to establish any broad propositions of the above kind, we require a far more extensive and accurate series of observations than any as yet at our disposal, not to say some more accurate and definite measure of the development of "organs," "apparatus," and "intelligence" in the various animals. That some such general rule will be found, we may fairly expect; but meanwhile, no *à priori* arguments entitle us to a premature generalization on insufficient facts. And while in such tables as those of our author we may see the specific gravity of both the blood and the serum increase in passing from a lower to a higher class of animal, the rule is interrupted, or even reversed, for occasional species. In like manner, it is difficult to find numerical grounds for assuming that the increase is chiefly due either to corpuscles on the one hand, or to serum on the other. Furthermore, the various sizes and shapes of the corpuscles make number and quantity anything but convertible terms; so little so, that we may imagine the same total corpuscular mass arranged to form an organ of very different efficiency, according as it is collected into a smaller number of large cells, or spread over a wider working surface by separation into a larger number of small cells. Finally, as regards the quantity of corpuscles, we have already adduced reasons for believing that the author's estimates are not only of necessity erroneous, but that the amount of error differing in the different instances (the quantity of corpuscles approaching accuracy much more nearly in the lower animals examined), they are not even comparable among themselves.

The author next investigates the effect of acetic acid, carbonic acid gas, carbonic oxide, and hydrogen, in altering the shape of the blood-corpuscles. He incidentally mentions the interesting fact, that both carbonic acid poisoning and asphyxia by ligature of the trachea caused the urine of the Chelonians (*Emys serrata* and *Emys terrapin*) examined to contain grape sugar, which is normally absent from this secretion.

The effects of starvation and thirst on the blood are next traced. The subjects of these observations are two alligators; a series of the *Emys terrapin*, *Emys serrata*, and *Testudo polyphemus;* and lastly, a single cur-dog. Here again, however, the method of inquiry adopted precludes much stress being laid upon any of those details, which would otherwise have been precisely the most interesting. Indeed, the chemical features of the inquiry could scarcely have supplied much definite information, owing to the difficulties, inherent and circumstantial, of the physiological elements.

Thus, starvation and thirst are associated in all the experiments—an association which, in our opinion, ought to be checked by an occasional comparison with each separately, in instituting such experiments upon any particular species of animal. Indeed, mere death by thirst supplies little useful material for physiological deduction. Then, again, to compare the blood of one individual of a species with that of another, gives us little right to conclude the precise amount of effect produced by the starvation of one of them, until we have some clue to the healthy deviations due to idiosyncrasy, age, sex, or circumstances. Again, if we bleed an animal repeatedly, as in the case of the dog thus experimented on, we are adding a further complexity to any difference of composition afterwards observed in its blood, as well as an additional cause of death, likely to be very effective in an animal deprived of food. Lastly, in the Reptiles a similar, but even greater, imperfection attaches to these observations—namely, the process of death by starvation and thirst was never observed at all, the animal being slain when still living, often when still vigorous. Indeed, in the Gopher (*Testudo polyphemus*) this imperfection so far reaches its maximum that we may really question whether the creature can be said to have been starved at all. This animal, our author informs us, inhabits a barren sandy country, where vegetation is scarce, and it is often impossible to obtain water. After fifty-one days of abstinence, a Gopher retained life, activity, and a capacity for considerable muscular effort. On killing it, its colon was found occupied by a considerable amount of vegetable matters, which were saturated with juices, and incompletely digested! If any one of our readers can allow for all these sources of error, and reduce our author's various estimates of the blood, to such as shall be comparable with the many admirable researches of the effects of inanition already before the scientific public, we beg to suggest his publication of the formula that effects such a desirable translation. In the meantime, we confess our utter inability to grapple with facts like these, save in the vague form in which they have long been known to physiologists. We quite believe our author when he assures us that

deprival of liquid aliment diminishes the amount of water in the body; that this effect occurs more slowly in cold-blooded than in warm-blooded animals; in active than in passive individuals; that it diminishes the blood-corpuscles, and still more the fat. But to deduce these propositions from a series of analyses like those here placed before us, recalls to mind the philosophers of Laputa, who drank tea by stratagem, and cut coats on principles of the strictest geometry. Similar objections might be taken to much that we find in the remaining pages of this contribution to physiology. They, too, teem with generalities always out of place—often utterly unjustifiable. A tabular statement of the proportionate weight of the heart to that of the body is no doubt interesting; though we do not think any such statement likely to throw light upon the mysteries of the circulation, until much more is understood upon some recondite points equally influential in the mechanism of this movement. No doubt the accumulation of materials is in itself a task of great importance, the right fulfilment of which deserves something more than praise. Still, when we find the frequency of the heart's beat in different animals taken as an index of the rapidity of the circulation, or chyme spoken of as a solution of albuminous matters, we doubt whether the book which contains such statements may not do harm as well as good.

Here, however, we may end a criticism which, though unsatisfactory, fulfils what we believe to be the duty of a scientific Review, when its attention is claimed by a series of researches so large, so laboured, and so pretentious as those we have in part analysed. We do full justice to the zeal and industry of Dr. Jones, and trust our readers will quite understand that this handsome quarto *brochure* contains scattered throughout its 140 pages many interesting and even important facts; many illustrations of physiological principles in animals hitherto little subjected to research. We may add, moreover, that it carries with it intrinsic evidence of much more conscientious and painstaking accuracy than our remarks might seem to imply. It is not so much that Dr. Jones shuns any trouble, or distorts any facts to sustain his deductions; it is rather that he wants that wide knowledge of his subject, and that firm grasp of its essentials, which are so valuable in an inquiry of this kind. Still more, perhaps, are his defects attributable to his having attempted too much. A series of observations cannot be made a physiological treatise; cannot even be successfully worked out so as to elicit a supply of new and useful facts, save by the inquirer confining himself strictly for the time to a few rigid *quæstiones naturæ*, arranged so as best to elicit a connected answer. And if (as this fault entitles us to suspect) Dr. Jones is either young in years, or unaccustomed to this kind of research, we venture, while exercising what we believe to be a duty at his expense, to assure him how gladly we should hereafter unsay whatever may appear disparaging to him, should the opportunity be afforded us. He may rest satisfied that the subjects of research he has chosen are invaluable to the sciences he professes; and that, with less haste and more prudence, his ability and industry may conduct him to a high eminence in physiology. In the

meantime, it is due to the great nation of which he is an unit, and to the admirable series of Smithsonian Contributions to Knowledge among which his researches are enrolled, to measure them by the ordinary standard of criticism. It is because America is taking up her proper position as a province in the great republic of science, and as such, putting forth works which may fairly rank as equal to any of the products of modern intellect in most of the departments of knowledge, that we feel the day is gone by for any affectation of measuring an American book by a different standard from an European one, for omitting all blame, and in bitter but true phraseology, "Damning with faint praise."

Review X.

Mémoires de la Société de Chirurgie de Paris. Tome Quatrième. *Paris*, 1856–7. 4to. pp. 281–722.
Memoirs of the Society of Surgery of Paris.

In continuing our account* of the fourth volume of the Transactions of the Paris Surgical Society, we next come to a paper on—

I. *Acute Sub-periosteal Abscess.* By M. Chassaignac.—He believes that the subject has not had sufficient attention paid to it; indeed, so little has it been dwelt upon, that, joining his own cases to those contained in the best works on periostitis, he has been enabled to collect only eleven instances, excluding as he does from consideration periostitis occurring after amputation, and syphilitic periostitis. This paucity is in some measure due to the inexactitude of the discrimination of some writers preventing him utilizing their observations. Although these abscesses do not exist independently of periostitis, he has not found this the initial phenomenon in all cases, and therefore he has not wished to exaggerate its influence by adopting it as the designation of the affection. Sub-periosteal abscesses may be defined as collections of matter forming between the bone and periosteum in the course of some days or weeks, frequently attaining a large size, and being accompanied by severe general symptoms. They have occurred hitherto exclusively in early life, and generally in scrofulous subjects, the bones of the lower extremity being those which are most liable, and especially the tibia.

Of the *symptoms*, *pain* is that which precedes all others; and it is severe and excruciating, resembling in character that of panaris. It is deep-seated and localized, commencing when the abscess affects the femur at its lower part. There are then severe pains in the knee-joint, although the abscess never extends, as it does in osteo-myelitis, into the joint itself. By its nocturnal exacerbation, the pain bears some resemblance to syphilitic pain; but such exacerbation, in point of fact, when observed in syphilis, is not due to that disease, but to the implication of bony or periosteal tissues. The radiating of the

* Vol. xix. p. 159.

pain along the shaft of the bone is not an essential feature, as it is in osteo-myelitis, but an indication of the progress of the disease and the greater detachment of the periosteum. The *swelling* observed is never accompanied by change of colour. At first limited to the spot where the affection commenced, it may become enormous, the thigh in some of the cases acquiring double its normal size. In osteo-myelitis, such enlargement is not attained, the greater rapidity of its course giving rise to a fatal termination, or rendering amputation necessary. A peculiarity of the tumefaction is, that it makes one, as it were, with the bone at the surface of which it exists; at least, this is observed at the commencement of the affection, and especially at the front of the tibia. As the disease advances, tumefaction increases, and the limb becomes œdematous, and at last great tension and redness of the parts are produced; but the pain, though considerable, is not of the same acute character as when it was confined to its original seat. A tumefaction persists after the disease is nearly or quite cured, but in place of being soft, elastic, and fluctuating, it is hard and compact, being due to the secretion of layers of osseous matter by the periosteum during reparation. Fluctuation, easily detected over a superficial bone like the tibia, is perceived with difficulty in the case of a deep-seated one, as the femur. Detection is best accomplished, not by applying the fingers flat upon the limb, but by grasping the upper part with one hand, and the lower part with the other, and submitting it to alternate compression by each. The quantity of *pus* is usually proportionate to the amount of detachment of the periosteum, and is in some cases enormous; but when the incisions have been sufficient, and the patient's constitution is not very bad, it usually gradually decreases in amount. It is characterized by the presence of oily globules, and by fœtidity, even when the incision is first made. The detachment of the periosteum, the prolonged suppuration, and the dry sound heard habitually in necrosis, combine to prove that in acute sub-periosteal abscess, more or less of the surface of the bone is affected with superficial *necrosis*. In general, the lamellary sequestra that result are discharged with the pus, or they may disappear under the influence of acidulated lotions or by resorption, except, indeed, when the bone becomes deeply necrosed, when a permanent sequestrum may result. The extent of the denudation of bone is proportionate to the size of the abscess, and is found to be far greatest in the case of the femur. The *general symptoms* are those which are met with in affections severely influencing the general economy. There is intense fever, the pulse rising to 140, and never being lower than 120, as long as the abscess remains unopened. It is small and feeble, while the face is pale, but sometimes flushed. The skin is hot, and great debility and emaciation are present. These symptoms, so serious at first, always undergo amendment after incision; but should there arise obstacles to the free discharge and cleansing out of the abscess, the alarming condition returns, and the patient is soon reduced to the last extremity.

Anatomical Lesions.—In the observations that have been related,

attention has been almost exclusively confined to the condition of the surface of the bone, and none paid to that of the medullary cavity and the spongy extremities. It is rare to find the detachment of the periosteum extending around the whole circumference of the bone, some points usually yet retaining their adhesions. It loses its transparency, and becomes thickened, vascular, and friable. When the abscess is situated on the cranium, it is accompanied by a corresponding detachment of the dura mater. In the author's opinion, this is sometimes not due to a propagation of the disease to this membrane, but to the fact of an affection of the bone being reflected upon the two membranes. In almost all cases of sub-periosteal abscess, necrosis of the bone takes place where it is in contact with the pus, and sometimes the diseased process is propagated as far as the epiphyses, even inducing their separation. As a consequence of the abscess, we always meet with bony tissue of new formation.

Complications.—The author relates two examples of these—in one of which an articular abscess, and in the other osteo-myelitis, constituted the complication. In former researches of his on osteo-myelitis he had always found that that affection gave rise to sub-periosteal suppuration; but he has not found sub-periosteal abscess playing the part of exciting cause of osteo-myelitis. In a case furnished to him by Dr. Foucher such sequence seems to have been observed.

Diagnosis.—The affections with which acute sub-periosteal abscess may be confounded are—very intense phlegmon, diffuse phlegmon, and osteo-myelitis; and the diagnosis is sometimes a matter of difficulty. Fortunately this is not of much consequence as regards treatment, inasmuch as the indication in each affection is the performance of prompt incision. Nevertheless, it is always desirable to establish a prompt diagnosis; and in aid of this it is to be observed that in respect, 1, to *phlegmon*, the pain is less excruciating, the phlegmon does not form, as it were, part with the bone, the general system suffers less, and the pus more promptly reaches the surface. 2. The sub-periosteal abscess is distinguished from *diffuse phlegmon* by the absence of œdema and change of colour of the skin; by the fluctuation being well circumscribed; by the less diffused form of the swelling; and by the more severe and more localized character of the pain. 3. As contrasted with *osteo-myelitis*, the fluctuation precedes the œdema; the painful œdema accompanying osteo-myelitis terminates by a hard and projecting border opposite the point where the disease of the bone ceases to extend to; in sub-periosteal abscess there is neither medullary suppuration of the bone, nor a general purulent infiltration of the whole limb; and while in osteo-myelitis the disease is propagated from one bone to another, passing towards the root of the limb through the articulations, which become purulent, sub-periosteal abscess generally remains confined to the portion of the limb in which it commenced. If it affect other parts of the same limb, it does not do so by direct propagation, but by distinct simultaneous or successive attacks, generally, too, leaving the joints uninjured.

Prognosis.—This must be looked upon as a very serious affection,

not that it directly menaces life, or even the loss of the limb; but because—let it go on as well as possible—confinement to bed for months must take place, and there is always a risk of the various complications which suppuration in contact with bone may give rise to. The gravity of the affection is, however, more than ordinarily dependent upon the mode in which treatment is carried out. If active and well comprehended, the most alarming symptoms may be dissipated; while when put feebly into force, death may result in either the primary or consecutive periods. The unhealthy condition of the patient's constitution, frequently the subject of scrofulous diathesis, is an unfavourable element in the prognosis. Five out of the eleven cases employed for the construction of this memoir have died, surgical interference in several having, however, only been sought for at too late a period to be of any service.

Treatment.—In a general manner this may be said to consist in—1. The employment of prompt incision; 2. The insertion of a Y-shaped canula, or a perforated seton; 3. At a later period the injection of acidulated douches; 4. Resort to amputation when the rapidity of the accident or the debility of the patient prevents further temporizing; and 5. The employment of various operative manœuvres when the sequestrum is too solid to yield to the acidulated douches. The opening and counter-openings must always be sufficient for the free discharge of the pus, but should not be excessive. Such discharge cannot, however, be secured by merely making apertures, the large putrid surface requiring also abundant washing out by water, freely injected by means of a pump or syringe. In the author's practice this is indeed adopted in all abscesses, as the means of freeing their cavities from the pus, which he regards as acting like a foreign body. At a later period acidulated douches are substituted, M. Chassaignac preferring the hydrochloric acid to any other, and commencing with the proportion of two grammes to a litre of distilled water, afterwards increasing the strength. For the purpose of facilitating the free discharge of pus from these and other abscesses, he employs what he elsewhere calls "drainage," by means of vulcanized caoutchouc tubes. Taking one of these, of the size of a quill, and from 12 to 15 centimetres in length, the extremity which is to enter the wound is cut into the shape of the reed of a flute. The other end is slit up into two, and when the tube has been introduced, these two branches are turned back over the surface of the skin, and secured by plaster. In this way the canula takes the form of a T or a Y. When a counter-opening has been made, the canula is replaced by the perforated seton, which establishes a permeable uninterrupted channel from one incision to the other.

II. *On the Treatment of Varices and Hæmorrhoids by Injection of the Perchloride of Iron.* By M. DESGRANGES.—This is the successful essay for a prize upon the above subject offered by the Society. The author doubts the applicability of the treatment to hæmorrhoids; and the following are the conclusions (founded upon 19 cases) he comes to with respect to the employment of the remedy for varices:

"1. The injection of the perchloride of iron into the veins is an innocent but an extremely delicate operation, that requires every care of the surgeon with respect to precautions and manual procedures. If well performed, the consequences are simple and easily managed. 2. Baumé's solution at 30° is that best adapted for varix. 3. Two drops suffice for the coagulation of all the blood contained in the largest varicose lobules. To inject more would be both dangerous and useless. 4. The injection must not be made unless blood issuing from the canula proves that we have entered the vein. 5. Only one injection should be made at a time; an interval being then allowed, and a spot next selected as remote as possible from the last injection. 6. We may, exceptionally, throw in an injection into each leg, when the two are subjects of varix, on the same day, if the patient is in good health. 7. We must pursue the varices as long as the veins are penetrable; so that the mean duration of treatment cannot be assigned. 8. As regards the pain, the accidents, and results of the operation, injection is preferable to other means of treatment. 9. The obliterated vein always remains impermeable to the blood; but we are unable as yet to pronounce on the curative value of this treatment. 10. The injection exerts the most excellent effect on the varicose ulcers, changing their nature, and giving rise to their rapid cicatrization." (p. 409.)

III. *On Amputation at the Ankle Joint.* By M. Verneuil.—The conclusions stated in this paper are chiefly based upon the examination of several stumps, which its author has made at a considerable period after the operations have been performed. One of the rules he lays down from having observed osteitis set up when the malleoli were left entire, is that they should be always more or less excised. Pressure on pointed eminences like these, may indeed give rise to the formation of adventitious bursæ, but such serous sacs easily pass into a state of inflammation. Another rule, applicable in the author's opinion to many other amputations, is couched in these words—

"Whenever the extremity of a stump is destined to support a direct continuous pressure, procedures by flaps are to be rejected, if the inflexion of these will place the great nervous trunks in such a position that their terminal enlargement would have to support such pressure; such procedures being, however, allowable on the condition of excising to a certain extent the nervous trunks that may give rise to these inconveniences." (p. 443.)

After passing an unfavourable criticism on M. Pirogoff's operation, M. Verneuil makes the following observations:—

"In following out the principle, that we should be as economical as possible of the hard parts in practising mutilations of the hand and foot, the catalogue of the procedures for partial amputation of these two important regions, is increased day by day by new operations, by which an apophysis, an articular extremity, &c. is preserved. The intention is certainly good, and the execution is sometimes crowned with success; but even good principles must not be pushed too far. Thus, for example, this opulence in operative procedures is of scarce any avail in traumatic cases, for in gun-shot wounds and in partial crushing of the hand and foot, experience shows that the best practice consists in leaving the cure to nature, favouring it by continuous irrigation. The surgeon remains a mere spectator of the process, or he contents himself with simply intervening by the excision of certain osseous points, or by the removal of splinters. This practice, so humble, but so fertile in good results, has become so common, that partial amputations of the foot and hand become more and more rare in our hospitals. Operative parsimony, it must be admitted, is sometimes mischievous in organic lesions; and when we have to do with cancer or fibro-

plastic tumours of bone, if amputation is considered advisable, we must make large sacrifices of the diseased parts, and cut into those that are healthy, in order to guard as much as possible against local relapse. In necrosis, we should be content in most cases with the removal of the sequestra. When osteitis or caries is present, we often find the disease return in portions of the bone which at the time of the operation concealed the germ of the disease. To sum up: the conservative principle pushed to excess, is frequently useless, if not hurtful. It is by keeping the patient in sight for a sufficiently prolonged period, that we may convince ourselves of this; and great would be the mistake to register as cures all those cases in which the patient quits the hospital after the cicatrization of the wound made by the hand of the surgeon." (p. 433.)

The following are the general conclusions of the memoir:—

"1. Tibio-tarsal amputation is not a very grave operation, as far as the immediate mortality is concerned; 2. Healing is always slow, and on this account the total duration of the consecutive treatment is longer than in most of the major operations; 3. The employment of the stump should be long delayed—in fact, until the flap and the extremity of the bone become sufficiently organized to support pressure. All traces of inflammation must have disappeared in the soft parts, while as regards the hard parts, if the malleoli have been preserved, they should have become in part blunted (*émoussées*); or, if they have been excised, the bony surfaces should have thoroughly healed. The treatment resorted to in old or commencing osteitis may be employed with advantage for consolidating or favouring the definitive cure. 4. Although this operation has been practised a great many times (there being perhaps from eighty to one hundred cases on record), it is impossible to know how many examples there are of complete success, understanding by this cases in which the mutilated leg can entirely replace the wooden one; 5. Instances of failure are unfortunately not rare. This may be immediate or distant, the latter being divisible into two forms. Thus, the stump may be irreproachable in its external configuration, and all the tissues forming it may be healthy in appearance, and yet progression may be impossible. In other cases the lesion is appreciable, and the ill result may be attributed to various causes, some of which are treated of in this memoir. Thus, progression may be rendered impossible by the sole fact of the procedure made choice of, or by the development of unforeseen accidents. The anatomical lesions that have been authentically proved are osteitis of the tibia and fibula, changes in the skin of the flap or in the accidental serous bursæ beneath the bony extremities, neuritis of the large nervous trunks, and osteophytes developed within the substance of the flap. As these consecutive accidents may only appear at a very late period, the result of the operation remains long doubtful; and in order that a cure may definitively be stated to have taken place, the stump must have gone through the ordeal of walking and of sufficiently prolonged standing. 6. This criticism does not imply the blame or proscription of the operation; for if many other operations were submitted to a similar examination, many illusions concerning their harmlessness and the benefits derived from them would be discovered; 7. All things taken into account, this amputation may be said to be a good operation in certain cases; but it is one that seems to me susceptible of improvements, among which, I believe, may be placed the excision of the posterior tibial nerve. To the present time, M. Jules Roux is the practitioner under whose hands the operation has made most progress, his mode of procedure being decidedly preferable to all others. 8. If surgeons are desirous of obtaining a definitive solution of this question, they should neglect no opportunity of the careful examination of individuals who have undergone the operation, and of very minutely dissecting any stumps that may come under

their notice. Detailed observations of this kind will, at a later period, allow of a more complete history being traced." (p. 447.)

IV. *On the Condition of the Cervix Uteri during the latter Half of Pregnancy.* By M. CAZEAUX.—Drs. Costilhes, Boys de Loury, and Bennett, maintain that during the early months of pregnancy ulcerations of the cervix uteri are of great frequency, and exert a powerful influence in the induction of various pathological conditions. M. Cazeaux believes these statements to be greatly exaggerated; and at all events in the latter half of pregnancy, to which his own observations apply, they are not borne out. Examined by the speculum, the mucous membrane of the vulva, the vagina, and the free surface of the os itself, is found of a dark colour, which becomes deeper and deeper, until towards the end of pregnancy it has attained a dark violet. A person unaccustomed to this examination, and especially if he has not previously ascertained the position of the cervix by means of the finger, may have considerable difficulty in engaging this part within the extremity of the instrument,—this arising from the anteversion of the body throwing the vaginal extremity backwards.

"As the *toucher* would lead one to expect, the modifications presented to the eye by the vaginal portion of the cervix, are very different in primiparous and in multiparous women. In both, the cervix is of a deep violaceous, wine-lees colour; but in the primipara, this is pretty uniform throughout its whole extent. The external orifice, the lips of which are much softened, is in general more or less rounded; but although it is larger than in the unimpregnated state, it admits of the penetration of the eye with difficulty, even when the valves of the speculum are considerably expanded. The circumference of the os, and the free portion of the cervix, rarely exhibit any traces of ulceration, but it is common enough to observe series of cherry-red granulations, true fleshy vegetations, varying in size from a pin's head to a large pea, which bleed on the slightest contact. In the woman who has borne a certain number of children, the cervix is in general much more voluminous, so that there is some difficulty in completely embracing it by the speculum. The lips of the os seem divided into several fragments, this segmentation, the result of lacerations that have occurred during former deliveries, rendering it very irregular. In consequence of these numerous solutions of continuity, the orifice is much larger and much more easily dilatable, so that the eye is enabled with ease to explore all the cavity of the cervix. The walls of this cavity are very unequal, and present irregular series of fungous projections, separated by more or less deep depressions. Some of these prominences are transparent, being probably due to hypertrophied follicles, but others resemble true flabby (*mollasses*) vegetations. Sometimes these are covered by a protective epithelium, but it is not unusual for them to be deprived of this, and then to bleed upon the slightest touch. It is especially within the furrows which separate them, that more or less deep linear ulcerations are often observed. These ulcerations sometimes so increase in size as to occupy a pretty considerable surface, and then they are easily seen; but generally they are hidden in the depths of the anfractuosities, and in order to perceive them, after well cleansing the surface, we must put the cervix on the stretch by opening the instrument widely. I have very often met with these ulcers in multiparous women, and I believe that I am within the truth when I say that I have observed them in seven-eighths of the cases, confining this statement to the last third of pregnancy. Supposing that a singular chance has not favoured my researches

for a long time past, it is probable that what I describe here is the normal condition, and should not be considered as a pathological state, but simply as a consequence of the progress of gestation. Resembling in this respect the deep colour, the tumefaction, the ramollissement, and the almost fungous condition of the walls of the cervix, which are proper to pregnancy, and in no-wise influence its progress, these ulcerations have the same origin, and should be considered as the result of excessive congestion. I believe that they are of no more importance. I am especially convinced of their innocence, and believe that all treatment of them is much more mischievous than useful. . . . . If I am not mistaken, then, and if the peculiarities I have been describing belong to pregnancy, and are only an exaggeration of the modifications of the structure and the vascularity of the parietes of the uterus, this condition should disappear with the cause that gave rise to it. Like vomiting, varices, hæmorrhoids, and all the sympathetic disturbances of pregnancy, it should cease with this. And that is precisely what takes place, and we may lay down as a rule, that no traces remain five or six weeks after delivery; the ulcerations which we sometimes meet with in women recently delivered, do not, in fact, present the same appearances, and generally are referrible to another origin." (pp. 453–456.)

The statements made by Boys de Loury, Bennett, and others, as to the frequency with which abortion and various puerperal diseases are produced by ulcerations occurring at an early period of pregnancy, are so discordant with the observations the author himself has made, that he cannot but tax them with exaggeration. It is of importance to distinguish between ulcerations that have preceded pregnancy, and have persisted and increased since its occurrence, from those which have only become developed subsequently to the formation of the germ. The former, becoming irritated under the influence of exertion, and especially by excessive coition, may easily induce the contractility of the body of the uterus, and bring about premature expulsion. But the latter, in the author's opinion, rarely exercise a similar influence; so that however proper treatment may be in the one case, it does not seem called for in the other. He also doubts the justice of Bennett's statement, that these ulcers are a frequent cause of obstinate vomiting in pregnancy; and since he has been in the habit of treating this affection by the application of belladonna to the cervix, he has had the opportunity of examining four primiparæ, reduced by it to the last stage of marasmus, in whom the cervix remained perfectly healthy.

M. Laborie, in his report upon this paper, observes that M. Coffin, drawing his materials from the practice of M. Richet, describes precisely the same fungous ulcerations as those treated of by M. Cazeaux; but that he attaches much more importance to their presence, at the same time that he admits that no kind of treatment has been applied with success. Of 7 women examined by M. Laborie himself in M. Cullerier's wards at the Lourcine, there was but 1 who did not exhibit ulcerations. She was a primipara, and had reached the fifth month. Two other primiparæ, exhibiting the ulcers markedly, were three months gone; and the 4 multiparæ had respectively attained the periods of five, seven and a half, and nine months. In these cases no special means of treatment were adopted, nor is it probable that

the ulcerations would ever have been discovered without the use of the speculum.

V. *On the Cicatrization of Arteries, and on the Form of Ligature which least exposes to the Danger of Secondary Hæmorrhage.* By M. NOTTA.—In a thesis published in 1850, M. Notta, deriving them from investigations made on ligatures of arteries applied after amputation, arrived at conclusions which differed from those announced by Jones, Béclard, Scarpa, and Manec. Thinking that this difference might have arisen from the fact of the investigations of these observers having been conducted on animals, while his own were made on the human subject, he determined to repeat the latter upon animals, and compare the results. The present memoir gives an account of these results, which are confirmatory of those formerly arrived at, and the preparations upon which the conclusions are founded are all deposited in the Musée Dupuytren.

"Authors are by no means agreed as to the primary effects of a simple ligature applied immediately to an artery. According to Amussat, who has, so to say, exaggerated the ideas of Jones upon this point, at the end of some hours the inner coats are found divided, and retracted from half a line to two lines or more above the ligature. There is therefore a space in which the arterial tube consists only of the cellular tunic, with which the base of the coagulum is in contact, and contracts adhesions. According to Guthrie and Manec, the inner and middle coats are not only divided, but are also so recurved inwards as to come into contact at the edge of their section. From this it results that the arterial canal is completely closed by the inner coats, which form a kind of diaphragm interposed between it and the cellular coat—so that the coagulum lies with its base upon the inner coat. In my numerous experiments, made on the horse and dog, I have in vain endeavoured to verify Amussat's assertions; while I have constantly observed the disposition mentioned by Guthrie and Manec, a disposition resembling in every point that which I have described as occurring in man.

"Immediately after the ligature has been applied, a deposit of fibrin takes place, and adheres to the lips of the divided inner coats, which project more or less into the interior of the vessel. This clot, small at first, increases in size until it fills the artery, with the walls of which it contracts adhesions. These adhesions of the coagulum to the internal coat were misunderstood by Jones and Amussat, merely alluded to in some of Beclard's experiments, and demonstrated by Scarpa and by Manec, who comprehended their importance, but for their explanation erroneously admitted the existence of a layer of plastic lymph. As in man, they are direct, without any intermediate substance. At all events, that is what I have observed in 19 out of 21 cases, comprised between the 3rd and the 122nd day after the operation. In all these preparations an adherent fibrinous clot is seen, and I have always found it impossible to distinguish the layer of plastic lymph spoken of by Manec. . . . . In these experiments, as in man, the extent of the adherent portion of the coagulum was always found dependent upon the presence of a collateral vessel, however small the volume of this might be. The twenty-one preparations only offer one exception to this rule. I will merely observe that in those cases in which there is a great distance between the ligature and the first collateral, as in the carotid, the clot, when entirely developed, does not completely fill the artery, the vessel remaining permeable for a space varying from five to ten centimetres between its cardiac extremity and the first collateral. In man, on the other hand, when the coagulum is well

developed, it almost always reaches as far as or very close to the first collateral." (pp. 479-482.)

The formation of a clot at the peripheric end of the vessel is not of constant occurrence. It was absent in 4 of the 21 cases, 3 of these being ligature of the carotid, and 1 of the aorta. In 3 other cases, the coagulum was less developed than that at the cardiac end, being much less adherent. In 8 cases, while almost as large as the cardiac clot, it differed from this in length, density, or adhesion. In 5 cases only no difference was observable in the two coagula. Thus, so far from the shock of the column of blood against the ligature being prejudicial to the formation of the coagulum, it would seem to favour this. This difference only confirms the conclusion already arrived at by the author in his investigations upon the human subject, that the formation of the coagulum is nowise dependent upon inflammation, as arteritis. Guthrie had already shown that the inferior development of the clot in the peripheric extremity of the vessel rendered secondary hæmorrhage from this end more common.

As the coagulum increases in age it contracts and augments in density; but although its adhesion to the inner coat becomes more intimate, it always remains distinct from this, and the most careful examination has never detected the slightest traces of vessels penetrating it. During its contraction, it draws the arterial tumours with it, so that when the vessel is examined four or five months after the application of the ligature, its calibre is found somewhat diminished. The author has never found the coagulum absorbed; and, examined four months after the ligature, it has continued quite distinct, and of good size, the arterial coats retaining their characters and thickness. If the observations of Jones, Béclard, and Scarpa be attentively examined, it will be found that, led away by the physiological error, that tissues admit of transformation, they have mistaken the decoloration and condensation of the coagulum for a fibrous transformation. When the artery is examined two or three months after it has been tied, if the coagulum reaches to or near the first collateral, the calibre of the artery will be found preserved up to this point. If a large space intervenes, as is the case with the carotids, the calibre of the vessel undergoes sensible diminution. This contraction is frequently very sensible at the end of fifteen days, and as the contraction of the clot takes place much more slowly, the artery presents opposite to it a bulging, which eventually disappears.

Having traced the normal development of the coagulum, M. Notta next proceeds to give some account of its pathological conditions. These he has already described in his thesis in relation to their influence on the production of secondary hæmorrhage. They are three in number—viz., arrest of development, putrid dissolving, and purulent dissolving; but although the same alterations are met with in animals, they are so less frequently. In but two instances out of twenty-one cases was arrest of development observed, and that was seen twice in the same animal. In another instance the putrid dissolving of the coagulum was observed, and in two the purulent dissolution.

The description of the effects of the ligature upon the two ends of the vessel is given at considerable length. We subjoin a portion of the account:

"When a ligature is applied in the course of an artery, the portion of the vessel actually embraced by the thread mortifies, suppurative inflammation tends to eliminate it; and when the small eschar has become quite detached, the two ends of the artery, now completely separated, become retracted by virtue of their elasticity, the interval of a variable extent which exists between them being filled up by the sheath of the vessel, which is open, and communicates externally by the wound made in the soft parts. The walls of this sheath become inflamed and thickened, their internal surface being covered with fleshy granulations, secreting pus. The inflammation of the sheath spreads to the surrounding cellular tissue, which becomes indurated to a distance of several millimetres, or one or two centimetres beyond the extremity of the two ends of the artery. Amidst this induration, the arterial coats preserve their normal colour and thickness, and remain exempt from inflammation. In the dog, the artery, when it has been well isolated, is divided from the third to the fifth day; and after the sixth day I have always found it divided." (p. 493.)

In some few cases in the human subject (but never in the author's experiments on animals), the falling ligature has attached to it two small arterial cones, produced by the sphacelation of the artery at the point where the ligature was applied. In both the same result follows division of the artery and retractation of its ends. The interval of separation sometimes only amounts to one or two millimetres, and then the two ends of the vessel may become so intimately united as to render it impossible to determine the point at which the ligature was applied. In an example of ligature of the external iliac, performed fifteen years since, a fibrinous coagulum obliterated the artery from the hypogastric to the femoral profunda, and it was impossible to determine the point at which the vessel had been tied. In general, the separation varies between one and several centimetres, the sheath communicating after a time with the exterior only by a fistulous track. It is usually about the tenth day that the wound becomes thus reduced to one or two fistulous openings, which close in a period varying from the fifteenth to the twenty-fifth day. The cicatrization of the external wound should be delayed until we are certain that the deeper-seated portion is entirely closed, otherwise the pus contained in the sheath cannot obtain issue.

IV. *On the Elephantiasis of the Arabs, and especially as affecting the Scrotum.* By Clot Bey; with a Report by Baron H. Larrey.—Clot Bey's paper consists chiefly in the narration of four instances of large elephantiasis of the scrotum, for which operations were resorted to. Baron Larrey's Report assumes the size and form of a monograph upon the subject, and is executed with great ability, giving a *résumé* of the entire literature of the subject nowhere else to be found.

Review XI.

The Physiology and Treatment of Placenta Prævia; being the Lettsomian Lectures for 1857. By Robert Barnes, M.D. (Lond.), Physician to the Royal Maternity Charity, &c.—*London*, 1858. pp. 208.

It would be difficult to overrate the value of lectures like the Croonian, Lettsomian, and similar institutions. If reasonable care be exercised in the selection of the lecturer, they afford to the profession in the metropolis a full and well-digested development of the subjects of the course, or perhaps the first announcement of new discoveries or novel theories; whilst to the lecturer, the prominent position gratifies a laudable ambition, and gives him the advantage of an educated audience of high scientific character—a tribunal at once indulgent and exact, before whom he may fearlessly unfold his views, and from whose criticism he is sure to derive benefit. Many admirable illustrations in support of this view must be within the memory of our readers, and one, the most recent and not the least important, it is our duty and pleasure to bring before them at present.

Dr. Robert Barnes, whose previous labours have been highly appreciated by the profession, was appointed Lettsomian lecturer for 1857, and having arrived at some novel conclusions concerning placenta prævia, he chose that for the subject of his lectures, and he has now republished them in the volume before us, with an appendix of cases in illustration.

As the truth of these views must, indeed *can only*, be tested in practice, it is of course too soon to pronounce any positive judgment upon their merits; but we can say, in truth, that not only are they very interesting and ingenious, but that if confirmed by further observations, they will have a most important influence upon practice. Our present purpose is to lay these views before the reader, with the grounds upon which they are based, and their practical application, together with such remarks of our own as we may deem necessary. But previously, we may take the liberty of mentioning that the ordinary opinion as to the hæmorrhage in placenta prævia is, that it arises necessarily (hence the term "unavoidable hæmorrhage") in consequence of the separation of the placenta from the cervix uteri, whilst the latter is undergoing the process of dilatation; that the source of the discharge is the exposed uterine sinuses; that the extent of the detachment is indefinite, and the hæmorrhage limited mainly by the pressure against the child.

Now, from this view of the extent of detachment to which the placenta is exposed in such cases, Dr. Barnes dissents entirely. In his opinion, "There is an *anatomical and physiological limit to the extent of placenta liable to detachment during the expansion of the womb.* This is why, after a certain stage of the labour, no fresh bleeding surface is exposed." And, taking Sir C. Bell's anatomy of the uterus as his basis, Dr. Barnes illustrates his physiological problem as follows:

"The inner surface of the womb may be divided into three zones or regions by two latitudinal circles. The upper circle may be called the Upper Polar Circle. Above it is the fundus of the uterus. This is the seat of fundal placenta—the most natural position. It is the zone or region of safe attachment. The lower circle is the Lower Polar Circle. It divides the cervical zone or region from the meridional zone. The space comprised between the two circles is the region of lateral placenta. When attached to this region, the placenta is not liable to previous detachment. It may, however, cause obliquity of the uterus, transverse position of the child, lingering labour, and disposition to retention of the placenta and post-partum hæmorrhage. Below this circle is the cervical zone—the region of dangerous placental attachment. All placenta fixed here, whether it consist in a flap overreaching downwards from the meridional zone, or whether it be the entire placenta, is liable to previous detachment. The mouth of the womb *must* be pulled open to give passage to the head. This enormous contraction or retraction of the longitudinal fibres is incompatible with the preservation of the adhesion of the placenta, which is within its scope. In every other part of the womb, there is an easy relation between the contractile limits of the muscular structure and that of the cohering placenta. Within the cervical region this relation is lost. The contraction of the uterine tissue is in excess." (p. 77.)

The extent of this "lower polar circle," Dr. Barnes thinks, may be described by a circular line three inches distant in all parts from the os uteri.

Assuming, then, that the detachment is limited to this extent, in what way is the hæmorrhage from this surface restrained?

"By precisely the same mechanism as that which stops the flooding after normal detachment of the placenta from its normal seat at the fundus. The longitudinal fibres of the lower segment *must contract* to pull open the mouth. Expansion, dilatation of the mouth is contraction of the cervix. This contraction, by shortening the cervical portion of the womb, casts off the placenta, and exposes the ruptured mouths of the utero-placental vessels. The *first* effect is, bleeding. The *second* is, to stop the bleeding. The contraction goes on either actively or passively, and tonically in most cases; and then further contraction constricts the orifices of the vessels—closes them; it is hæmostatic. If hæmorrhage be renewed, it does not proceed, except under circumstances of excessive muscular relaxation—the passive hæmorrhage of Dr. Chowne—from the surface bared by the preceding active contraction: it proceeds from a fresh zone or arc further from the os, bared of placenta by another contraction. This zone or arc is, in its turn, in like manner sealed; and there is another pause in the flooding. Zone after zone is thus bared by recurring contractions, and successively sealed up, until that physiological limit, that line of demarcation between normal and abnormal placental implantation, the boundary line of placental detachment, which I claim to have discovered, has been reached. *This zone attained, the labour is a natural labour.*" (p. 54.)

So far, we think that these extracts give a pretty full and correct view of Dr. Barnes's physiology; and with regard to the limitation of the detachment, in lateral placenta at least, we are inclined to agree with him, for the following reasons—viz., that as the detachment in the lower zone proceeds from the necessity of the cervix being dilated, whether mechanically or by muscular effort—in order to allow the child to pass; so the figure and expansion of the uterus above the lower zone show this to be unnecessary, and the only effect of uterine action here would be contraction. No doubt the effect of this con-

traction must be to detach the placenta, more or less; but it will not do this till near the end of labour, just as we believe happens in all cases of normal labour.

Nor do we doubt that the cause assigned may arrest the hæmorrhage; still we should not be inclined to reject the pressure of the child or the entire contents of the membranes when pressed downwards against the placenta, as another influential cause; but the real practical question is—Can we rely upon either with confidence sufficient to supersede more active interference? How far must we assume complete uterine action (labour pains) to be present in order to the complete effect? Again, when the hæmorrhage has ceased, shall we be justified in considering the patient free from the danger of any further bleeding from the old exposed surface? These questions, out of many others of great importance, bring us to the cases upon which Dr. Barnes bases his theory. He adduces a number of examples in which hæmorrhage occurred before or near the time of delivery, and in which it ceased after a time, and did not recur, the labour often terminating naturally; and he contrasts these with others in which death resulted from forced delivery. Now, we must take the liberty of remarking of some of them, that there is no evidence of these being placenta prævia at all; and of the others, that they were all cases of lateral placenta, where this organ came down to the edge, or partly over the os uteri. There is not a single case, as far as we see, of central implantation, and yet it is in these cases that there is most difficulty and most danger, and where, at least in the large majority of cases, the labour could not merge into a natural one—even if the hæmorrhage were stopped by Dr. Barnes's or any other method. And unless experience had shown positively that artificial detachment would surely arrest the hæmorrhage, should we not in such cases increase the mischief without promoting the delivery?

We would wish to speak cautiously on a subject of such importance; and where facts are not sufficiently numerous to enable us to decide, Dr. Barnes is entitled to the benefit of these doubts, and we trust that his future investigations may resolve them.

As to the source of the hæmorrhage, notwithstanding the very high authority of Dr. Simpson, we have no hesitation in agreeing with Dr. Barnes in attributing it mainly to the uterus, and not the placenta; and although we are as unable to explain the causes of placenta prævia as previous writers, we cannot go so far as Dr. Barnes, when he says, "that it is a matter of doubt whether there be one part of the superficies of the chorion more especially destined to be developed into placenta than the rest," for to whatever part of the uterus it may be attached, we are quite satisfied that the portion of the chorion to be "developed into placenta" is that part in the inner surface of which the umbilical cord is inserted. We have seen an example of the contrary, and the fœtus perished immediately.

But a very important practical point is the diagnosis. Dr. Barnes has given the signs which are considered reliable by Levret, Gendrin, Cohen, and Moreau, and then announces his own conclusions:

"1. The general signs, such as flattening of the abdomen, division of the abdominal tumour, and especially swelling and pain on one side of the pelvis, pointed out by Levret, should lead to minute exploration by the finger and stethoscope. 2. Abortions, disease of the placenta, dead children, and placenta prævia in former pregnancies, should also lead to minute physical exploration." (p. 34.)

We cannot say that we are inclined to attach much importance to any of these signs, with one exception. If they excite suspicion, they may lead to further investigation, which may yield fruit. A pulsation at the os uteri, not synchronous with the mother's pulse, or (the child being alive) our being unable to feel "ballottement," as Gendrin suggests, would certainly give ground for suspecting placenta prævia; but neither might be found, if the placenta only came down to the edge of the os uteri. The occurrence of hæmorrhage at the seventh or eighth month, as mentioned by Moreau and others, if there be no external cause, is a very suspicious symptom, and ought immediately to lead to an investigation with the stethoscope, which we regard as by far the most valuable means of exploration. We have repeatedly decided between "accidental" and "unavoidable" hæmorrhage by its assistance. The placental murmur can be heard at any part of the anterior half of the uterus, and by a little management, when somewhat posteriorly situated.

Now, let us consider the practical application of Dr. Barnes's theory. He states that "in cases of complete placental presentation, and in those of partial where the flooding is unusually profuse, we have to choose between two methods—forced delivery and artificial detachment of the placenta." As regards the latter, some,

"With Kinder Wood, would detach the placenta in those cases of exhaustion where forced delivery cannot be had recourse to without peril to the life of the mother; or with Dr. Radford, in those cases, also, where the os is partially dilated, when the membranes are ruptured, and where strong contraction exists—a condition, I presume, considered to contra-indicate turning; and also in cases of narrow pelvis. Others extend their faith in this method to the full latitude of the precepts of Dr. Simpson."

In the latter category we certainly cannot include ourselves, nor are we as yet prepared to agree fully with Drs. Radford and Wood.

Dr. Barnes does not propose always to interfere promptly in the earlier cases, but to wait for some time until the os uteri is expanded—

"It may be justifiable to trust some time to the plug; but if the symptoms are too urgent for delay, one or two fingers may be at once passed through the os. Seek to determine which side of the uterus bears the great bulk of the placenta; feel for the edge of the placenta on the opposite side; rupture the membranes during a pain; tear the membranes freely from the border of the placenta, and sweep the finger round half the circumference of the os uteri internum, so as to detach the placenta completely from that side of the uterus to which the lesser portion adhered. This done, there is nothing to prevent the os uteri from expanding, and carrying the liberated portion of the placenta over to the side where the bulk of the organ adheres. Nature herself will do the rest." (p. 105.)

Again,

"If the hæmorrhage should continue or be renewed with urgency, I propose as the next step, the *total cervical detachment of the placenta.* The detachment must be carried further, by sweeping the finger round between the placenta and uterus on that side to which the main bulk of the organ adheres. In this manner, the whole of that part which had been seated in the cervical zone will have been detached. . . . . Now, this detachment will not *of itself* stop the hæmorrhage. We may, I think, tranquillize our minds as to the effect on the mother, of that small portion that will escape from the detached placental surface. But the uterine vessels may pour forth blood until the hæmostatic resources of nature or art come into play. The resources of nature are those I have before described; continued contraction of the muscular structure of the womb, the contractile action of the coats of the uterine arteries, and the formation of coagula in their mouths. In the majority of cases these resources are sufficient; the hæmostatic process may, however, be further assisted by plugging again. A method of plugging is recommended by high authority, which appears deserving of attention. Moreau advises the application of a lemon, the end of which has been cut off, to the os uteri, and to retain this '*in situ*,' by firm pressure through rags or a sponge. The acid juice favours coagulation, as well as the pressure and retention of the blood. The soaking of the sponges or rags, used for plugging, in vinegar, is a common practice, but it seems to me reasonable that more powerful styptics should be used. I suggested some time ago the injection of the sesquichloride of iron. . . . . By these proceedings I have described, we may reasonably hope, in the vast majority of cases, that the hæmorrhage will cease. If it should not, time will have been gained, the os uteri may have become soft and dilated, and in the event of its being felt to be necessary to resort to forced delivery, this operation may be performed with comparative ease and safety." (p. 107.)

These extracts, we think, give a very fair account of Dr. Barnes's proposal, and we have preferred using them to giving the substance in our own words. In order, however, to a fair appreciation of this new procedure, it will be well to classify the principal cases of unavoidable hæmorrhage met with in practice. 1. Let us suppose a case in which the hæmorrhage occurs at the seventh month, to a considerable extent, without pains and without dilatation or dilatability of the os uteri. 2. Or a case at the full period of pregnancy, without pains, the os uteri being soft, and the edge of the placenta coming down to the edge of the os uteri (lateral placenta). 3. A case, similar to the last, with labour pains, and the placenta coming down to the edge of the os, or perhaps a little beyond it. 4. A case of central implantation of the placenta. Let us see how far our present experience would justify our depending upon Dr. Barnes's method.

1. We quite agree with Dr. Barnes in repudiating "forced delivery" in the first class of cases: it would be attended with great danger, and in our opinion is quite unnecessary. As the uterus is filled and the membranes unbroken, internal hæmorrhage is almost impossible to any serious extent, so that the plug may be unhesitatingly used; and if the vagina be thoroughly filled, external hæmorrhage is almost equally impossible. The best material for plugging we have ever tried, is finely-carded cotton wool, and the easiest and least painful mode of introducing it is by means of a cylindrical speculum. Under this

treatment, we believe that in most cases the hæmorrhage will be arrested for a time.

2. But if it should not, and if it should become alarming, whether premature or at the full term of pregnancy, Dr. Barnes advises the detachment of the placenta. Now, in cases where there are no pains —where there is no effort at uterine contraction—are we sufficiently sure of the success of Dr. Barnes's plan to warrant our depending upon it? If the result were tolerably certain, it doubtless has great advantages; but if it fail, do we not necessarily increase the hæmorrhage, and place the patient in a worse condition than before? We must frankly confess that, whilst we are open to conviction on this point—nay, are anxious for it—we do not think that any of the cases adduced by Dr. Barnes sufficiently establish the point. Indeed, the latter sentences of the extract we have last given, admit that it will not *necessarily* do this, but "that the uterine vessels may pour forth blood until the hæmostatic resources of nature or art come into play." In cases of slight flooding this may possibly not signify much, but surely in many of the cases of alarming hæmorrhage such an increase might make the difference between life and death. We have seen many in which every additional ounce lost was a matter of extreme importance. Might it not be better to have recourse to the hæmostatic resources of art, until uterine contraction is established, without further detaching the placenta?

3. When regular pains exist, with the placenta down to the edge of the os uteri, as in many of the cases related by Dr. Barnes, we see less objection to his proposal. The cervix will be freed thereby to contract and close the open mouths of its vessels, and the rupture of the membranes will increase the pains, and bring down the child's head. But even supposing there be no objection, we may still ask, Is it necessary? We have repeatedly treated such cases by rupturing the membranes, and the exhibition of the ergot of rye, and as yet we have always found the hæmorrhage cease, and have never been obliged to introduce the hand and deliver.

If the placenta extend half over the os uteri instead of being limited to its edge, we think that Dr. Barnes's plan may be very useful, for unless the pains are very strong, the small covering portion of the placenta may be an obstacle to the descent of the head, even after the waters are evacuated; whereas, if it were freely detached, it would be easily pushed into the vagina by the head, and labour facilitated. If the pains were good, we should have no fear of much increase of the hæmorrhage

4. The severest test of any new proposal would be the cases of central implantation of the placenta, where the hæmorrhage is always excessive, and which every pain increases. The extent of detachment by the pains alone will probably be much greater; it may be two or threefold as much as in lateral placenta; and if we may judge from our own experience, these cases have been by far the most formidable that we have met. It is true that in such cases the uterus is in action; but may it not be reasonably doubted whether the presence of the

placenta would not prevent such a contraction as will arrest the hæmorrhage? And, in such extreme cases, should we be warranted in incurring the risk of even a moderate addition to the flooding? If detachment of one side of the placenta will lead to that portion being protruded into the vagina, with or without a little help, it might be advantageous, as it would permit the descent of the head after the waters were evacuated; and this peculiarly in cases where the loss has not been great, and where the os uteri is not very dilatable. It has been frequently remarked—and our own experience has corroborated the observation—that in the extreme cases, the os uteri is generally so much relaxed that the introduction of the hand can be effected without much force.

We must ask pardon of the reader for these observations; but in placing before him the very ingenious views of Dr. Barnes, we have felt it right to test their applicability to some of the cases of daily practice. If we have thrown out doubts, it has been because as yet the novel method has not been sufficiently tested; and where life and reputation are at stake, no step should be taken but upon adequate grounds.

We have read Dr. Barnes's book carefully, and with very great pleasure. It has increased our respect—which was very high before—for his accurate knowledge, his professional acumen, and his ingenuity. Though belonging to an older school than Dr. Barnes, we are not too old to learn, and we shall thankfully receive from him proofs that our caution has been unnecessary, and our doubts unfounded.

Review XII.

1. *A Practical Treatise on the Diseases of Children.* By J. Forsyth Meigs, M.D., Fellow of the Royal College of Physicians of Philadelphia, &c. Third edition, carefully revised.—*Philadelphia*, 1858. pp. 724.

2. *Recherches sur les Maladies des Enfants Nouveau-Nés.* Par V. Seux, Médecin-en-chef de l'Hospice de la Charité de Marseille. Arts., *Du Pouls chez les Nouveau-Nés—Entérite simple.—Paris*, 1855. pp. 288.

Researches on the Diseases of New-born Children. By M. Seux, &c. Arts., *On the Pulse in New-born Children—Simple Enteritis.*

3. *Der Abdominal-Typhus der Kinder.* Von Edmund Friedrich, Doctor der Medicin und Pract. Arzt zu Dresden.—*Dresden*, 1856. pp. 102.

The Abdominal Typhus of Children. By E. Friedrich, M.D., &c.

4. (1.) *Fièvre Typhoide.* (2.) *Généralités sur les Catarrhes, Congestions, Phlegmasies et Ramollissements de la Membrane Muqueuse Gastro-Intestinal.* (Articles contenus dans le 'Traité Clinique et Pratique des Maladies des Enfants.' Par MM. Rilliet et Barthez.—*Paris*, 1853. Tomes i. et ii. pp. 655, 663.)

(1.) *Typhoid Fever.* (2.) *Catarrh, Congestion, Inflammation, and*

Softening of the Gastro-Intestinal Mucous Membrane. (Articles contained in the 'Practical and Clinical Treatise upon the Diseases of Children.' By MM. RILLIET and BARTHEZ.)

5. *An Inquiry into the Nature of Typhoidal Fevers, based upon a Consideration of their History and Pathology.* By HENRY FRASER CAMPBELL, A.M., M.D.—*Philadelphia*, 1857.

6. *On the Identity or Non-Identity of Typhus and Typhoid Fever.* By GEORGE YATES, M.R.C.S., Surgeon to the General Dispensary, Birmingham. ('Midland Quarterly Journal,' Oct. 1857, p. 200.)

7. *Ueber die Veränderung der Peyer'schen Drüsengruppen und der isolirten Darmdrüsen bei ganz kleinen Kindern. Zweite Abhandlung über die Veränderung der Peyer'schen und isolirten Darmdrüsen bei Kindern im zarten Alter.* Von Dr. E. HERVIEUX. ('Journal für Kinderkrankheiten,' Band xxvi. s. 131; Band xxix. s. 213. 1856, 1857.)

On the Changes undergone by the Glands of Peyer, &c., in very Young Children. A Second Treatise upon the Changes undergone by the Glands of Peyer, &c., in Early Age. By Dr. E. HERVIEUX, of Paris. Translated from the French in the 'German Journal for Diseases of Children' for the years 1856–7.

8. *Bemerkungen über Dr. Carl Schneemann's Schrift: Die Fett-einreibungs-Methode in ihren Heilwirkungen gegen Scharlach- und Masernkrankheit.* Von Prof. MAUTHNER, in Wien. ('Journal für Kinderkrankheiten, Band xxi. s. 289.)

Remarks upon Dr. Schneemann's Essay upon the Infriction of Fat as a Therapeutic Agent in Scarlet Fever and Measles. By Professor MAUTHNER, of Vienna. ('Journal for Diseases of Children,' vol. xxi. p. 289.)

NEARLY ten years have elapsed since we introduced Dr. Meigs to the notice of our readers. At that period *pædiatrics* had just entered upon a stage of advancement which, happily continuing, has caused this department of pathology to keep pace with the most progressive branches of the science and art of Medicine. Before that time the well-known work of Dr. Underwood was the authority concerning the numerous ills that children are subject to. But latterly it became displaced by the far more scientific treatise of Drs. Evanson and Maunsell—a work which, considering the way in which the diseases of early life were studied in Great Britain, was one of considerable merit, though very much of a compilation. Year after year the *affiche* of each hospital displayed upon its face the comfortable assurance that it continued to be the anxious duty of the lecturer upon midwifery to initiate the student into a knowledge of "the Diseases of Women and Children." What amount of information was ever proffered upon the latter subject we are yet ignorant of. We ourselves passed through the usual novitiate, but the "Diseases of Children" were known to us only as a myth. Were others more fortunate? We doubt it. Even at the present day—

but we will not be personal; though we must say it would *look better*, for the future, to say the least of it, to discontinue *pædiatrics* as the mere caudal appendage of the obstetric physician. On the Continent, the systematic investigation of the diseases of early life had already made great progress—as evidenced by such works as those of Gölis, Billard, Berton, Barrier, Meisner, Valleix, &c.; and since the eighteenth Floreal of the tenth year of the Republic, Paris could boast of a hospital for sick children. But abroad a new era of progress commenced with the appearance of the now classic work of MM. Rilliet and Barthez (1843). Few voluminous treatises have more rapidly attained a safe and worthy hold of the profession than have theirs; and we do these writers no more than justice, when we say that the present high development of this branch of practical medicine owes very much to their influence. In this country, the publication (1848) of the Lectures of Dr. West was certainly the means of spreading amongst the profession generally a scientific knowledge of the information then possessed by a few persons of the diseases of infancy and childhood. The success of the treatise of MM. Rilliet and Barthez gave rise to several more or less successful compilations or *réchauffés* upon the Continent; whilst here, the Lectures of Dr. West have prompted to the same proceedings. The scope and merits of these various undertakings, both British and Foreign, have, as they appeared, been brought before the notice of the readers of this Journal, together with such references to the more important articles in the very useful German 'Journal for the Diseases of Children' as might serve to keep such as were interested in pædiatrics fairly *au courant* with the progress of the day. In continuance with such views, we again bring forward Dr. Meigs, with a commentary upon some of the more important essays which have lately appeared upon the important subject of typhoid fever in the child.

When we before subjected Dr. Meigs to critical revision, we felt compelled to animadvert upon one or two drawbacks which existed to the utility of his treatise. We are glad to find that in the present edition several of them are materially diminished. In the first place —though still much of a compilation—his work has the advantage of the results of ten years' practical experience. Secondly, in place of that undecided, indefinite way of discussing *treatment* followed in the first edition, we have here a clearer and more precise style adopted; and we now learn what Dr. Meigs has done, and what he believes should be done—instead of, as before, only what he *would do*. Further, several articles have been re-written, new ones added, and the work generally sought to be made a repertory of the latest researches in its particular department. But it is yet capable of much improvement. It is true we have so constantly MM. Rilliet and Barthez before our mind, that we become rather fastidious critics—the more so as we frequently feel disposed to inquire, where would be most of the works published within the last fifteen years, if these able investigators had themselves chosen to remain silent?

We shall now proceed to notice some of the principal changes and

additions made to the work since its former appearance. The first in occurrence is the "Introductory Essay on the Clinical Examination of Children." Upon this point we could scarcely expect much novelty, nor have we found it, preferring, too, the manner of M. Bouchut's more free and facile sketches. There is room for a few remarks, notwithstanding. Alluding to the pulse and rate of the circulation in very young infants, Dr. Meigs follows M. Billard, giving it as from 101 to 102 in the minute, qualifying the assertion that though this was the average frequency in 40 children, it was less than 80 in 18, in 14 between 100 and 125, and in 6 between 130 and 180; all these children presenting the appearances of good health. Upon this subject great difference of statement has existed. Floyer fixed the number of pulsations of the new-born child at 134 in the minute, Haller at 140, Sœmmerring at 130, Valleix the minimum at 76, the maximum at 104, Trousseau, minimum 96, maximum 164; whilst Jacquemier numbered from 96 to 156 pulsations in children twenty-four hours old, and M. Lediberdier 140 to 208 in those born only four minutes. This great variation of 132 recorded, lately induced M. Seux to re-investigate the matter, and the *résumé* of the tabulated results of the observations of 75 cases is as follows:

"The existence of 164 pulsations in the new-born child during repose does not in itself imply a state of disease—in fact, the pulse may vary in health and quietude from 80 to 164 pulsations in the minute. Nevertheless, the numbers comprised between 120 and 140 are those more frequently noted in nearly half of the cases. Next come those between 100 and 120, then those above 160, and finally those below 100. . . . . The sex, constitution, the greater or less salubrity of the habitation, and the time of year, do not exert any influence upon the frequency of the pulse. The latter is more frequent during the first hours of life, but then from the first day to two months, no differences can be determined as really due to the age of the child." (p. 12, op. cit.)

Both M. Seux and Dr. Meigs rightly prefer expressing by the term "irregularity" that change in the rhythm of the infant's pulse alluded to by M. Bouchut as an *intermittence*. We assume from the conjoint observations of M. Seux and Becquerel, that this irregularity is far less frequent in new-born children than in those above a year old, and the greatest irregularity occurs when the pulse is lowest, as in sleep. According to Dr. Meigs, the chief practical bearing of this fact is that—

"We should be careful not to lay too much stress upon slowness and irregularity of the pulse as signs of tubercular disease of the cerebral meninges, unless they are observed during the waking state, and in connexion with other symptoms, particularly with vomiting, constipation, and severe headache." (p. 36.)

The following remarks are worthy of remembrance:

"Violent and obstinate crying is almost always caused by severe pain—such as the pain of ear-ache; indeed, obstinate and long-continued crying lasting for hours is rarely met with, except from one of two causes—ear-ache or hunger. The cry of ear-ache is often incessant and unappeasable, the pain being generally constant and not paroxysmal, as are most other pains. It is to be silenced only by the application of remedies to the ear or by the internal

administration of opiates. I have known an infant three months old to scream with ear-ache for two days and nights, with only short lulls of a few hours, when brought under the influence of large doses of laudanum. As soon as the ear began to discharge, the cry ceased. I am constantly called to see infants and young children who have been crying most violently for hours, and who are thought to have colic or to have hurt themselves, and who are, in fact, tortured with that most violent of all pains, ear-ache. I have met with few instances in which such severe and *constant* crying has depended on other causes, for though children scream violently and obstinately from hunger and thirst, they may always be quieted by the supply of either want, whilst in ear-ache, the infant generally refuses the breast, or takes it only for a few instants, and then lets go to resume his almost automatic scream." (p. 25.)

In his remarks upon the "Examination of the Abdomen," the author recommends a plan as first proposed by M. Valleix in the case of young infants, and by which tenderness upon pressure may easily be recognised. The child is to be carried suddenly before a bright light, one of its greatest pleasures apparently consisting in gazing at such a thing. It almost always ceases to scream and continues quiet whilst thus attracted. Seizing such an opportunity, the attendant should pass his hand under its clothes, and applying it directly over the abdomen, he may first learn by a rapid palpation of its surface its general characters, and then ascertain by sudden and decided pressure whether the stomach be abnormally sensitive. If the pressure cause pain, the child will cry out at the moment, while at the same time a sudden contraction of the countenance will assist in showing the perception of some painful sensation. Should the child, on the contrary, continue to gaze fixedly at the light without heeding the interference of the physician, it will be fair to assume there is no inflammatory tenderness present.

The several chapters relative to affections of the upper air-passages have been re-arranged and re-written. The conclusion to which Dr. Meigs has come is that—

"There are but three distinct diseases of the larynx, which deserve to be considered as separate and distinct affections; these are simple ordinary erythematous inflammation of the larynx, unattended with spasm of the glottis, or, as that symptom has been emphatically named, laryngismus; simple erythematous inflammation of the larnyx, attended with laryngismus, and called most properly, spasmodic simple laryngitis, or more commonly, simple false spasmodic or catarrhal croup; and, lastly, pseudo-membranous inflammation of the larynx, properly named pseudo-membranous laryngitis, and more commonly called true or membranous croup." (p. 62.)

Laryngismus stridulus, or "spasm of the glottis," is viewed by the author as "but one of the many symptoms that mark the dependence of the disease upon disordered action of the reflex portion of the general nervous system." We cannot entirely coincide in Dr. Meigs's opinions; our own views may thus be expressed:—1stly. There is a more or less acute and sthenic form of inflammation of the larynx chiefly or almost alone, or of the trachea, with more or less involvement of the larynx, bronchia, &c., accompanied by croupose deposit or membranous exudation. This disease *commences in the parts below* the epiglottis, and constitutes the ordinary disease known as "croup," in

bleak northern climates like our own. 2ndly. There is a low asthenic and less acute form of inflammation, accompanied by a darker coloured, softer, and more pultaceous deposit, *commencing in the parts above* the epiglottis (uvula, tonsils, throat, &c.), which afterwards spreads into the air passages, destroying life. This affection constitutes the ordinary croup of many malarious regions, is the common form of croup in a great part of France, and in this country is more frequently seen as complicating the exanthemata, or as occurring in warm, damp autumns, and near the banks of low rivers and streams. It is the *diphthérite* or "diphtheritic croup" of authors. 3rdly. There is a disease marked by symptoms intermediate between laryngeal or tracheal croup and "laryngismus stridulus." It partakes of a certain amount of the inflammatory action of the former and of the nervous symptoms of the latter, the intensity and ratio of which vary in different instances. In some cases the spasmodic element is exceedingly prominent, whilst in others the inflammatory one is so decided as to give rise to apprehensions for the safety of the child. In some cases a chronic form of this malady is continued even for months. Certain instances of this variety of laryngeal disorder are confounded with the exudative one, or croup; whilst others in which the spasm of the glottis is severe, are often regarded as examples of *laryngismus stridulus*. This disease constitutes the "spasmodic laryngitis" of many authors. Lastly, there is *laryngismus stridulus*, or "spasm of the glottis," a secondary functional disturbance following in the train of more primary or of organic changes of diverse kinds. Dr. Meigs discusses at considerable length the question of "tracheotomy" in "pseudo-membranous laryngitis." He is an advocate for its performance under proper circumstances, and concludes by remarking that—

"Even if we adopt the worst view of the case, and conclude that it is impossible by any means now within our reach to determine the extent to which the exudation may have invaded the air-passages, it is still very doubtful whether for that reason we ought to abandon the operation. In effect it has been well established by numerous observations which have already been detailed, that the membrane extends into the bronchia only in a third of the cases, leaving two-thirds in which it remains limited to the larynx alone, or to the larynx and trachea. The question becomes, therefore, one of expediency, whether to leave two-thirds of the patients, some of whom might be saved by the operation, to perish without an effort to save them, because one third *must* probably die, or to perform it almost without a prospect of success in a third for the sake of the chance of saving some of the remaining two-thirds who must otherwise die." (p. 116.)

Upon this subject we may refer the reader to a paper read by Dr. Fuller before the Medico-Chirurgical Society at the beginning of last year, an abstract of which will be found in the 'Medical Times and Gazette' for February 7th, 1857.* The next addition to our author's treatise is a pretty full chapter upon "atelectasis pulmonum, or imperfect expansion of the lung." Following the majority of recent writers on pædiatrics, Dr. Meigs recognises the distinction between

* See also Ranking's Abstract, vol. xxv. p. 61.

non-expansion and *collapse*. He, however, discusses separately "collapse in the early weeks of life," because the symptoms which it gives rise to resemble much more those of congenital atelectasis than those of collapse at later periods.

"They are, in fact, those of cyanosis, and in some instances are as strongly marked as those observed in the worst cases of that condition caused by malformation of the heart or great vessels. . . . . In this form of atelectasis the child may have been born perfectly healthy, or only weaker than usual, or it may have had some difficulty in establishing the respiration, which, however, has afterwards been effected in the most complete manner. Some days or even weeks after birth, from a cause disturbing the function of respiration, portions of the lung may collapse, and give rise to the different symptoms of that condition in the manner above described. The most important of these symptoms are difficulty of breathing, consisting either in an increased or diminished rate of that function, diminution of the muscular power, cyanotic hue of the skin, and slight or severe spasmodic phenomena." (p. 123.)

The histories of four cases are related and a fifth referred to; in three, recovery took place in spite of the most dangerous and alarming symptoms, while in two death occurred in a period of about twenty-four hours. We have no account of the post-mortem appearances in the latter. This is much to be regretted. Dr. Meigs is inclined to think that the great difference between the symptoms of this form of the disorder and "post-natal collapse" of later periods, may be explained, in part at least, by the fact that the fœtal openings are still patulous, or in such a condition that they may be re-opened under pressure, and so allow a portion of the contents of the over-loaded and congested right side of the heart to pass into the left auricle and aorta, and so to the whole body. The subject of pneumonia has received fresh consideration; but upon the litigated question, as to whether the so-called "lobular pneumonia" of children in its different forms is or is not pulmonary congestion, or bronchitis with collapse of the air-cells, Dr. Meigs has nothing of his own to offer us. He remarks, however,

"I have been led to the belief that the former method of dividing the pneumonia of children into the two forms of lobular and lobar is incorrect, and I have determined to substitute for the term lobular that of *partial*, which is the one employed by M. Legendre, and very lately also by Dr. Alois Bednar, while I shall describe the other form of the disease under the usual title of *lobar*." (p. 146.)

In reference to the more frequent seat of pneumonic inflammation, the author's experience is in unison with that of MM. Rilliet and Barthez, Rufz, Berrier, and opposed to Dr. West's, in so far as its greater frequency in one lung alone is concerned. But it differs from the former as to the relative frequency with which the two lungs are attacked. In our author's practice, the inflammation occurred with equal frequency in the two lungs; whilst of 129 cases of unilateral pneumonia observed by the above authors, 84 were seated in the right, and 45 in the left lung. Whilst the author agrees in the general doctrine that pneumonia of the lower lobe is more common than that of the upper lobe, he observes:

"As it seems to be a general opinion in the profession that inflammation of

the summit of the lung is rare in comparison with that of the base, I wish to call attention again to the fact stated above—that of 33 cases in which I ascertained accurately the seat of the disease, it was in the upper lobe in 13, and in the lower in 20." (p. 151.)

We quote the following as bearing upon one of the questions of the day:

"I know that the opinion has been advanced and vigorously maintained by several more recent writers of extensive observation and experience, that pneumonia recovers as well, or almost as well, without as with bloodletting. I freely acknowledge, moreover, that of late years I have used this means more rarely and more sparingly than formerly; but I cannot doubt the evidence of my senses, and I have too often witnessed a great and immediate mitigation of the symptoms, both general and local, of the disease, from a bleeding, leeching, or cupping, to be willing to abandon the use of these means, and trust the case entirely to its natural course." (p. 170.)

The subject of bronchitis has received from the author an equal revision to pneumonia; but we do not find anything that needs particular comment.

The article on *scarlet fever* has been almost entirely rewritten, and the results of more than a hundred fresh cases have been added to those contained in the first edition. The disease is very fully considered, it having been so rife and so fatal in America in latter years, as to have become of much greater importance than formerly. Nearly seventy pages are hence devoted to its consideration. The chief points worthy of notice are as follows. The division of the disease into the *simple*, *anginose*, and *malignant* forms constitutes, according to Dr. Meigs, a faulty arrangement, and one not consonant with the nature of the malady.

"The simple form of the English writers, or that in which there is no anginose affection, has no existence whatever. So far as I have been able to discover, I believe that inflammation of the mucous membrane of the fauces constitutes an essential element of this disease; for I have never yet seen a case of scarlatina in which it was not present to a greater or less extent. It is often very slight—so slight, indeed, as to be unaccompanied by any evidence of pain in the part; but in all that I have examined it has been decided and obvious. This supposed form of the disease does not, therefore, in my opinion, exist. The two other forms usually described, the anginose and malignant, are also of little value practically, since I have found that in all severe or grave cases in which the patient did not die with violent nervous symptoms under the first shock of the scarlatinous poison, there has been developed a severe and dangerous anginose inflammation about the third or fourth day; so that it is fair to say that I cannot imagine any malignant case lasting over the third or fourth day which is not anginose, nor any severe anginose case which might not also be styled, from its dangerous character, malignant." (p. 498.)

We admit that Bateman does differentiate *scarlatina simplex*, as consisting "merely of the rash with a moderate degree of fever," and that Rilliet and Barthez do also maintain that in it "les amygdales et le voile du palais ne sont pas tuméfiés et à peine rouges." We ourselves believe we have also seen occasionally the throat so normal looking, that if the patient had not the cutaneous efflorescence, we should have had some misgivings about the matter, more particularly when bearing

in mind the very variable condition of the tonsils and adjacent parts in children. However, be this as it may, very many English writers allude to the fact, that, to use the words of Dr. West, "in most instances, even when the disease is mild, a slight degree of soreness of the throat comes on on the second or third day, the palate and tonsils appear red, and the latter are generally somewhat swollen, and deglutition is slightly impeded." Dr. Meigs settles down at last into a division of "*mild* and *grave* cases, since the only real difference between the cases is a difference in the degree of severity they exhibit." Now to our mind, the qualifying terms, "simple," "anginose," and "malignant," serve to express this degree of severity very well. The author seems to regard "cold" as the *exciting* cause of the subsequent *dropsy*, but does not appear to have made up his mind in regard to what he calls the "exact *pathologic* cause" of it.

"The form which the dropsy takes varies greatly in different cases, and seems to depend on inappreciable causes. Of the 29 cases that I have met with, anasarca alone was present in 22. In 5 hydrocephalus was present, and in 4 of these anasarca also existed. In 1 there were some symptoms which indicated the probable existence of a small amount of effusion in the brain; but they were not at all violent. Lastly, in 1 there was extensive anasarca, hydrothorax of the right side, hydropericardium, and ascites.

"The degree of danger to be apprehended from this complication depends upon the form which it assumes. M. Cazenave says that there is no danger from it so long as it remains confined to the subcutaneous cellular tissue; and this is probably true. When, however, it attacks the brain or lungs, it becomes exceedingly dangerous. Dr. Wood says that he has seen but one fatal case from dropsy, and in that the heart was diseased. Of the 29 cases that I have had under charge, 6 were fatal. Of the 22 cases in which the effusion was anasarcous alone, but 1 was fatal." (p. 528.)

About ten years back, Dr. C. Schneemann published an essay* upon the great value of treating scarlatina by rubbing the body all over with the *fat of bacon*. By a reference to Meissner's 'Grundlage der Literatur der Pädiatrik,' we find, however, that in 1810, Dähne proposed† general infriction with oil for the same purpose. According to Schneemann, as soon as we are certain as to the nature of the disease, the patient must be rubbed, morning and evening, over the whole body with a piece of bacon, so that a superficial covering of fat everywhere exists. In order to render this infriction easy, it is advisable to take a piece of bacon fat of the size of the hand, choosing a part still armed with the rind, that we may have a firmer grasp. On the soft side of this piece slits are to be made in various directions, that the fat may ooze out, and which may be still further promoted by previously warming the bacon. From this procedure, Dr. Schneemann affirms hat the most happy results accrue. The dry brittleness and itching of the skin are, for the most part, fully put a stop to; the oxidation of the

* Die sichere Heilung der Scharlachkrankheit durch eine neue, völlig gefahrlose Heilmethode nach zahlreichen Erfahrungen mitgetheilt. Hannover, 1848. 8vo.

† Beiträge zur Aetiologie und Kur des Scharlachfiebers nebst Empfehlung der Einreibung von Oel gegen dasselbe, Leipzig, 1810. 8vo. Zweite Auflage, 1821.—See also Ranking's Abstract of the Medical Sciences, vol. xii. p. 13; and the Journal für Kinderkrankheiten, subsequent to vol. ix. passim.

blood promoted; the affection of the throat modified; the period of desquamation is shortened; and the patient protected against the sequelæ of the disease. Finally,

"Owing to the fatty covering, the skin is kept moist, and the cuticle prevented from being driven about the room by currents of air, and thus one fertile source of infection is kept closed up; it being well known that infection is most easily communicated at the period of desquamation."

The proposer admits that complications may demand a modification of the practice; that ordinary therapeutic agents may be employed at the same time; and that the rubbing in is to be kept up twice a day for three weeks, and once a day during the fourth. Soon after this mode of treatment was proposed, no less an authority than Professor Mauthner, of Vienna, put it in force there at the hospital for children. He stated his opinion of it in high terms, and affirmed that he would, without hesitation, thus treat his own children if they were attacked by the disease. He had not long to wait for this latter opportunity, as the following extract will show:

"Infriction with bacon still increases in repute. I have treated my daughter (fifteen years old) by it alone, and carried her through a bad attack of scarlatina. I employ it now in *measles* (which is just commencing to rage in an epidemic form)."*

The next information we can obtain is from Dr. Ilisch, a Russian physician at Ssergiewfsk-Bad, in Orenburg.† As he was passing through the village of Dmitrewka, on a journey, he learned that in twelve days fifteen children had been attacked by scarlatina, and that all had died. He advised that other children affected should undergo "infriction," which being done, the happiest effects ensued. Acting upon this, he has employed it more extensively, and has arrived at a judgment highly in its favour (op. cit.) A third time Professor V. Mauthner‡ offers us evidence of the value of the method in question; and again a fourth time (in the paper whose title is given at the head of this article), when he likewise administers a little quiet bantering castigation to the illustrious proposer of the plan—viz., Der Herr Medizinalrath Doctor Carl Schneemann. The latter, it appears, issued a new manifesto on the 'Heilwirkungen,' &c., in which Professor V. Mauthner's support is strongly boasted of. But although the latter continues willingly to afford this support up to a certain point, he will not go beyond it, and sarcastically jokes Dr. Schneemann upon the daring peculiarities he has otherwise introduced into his treatment for scarlet fever. For what these consist in, we must refer to V. Mauthner's 'Observations.' In 1850–51, no less than 77 children were treated by "infriction," in the hospital for children at St. Petersburg. The Report says:

"It was certainly found agreeable to the children, as it cooled the burning, hot skin, and lessened its tension. But we were not able to perceive that the

* Jahresbericht &c. des St. Annen-Kinderspitales in Wien während des Jahres 1849.

† Ueber Speckeinreibung bei Scharlachkranken: Journal für Kinderkrankheiten, Band xvii. p. 128. 1851.

‡ Aus Briefen des Herrn Prof. V. Mauthner, in Wien: Ibid., p. 220.

course of the disease was specially altered; and in 25 cases desquamation by large scales followed. It was unable, also, to avert death in some bad instances of the malady."*

We heard of the importance of "infriction" in diminishing the febrile state accompanying pneumonia as referred to by a physician at a late meeting of one of the medical societies of this metropolis: let us now upon this treatment hear Dr. Meigs:

"Within the last few years *inunction* has been highly recommended in the treatment of scarlet fever. . . . . For my own part, I have never made use of the bacon fat for the purpose of inunction, except in two instances, being deterred by its disagreeable character. I have, however, employed inunction with other unctuous substances very frequently. Indeed, for the last eight years I have made use of it habitually in all my cases. The ointment I prefer is the following. I have tried others, but find this one the most agreeable and most convenient:—℞ Glycerinæ, ʒj; ungt. aq. rosæ, ʒj. M.

"There can be no doubt at the present time that the employment of inunction in scarlet fever has proved a most useful addition to our former means of treatment. In my hands it has had the effect of allaying, in all cases, the violent irritation caused by the intense heat and inflammation of the skin. In nearly all cases it sensibly diminishes the frequency of the pulse, and in many this effect is very strongly marked. It removes, of course, the dryness and harshness of the skin, keeping it instead soft and moist. It lessens or even removes the burning irritation and itching caused by the eruption. By these effects—to wit, lowering of the pulse, and alleviation of the external heat, dryness, itching, and irritation, it cannot but, and evidently does, modify and diminish most happily the injurious effects of the disease upon the constitution at large; so great is the comfort it gives to the patient, that I have several times had young children, still untaught to speak, make signs and motions at shorter or longer intervals, showing their desire to have the application renewed. The frequency of the application must depend upon the case. Where the eruption is intense, the skin very hot, and the febrile symptoms marked, they should be made every two or four hours, or even oftener. In milder cases they need to be repeated only three or four times in the twenty-four hours." (p. 539.)

Of this particular application of an ancient therapeutic measure† we ourselves have no experience. We have for some time felt the necessity of caution in paying attention to out-of-the-way recommendations in therapeutics, as made by our German brethren. We remember with what excited interest and curiosity we began a paper a few years back, in the German 'Journal for Diseases of Children,'‡ entitled "A strange Remedy in the Eklampsia of Children." It was, *risum teneatis*, to hold the rump of a pigeon against the anus of the little patient during the paroxysm (*so stirbt das Thier schnell und der Anfall hört rasch auf*). The remedy was too good to be forgotten. So, when we received our next number of the 'Journal,' behold in it "An Addition to Dr. Blik's Communication on the 'Pigeon's Rump-cure' in the Eklampsia of Children, by Dr. J. F.

* Berichte über das Kinderhospital zu St. Petersburg die Jahre 1846–1851 umfassend von Dr. J. F. Weisse.

† "Ungi enim leniterque pertractari corpus, etiam in acutis et recentibus morbis oportet."—Celsus, Lib. II. cap. xiv.

‡ Vol. xvi. pp. 159, 381.

Weisse, of St. Petersburg." He had tried it in two cases, and it had been successful; "the bird, soon after its application, several times gasped for breath, and closed its eyes from time to time; it then convulsively twitched its feet, and finally vomited;" the convulsions in the child then lessened materially, but the bird was done for, and it soon died. Finally, the learned author calls upon his medical brethren to institute investigations respecting this method of treatment, as if really to be depended upon, "it will be such a great gain in children's practice, particularly amongst the lower orders." We mentally offered (such as were so disposed to act in conformity with Dr. Blik's request) the advice of an old French physician, who, on being asked his opinion of a new remedy that was highly praised for its extraordinary virtues in a certain disease, very gravely replied, "Dépêchez-vous de vous en servir pendant qu'il guérit."

Dr. Meigs is shy of the "cold affusion" over the whole body, according to Currie's plan, preferring "the use of the tepid affusion bath, immersion baths, and of lotions with tepid or cold water." The whole of the author's discussion of this subject merits perusal. In a clinical lecture lately published (Gaz.-Hebdomad.) by M. Trousseau, occur some interesting observations upon this topic, and likewise upon the scarlatina of puerperal women. In the month of May, a girl, twenty years of age, who had been ill two days with a violent attack of the disease, entered the "clinic" of M. Rostan. The latter requested M. Trousseau to see her, or to take her into his department, asking his opinion at the same time as to the propriety of a lowering treatment. M. Rostan was inclined to enforce venesection, whilst M. Trousseau proposed cold affusion. The pulse was 144, the heat of skin considerable, and delirium with violent excitement present. Cold affusion (with water at a temperature of 59° Fah.) was successfully employed. But, says M. Trousseau,

"I must observe, that my ordinary treatment of scarlatina does not consist in the use of this agent; it must only be employed against violent ataxic nervous symptoms; nor must its value be considered as equivalent with that of a bath. Cold affusion lasting only for a few seconds is alone of any effect." (*op. cit.*)

Dr. Corson, of Pennsylvania, a strong advocate among the American practitioners for "cold affusion," has also advised the local application of cold to the throat. In cases attended with great heat of skin and fully-developed pulse, Dr. Meigs has satisfactorily employed "pieces of ice wrapped in flannel, and applied behind the angles of the jaws." Of the prophylactic virtue of *belladonna* he has received "an impression decidedly favourable," but his experience of its employment appears very limited. More than a hundred pages of new matter upon the "Diseases of the Skin" have been added since the first edition of Dr. Meigs's treatise. We should greatly have preferred if the author had sought to make the preceding portion of his work more complete, than to have touched upon (and necessarily very imperfectly) what most prefer to see treated as a distinct speciality. Nothing is said concerning diseases of the heart or of the urinary organs; fever, scrofula,

with tuberculosis and rachitis, remain untouched, along with all their important relations to the lungs, peritoneum, &c. Even the subjects of *rubeola*, as distinct from morbilli, and *scarlatina rheumatica*, as it has been called, in its relations to rheumatism and scarlet fever, are unnoticed, notwithstanding stress is laid by the author upon the elaboration of his chapters upon the two typical affections. We have missed also any reference to the cardiac relations of chorea. In his next edition let Dr. Meigs cancel all that he says about the non-febrile cutaneous affections, and give us something in reference to the above and other important topics connected with pædiatrics that he has hitherto let slip.

There are few diseases in connexion with which those who pay particular attention to pædiatrics would express themselves with more caution than in respect to the *fevers* of children. In this country, for years past, the terms "remittent fever," "infantile remittent," as also "gastric remittent" and "gastric fever," have been usually employed to designate a general febrile condition, with more or less disturbance of the alimentary canal. The designation "infantile remittent" has been the chief favourite, and intended to express the "fever" *par excellence* of early life. But few who know much about our present subject would deny that this expression has been most loosely employed, and been made to indicate fever of a primary and idiopathic kind, as well as reactional pyrexia secondary to many sorts of local lesion. We do not go beyond the truth when we say that typhoid fever, catarrhal fever, forms of enteritis, acute tuberculosis, ague, hepatic and portal congestion, &c., are being constantly included under "remittent fever," to say nothing of other affections, in lieu of the diagnosis of which "infantile remittent" is "a refuge for the destitute." But it must be admitted there are some very great difficulties surrounding the differential diagnosis of idiopathic and reactional fever in the child in many cases, and which difficulties those who are best acquainted with disease in childhood experience no less than do those who care little about treating it. We shall here refrain, however, from entering upon this part of the question further than relates to the discrimination between typhoid fever and the reactional febrile erethism attendant upon some diseases of the alimentary apparatus, and we premise *in limine*, that without meaning to express any decided opinion upon the specific, essential, or fundamental difference between *typhoid* and *typhus*, we assume many of their clinical manifestations are so generally distinct as to permit of their separate study, and that our typhoid is the typhoid fever of Jenner. We before remarked that this affection is very frequently included under "remittent fever." The recognition of it as "remittent fever of a low type" is occasionally seen, but the admission of *typhoid fever* as a disease of childhood is by no means common. In the reports of the Registrar-General fevers are classified as *ague, remittent fever, infantile fever,* and *typhus*, the mortality under this head (typhus) including all the forms of common continued fever.* Yet, strange to say, eighty years

* Eighteenth Annual Report, p. 181.

ago, Hamilton and Underwood differentiated fever in the child, and separated what they called "typhus" from remittent, mesenteric, hectic fever, and marasmus. Underwood writes:

"A fever of much importance, however, is mentioned by Dr. Hamilton, and is a true low fever or typhus. It is very accurately described by him, and particularly as commencing generally rather with marks of languor and fatigue, than with any distinct rigor; the stomach is frequently sick, and pain in the head soon succeeds, with great thirst and restlessness. On the second day there is an evident remission, which is at the first very regular, and continues—though in a less degree—till the termination of the fever; and yet, as far as my experience has gone, the little patient recovers, if duly supported."

But, behold the wisdom of modern days! Drs. Merriman and H. Davies, Dr. Underwood's editors, correct the above *error*, and affirm this distinct and "true low fever" to be *infantile remittent*. According to Dr. Merriman, "the typhus or low fever next mentioned is a more aggravated form of the remittent with increased debility." So, replies Dr. Davies, "we have deemed it advisable to let the title remain, considering it, with Dr. Merriman, as one of the grades of the infantile remittent fever."*

So firm a hold, indeed, has this latter term got of us here, that it is retained even in the work of Dr. West, in which the true nature of the affection so designated is of course known, and as frankly admitted. It is not before 1831 that we find *typhoid fever* distinctly recorded by this title as occurring in the child; from when up to 1836 several cases are to be found in the 'Clinique des Hôpitaux,' 'Gazette Médicale,' and 'Lancette Française.' In 1836, M. Hutin published, in the 'Revue Médicale,' a short notice "On an Epidemic of Typhoid Fever specially attacking Young Children." But it was not until 1840 that the first complete monograph on the disease appeared; in this year M. Rilliet published one as his inaugural dissertation; and about the same time, M. Taupin issued a series of 'Clinical Researches' upon the same subject. In German literature we do not meet with the recognition of typhoid fever in the child until 1846, when Löschner treated of it in the 'Prager Vierteljahrschr.' iii. Jahrg. 1 Band. In addition to the above names we may add those of Rufz, Audiganne, Stöber, Roger, Friedleben, Hennig, &c., as indicating some of those to whom within the last ten years we stand indebted for extending inquiry upon the subject before us.† But until the recent appearance of Dr. Friedrich's monograph, no approach had been made to the able essay upon "typhoid fever" contained in the second volume of MM. Rilliet and Barthez's classic work, 'Traité Clinique et Pratique des Maladies des Enfants.' The conclusions of the latter are based upon 111 cases, including the results of 12 necroscopies; those of the former are built upon the records of 275 examples, and of 10 post-mortem examinations. As most of our

* Dr. Underwood's Treatise on the Diseases of Children, with Directions for the Management of Infants. Tenth Edition, with Additions, by Henry Davies, M.D., &c. pp. 312–14. London, 1846.

† Some papers by Dr. Lederer on Typhus in Children, in the Mediz-Wochenschr. 1857—Nos. 6, 7, 8, 12, 13—came to our knowledge too late for analysis in the present article. We shall refer to them in a future number, when discussing the merits of the new edition of Dr. Underwood's treatise, and bringing under notice Dr. Tanner's compilation.

readers who have much to do with sick children are no doubt well acquainted with the labours of the French writers, we shall here make Dr. Friedrich our *point de depart*, indicating some of the chief conclusions he has arrived at, and comparing them, *en passant*, with those of MM. Rilliet, Barthez, and others. We shall follow the same sequence adopted by the author in the consideration of his subject—premising that the term "abdominal typhus" is synonymous with our typhoid, whilst the Germans employ "petechial typhus" to signify our typhus fever.

1. It is clearly established that "typhoid fever" is not an unfrequent disease amongst children, occurring sporadically and as an epidemic. Boys are more liable to it than are girls. These conclusions, we may remark, are in consonance with those of MM. Rilliet and Barthez, Taupin, and Löschner, whilst Friedleben's rate of frequency as regards males and females is in the opposite way—viz., 46 boys to 52 girls in 104 cases.

2. The mortality is less in children than in adults, and higher amongst girls than amongst boys. Friedleben's experience agrees with the latter statement, whilst Löschner's is somewhat opposed to it, and Rilliet and Barthez remark, "Girls appear to us less exposed than are boys to the very grave form. In 23 cases of very severe typhoid fever, 19 were boys, 6 girls; the proportion of the whole number of girls to boys in the other forms of the disease being as 27 to 61."

3. It is most frequent between six and eleven years of age, and is rare during the period of lactation. According to Löschner, from five years to nine is the time of greatest liability; to Rilliet and Barthez, from nine to fourteen; to Wunderlich, six; Friedleben, five to eight. Most writers are agreed upon the rarity of its occurrence during the first year of life, some even denying its existence then altogether. But this is going to too great a length, notwithstanding a source of fallacy then surrounding the diagnosis of typhoid fever, and to which we shall presently particularly refer. We may, however, observe that Hennig has recorded it as occurring at the age of three months; Wunderlich between two and three months; Rilliet at seven, ten, and thirteen months; and Dr. Friedrich at six months. We find on reference to our own note-book, a case of "typhoid" recorded in a girl one year and seven months old, and in a boy two years of age. Dr. Friedrich refers to Bednar as having certainly observed it in a child only five days old, and as recording two problematic cases of fifteen and eleven days respectively. Being somewhat curious about these instances, we went direct to Bednar. We found that the mother of the first child died of "metritis," and the child itself exhibited, amongst other signs, enlarged spleen, and perforation of the bowel next the ileo-cæcal valve. In the second case, there existed "gangrene" of the left axilla, &c., swelling and injection of the Peyerian patches, and splenic enlargement. In the third instance, the infant (a female) showed signs of pyæmia, with commencing destruction by decomposition of Peyer's glands. Bednar remarks* upon these cases:

* Die Krankheiten der Neugebornen und Säuglinge, &c., Theil i. p. 114. Wien, 1853.

"The three examples adduced form a gradual transition to a diseased state, having its origin in purulent infection of the blood, and only observed in children within the first fourteen days of life born of mothers who have been attacked by 'puerperal metritis.'"

4. Typhoid and scarlet fevers have an inverse ratio as respects epidemic occurrence. When one prevails, of the other only individual cases are met with. This law was first advanced by Stöber, was acquiesced in by Löschner, and supported by Friedleben, as one easily to be proved. Dr. Friedrich appears to have taken particular trouble in inquiring into this point, and his statistics unquestionably support the doctrine.*

5. We shall here quote the author's own *résumé :*

"The morbid anatomical changes of abdominal typhus in the child exhibit either the same variations, or the like constant relations, as are seen in the adult in the same malady. In the latter category, *splenic enlargement* is particularly noteworthy. Still it is extremely rare that the formation of a slough ensues, with the consequent peculiar typhous ulceration following previous infiltration of the Peyerian patches. Mostly only single follicles in the glandular assemblage are infiltrated, and which, either from resorption of the infiltrated material, or more often from rupture of the follicle within the intestinal canal, revert to a normal condition without the formation of any cicatrix. The rupture and evacuation of the follicle within the bowel take place generally only to a limited extent. Ulceration of the mucous membrane of the pharynx, of the œsophagus, of the trachea, &c., is of exceedingly rare occurrence in the child." (p. 100.)

"The spleen was enlarged in *all* of our cases; once it was of normal consistency and colour, six times dark and firm, and three times soft and mashy." (p. 39.)

The experience of almost all observers since the time of Barrier is in accordance with the statements of the author relative to the aggregated glands. These latter usually present in the child that change known, since the time of Louis, as "*plaques molles,*" and very rarely that implied as "*plaques dures.*" Changes of the sub-mucous tissue and ulceration are rare, still, the "*plaques molles*" may ulcerate, and in extreme cases even perforation may ensue. MM. Rilliet and Barthez have proposed the question, whether

"This predominance of the '*plaques molles*' is an accidental circumstance? Or, on the contrary, is it due to the special structure of the 'patches' in children, to the physiologic office this secretory apparatus fulfils, or rather to the special mode of irritation which in early life produces tumefaction, and engorgement of the intestinal follicles? This is a point difficult to determine." (p. 666, op. cit.)

With reference to the enlargement of the spleen so constantly met with by Dr. Friedrich, we may observe that his experience is supported by that of Barrier, Löschner, Friedleben, and Bednar, though they all differ somewhat amongst themselves as to the consistence of that organ. MM. Rilliet and Barthez appear to attach but little value to changes in the spleen.

* Upon some points connected with the relations of these two diseases there are some remarks by Dr. Mayr in the Wochenblatt der Zeitschrift d. k. k. Ges. d. Aerzte in Wien, 1856, p. 47, worth referring to. See also Lehrbuch der Kinderheilkunde, 1857, Heft 2, Analecten, p. 27.

6. Amongst the symptoms of typhoid fever in the child, the following are the chief: splenic enlargement, diarrhœa, meteorism, gurgling; pyrexia, quickened respiration, bronchial catarrh; delirium, somnolency. Shivering, intestinal hæmorrhage, and lesions of motility are rare. According to M. Taupin, out of 121 cases, 109 exhibited considerable, and 10 a moderate, tumefaction of the spleen; whilst MM. Rilliet and Barthez in 61 cases met with it only 18 times; and in 44 other instances observed since their previous publication, it was only 10 times that it projected two inches beyond the ribs.

7. As the cutaneous eruption, *a roseola* is the common form, more rarely a papular exanthem. By the term roseolous rash, the author evidently implies the "*taches rosées*" and "rose spots" of other writers. By the "papular" rash, we can but assume he refers to the more discoid and elevated of the "taches rosées." At any rate, being in default of a fuller explanation from Dr. Friedrich, we referred to Griesinger,* who remarks:

"Whilst herpes labialis occurs so frequently in intermittent but so very rarely in typhus, that its presence in a patient alone renders the diagnosis of 'typhus' in the highest degree improbable, so in a like manner is the *roseola* exanthem characteristic of the latter. It consists of round rose-coloured spots of a scarcely appreciable elevation, from 1 to 1½ line in diameter, which under the finger's pressure at first at any rate entirely disappear. To call it a characteristically papular exanthem is scarcely correct—there are no true pimples."

If we understand the author aright, some amount of "roseolous" rash was always present in his cases; his experience, therefore, would agree with that of Fabre, who says:†

"The lenticular 'taches rosées' and sudamina are nearly constant in children, and what is peculiar is that the former are especially frequent on the back and extremities, so that if the abdomen and chest only are examined, their presence may often not be apparent."

The frequency of occurrence of the "rash," and its degree of profuseness, are points on which there is great diversity of opinion, however; and in connexion with this department of the subject, MM. Rilliet and Barthez give utterance to opinions with which we confess we are at open war. They observe, in reference to the spots:

"They are to be seen principally upon the abdomen, the chest, and the upper parts of the thighs. . . . . In the greater number of cases they are but few—from one to six at a time. This result (to which we had arrived in 1840) is the inverse of that obtained by M. Taupin, but conformable to the experience of MM. Rufz and Stöber. . . . . The lenticular 'taches rosées' are not constant in the typhoid affection. This we had already affirmed, and the more recent facts collected by us afford not less support to the opinion, though slightly modifying the rate of frequency—viz., in three-fourths instead of two-thirds of the patients. Further, *the spots exist in other affections which are not typhoid fevers.*" (p. 684.)

The italics are our own; the doctrine conveyed by them we abro-

* Handbuch der Speciellen Pathologie und Therapie, Zweiter Band, Zweiter Abtheilung: Infections-Krankheiten, p. 162. (Red. von Virchow.)

† Bibliothèque de Médecin-praticien, tom. vi., Maladie des Enfants, p. 516.

gate entirely—i.e., that the *true taches rosées* found in typhoid fever may be met with under other circumstances. Spots that may be negligently or otherwise unwittingly mistaken for them possibly may be, but if there is any meaning in the true ones, it is that they *clench the diagnosis of typhoid fever.* Yet can MM. Rilliet and Barthez be mistaken? We shall inquire further presently. But are the rose-spots not often wanting in children? We certainly diagnose what we consider "typhoid," and yet do not necessarily *find* them. Drs. West and Jenner* are sometimes equally at a loss; and Dr. Campbell remarks:

"According to our observations and those of many physicians with whom we have conferred in relation to the eruption of typhoid fever in those cases which occur in the Southern and Middle States, even where the disease has prevailed as an epidemic, these eruptions are very rarely observable, although looked for with the utmost diligence throughout every stage of the disease. This absence of eruption in cases where every other symptom of typhoid fever was present, has been so uniform that many of our most intelligent practitioners have been disposed to doubt the diagnostic importance of eruptions in typhoid fever, and to look upon them as of accidental occurrence, not having any important connexion with the true pathology of the disease." (p. 13.)

We are as little inclined to believe this latter doctrine as the former one; but in children, certainly, the *taches rosées* are not so frequently observed as they are in adults.

8. The disease assumes three forms—viz., a mild, a severe, and a very grave form. As a rule, it appears generally in the mild form. In these statements the author is in accord with the French and other writers.

9. The prognosis is in general favourable, but of course varying with the form the disease has assumed.

10. The disease cannot be cut short, and the best method of treatment is the *expectant,* with medium doses of calomel between the fifth and eighth day of the disorder. Under all circumstances, the powers of the patient must be preserved, and proper nourishment early enforced. MM. Lombard and Fauconnet have been strong advocates of calomel, whilst MM. Rilliet and Barthez, though not entirely deprecating it as a purgative, quite discountenance what may be called a mercurial plan of treatment. Our author, too, it should be borne in mind, limits the period for its administration to between the fifth and sixth day, when it is chiefly serviceable.

11. The influence of contagion in propagating the disease is very doubtful. MM. Rilliet and Barthez think much in the same way. "Without pretending to deny the influence of this cause, we will rest satisfied with affirming that it is less evident and less frequent than in respect to many other maladies."

12. The influence of particular months of the year in inclining to typhoid fever, or to its greater mortality, cannot at present be indicated. The following remarks of Hennig† may not be here out of place. He resides in Leipzig, it should be remembered.

* Medical Times, vol. xx. pp 491 et seq. 1849.

† Lehrbuch der Krankheiten des Kindes, &c., p 91. Leipzig, 1855.

"Abdominal typhus is an extremely uncommon disease in children amongst us. Out of 3000 sick children whom I have observed, I have only been able to diagnose its existence with surety three times. . . . . Elsewhere it appears more common, particularly in Vienna, where Mauthner once counted 26 cases of it amongst 556 little patients. Several of the former were still at the breast."

Although the idiopathic fever we have been discussing had not been recognised as, and termed, *typhoid fever* in the child, except by those writers whom we have before pointed out, there cannot be the least doubt its more important morbid manifestations were well known to others, though they regarded them in their totality as constituting quite another disease. Abercrombie in 1820, Billard in 1828, Meisner in 1828, Evanson and Maunsell in 1836, and several others more recently, have more or less accurately described both the symptoms and post-mortem lesions of typhoid fever as those of a primary inflammatory affection of the intestinal mucous membrane and its appendages (glands, follicles, &c.), accompanied by a secondary fever of a more or less adynamic type. Numberless instances of ileitis, follicular enteritis, dothinenteritis, &c., have thus been nothing less than typhoid fever. But it behoves us not to be too boastful of our own increase of knowledge. Most of the older writers seem to have had no suspicion of the occurrence of typhoid fever in the child; and even when attention began to be attracted that way, it was thus that M. Chomel, in 1834, spoke upon the question:

"We have not the fear of erring before our eyes, when we say, that below fifteen years of age the number of children attacked by typhoid fever continually diminishes to ten years, after which it appears that children are but very rarely affected by the disorder."

But now practitioners, convinced of its frequent occurrence in early life, well acquainted with its signs and symptoms, and not forgetful of the rocks upon which they may split, are, it is feared, frequently unable to determine whether they have before them primary disease of the intestine, with a reactional pyrexia of a low type—or, on the contrary, typhoid fever. When the course of the malady is short, the child not very young, and a combination of particular causes not being in operation, we may be generally able, even at an early period of the disorder, to diagnose typhoid fever when it is present. We would speak advisedly, however, since MM. Rilliet and Barthez express themselves not less doubtfully in reference to childhood, than do MM. Friedleben and Fleisch in respect to early infancy. After describing the morbid anatomic changes in primary follicular enteritis, MM. Rilliet and Barthez go on to say:

"It is unnecessary further to dilate upon this description, since this inflammation of the 'patches' is identical, with exception of intensity, with that which is seen in typhoid fever. In fact, the appearance is the same; and if there exist differences between these two diseases, it is elsewhere than in the lesion of the 'patches' that they must be sought. . . . . Further, this development of the 'patches,' so common in children, is accompanied by symptoms which are analogous to those of *non-follicular* enteritis; whilst we

shall find certain examples of the latter assume the typhoid form, from whence there further arises an approximation between typhoid fever and enteritis in the child." (Vol. i. p. 188, op. cit.)

But the difficulties of diagnosis, according to these eminent writers, become insuperable when we arrive at what they term "*typhoid enteritis.*" In the follicular form the *morbid changes* are those of the idiopathic fever; in the typhoid variety it is the *symptoms* which are so markedly the same.

"Does there exist an enteritis whose symptoms are typhoid?

"We should think that the disease is of the same nature as dothinenteritis, with a different anatomical expression. . . . . We have seen *elsewhere* that there exist in the child alterations of Peyer's patches which recal those of typhoid fever, though the symptoms of the disease of which they are the expression are very different from those of this pyrexia. *Here* we find typhoid symptoms without follicular or mesenteric changes; farther, we shall afterwards see examples of dothinenteritis of the form of the simple affection. It appears to us natural to group together all those facts which would constitute an incomplete, bastard, or simply anomalous typhoid fever—so that between typhoid fever and acute catarrhal diseases of the intestine it may be shown there exists an intermediate affection (typhoid enteritis), connected to the former by symptoms, to the latter by morbid anatomic changes, and which would establish the union between them." (Vol. i. pp. 787 et seq., op. cit.)

Now, with all deference to the reputation of the two French pathologists, we must confess that we do not always perceive that clearness and consistency of description in their writings upon the above and correlated topics which we have a right to expect, notwithstanding the difficulties which undoubtedly exist. In the first place, we would observe, that although we are told (vol. i. p. 787) that the symptoms accompanying follicular enteritis are very different from those of typhoid fever, we are informed (vol. ii. p. 695) that in young children the *symptoms* of the one may so simulate those of the other malady, that they may be confounded together; indeed, M. Rilliet has himself confessed to making the mistake. And "if the acute catarrhal affection of the bowel may in very young children be easily confounded with dothinenteritis, the mistake is still easier when the intestinal inflammation assumes the typhoid character." (See also vol. i. p. 704.)

Secondly, at vol. i. p. 787, it is affirmed that in typhoid fever the lenticular spots *manque rarement*, whilst elsewhere (vol. ii. p. 684) it is maintained *que l'éruption manquait assez souvent*, and also that they have the same characters as the rose spots of enteritis! We have before seen that MM. Rilliet and Barthez maintain the "rose spots" are met with in other affections, as well as in typhoid fever—as in "slight gastritis," &c. They, however, admit that "in all these cases it is true they have not been very abundant, and were of short duration." (Vol. ii. p. 684.)

Now, our readers will understand how difficult must be the diagnosis where fever exists with diarrhœa, slight tympanitis, splenic enlargement (vol. i. p. 703), and rose spots: between these as manifestations of primary enteritis, or, on the contrary, of typhoid fever! No wonder that MM. Rilliet and Barthez have come to the conclusion, that "in certain cases it is impossible to distinguish between the two

affections." (Vol. ii. p. 698). That this must be the case, we would be the last to deny, but we must agree with Dr. Parkes in his assertion,* that "the diagnosis of typhoid fever is absolute when, in a febrile disease attended by looseness of the bowels, unequivocal rose spots appear on the sixth or eighth day." If they do not appear then, the diagnosis cannot be perfected until the case has been watched for several days, and the age of the patient and history of the malady been carefully studied; but the rose spots appearing in a hitherto doubtful case posits the diagnosis. But if the rose spots are not found? Then in many cases, particularly in children between one and five years of age, an absolute diagnosis is scarcely possible. The truth of this and some previous statements will be sufficiently apparent on consulting M. Hervieux's essays (referred to at the head of this article), containing the results of minute investigation into disease of the glands of Peyer during early life. In his first memoir, children up to a month old are included; in his second, those from one to twelve months old; and in a third essay, yet to appear, children from one to five years will form the subjects. The first series of observations tend to show that it is not in *very early* infancy that great difficulty of diagnosis can arise, as the symptoms (speaking generally) are then more those of entero-colitis or diarrhœa and marasmus, than of typhoid fever. But as the child gets older,

"The peculiar typhous manifestations (which are entirely absent in the new-born child) begin in the second infancy—i.e., from one to twelve months of age—to make themselves evident, particularly in children approaching the end of their first year; yet more prominently shall we perceive this to be the case in patients between one and five years old." (p. 232).

"In children of the second period, the diagnosis of the morbid changes of the Peyerian and Brunnerian glandular apparatus is capable of no greater surety than in those of the first, and we could very easily prove that none of the many manifestations we have as yet adduced belong directly to these morbid changes, and that only the *totality of these manifestations* determines to some extent the diagnosis." (p. 236.)

According to M. Hervieux, the *toute ensemble* here referred to "produces, in a word, just such a physiologic and general expression which forcibly recals to mind the typhous condition of the adult." (p. 243.)

Further, it is worthy of remark, that although *gurgling* and *splenic* enlargement were present in a great many cases, along with the other signs, yet we do not think M. Hervieux refers to the occurrence of the rose spots in any of his cases.

But independently of the difficulties arising from a confusion of the symptoms of typhoid fever and its intestinal complication with those of certain forms of enteritis and their reactional typhoid pyrexia, there are several others which complicate the diagnosis here involved. *Acute tuberculosis, simple* and *granular meningitis, bronchio-pneumonia,* may all be mistaken for typhoid fever, or *vice versâ*.† We have no space,

* Medical Times, vol. xxi. p. 396. 1850.

† The new edition of Dr. J. Hughes Bennett's Clinical Lectures may be referred to (p. 882), where the question, "Remittent fever—can it be separated from acute hydrocephalus?" is discussed, and answered in the negative; p. 873 also, *quoad* the value of the febrile exanthems.

however, for discussing their differential diagnosis, but would limit ourselves to the following consideration—What, viz., is the nature of those numerous cases, of comparatively slight febrile movement, of *very marked remittency*, in which the bowels are generally constipated, the thirst great, and the emaciation occasionally by no means inconsiderable? What, too, is the nature of those allied cases, occurring more particularly in the summer months, in which vomiting is allied to the constipation, and intense heat of skin present with, apparently, cephalalgia? No doubt such forms of disease are also, along with others we have mentioned, frequently called "mild remittent," "gastric remittent," "gastric fever." But in these, is the pyrexia a primitive one? Or is it not rather reactional upon what the French writers denominate, though perhaps a little vaguely, *embarras gastrique?* We confess we have not yet been able to determine the matter; nor perhaps should we be surprised at this, seeing that Dr. West observes:*

"There are still many questions that might be proposed with reference to the remittent fever of children, but on which I do not enter now because I am at present unable to give you what would be, even to my own mind, a thoroughly satisfactory solution of them."

And that MM. Rilliet and Barthez are so puzzled to know what to make of these varieties of "mucous," "remittent," and "gastric" fevers, as to be obliged to invent a new theory and name for some of them—viz., "*fièvre catarrhale gastro-intestinale*," and in which the fever is said to be "*non*-reactional," whilst others in which the fever is "reactional" (though they are admitted to be occasionally tainted by the *taches rosées*), are included under the *embarras gastrique* of old acquaintance. Speaking of the former class of cases, they remark:

"It is easy to understand the possibility of the existence of a catarrhal fever having a remittent, or an intermittent, or a continuous type. . . . . The admission of this pyrexia into the nosologic list, contradicts in nothing the opinions we have expressed upon the nature of catarrhal diseases. So far from that, if we compare with the former the analogous pyrexia of which we have spoken under the name of *fièvre catarrhale broncho-pulmonaire*, from such comparison a new proof will result of the identity of catarrhal maladies (wherever they be seated), as well as a confirmation of our theory." (Vol. i. p. 728.)

A careful review of much that has been written upon our present subject, together with a very fair experience derived at the bedside of the sick child, forces us to admit that although the diagnosis of typhoid fever in the mass of cases may with due attention be substantiated, yet there occur numerous instances where the diagnosis cannot be made absolute until such time as it is next to useless for purposes of treatment, and that this is more particularly the case where the differential diagnosis lies between "follicular enteritis" with reactional pyrexia of a low form, and the fever in question.

Review XIII.

On Squinting, Paralytic Affections of the Eye, and Certain Forms of Impaired Vision. By Carsten Holthouse, F.R.C.S.E., Surgeon to the Westminster Hospital, and Lecturer on Surgical Anatomy

* Lectures on the Diseases of Infancy and Childhood, p. 502, second edition.

at its Medical School; Surgeon to the South-London Ophthalmic Hospital; and late Surgeon to the British Hospital, Smyrna.—*London*, 1858. pp. 210.

IN the preface to this work, Mr. Holthouse expresses his conviction, that common as strabismus is, there are few deformities the pathology of which is less understood; the array of conflicting opinions adduced is certainly not flattering to medical science, nor can reflecting men look back with feelings of satisfaction to the undignified competition, the literal searching of highways and byeways for cases, the puffing announcements of operations astounding in number and incredible in results, which followed the introduction of ocular myotomy into this country. A natural reaction took place, and with many surgeons the operation is now discountenanced, except in extreme cases. Nor has the warfare, which has almost constantly raged upon this subject, elevated it in public estimation. Not long since, the leading journal of the day, commenting on the speculative character of the age, stated its opinion that "paper," in the commercial sense, was the bane of the present day. The character of many of the communications to serial journals leads us to suspect that "print" is scarcely less injurious from its profuseness in certain professions; and the subject of strabismus has proved as vexed a question, and has led to as much ink-shed as any we happen to remember. These remarks, however, do not apply to the little book before us; it is modest in its pretensions, and is evidently written by one who has observed and thought for himself.

Much pains have been bestowed by Mr. Holthouse in determining the true pathology of strabismus; and his conclusions are these:

"1st. The most frequent *exciting cause* of strabismus is some lesion of the nervous centres or nerves; and next in frequency are inflammatory affections of the eyes.

"2nd. The *essential or immediate cause* of confirmed non-paralytic strabismus is a shortening, with or without hypertrophy; or simple hypertrophy of the orbital muscle in the direction of which the eye is drawn.

"3rd. These muscular changes may be associated with thickening and contraction of the conjunctiva and sub-conjunctival tissue, and an adhesion of these to the sclerotic coat of the eye.

"4th. The above-named changes may affect both eyes, though they are more commonly confined to one eye. In the former case 'it is immaterial which eye is operated on.' In the latter it is not immaterial, but on the contrary essential that the affected eye should be distinguished and selected for the operation.

"5th. The imperfect vision of the strabismic eye may either precede and be the cause of the distortion, or may follow and be the consequence of it. In the former case, the operation of dividing the affected muscle will not remove the imperfection of sight. In the latter it will.

"6th. The morbid changes referred to in deductions 2 and 3 are competent to explain all the phenomena of strabismus.

"7th. The phenomena of strabismus cannot be accounted for on any other hypothesis." (p. 128.)

The effect produced on the sight by division of the muscles, was one of the facts which early attracted notice, after the introduction of ocular myotomy. So far back as 1841, Guerin recommended division

of the recti muscles as a cure for myopia, and other practitioners performed it not only for the relief of that affection, but for asthenopia also. In some cases it answered, in others it signally failed, mainly from the class of cases to which it was really applicable not being fully recognised. On the whole, the treatment was too heroic to gain a firm hold on popular favour, seeing that spectacles supplied the coveted want; but there can be no doubt that where imperfect sight exists in connexion with strabismus, the division of the rectus generally improves not merely the aspect, but the sight of the patient; had John Wilkes lived in our days he would doubtlessly have been rendered not merely a handsomer, but a more far-seeing man, by the rectification of that villanous squint of his.

The imperfect sight attending strabismus is regarded by Mr. Holthouse as a very important aid in diagnosing the squinting eye, especially in cases in which it is by no means easy to distinguish between the eye which is primarily and structurally affected, and that which is secondarily and often only functionally involved.

"Here, then" (says he), "some reliable means of diagnosis is required, and this is to be found in the condition of vision which is nearly always imperfect in the worse eye; if, however, no difference should be discovered in the power of either, it is immaterial which is selected for operation. Mr. Walton, in a letter published in the 'Medical Times and Gazette,' of October, 1856, has called in question the value of this test, and vaunts one which he has proposed as superior; but I have had occasion already, in several parts of this work, to point out its shortcomings, and shall therefore now merely refer my readers to Cases 11, 23, and 24, where its failure was too patent not to be noted. The objection which Mr. Walton urges to the vision test, rests on a solitary case of strabismus, in which he says he satisfied himself that the worse seeing eye was the one that did not squint." (p. 141.)

So much for the test, which is important, "because," says Mr. Holthouse, "on the selection of the eye to be operated on, will ofttimes depend the necessity, or otherwise, of a second operation." (p. 143.)

Mr. Holthouse was, we believe, the first in the country to draw attention to the merits of the subconjunctival section for strabismus, and to that operation he still gives the preference. In many cases undoubtedly this operation is of great service, but no one should attempt it on the living subject until he has thoroughly familiarized himself with it on the dead. To perform it satisfactorily and with success, requires more practice than falls to the lot of the majority of surgeons, and failure is annoying to both patient and operator. The author points out that there are cases in which the ordinary operation is to be preferred to the subconjunctival one—those in which mere division of the shortened muscle is not sufficient, but the conjunctiva and subconjunctival fascia require to be freely divided before the eye can be brought into a central position.

In addition to the subject of strabismus, Mr. Holthouse devotes a chapter to paralytic affections of the eye, and another to certain forms of impaired vision. The cases narrated possess considerable interest, and add to the value of this book, which may be consulted with advantage by the student and by the practitioner.

PART SECOND.

Bibliographical Record.

ART. I.—*On Malformations, &c. of the Human Heart; with Original Cases.* By THOMAS B. PEACOCK, M.D., Fellow of the Royal College of Physicians, Assistant Physician to St. Thomas's Hospital, and Physician to the Hospital for Diseases of the Chest, Victoria Park.—*London*, 1858. pp. 143.

THE present volume is in the main a reprint of lectures and papers that have already appeared in various periodicals; important and interesting as the subjects are with which it deals, we cannot but feel grateful to Dr. Peacock for having collected the *disjecta membra*, and presented them in an easily accessible form to the profession. While the author carefully investigates the history of the individual malformations, he presents us with a large number of illustrative cases which have fallen under his own observation. The value of these cases is further enhanced by numerous lithographs, which have the double merit of being perfectly intelligible, while they are well drawn.

The subject-matter of the book is considered under five main heads. Under the first, Dr. Peacock discusses misplacements of the heart occurring congenitally; under the second, congenital deficiency of the pericardium. The third division is devoted to malformations of the heart, in the strict sense of the term; this includes malformations dependent on arrest of development at an early period of fœtal life; malformations preventing the changes which should ensue after birth; and such as lay the foundation of disease in after life, while they do not in themselves interfere with the functions of the heart. Those malformations only are spoken of which are compatible with extra-uterine life, whether of long or brief duration.

The first class of malformations comprises cases in which the heart consisted respectively of two cavities, of three cavities, and of four cavities, with one or both septa imperfect; the presence of a supernumerary septum, to which the author attributes "the majority of cases of apparent duplicity" of the cavities of the heart which are on record, is discussed under the same head. It appears that the right ventricle is the part most liable to this malformation, and that its morphology is explained by reference to the hearts of the higher reptiles. We here find the heart consisting

"Of three imperfectly-separated ventricles; the right and left systemic

ventricles, from which the two aorta arise, and a small anterior ventricle, which gives rise to the pulmonary artery. The latter is entirely separated from the left, but communicates with the right aortic ventricle. The sinus and infundibular portion of the right ventricle are in man the analogues of the right systemic and pulmonic ventricles of the turtle; the right ventricle in the well-formed human heart always shows, at the point at which the two portions unite, some indications of division by the muscular columns to which the folds of the tricuspid valves are attached; and in cases of malformation this is still more marked. When also a supernumerary septum is developed, it is at the point of union of the sinus and infundibular portion that it occurs."

The malformations of the second class comprise premature closure of the fœtal passages, and patency of the foramen ovale and ductus arteriosus. The third subdivision is devoted to irregularities of the valves, such as excess or deficiency.

The fourth division of the book presents us with an historical analysis of the malformations consisting in the irregular development of the primary vessels, together with Dr. Peacock's personal experience in these deviations from the normal state.

The work concludes with a valuable inquiry into the development of the malformations previously spoken of, and with remarks on the diagnosis and treatment of these affections. The whole deserves a careful study, and while we regret our inability to give a more detailed account of the information it contains, we cannot refrain from extracting the following remarks on some of the diagnostic features presented in the young child:

"Where an infant suffers from great difficulty of breathing and palpitation, and is intensely and constantly cyanosed at or immediately after birth, it may be inferred that it labours under some serious malformation, occasioning great obstruction to the circulation of the blood—as obliteration or contraction of the pulmonic orifice, or transposition of the aorta and pulmonary artery. On the contrary, when the symptoms do not manifest themselves at so early a period, and are less constant and intense, there is probably only some slight malformation, as a moderate amount of contraction at the pulmonary orifice. Of 153 cases of various forms of decided and important malformations of which I have collected notes, in 74 there existed more or less contraction of the orifice of the pulmonary artery, or other sources of obstruction to the exit of the blood from the right ventricle; and in 25 others the orifice or trunk of the vessel was obliterated. In those patients who survived the age of twelve, the entrance of the blood into the pulmonary artery was interfered with in a much larger proportion of cases, or in 32 out of 39; so that, in any given case of malformation, especially after the age of fifteen, the probability is that the pulmonary artery is contracted. If this be the case, a loud systolic murmur will be heard in the præcordial region, and most intensely at the level of the nipple, and between that body and the sternum."

Obstruction of the pulmonic orifice almost necessitates deficiency in the septum of the ventricles or patent foramen ovale; but Dr. Peacock considers it doubtful whether we possess diagnostic characters by which these can themselves be recognised. Collateral evidence may sometimes aid in the diagnosis.

Of the treatment recommended to be pursued, it suffices to say, that it aims at meeting the physiological requirements of the case, and is perfectly rational.

In concluding our observations on Dr. Peacock's book, we would only add, that it is one which must take a high rank in medical literature, and cannot fail to serve as an important aid to all students of cardiac disease.

ART. II.—*The Diseases of Children.* By FLEETWOOD CHURCHILL, M.D. T.C.D., M.R.I.A., Fellow of the King and Queen's College of Physicians; Professor of Midwifery, with Diseases of Women and Children, in the King and Queen's College of Physicians; Honorary Member of the Philadelphia Medical Society, &c. Second Edition.—*Dublin*, 1858. pp. 782.

IT is now eight years since we had occasion to present to our readers an analysis of the first edition of Dr. Churchill's work 'On the Diseases of Children,' in which we were compelled to point out certain deficiencies. It affords us pleasure in stating that he has consulted his own distinguished reputation in the additions and improvements which he has made to the second edition, and that many of the subjects which were not adverted to in its predecessor now receive ample attention. Mesenteric disease, phthisis, paralysis, scleroma, atelectasis, syphilis, which were passed over in the former work, now form the subjects of so many new chapters, while throughout the volume we discover evidences that the author has watched the progress of science, and gathered its fruits for the benefit of his readers. The additional matter introduced in the present volume amounts to a hundred and twenty-six pages. It may appear almost ungracious, where so much labour has been expended, to ask for more, yet we cannot but think that when a third edition is required, by the condensation of some parts, space may be obtained without further enlargement of the volume, for the introduction of some further matter which we are of opinion ought not to have been omitted; thus, a chapter on scrofula would be very acceptable; rachitis, purpura, diabetes, nephritis, are also among the topics which, in a volume like the one before us, deserve a prominent place, and of which we have found no notice.

ART. III.—*Evil Results of Overfeeding Cattle. A New Inquiry, fully Illustrated by Coloured Engravings of the Heart, Lungs, &c., of the Diseased Prize Cattle lately exhibited by the Smithfield Cattle Club*, 1857. By FREDERICK JAMES GANT, M.R.C.S., Surgeon and Pathological Anatomist to the Royal Free Hospital.—*London*, 1858. pp. 39.

ANXIOUS to demonstrate that the system which is at present pursued by persons ambitious of obtaining prizes at cattle-shows for their farm produce is a vicious one, Mr. Gant has applied the test of pathological anatomy. He has been at some pains to follow the prize oxen, prize sheep, and prize pigs, from their pens in Baker-street, to the slaughter-

house in Hampstead, and the result of his researches is, that these animals, without exception, suffer from fatty degeneration of the muscular tissue, and that this disorganization is particularly perceptible in the heart. The delineations are truthfully and artistically executed, and entirely confirm the author's descriptions. Mr. Gant argues justly, that meat so got up cannot possess the nutritive qualities of muscle in which the fibre is not degenerated, and that it is more to the interest of breeders and of the public to follow a system of training cattle which shall preserve the animals in health, while it produces the maximum of food that they are capable of yielding.

We recommend the supporters of the Smithfield cattle-shows to pay attention to the facts ably and clearly put before them by Mr. Gant; and a little reflection will show them that there is an error in pursuing the system of fattening animals intended for human food, to the extent to which it is now carried; they are perpetuating an error alike disadvantageous to the producer as to the consumer.

ART. IV.—*Rheumatism; its Nature, Causes, and Cure. Gout; its Nature, Causes, Cure, and Prevention.* By JAMES ALEXANDER, M.D., Member of the Royal College of Physicians, Fellow of the Royal College of Surgeons, &c.—*London*, 1858. pp. 266.

THAT there is an analogy between gout and rheumatism nobody would be disposed to deny; that there are forms of the two diseases which so closely resemble one another as to render the diagnosis difficult, is also indubitable; but we can scarcely admit that diseases, which in their typical manifestations present such characteristic distinctions, are due to a retention of the same poison in the blood. This is essentially the doctrine advocated by Dr. Alexander in the book before us. He regards urate of soda as the materies morbi in both these diseases, the difference in form and degree between them depending

"Partly on the difference in the chain of events which has preceded and led to the formation of the morbific matter, and which has at the same time exercised an important influence on the character of the vital fluid itself, and partly to the different effect which this animal irritant exercises on two opposite conditions of the blood."

The urate of soda is raised to a pinnacle of importance which it has not hitherto enjoyed, and if we adopt Dr. Alexander's version, we shall be compelled to return to the mechanical theories of Asclepiades, or to the analogous doctrines of the iatro-mathematical school of later days. The pain of rheumatism and gout is attributed to the irritation caused by the entanglement of the particles of urate of soda in the fibrous structures which are the chief seats of the local phenomena of the disease; at the same time, owing to its being "a very powerful animal irritant," it stimulates the lining membrane of the heart, causing increased action of the heart, and consequently a quick bounding pulse. That we may not be supposed to misrepresent the author, we quote his own words. In speaking of the fibrous structures which

are the seat of rheumatism, he says: "The particles of urate of soda become mechanically arrested between the fibrillæ of which these dense structures are composed." And again:

"The pain of rheumatism in the acute fibrous form of the disease is dependent on the particles of urate of soda which accumulate in the myolemma and sarcolemma of the muscles pressing on the nervous fibrillæ which traverse these structures, and temporarily paralysing them by the pressure occasioned by their accumulation."

Although a great deal of the book is purely theoretical, and calculated to excite controversy rather than to convince the reader, we have no objections to raise to Dr. Alexander's method of treatment, which is rational and judicious, inasmuch as his chemical and mechanical theories regarding the diseases in question fortunately lead to conclusions which are in harmony with the therapeutic proceedings adopted by the most enlightened practitioners of the day.

ART. V.—*On Medicine and Medical Education.* Three Lectures, with Notes and an Appendix. By W. T. GAIRDNER, M.D., Fellow of the Royal College of Physicians, and Lecturer on the Practice of Physic, Edinburgh.—*Edinburgh*, 1858. pp. 130.

NOT long ago, we introduced to our readers an ethico-medical trilogy, by Dr. Simpson, of Edinburgh; we have now to recommend to them —and we do so heartily—another similar production by Dr. Gairdner, of the same town. There is necessarily a great resemblance between essays of this kind if the different authors stand on the same ground of science and morality; and yet we are glad to see them multiplied so that each successive generation of students may at least have an opportunity of becoming imbued with the high and able principles that animate their teachers. Dr. Gairdner's little book is not, however, intended for students of medicine only. His second lecture was addressed to the members of the Scottish Educational Institute, and places before them, in suitable terms, the fallacies which prevail among the general public regarding medicine and its professors. Not the least interesting part of the collection is contained in the notes to the last lecture, where we find succinct accounts of the lives and teachings of Paracelsus, Brown, and Hahnemann. An appendix concludes the book, in which Dr. Gairdner strongly advocates a system which we, too, with many other teachers, would gladly see carried out—viz., the institution of examinations by the medical corporations at the end of every session, or, as he terms them, sessional examinations. To be of any service, they would necessarily be compulsory; and that they would exercise a beneficial influence upon the student, by insuring continuous instead of spasmodic study, who can doubt that knows students and student life. The only question with us is, whether it would not, for various reasons, be better that the examinations should be instituted by the teachers themselves than by the Halls and Colleges. The prize system as it now prevails is acknowledged to be vicious—the

chief objection to it would fall to the ground if all students were required to enter for competition, and by that means a uniform emulation in all the classes maintained.

ART. VI.—*Transactions of the National Association for the Promotion of Social Science*, 1857. Inaugural Addresses and Select Papers. *London*, 1858. pp. 608.

OUR readers will probably remember that in the month of October of the past year, a meeting was held at Birmingham, presided over by Lord Brougham, to which all were invited who had worked, or were supposed to be interested, in the cause of social science. As set forth in the introduction to the Transactions by Mr. Hastings, with whom the idea appears to have originated, the object was to form "an association for affording those engaged in all the various efforts now happily begun for the improvement of the people an opportunity of considering social economics as a great whole." The plan was most successfully realized, and the various phases of social science were represented in five departments, each separately organized, but working together to the one great object of the advancement and growth of Christian society and civilization. The first department, devoted to Jurisprudence and Amendment of the Law, was presided over by Lord John Russell; the second, for Education, was under the chairmanship of Sir John Pakington; the third, under the presidency of the Bishop of London, investigated the questions connected with Punishment and Reformation; Public Health was the topic to which the fourth department turned its attention, with Lord Stanley at its head; while the fifth department, under Sir Benjamin Brodie's leadership, was devoted specially to Social Economy.

No man could be more suitably selected to head such an undertaking than Lord Brougham; second to none in zeal for the advancement of man in all his social relations, superior to most of the living in profound acquaintance with the intellectual produce of mankind. In the opening address delivered by this great man we receive the strongest evidence in favour of such combinations as that of the Association for the Advancement of Social Science. No one has had greater experience than he of the beneficial effects of combined action in the advancement of civilization; and few could, from personal knowledge of his contemporaries, or from appreciation of the labours of the past, speak with greater authority on all the topics which this Association proposed to inquire into. It was well done, then, to request Lord Brougham to preside, and to tell the assembly, and the country at large, both what the Association was called upon to undertake, and what it had in its power to achieve.

The present volume is a proof how earnestly men of all denominations and professions are working at the different problems of modern times. It contains valuable papers read at the meetings of the different departments, besides the very interesting addresses of the

respective chairmen; and no small credit attaches both to the originators of the movement and to those who thus far have brought it to a successful issue.

ART. VII.—*Lehrbuch der Allgemeinen Chirurgie.* Von C. F. LOHMEYER, Dr. Med., &c.—*Lahr*, 1858. 8vo, pp. 253.
A Compendium of General Surgery. By Dr. LOHMEYER.

THIS volume is one of a circle of the medical sciences of which Dr. C. H. Schauenburg is editor. The plan which the author follows in treating the subject may be deduced from his introductory remarks:

"General surgery," he observes, "takes cognizance, generally, of those disorders which admit of mechanical aid; it has to investigate the laws of their origin, development, and changes, and to furnish means for their cure. If a comprehensive view be taken of those maladies which are regarded as objects of surgical treatment, it will be found that they naturally divide into three leading groups, according to their more prominent appearances, but of which it is not possible accurately to define the limits. Thus we observe, on one hand, a great series of changes, which we must regard as signs of irregular nutrition, as, for instance, inflammatory processes, mortification, hypertrophy, the formation of pathological tissues, &c. On the other hand, we meet with anomalies of coherence—partly as abnormal separations, partly as faulty unions—conditions which are often the sequel of disordered nutrition, but frequently originate in other causes, such as direct mechanical injuries, spasmodic affections of muscular structures, and the like. Thirdly, we encounter abnormal relations which depend on the irregular form or situation of individual parts of the body—curvatures, luxations, hernias, prolapsus, pathological phenomena, again, which may result from very different causes."

A considerable portion of the work is devoted to surgical pathology. A number of woodcuts are interspersed throughout the text, mostly illustrative of morbid changes of structure; and two lithographic plates, containing figures taken from the writings of Paget, Förster, and others, are inserted at the end of the volume. On the whole, we consider the author has produced a useful compendium, and one likely to prove acceptable to the junior practitioners of his country.

ART. VIII.—*Das Wesen und die Entstehung der Spondylolisthesis.* Von Dr. WILHELM LAMBL, Docent an der Universität in Prag. —*Würzburg*, 1857. pp. 79.

The Nature and Origin of Spondylolisthesis. By Dr. WILLIAM LAMBL, Teacher in the University of Prague. (A Reprint from Scanzoni's 'Beiträge,' Band iii.)—*Würzburg*, 1857.

THE purpose contemplated in Dr. Lambl's work is the anatomical examination of all the hitherto known cases of pelvis with a dislocation of the fifth lumbar vertebra forwards, with consecutive lordosis of the loins, so as to contract the pelvic space, and increase the danger of childbirth. Dr. Lambl's researches show that the anomaly, though rare, is very important. The recorded cases of this remarkable pelvic

deformity are the following. We repeat the list here in order to multiply the opportunities to our readers of inspecting a preparation of the deformity for themselves: 1, The Prague case, the preparation of which is in the collection of the Lying-in Institution at Würzburg; 2, A preparation in the Anatomical Museum at Munich; 3, A gigantic pelvis, and 4, A small pelvis, with this deformity, both in the Pathologico-Anatomical Museum of Vienna; 5, A pelvis in the possession of the director of the Midwives' School of Dr. Everken, of Paderborn. To these are added descriptions of other pelves presenting analogous or illustrative features met with by the author in his travels. His theory of the origin of the deformity may be briefly summed up as follows: Lumbo-sacral hydrorachis, traces of which disease were discovered in all the preparations, is the primitive causal condition of a deformation of the fifth lumbar vertebra, which leads, through dilatation of the canal, to thinning and lengthening of the vertebral arch. With this elongation of the vertebral arch and dilatation of the canal, a real displacement of the articular processes is observed. Together with this divergence of the articular processes, which is produced by the elongation of the inter-articular substance, the inferior oblique processes suffer a twisting of their articular surfaces, so that these last, deviating from their oblique normal direction, obtain an abnormal parallel direction. Under these circumstances the weight of the body brings about the further deviation of the vertebral column. The size and compressibility which the intervertebral cartilage of the lumbo-sacral joint possesses in so high a degree, and to which the freer mobility of the lumbar segment of the vertebral column at this point is owing, permit the lumbar vertebra, if its ligaments do not hinder, to *slip out* of its position under the weight of the body. This always happens in a forward direction, because the body of the vertebra is deeper in front than behind, because the normal bow of the column has the same direction, and lastly, because the lumbar surfaces of the sacral bone shelve forwards and downwards, favouring this forward gliding of the body of the superincumbent vertebra. In consequence of this dislocation, which may be complete, of the lumbar vertebra on the slanting upper surface of the first sacral vertebra, the intervertebral body is so compressed, that in the end its structure may degenerate and soon disappear. The consequent apposition of the bared articular surfaces entails other modifications. Ankylosis follows, distortion of the upper vertebral column, pelvic deformity, the most remarkable features of which are, defective space at the brim, projection of the symphysis, curvature of the sacrum, and elongation of the pelvis in its oblique diameter.

This summary is enough to show the leading features of this species of deformity, and to indicate its distinctness from the ordinary forms of rachitic distortion. The work itself is one exhibiting great minuteness of research, and will no doubt long remain the standard authority upon this rare but interesting form of pelvic contraction. The anatomical descriptions are illustrated by nine carefully-executed plates taken from the preparations.

ART. IX.—*A Concise History of the Entire Abolition of Mechanical Restraint in the Treatment of the Insane, and of the Introduction, Success, and Final Triumph of the Non-Restraint System, together with a Reprint of a Lecture delivered on the Subject in the Year* 1838, *and Appendices containing an Account of the Controversies and Claims connected therewith.* By ROBERT GARDINER HILL, F.S.A., M.R.C.S. Eng., late one of the Visitors of the Lincolnshire County Lunatic Asylums, and formerly Resident Superintendent of the Lincoln Lunatic Asylum.—*London*, 1857. pp. 318.

THE non-restraint system of treatment of insanity is now very generally practised throughout England, and it forms the distinctive feature between the treatment in this country and that pursued in nearly every other part of the civilized world. The object of Mr. Hill's book is to place on record his claim to be the sole originator of the system. We are not disposed to depreciate Mr. Gardiner Hill's claims to be the sole originator of a great and good work. The following appears as impartial a statement of the case as we can gather of the claims and merits of Mr. Hill connected with the introduction of the treatment of insanity by non-restraint. Mr. Gardiner Hill was elected to the post of house-surgeon to the Lincoln Lunatic Asylum in 1836. Dr. Charlesworth was, and had been for some time, the visiting physician to the same institution. Dr. Charlesworth, following the example of Pinel, laboured assiduously to lessen the number and mitigate the severity of all mechanical restraints. Mr. Hill speaks of what had been achieved prior to his connexion with the asylum in the following terms: "the mildest period of restraint just prior to my connexion" with the Lincoln Asylum. Mr. Hill greatly assisted Dr. Charlesworth in his benevolent purpose, when, after reducing the number of applications of mechanical restraint to a very low ebb, it occurred to Mr. Gardiner Hill to abolish restraint altogether. Here precisely is the difficulty that has prevented Mr. Hill's claims from being rightly understood by some, and has afforded arguments to those who have opposed him. Restraint, Mr. Hill admits, had been reduced to the lowest point. But this reduction is not, as it may not appear to those not practically acquainted with all the details of the treatment of insanity, anything like the establishment of the principle of non-restraint—in fact, it is the very contrary. The merit consists not only in the origin of the idea, but in the elaboration of the system, founded upon the entirely novel principle of abandoning brute force, and entirely ruling by moral influence. What, says Mr. Hill, is the substitute for coercion? The answer may be summed up in a few words—viz., "Classification and watchfulness; vigilant and unceasing attendance by day and night; kindness, occupation, and attention to health, cleanliness, and comfort; and the total absence of every description of other occupation of the attendants."

The controversy following the enunciation of the doctrine ran high at the time, and Mr. Hill found himself at last compelled to abandon his office. The system was taken in hand, however, by Dr. Conolly,

and applied on the large scale at Hanwell Asylum in 1843, and since, under the fostering auspices of the Commissioners in Lunacy, it has spread to all the large asylums in England. The claims of Mr. Gardiner Hill were brought forward and acknowledged by Dr. Conolly, and also acquiesced in by Dr. Charlesworth, on the occasion of the presentation of a public testimonial to Dr. Conolly, and Mr. Hill was again involved in a controversy. The pleas of his opponents appear to have taken at different times every change, and resemble in this respect those of the defendant in the celebrated case put forward, when sued for damages for the broken looking-glass, which were, first, that the glass was broken when hired; second, it was whole when returned; and, third, it was never hired at all. Mr. Hill's opponents appear to have stated, first, that the non-restraint system possessed no merit; second, that the merit of it was due to others; third, that it was no new system at all.

This work may serve the purpose of the author in placing his claims on record, but we cannot say that it has any very great interest to the scientific reader.

ART. X.—*Researches on Epilepsy: its Artificial Production in Animals, and its Etiology, Nature, and Treatment in Man.* First Part of a New Series of Experimental and Clinical Researches applied to Physiology and Pathology. By E. BROWN-SÉQUARD, M.D., Professor of Physiology at the Cooper Institute, N. Y., &c.—*Boston*, 1857. pp. 82.

IN the general review of Dr. Brown-Séquard's researches which we gave a little more than two years ago,* we adverted to the interesting fact, that in animals in whom a complete or semi-transversal section of the lower spinal cord had been made, a convulsive affection supervened after the lapse of twelve or fourteen days, which closely resembled epilepsy. With the many other gentlemen who have had the advantage of attending the lectures of this distinguished physiologist, recently delivered in London, we have convinced ourselves of the epileptiform character of the convulsions thus produced. They occur spontaneously, but may be most readily produced by irritating a portion of the skin supplied by the second division of the fifth pair of nerves. In the book of which we have given the title above, Dr. Brown-Séquard presents to his readers a collection of the various papers which he has published on this branch of his inquiries into the functions of the nervous system since 1853. He investigates the history and pathology of epilepsy, with special reference to the results of his vivisections, and arrives at some conclusions which cannot fail to command the attention of the medical profession.

Our readers will find in the Physiological and Obstetric Reports of the present number, summaries of several of his recent papers, but we hope ere long to be able to offer to them an analysis of the la-

* Vol. xvii. p. 412. April, 1856.

bours of Dr. Brown-Séquard since the publication of our former review; we shall then specially examine his researches illustrative of epilepsy. We will now only express the hope that Dr. Brown-Séquard may see fit to publish in a collected, and, we would add, illustrated, form, the entire course of lectures which he has recently delivered in London. The great interest which they have excited in the profession would be a guarantee to any publisher that he was consulting his own private advantage, quite as much as the advancement of science, by undertaking their speedy issue. In the mean time, we would express our thanks to Dr. Brown-Séquard for again coming among us, and, with all the ardour and liberality of a genuine man of science, expounding to us the laws of nervous action which he has so laboriously, so patiently, and so successfully investigated.

ART. XI.—*Observations on Venereal Diseases.* By HAMILTON LABATT, A.B., F.C.D., Licentiate and Fellow of the Royal College of Surgeons, Ireland; Medical Officer of the South-Eastern Dispensary, Dublin; late in Medical Charge of the Second Battalion 60th Royal Rifles, &c.—*Dublin.* pp. 283.

M. LABATT'S observations are founded chiefly upon his experience of the venereal diseases among soldiers. He informs us that 470 cases were admitted under his charge in the Dublin garrison during the late Russian war. In the 60th Royal Rifles 106 cases occurred, but no instance of secondary syphilis presented itself during eight months that he had charge of the depôt! (p. 9.) With a view of pursuing the inquiry still further, M. Labatt obtained a return of the amount of secondary syphilis in the regimental and depôt battalion hospitals in Ireland during the months of December, 1856, and June, 1857.

"In the former month, 35 hospitals were open, and the average amount of secondary syphilis in each hospital was $1\frac{12}{35}$ths—the whole garrison being 27·379. In the latter month—viz., June 1857, 31 hospitals were open, and the average amount of secondary syphilis in each was $1\frac{24}{31}$. The command at this period numbered 19,019. These facts can only be appreciated by the military surgeon who is aware of the very great prevalence of syphilis in the army." (pp. 19, 20.)

The consideration of the causes of these favourable results forms the chief object of M. Labatt's work; and the solution M. Labatt thinks is to be found in those valuable restrictions which the characteristic discipline of the army enables the surgeon to carry out. Even for the simple primary ulcer, M. Labatt says that "perfect rest in the recumbent posture must be enjoined." (p. 59.) For the ulcer with elevated margin, smooth surface, and devoid of fungus, M. Labatt also recommends "perfect rest in the recumbent position." Now, if it were intended to imply that the favourable results obtained depend upon the peculiar mode of treatment, we may firmly say that the figures above given prove a great deal too much. In the first place we would question whether primary sores of the nature here described would, if

left to themselves, produce, as a rule, any secondary symptoms; secondly, we have other statistics from the army, in which it is not stated that any particular care was taken to keep the patients in a recumbent position, affording very favourable results. (See p. 31.) In the often-quoted cases treated by Mr. Rose, we have heard from authority which we cannot doubt, that while Mr. Rose believed he was treating his patients without mercury, they were in the constant habit of going to unauthorized venders of medicine, who supplied them plentifully with that drug. They could not, therefore, have been kept at rest during their treatment, and yet the results published are sufficiently favourable. We must, then, seek some other explanation of the favourable results published than that of rest or other restrictions upon which our author lays so much stress. This problem at first sight appears by no means easy of solution, and we had thought of it for a considerable time before. In reading page 89, we came upon what appears to be the true explanation. M. Labatt there states that during the whole time he had charge of the military hospitals, but one example of the Hunterian chancre came under his observation. The remarkable coincidence of the rare occurrence of the Hunterian chancre and of secondary symptoms in M. Labatt's practice, confirms, as far as it goes, the non-infecting nature of the ordinary forms of sores which have come under his notice. And this we believe to be the true explanation of the comparatively few cases of secondary disease which presented themselves under the treatment which he has advocated; nor do we believe that they would have been much more numerous under any other treatment. According to our view, the sores were in their nature non-infecting, and although a different plan of treatment might not have healed them so quickly, it would not have changed their nature so as to have converted them into infecting sores,—by which we mean sores which have the property of infecting the patient's system. But still the question remains, why so small a number of infecting sores should present themselves in military practice? and this question is not very easily answered.

We believe that the secretion from a soft chancre will often give rise to another soft chancre, and that the discharge from a hard chancre will often produce a hard chancre. We also believe that after repeated exposure, the ulcerations almost always assume a soft character; they are communicated from, and give rise to, some form of suppurative inflammation. Now it is with persons who have been exposed to these frequent infections, and whose parts are very frequently affected by suppurative inflammation, that our common soldiers have to do. It would then appear, *à priori*, probable that often indulging in gregarious intercourse, they would, as a rule, be affected with soft or suppurating sores; and when we add to this the consideration of the early and frequent use of caustics in the army as tending to produce the same kind of inflammation, we have advanced some way in explaining the rare occurrence of secondary disease to which M. Labatt has so forcibly drawn our attention. We regard M. Labatt's work as a faithful record of what he has himself witnessed.

ART. XII.—1. *The Nature and Treatment of Cancer.* Being the Address in Surgery read before the Twenty-fifth Meeting of the British Medical Association. By GEORGE SOUTHAM, F.R.C.S., Surgeon to the Manchester Royal Infirmary.—*London*, 1858. pp. 47.

2. *On the Painless Extirpation of Cancerous Growths by Congelation and Caustic, including a Report of the recent use of Prolonged Congelation in the Cancer Wards of the Middlesex Hospital.* By JAMES ARNOTT, M.D., late Superintendent of the Medical Establishment at St. Helena.—*London*, 1858. pp. 28.

IN the able address delivered by Mr. Southam, before the meeting of the British Medical Association held in Nottingham, in 1857, we receive a lucid account and well digested critique of the prevailing doctrines regarding the nature and treatment of cancer. The author, while allowing the difficulties that surround the questions bearing on the origin and nature of the disease, maintains that no one pathological or clinical symptom is characteristic of malignant disease. He finds—

"In the tendency of the growths to multiply themselves in the different textures of the body, to return after removal, and to present during some stage of the affection symptoms indicating constitutional vice, with its almost invariably fatal nature, the leading characteristics of cancer."

The author commences his remarks on the treatment of the disease, by expressing his belief that although a rare occurrence, it is occasionally cured. In proof, he quotes cases and statements of Sir Astley Cooper, Sir Benjamin Brodie, Mr. South, and M. Velpeau, and relates four cases of his own in which there were complete recoveries for nine, ten, nineteen, and twenty-two years respectively, or more properly speaking, in which after the lapse of these protracted intervals the disease had returned.

Mr. Southam is decidedly in favour of excision, as compared with caustic treatment, though he admits the occasional utility of the latter. The analysis of 150 cases examined by the author leads him to make the following statement—

"Taking the average duration of life, from the first detection of the disease in those *not* operated upon, at four years, I think it may be inferred, that if the patient remain in perfect health and free from any return of the growth for three years after operation, it has added to the duration of life. Twenty-five cases exceeded this period, five of which lived for ten, twelve, twenty-seven, forty-five, and fourteen years respectively, after the removal of the tumour, the last mentioned being still alive. Symptoms indicative of the malignant cachexia do not appear to have been present in any of the twenty-five at the time of operation."

We cannot follow Mr. Southam further than to say, that he regards the application of ice and salt to produce local anasthesis as a valuable adjunct in the treatment by caustic. To the recommendation of this procedure, Dr. Arnott's pages are devoted. By the aid of frigorific mixtures graduated according to the depth to which he wishes to pro-

duce congelation, he destroys the sensibility of the parts, and is thus enabled to apply caustics without the production of the pain and its consequences, which result from the ordinary mode of application of these remedies. The subject is well deserving the attention of surgeons.

Art. XIII.—*Summary of New Publications.*

The first work that we have to mention in this quarter's Summary of New Publications, and to which we hope in our next issue to draw the attention of our readers more especially, is the second edition of Dr. Bennett's 'Clinical Lectures on the Principles and Practice of Medicines.' 'An Essay on Physiological Physiology,' by Mr. Dunn; 'A Manual of Psychological Medicine,' by Drs. Bucknill and Tuke; a work by Dr. Noble, entitled, 'The Human Mind in its relations with the Brain and Nervous System;' and Dr. Roberts' 'Essay on Wasting Palsy,' or progressive muscular atrophy, mark the results obtained by various labourers in the field of neuropathology. This branch of science has received a fresh impulse from the brilliant experimental researches of Dr. Brown-Séquard, whose lectures have recently created such unprecedented interest in the metropolis, and the first number of whose 'Journal de Physiologie' will, with the second number, receive special notice in our Half-yearly Report on Physiology.

'The Institutes of Medicine,' by Dr. Martyn Paine, of New York, will command a very different class of admirers from that led by Dr. Bennett; we hope again to advert to it, as to the 'Essay on the Pathology of the Blood and its Containing Vessels,' by Dr. Wise. Mr. Lister, in an interesting paper 'On the Causes of the Coagulation of the Blood in Diseases of the Bloodvessels,' *àpropos* of a case of spontaneous gangrene from arteritis, follows up his former researches into the nature of inflammations, and arrives at the conclusion, that in the early stages of arteritis and plebitis, the impaired vital energy of the vascular coats give rise to an evolution of ammonia and consequent coagulation, with its sequelæ. The Proceedings of the Royal Society for 1857 contain the abstract of a paper, by Mr. Lister, 'On the Early Stages of Inflammation,' in which he reconciles solidism and humoralism, by showing how, in the production of inflammation, the withdrawal of the influence of a healthy part in the vicinity of the bloodvessel operates in inducing the changes seen within the vessels. From France we have received a work, by Dr. Willemin, 'On the Waters of Vichy,' in which the author discusses their value in the treatment of chronic affections of the uterus, which his cases would argue as being very considerable.

Under the head of History of Medicine, we introduce to our readers the first volume of a work by Dr. Simon, of Hamburgh, entitled 'Kritische Geschichte des Ursprungs der Pathologie und Behandlung der Syphilis.'

Under the head of Hygiene, we introduce to our readers a work by

Dr. Armstrong, the well-known medical officer of the *Investigator*, the first vessel that achieved the North-west passage. The title is, 'Observations on Naval Hygiene and Scurvy.' Mr. John Roberton, of Manchester, again favours us with valuable 'Suggestions with a view to the Improvement of our Hospitals.' We are glad to find that he has not laboured in vain, and that a new hospital is about to be erected at Blackburn, in which the old principle of construction—by which proper ventilation was impossible, and the generation of a hospital atmosphere unavoidable—is cast to the winds. This building will be erected upon a plan already adopted in the great Hospital of Bordeaux, in the Hôpital St. Jean of Brussels, the magnificent Lariborisière of Paris, and, last not least, the admirable but now historical hospital of Renkioi*—viz., of having detached wards, with windows on each side, connected only by a common corridor. 'Suggestions and Instructions, in reference to Sites, Construction, and Arrangement of Buildings, and Plans of Lunatic Asylums,' come to us as the first achievement of the new Lunacy Board of Scotland. A lecture 'On the Sanitary Condition of the British Army,' by Dr. Guy, places certain points of the recent Sanitary Report on the Army, especially the evil results of overcrowding, in a very strong light. On the other hand, Dr. Barker gives us experimental proof of the noxious influence of sewer emanations, and of the individual gases which prevail in the cloacæ, and make their way to the lungs and noses of her Majesty's lieges through the untrapped gully-holes of the streets and open mouths of conduits discharging into the Thames. It is interesting to observe the close resemblance of the symptoms produced in the animals experimented upon to the typhoid fever of man, especially when subjected to the influence of sulphide of ammonium. We trust that if the new Public Health Act, of which a copy is before us, be carried in the present session, the good sense of our corporations will prevail to cause its universal adoption, and the consequent abatement of all removable causes of disease. In connexion with sanitary matters, we have to mention the Second Report of the Commissioners of Her Majesty's Customs, including the Medical Report 'On the Health of the Waterguard and Waterside Officers,' by Dr. McWilliam.

The Surgical works that claim our attention are Mr. Zachariah Lawrence's work 'On Surgical Cancer,' which has attained the honours of a second edition; the second number of the 'Ophthalmic Hospital Reports,' edited by Mr. Streatfield, containing communications from Messrs. France, White Cooper, Dixon, Bader, and the editor, besides a polemical article of Dr. Von Gräfe; and Mr. Marion Sims' address, in which he dwells with great unction on the importance of silver sutures in surgery. As a branch of surgery, we next advert to Odontology, which now has its separate representative in the republic of literature under the title of 'Transactions of the Odontological Society,' to the first number of which Mr. Rogers, Mr. Bate, Mr. Tomes, Mr. Shelley, and other well-known dentists, have contributed.

* See British and Foreign Medico-Chirurgical Review, Oct. 1856, p. 451; and Oct. 1857, p. 445.

Materia Medica and Therapeutics are represented by a large work by Dr. Tully (United States), of which the two first parts of the first volume have already appeared, extending already to 1534 octavo pages. Under this head we would also mention a new work by Dr. Lawrance, 'On Localised Galvanism,' a subject which, with daily improving knowledge, attracts daily greater attention. We intend shortly to lay before our readers a summary of this department of therapeutics, when we shall revert to Dr. Lawrance's volume. Dr. Tyler Smith's Manual of Obstetrics, a similar German work by Dr. Spiegelberg, and the reprint of a paper by Dr. Bozeman, of North America, 'On Urethro-Vaginal and Vesico-Vaginal Fistules,' comprise the productions of Midwifery that the past quarter has brought.

In Medical Jurisprudence, we have received the sixth edition of Dr. Taylor's well-known, and evidently well-appreciated, manual—one of those works upon which the deserved applause of a discerning public has already conferred the highest distinction to which an author aspires. The proposed scheme of Mr. Pearson* to offer certain facilities to medical students in taking their degree at Oxford, is discussed by Dr. Child and Mr. Hussey; both speak as if they were treading on dangerous ground. It becomes daily more apparent that if Oxford is really to do her duty as the *alma mater* of all the learned professions, a more searching reform is necessary than that implied in Mr. Pearson's well-meant proposition. Let those who do not clearly apprehend the intimacy of the relation between the physical and moral and mental training of man, read Mr. Buckle's introduction to the 'History of Civilization' as an indication of the advantages to be derived from a symmetrical development of the intellect. We wish to see Oxford the representative of the whole mind of the nation; and for this reason, more than for the benefit that will accrue to the medical profession, we hope yet to see medicine occupying its legitimate position there. We are glad to perceive by the daily papers that Cambridge is also agitating reforms in a similar direction; the Council of the Senate having submitted to the Senate for adoption, certain regulations which will much facilitate the graduation of medical students at that University.

It remains for us to notice a well-written and interesting book, by a Fellow of the Royal College of Surgeons, entitled the 'Unity of Medicine,' to which we shall again recur; the essays of Mr. Herbert Spencer, collected from the Quarterly Reviews, and a second edition of the 'Handbook of Chemistry' of Messrs. Abel and Bloxam, which on a cursory glance makes the impression of being valuable as a practical guide to the student; if to this we add Mr. Chalmers' work 'On Electro-Chemistry,' and Dr. Harvey's letters 'On Administrative Reform,' which have reached us at the last moment, we have exhausted the list of the more important productions to which in this summary we wished to advert.

* See British and Foreign Medico-Chirurgical Review, p. 558. April, 1858.

PART THIRD.

Original Communications.

Art. I.

On the Pathology of Asthma. By Hyde Salter, M.D., F.R.S.

There are three motives that have induced me to put together the following thoughts on the pathology of asthma. One is the extreme interest of the subject in a pathological point of view, the beautiful illustration which the clinical history of the disease affords of the phenomena and laws of that particular form of perverted nervous action in which I believe it essentially consists. Another is the importance of correct pathological views in relation to the therapeutics of the disease. A third is a desire to refute the various erroneous theories that have been, and still are, advanced with regard to the pathology of asthma, and to supplant by something precise and definite those muddy and loose notions about it that I find, both from conversation and from their writings, to be so common among medical men. Some, I find, even now-a-days, do not believe in stricture of the bronchial tubes at all; others do not believe that such a condition would explain the phenomena of the disease; and others again, while admitting that spastic contraction of the bronchial tubes does exist in asthma, believe it to be only a part of a complex condition, that it does not *constitute* the disease, but that some special derangement of the pulmonary circulation shares, equally with it, in the production of the symptoms. I may even add a fourth class—those who deny the existence of asthma as a substantive disease altogether; who affirm that you never have it without organic disease of the heart or lungs, and that it is merely a particular form of dyspnœa induced by that organic disease. No doubt there are many circumstances peculiar to asthma that go far to explain why such vague and erroneous notions should be entertained with regard to its absolute nature, such as the rarity of death in cases of uncomplicated asthma, the slightness and unconspicuous character, or the total absence of anything special and peculiar in the post-mortem appearances in such cases, and the remoteness of the actual morbid condition from the manifest phenomena of the disease. But, at any rate, such is the fact; and if any doubt of it existed, a reference to the literature of the subject, or ten minutes' conversation with any half-dozen members of our profession, would soon dissipate it.

What I shall endeavour to show will be this:

1. That asthma is essentially, and, with perhaps the exception of a single class of cases, exclusively, a nervous disease: that the nervous system is the seat of the essential pathological condition.

2. That the phenomena of asthma—the distressing sensation and the demand for extraordinary respiratory efforts—immediately depend upon a spastic contraction of the fibre-cells of organic or unstriped muscle, which minute anatomy has demonstrated to exist in the bronchial tubes.

3. That these phenomena are those of excito-motory or reflex action.

4. That the extent to which the nervous system is involved differs very much in different cases, being in some cases restricted to the nervous system of the air-passages themselves.

5. That in a large number of cases the pneumogastric nerve, both in its gastric and pulmonary portions, is the seat of the disease.

6. That there is a large class of cases in which the nervous circuit between the source of irritation and the seat of the resulting muscular phenomena involves other portions of the nervous system besides the pneumogastric.

7. That there are other cases in which the source of irritation, giving rise to the asthmatic paroxysm, appears to be central—in the brain; consequently, in which the action, though excito-motory, is not reflex.

8. That there is yet a class of cases in which the exciting cause of the paroxysms appears to be essentially humoral.

Let us now examine these propositions in the order in which I have stated them, and see what proofs can be brought in their support.

1. The reasons that force upon one's mind the conviction that asthma is essentially a *nervous* disease are very numerous, and not less forcible and convincing; but I shall here be able only briefly to indicate them, as their fuller consideration would involve too great space. They are principally derived from the following considerations:—α, The *causes* of asthma; β, its *remedies;* γ, its *associated and precursory symptoms;* δ, its *periodicity*; ε, the *absence of organic change*; ζ, the circumstance that the phenomena of the disease are *muscular.*

α. We see, in the first place, that the *causes* of asthma are such as affect the nervous system, and such as give rise to other diseases acknowledged on all hands to be nervous. Thus, fatigue and physical exhaustion, and sudden or violent mental emotion, will bring on an attack. I was informed, some short time since, by my friend, Dr. Theophilus Thompson, of a case in which, on two occasions, severe asthma was brought on in a gentleman by sudden fear, from his having, as he imagined, administered accidentally something deleterious to his wife. I knew the case of an asthmatic boy, some years ago, who used constantly to be warned by his parents not to over-excite himself, as if he did he would be sure to have the asthma the next day; and lately I met with another case, in which I was told that when the asthmatic was a little boy, he found in his disease a con-

venient immunity from correction; "don't scold me, papa," he would say, if he had incurred his father's displeasure, "or I shall have the asthma;" and so he would; his fears were as correct as they were convenient. Venereal excitement will bring on asthma; a gentleman once told me that one of the severest attacks he ever had in his life was brought on in this way. Moreover, many well-known and recognised causes of asthma can only act on the lungs through the intervention of certain parts of the nervous system; thus, gastric irritation can produce spasm of the bronchial tubes only through the intervention of the pneumogastric nerve—it is the only connecting link between the two organs, either physiologically or anatomically.

β. Again, the *remedies* of asthma are such as appeal to the nervous system—as antispasmodics, sedatives, direct nervous depressants, &c.; tobacco, for example, stramonium, antimony, chloroform. Perhaps the effect of chloroform is, of all remedies, the most striking, and at the same time the most illustrative of the purely nervous nature of the affection—a few whiffs, and the asthma is gone; a dyspnœa that a few seconds before seemed to threaten life is replaced by a breathing calm and tranquil—it acts like a charm. Now, remembering the action of the drug, that it is the nervous system to which it appeals, it is impossible to help seeing in this the most conclusive proof that the symptoms are due to a nervous cause. And besides these ordinary remedies, there are other circumstances that will put a stop to the paroxysm, that eminently prove its nervous nature; one of these is mental emotion—any strong or sudden passion, such as fear, fright, or surprise. It is a curious thing that mental emotion should have the effect both of inducing and relieving asthma, but so it is; and we are not without analogies—e.g., shock will bring on chorea, and shock will cure it. I think the *immediate* effect of emotion is always to cut short asthmatic spasm, if it exists, by a sort of nervous revulsion; whereas its tendency to induce it is *remote*, and only shown after some time, as the next day; and it acts, I think, by producing an exaltation of nervous impressibility—and thus facilitating the induction of excito-motory action. Nothing, indeed, in the whole range of pathological phenomena is to my mind more remarkable than the effect of emotion upon asthma. Dr. Todd has told me that he has had patients come to him who have lost their asthma the moment they have entered his house; suddenly, and without any apparent cause, except the mental perturbation at being within the precincts of the physician, the difficulty of breathing has vanished. We see just the same thing in tooth-ache—the sight of the dentist's house is enough to cure it. I witnessed once myself so striking an example of this sudden disappearance of asthma under the influence of alarm, that I cannot forbear relating it.

A gentleman, a confirmed asthmatic, was suffering an unusually bad attack of his complaint, so bad that he was unable to move from his chair, or speak even, except in catchy monosyllables. He had been suffering all day, and in the evening his sister was going to give him an emetic of ipecacuanha, when she suddenly fell down in an hysterical

fit, to the occurrence of which she was subject. The suddenness of her attack, and the severity of her sufferings, so alarmed him that he sprang from his chair and ran to her relief, and as soon as he had placed her in a position of safety, ran down two flights of stairs to procure the restoratives that were usually administered; having run upstairs again with the same speed, and applied the remedies, he found to his surprise that his asthma was gone, and indeed it was its sudden departure under the influence of alarm that had alone enabled him to perform such a feat; a man who, two minutes before, was unable to speak or move, had, under the influence of an absorbing alarm, ran down two flights of stairs and up again, and found himself after his exertion breathing with perfect freedom. The asthma gradually returned, and within an hour he was as bad as ever. I do not think it possible to adduce a stronger proof than this of the purely nervous nature of asthma. I might cite many such cases.*

γ. Again, the *periodicity* of asthma implies its nervous nature—that is, such a periodicity as characterizes asthma. There are three kinds of periodicity in disease. One, in which it is produced by the periodical return of its cause, as in the recurrence of hay-fever every summer, the morning expectoration after a night's rest, indigestion every day at a certain time after dinner. Another, in which the periodicity seems to depend upon that rhythmical impress which is stamped on the functions, that sort of diurnal oscillation in which the body is swung by the constant recurrence, at one unvarying daily interval, of the habitual actions and passions of the body; I think that hectic and ague acquire their periodicity from this diurnal beat into which the body falls. But there are other diseases whose rhythmical recurrence cannot be explained on either of these suppositions, whose periodicity has no relation either to the diurnal interval or to the renewal of the cause, but which must be intrinsically periodic; such are epilepsy and asthma. In these the interval is long and of no certain standard—that is, though tolerably constant in the same individual, it differs very widely in different cases—the period is peculiar to each case, is an integral part of the pathological condition. This last kind of periodicity, and this alone, it is that points at all to the nervous nature of a disease.

δ. Furthermore, the associated and precursory symptoms of an asthmatic attack are such as point to its nervous character. The quantity of limpid water passed in the early part of the paroxysm, white as pump-water, like the nervous water passed in the student's "funking room," or like the urine of hysteria, or that of nervous headache; the neuralgia, which I have often noticed; the frontal headache; the drowsiness and languor of the previous day by which the approach of the attack is foreknown; or, on the other hand, a peculiar and unwonted hilarity and animation and sense of health,—all these are just such symptoms as we meet with in various diseases of the nervous system, such, for example, as hysteria and epilepsy.

* A remarkable exception to this curative power of excitement once came under my observation, in the case of a woman who suffered from a violent attack of asthma the whole of the time she was in labour.

ε. Another circumstance in favour of this view, in fact consistent with no other, is the possible absence of appreciable organic change, as shown by post-mortem examination, in cases where the disease has not been of long standing. A man may have been known during his life to have had attacks of asthma, he may have seemed over and over again almost *in articulo mortis* from want of breath; and yet, if death from some other cause* gives an opportunity of examining his lungs, they may be found apparently in every way healthy—no trace of inflammation or its products, the vesicular structure perfectly normal, the passages leading to it lined by a healthy and unchanged membrane, the cavities of the pleura free from all abnormal contents, their surfaces smooth and apposed, the heart sound. The disease shows no cause, and has left no trace, either in the respiratory or circulatory systems—in fact, no trace anywhere. Where, then, shall we locate it? What is its starting-point? We may, I think, lay it down as a rule, that all those diseases that leave no organic trace of their existence produce their symptoms through the nervous system.

ζ. Lastly, the phenomena of the disease are muscular, the proximate diseased condition is situated in the muscular system, and whenever the proximate derangement is muscular, we may always, with one or two exceptions, safely affirm that the primary disease is nervous. In epilepsy, tetanus, chorea, paralysis agitans, hemiplegia, child-crowing,—in all these, the obvious departure from health is in the muscular system; but the essence of the disease is nervous. The only exceptions that I know of are, the cases in which the muscles are either poisoned by some material present in them as, for example, in the paralysis of lead-palsy, the cramps of cholera; or disorganized by fatty degeneration, as we see in the heart, the muscles of disused limbs, &c. In these cases, the disease is radically and primarily muscular. In all other cases, muscular disturbance is but the index of nervous disease. Hence the very fact that the phenomena of asthma are muscular, is all but proof positive that the nervous system is the seat of the primary derangement.

2. *That the phenomena of asthma—the distressing sensation and the demand for extraordinary respiratory efforts—immediately depend upon a spastic contraction of the fibre-cells or organic muscle, which minute anatomy has demonstrated to exist in the bronchial tubes.*

Although this is a proposition that many perhaps might think it hardly worth while to set about proving, yet I think it will be well not to assume it, partly for the reasons I have already mentioned (the general absence, namely, of precise pathological views on the subject), partly because it is a necessary stepping-stone to the succeeding propositions, and partly because I think its proof will be the best way of expressing my notions of the ultimate pathology of asthma, and my reasons for them. It will certainly be an advantage if it can be shown beyond cavil that spasmodic stricture of the bronchial tubes is

* I say *from some other cause*, because if asthma kills it, always does so by producing organic change in the heart or lungs, or both.

the only possible cause of asthma, that it is adequate to the production of all the phenomena, that it is a form of perverted physiology that may exist pure and uncomplicated with any organic disease, and that the view that would assign it as the sole essential condition in asthma is—what all pathological views should be—physiological and rational. I think perhaps the eliminative or exhaustive method of proof will be as good as any.

I will suppose a case of severe uncomplicated asthma, such as we sometimes see. Now, what have we here? We have, as the sole constituent symptom, *dyspnœa*—dyspnœa of a peculiar kind—sudden in its access, intense, and agonizing, following a state of perfect apparent health and ease, and relapsing as suddenly, possibly, and speedily, and perhaps without any expectoration, into ease and tranquillity again. What, then, can give rise to such phenomena as these? We know that the only way in which such an arrears in the respiratory changes as produces a sense of dyspnœa can be brought about, is by a derangement of the supply of one or both of the two fluids, the air or the blood, or by a disorganization of the functioning portion of the lung. On what recognised diseases, then, can we fall back, as supplying in such an instance the necessary conditions? On heart-disease, possibly, bronchitis and emphysema. But if we examine the heart, we find it all right,—it cannot then be that. The mucous membrane of the air-passages could not assume and relinquish a condition of inflammation so suddenly; and moreover, to produce such dyspnœa, the inflammation must be intense, and could not fail to give rise to the results of inflammation, yet none such are thrown out. There is not necessarily any mucous exudation; crepitation or expectoration may both be absent. Besides, it would be impossible for bronchitis to exist to such an extent as to give rise to the amount of dyspnœa, without producing the constitutional signs of inflammation; but none such are present—the patient is not ill, he is wheezing and labouring—he passes from a state of health to a state, not of illness, but of dyspnœa, and back again from dyspnœa to health: there are no sequelæ, there is no convalescence. It cannot therefore be bronchitis. Emphysema we know it is not, for the dyspnœa of emphysema is constant and unvarying, and moreover, we listen to the breathing before and after the attack, and find evidence that the spongy structure of the lung is perfectly healthy. We see, then, that in none of the three ways in which dyspnœa is ordinarily produced—on the side of blood-supply, on that of air-supply, or on that of injured functioning structure,—by heart-disease, emphysema, or bronchitis respectively—can the symptoms of asthma be explained. Moreover, the character of the dyspnœa is altogether peculiar; it is utterly unlike either of the three dyspnœas that have been mentioned. Heart-dyspnœa is intolerant of the slightest exertion, or of the recumbent position, and sitting up, or stillness, may cure in two minutes the most violent paroxysm. The breathing, too, has rather a panting and gasping than the wheezing, labouring character of asthma. Bronchitic dyspnœa is short, crepitous, and accompanied with cough; asthma, often long-drawn, dry, and without cough. In

pure emphysema, the dyspnœa is abiding, varies but little, and has no wheeze.

But the dyspnœa of asthma tells a plainer tale than this; it tells us not only what it is not, but what it is. It gives the most positive evidence of narrowing of the air passages. The asthmatic's breathing is what our forefathers called "strait," what we call "tight;" he feels as if a weight were on his sternum, as if his chest were compressed, as if a cord bound him, as if it would be the greatest relief to him if some one would cut his breast open and allow it to expand; he rushes to the window to get air, he cannot tolerate people or curtains about him, his clothes are loosened, and all the muscles of respiration tug and strain their utmost to fill his chest. But he can neither get air in nor out, he can neither inspire nor expire—his respiration is almost at a deadlock; he cannot blow his nose, can hardly cough or sneeze, cannot smoke a pipe, and if his fire is failing, cannot blow it up; he has hardly air enough to produce the laryngeal vibrations of speech. The chest is distended, indeed, to its greatest possible limit, the cavity of the thorax is enlarged both in the costal and diaphragmatic directions; the costal distension is shown by the fact that a waistcoat that would ordinarily fit will not meet over the chest by two inches, while the descent of the diaphragm is shown by the increased girth of the abdomen and by the heart being drawn down to the scrobiculus, where it is seen beating plainly; such are the violent instinctive efforts of the respiratory muscles to overcome the obstruction to the access of air. But they are unavailing; the air that is without cannot get in, and that which is within is locked up. In spite of the violent muscular effort there is hardly any respiratory movement, the parietes of the chest cannot follow the action of the muscles; on listening to the chest, the respiratory murmur is inaudible, even when not drowned by the wheezing; respiration is almost *nil*. Where, then, can this obstruction to the introduction and exit of air be? It must be in some part of the air passages—the larynx, trachea, or bronchial tubes. In the larynx and trachea we know, from the symptoms, it is not. The fact of bronchial stricture, then, is certain.

The very intensity of the dyspnœa, too, its agonizing and laborious character, implies that the seat of the mischief is in the air-passages. Dyspnœa is essentially remedial, and tends directly, both by its sensory and muscular phenomena, to diminish and relieve its cause. As soon as respiration is not going on satisfactorily, the sense of dyspnœa or want of breath, at once prompts to more violent respiratory efforts, which tend to relieve it. The distressful sensation is an essential link in the chain—it gives warning of the condition to be remedied, and is the irresistible stimulus to the remedial efforts. But this sense of dyspnœa, being in its nature remedial, would be likely to be felt only in those cases in which the condition giving rise to it could be remedied by those extraordinary respiratory efforts to which it irresistibly prompts. Now, consistently with this view, I think I have noticed a very curious law with regard to dyspnœa—it is this, that it is proportionate, not to the amount of injury done to the organ, but to the

amount of relief that the condition admits of by extraordinary respiratory efforts. If the parenchyma of the lung, its functioning structure, is injured, no amount of respiratory effort will better the condition, and accordingly violent dyspnœa is not induced. Thus, half the lung may be destroyed by phthisis or solidified by pneumonia, and the tranquillity of the respiration be hardly interfered with: a little hurried, perhaps, but no distress or violent effort. But if, while the lung-substance is healthy, the free access of air is prevented, violent and distressing dyspnœa is immediately induced—as in croup, laryngitis, the sudden infarction of a large bronchus. For here, if the air could only be got in sufficient quantity to the healthy functioning structure, the balance of the function would be completely restored; hence it is that such cases are always characterized by those violent respiratory efforts which have for their object the freer introduction of air, and that urgent sense of want of breath which is the constituted stimulus to these efforts. We recognise, therefore, in the very urgency of asthmatic dyspnœa, evidence that the mischief is in the air-passages, and that it is of such a nature as to shut off the air-supply.

But the *sounds* of asthma give us perhaps still more certain and circumstantial evidence as to the condition of the bronchial tubes. We know in health that respiration is noiseless, but that when the breathing becomes asthmatic it is accompanied with a shrill sibilant whistle. We know, too, that hollow tubes give no musical sound, when air rushes through them, if they are of even calibre, but if they are narrowed at certain points, if their calibre is varied, the air in them is thrown into vibrations, and they become musical instruments. The wheezing of asthma, then, is as positive evidence of bronchial contraction as if we could see the points of stricture—it is physical demonstration.

Now, in what ways may the bronchial tubes be narrowed? In four, I think, as shown in the accompanying diagram: by a plug of tenacious mucus partly closing the passage, Fig. 1, *a*; by congestive or inflammatory thickening of the mucous membrane, Fig. 2, *b*; by plastic exudation thrown out in the sub-mucous areolar tissue in severe bronchitis, and undergoing subsequent slow contraction (as we see in œsophageal and urethral stricture), Fig. 3, *c*; and by contraction of the circularly-disposed organic muscle which exists in the bronchial wall, Fig. 4, *d*. This last is spasmodic stricture; the other three are not; the first is no stricture of the tube at all; and the second and third are inflammatory stricture; the second recent, vascular, and mucous; the third old, fibrous, and submucous. In all these ways the column of air in a bronchial tube may be constricted, and the tube converted into a musical instrument—the seat of a sound that will be rhonchus or sibilus, of high or low pitch, according as the tube is large or small. Now, which is the cause of the sound in the case before us—the sibilus of asthma? The sibilus depending on a plug of tenacious mucus sticking to the side of the tube is generally (always *ultimately*) relieved by coughing; the sibilus of asthma is never affected by coughing. Vascular tumidity of the mucous membrane can never be

dissociated from the symptoms of existing bronchitis, and the sibilus arising from it is not of transient appearance and disappearance; the

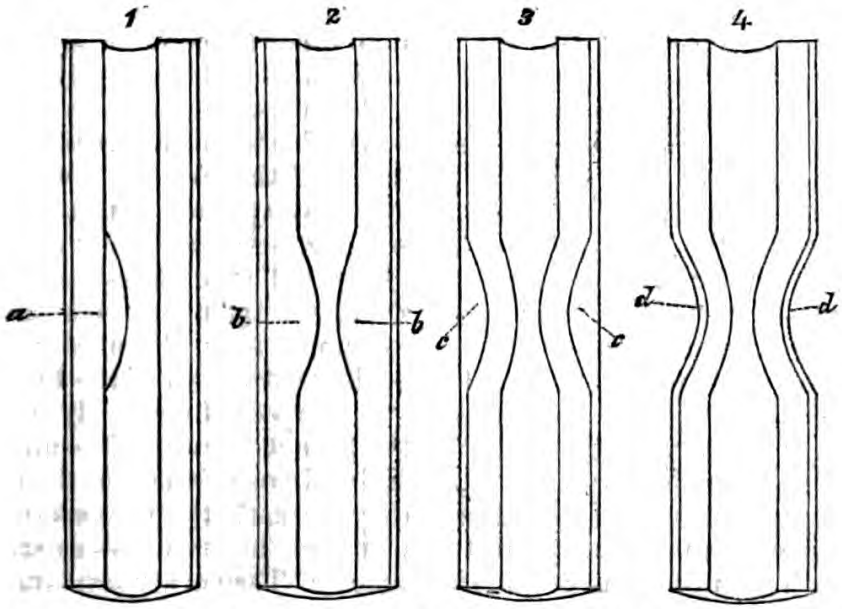


Diagram showing the four ways in which the bronchial tubes may be narrowed.
1. Bronchial catarrh. 2. Recent bronchitis. 3. Old bronchitis. 4. Asthma.

sibilus of asthma, however, may come one minute, and the next be gone, and is ever changing; moreover, the signs of bronchitis are absent. The sibilus arising from the contraction of plastic exudation thrown around the tube is unvarying and irremediable—a permanent condition, and must have been preceded by some recognised attack of severe bronchitis; the wheeze of asthma, on the other hand, ceases with the paroxysm, and there need not have been bronchitis in any part of the previous history of the case. We have thus got rid of three of the possible causes of sibilus—we have seen that in the case before us (asthmatic wheezing) it cannot be produced by mucous plugging, by vascular tumidity of the mucous membrane, or by the slow contraction of old plastic exudation thrown around the tube. Muscular spasm alone remains. And should we have in this a condition consistent with all the phenomena, and sufficient for their production? Perfectly. The supposition of spasmodic stricture of the air-tubes would explain the sudden access and departure of the dyspnœa, for it is a state that may be instantaneously induced, and may instantaneously vanish; it is consistent with perfect health in all other respects, with the absence of all organic disease or vascular disturbance in the lungs (except that which results from it), with the kind and characters of the sounds generated, with the particular type of the dyspnœa, with the effects of remedies, and with all those circumstances that point to the nervous nature of the disease, such as its causes, the effect of emotion, its periodicity, &c.; for only by the production of muscular contraction of their walls can nervous stimuli affect the condition of the bronchial

tubes; everything, therefore, that points to the nervous nature of the disease, points to spasmodic bronchial stricture as its proximate pathological condition.

Thus we see, by evidence as certain as sight, that in asthma bronchial spasm must and does exist, and that no other conceivable supposition will explain the phenomena. And we see this independently of that anatomical and physiological support that dissection and experiment supply, and that has hitherto been the chief evidence adduced.

But we find in the muscular furniture of the bronchial walls and the nervous furniture of the whole bronchial system, a valuable confirmation of the correctness of these views, both negatively and positively, for while their absence would be a sad stumbling-block in the way of our inferences from other evidence, their presence supplies exactly the required machinery. Nay, more, it is the most positive proof that could possibly be, that muscular contraction of the bronchial tubes does take place. For, what is the purpose of circularly-disposed muscle, if not to vary the calibre of the tube it invests? The muscular coat of the bronchiæ consists of circularly-disposed bands of fibre-cells, forming a continuous layer immediately beneath the mucous surface; these fibre-cells may be seen in tubes of great minuteness—as small as one-tenth or one-twelfth of a line in diameter. The nervous system of the lungs is derived from the vagus and the sympathetic and the anterior and posterior pulmonary plexus, and is from these origins furnished with wide-spread and varied connexions. It is these wide-spread nervous connexions that can alone explain some of the phenomena of asthma to which I shall have presently to refer. The nervous system of the lungs, thus derived, consists of ramifying plexuses, supported by the bronchial tubes as upon a scaffolding, and conducted by them to every part of the lungs. These plexuses form a sort of network, investing the bronchial tubes even to their finest ramifications, and are furnished with microscopical ganglia.

But besides this anatomical evidence, we have the positive proof of direct experiment; for Volkmann, C. J. B. Williams, and others, to whose accounts I must refer the reader, have clearly shown that the bronchial tubes undergo contraction, in some cases even to complete occlusion, from the application of various stimuli, both to the tubes themselves and to the trunks of the pneumogastric nerves. This completes the chain of evidence.

3. *That the phenomena of asthma are those of excito-motory or reflex action.*

Whenever the peripheral application of a stimulus results in muscular motion, we say that the phenomena are reflex. And so they are, universally. As far as our present knowledge goes, we believe that a stimulus applied to a sentient surface or organ must first be transmitted to a nervous centre by incident, and thence reflected by motor filaments, before it can affect the muscular tissue and stimulate it to contraction. The nervous centre may be a ganglion of microscopical minuteness, and the filaments emanating from it to their peripheral distribution may be of extreme shortness; but still, however near the

seat of movement may be to the seat of stimulation (and they may be completely coincident), such a centripetal and centrifugal course, and such an intervention of a centre, are essential. We see a very good example of this kind of reflex nervous action in the peristaltic movement of the intestines. In this case the stimulus travels along a perceptive filament to one of the ganglia of the abdominal portion of the sympathetic; there it comes into relation with a motor filament, and is by it transmitted to the muscular wall of the intestine. Of just such a nature is the contraction of the bronchial tubes in obedience to sources of irritation applied to their internal surface: the filaments distributed to the mucous surface receive the impression, along them it travels to some of the scattered ganglia of the pulmonary plexuses, and thence returns by motor filaments to the bronchial muscles to which they are distributed. This is the normal function of the bronchial nervous system; it is for the production of bronchial contraction, in obedience to stimulus thus applied, that it is especially organized. It is by this reflected path that surface-stimulation arrives at and contracts the muscular wall. It is in this way that the bronchi know when and where to contract; that a plug of mucus produces a circumscribed strait through which cough drives it with greater force; that exudation occupying the capillary bronchial tubes is expelled by their peristaltic contraction; that offending material that has found ingress through the glottis is shut off by bronchial stricture from reaching the ultimate lung-structure—supposing, that is, such actions really to take place.* In asthma from the effluvium of hay, and of certain animals, as cats and rabbits; asthma from inhaling the emanations from ipecacuan powder; asthma from breathing ammoniacal or carburetted fumes; asthma produced by certain airs; asthma complicating bronchitis,—in all these the bronchial spasm is of this natural, physiological character; the seat of the application of the stimulus, and its reflected path, being the same as that by which ordinary stimuli arrive at, and produce contraction of, the bronchial muscle.

But one of the peculiarities of asthma is that it may be induced by stimuli applied to remote parts; in these cases the nervous circuit is much longer, and the phenomena of reflexion clearer and more conspicuous. Take, for example, that most common of all the varieties of asthma, what we may call *peptic asthma*, in which the induction or prevention of attacks is entirely controlled by the state of the digestive organs, in which an error in diet—the eating some particular thing, eating too largely or late in the day—is sure to bring on an attack; while a certain dietetic abstention is as certain to be attended with immunity from the disease. Here the reflex character of the phenomena is clear, and the nervous circuit by which the reflexion is completed conspicuous and evident. I think there are three degrees of remoteness of the application of stimulus producing asthma, and thus three groups into which we may divide these clearly reflex cases.

1st. Those that I have just mentioned, in which the source of irrita-

* For a further discussion of this point I must refer the reader to the concluding paragraphs of this paper.

tion is alimentary, and generally gastric. Here the nerve irritated is the gastric portion of the pneumogastric, by which the stimulus is conducted to the medulla oblongata; this is probably the seat of the central reflexion, and transmits the stimulus immediately to the lungs by the pulmonary filaments of the same nerve, the bronchial muscles contracting in obedience to this reflected stimulation, just as they would have done if it had been primarily pulmonary. Here we have only one nerve concerned—the pneumogastric, but two portions of it, one of which plays an afferent and the other an efferent part, while the portion of the centre involved is confined to the origin of the nerve—to the seat of implantation of these respective gastric and pulmonary filaments.

2nd. Those cases in which the irritation is more remote, but is still confined to the organic system of nerves—e.g., asthma produced by a loaded rectum.

3rd. Where the cerebro-spinal system is the recipient of the irritation which is the provocative of the attack—for example, that remarkable case related to me by Dr. Chowne, in which the application of cold to the instep immediately produced the asthmatic condition.

Of these two last groups of cases, I know of very few examples, and I may dismiss them with a few words. A case came under my observation some years ago in which a patient could regulate his asthma entirely by the condition of his bowels. They were, as a rule, relieved every evening; if the customary relief took place, and he retired to bed with an empty rectum, he awoke the next morning well; but if he neglected to relieve his bowels, or his efforts to do so were abortive, he was quite sure to be awoke towards morning by his asthma. Strange as this may appear, it is strictly true. Dr. Copeland remarks that the attack is often preceded by costiveness and inefficient calls to stool—an observation quite in accordance with the case I have just related. I should be disposed to think myself that these were not only precursory and premonitory symptoms, but that they had something to do with the causation of the asthma that followed them—that the attack had a *propter* as well as a *post* relation to them. Of a strictly analogous nature are those cases of hysterical asthma in which the attacks are preceded by recognised symptoms of uterine irritation. The remarkable case communicated to me by Dr. Chowne to which I have already referred, was as follows:—J. G., a man of about fifty years of age, made application to an insurance office for the assurance of his life. In reply to the questions of the physician of the office, he stated that he was liable to spasmodic asthma. He stated that he was subject to these attacks if by any accident cold water fell upon his instep, or his instep in any other way became cold. The impression on the mind of my informant, who was the medical man who examined him, was that this was the commonest, but he is not sure it was the only, cause of the attacks. The asthma came on suddenly and *immediately*, and the attacks were very severe. The circumstances were considered so curious, that great pains were taken thoroughly to sift the case, and the result was that the facts were clearly established, and the man's life refused in consequence.

But while cases illustrating in this remarkable way the excito-motory nature of asthma, and the distance from which stimuli may reach and influence the innervation of the lungs, are rare, cases of peptic asthma, in which the attacks are caused by pneumogastric irritation, are so common, that I think few cases could be found of true spasmodic asthma in which the disease is uninfluenced by the state of the digestive organs, while in a very large number it is entirely under their control. This fact is so patent and so generally recognised that it has by many writers been made the basis of their classification of asthma; thus Dr. Bree and Dr. Young erect into a distinct species those cases that are dependent on gastric irritation. Therapeutically, the full appreciation of this fact is most important; more is to be done for our patients on the side of the stomach than in any other direction. An observant and thoughtful physician once said to me that he considered dietetic treatment *the only* treatment of asthma.

But there is yet again another class of cases that have suggested to my mind the belief that asthma is sometimes *central*, not reflex, in its origin; that it may originate in irritation of the brain itself, or the spinal cord. The two following cases appear to me to be examples of this kind of "*central*" asthma. The first was communicated to me by my brother, Mr. James Salter, and occurred under his own observation. The patient was a boy of about ten years old, and the disease acute hydrocephalus, which ran a fatal course in about a fortnight. Five days before his death he was suddenly seized with an attack of dyspnœa of the asthmate kind; it was very severe, lasted about half an hour, and then entirely vanished. The following day he was seized with a precisely similar attack; but this was the last; the symptoms never reappeared, and the patient sank in the ordinary way, from the brain disease, about four days afterwards. He had never before suffered from asthma; there were no chest symptoms either before or after the attacks—no cough, no expectoration. My brother is very precise as to the nature of the dyspnœa; he says there was nothing cardiac about it—no panting, no orthopnœa—but that it had the labouring "difficult" character of asthma. I conceive that in this case the bronchial spasm was a phenomenon of deranged innervation from central irritation, analogous to the jactitations, rigidity, and convulsions characteristic of hydrocephalus.

The other case was that of a man of about fifty years of age, subject to epilepsy. His fits had certain well-known premonitory symptoms, and occurred with tolerable regularity, I think about once a fortnight. On one occasion his medical attendant was sent for in haste, and found him suffering from violent asthma; the account given by his friends was, that at the usual time at which he had expected the fit, he had experienced the accustomed premonitory symptoms, but instead of their being followed, as usual, by the convulsions, this violent dyspnœa had come on. Within a few hours the dyspnœa went off and left him as well as usual. At the expiration of the accustomed interval after this attack, the usual premonitory symptoms and the usual epileptic fit occurred. On several occasions (I do not know how many) this

was repeated, the epileptic seizure being, as it were, supplanted by the asthmatic. Of these four points my informant, who was the medical attendant, seemed certain; that there was nothing amiss with the lungs either before or after the attacks, that the character of the dyspnœa was asthmatic, that each attack of asthma occurred at the usual epileptic period, and that they were preceded by the premonitory symptoms that ordinarily ushered in the epilepsy. I think such a case admits of only one interpretation—that the particular state of the nervous centres, that ordinarily threw the patient at certain periods into the epileptic condition, on certain other occasions, from some unknown cause, gave rise to bronchial spasm; that the essential diseased condition was one and the same, but that its manifestations were altered, temporary exaltation and perversion of the innervation of the lungs supplanting unconsciousness and clonic convulsion. Bearing in mind the many points in their clinical history that asthma and epilepsy have in common, this case is one of peculiar interest.

To this same category of *central* asthma we must, in strictness, assign those cases in which the paroxysm is brought on by violent emotion, as in that remarkable instance I have related of the gentleman who had, on two distinct occasions, violent spasmodic asthma suddenly induced by alarm, from the fear that he had poisoned his wife. In such a case, the seat to which the stimulus is primarily applied is the brain itself.

Lastly, there is a class of cases in which the exciting cause of the paroxysms appears to be essentially *humoral.* I have stated that the most frequent of the exciting causes of asthmatic attacks are alimentary, and that an error in diet, or the mere introduction of food into the stomach, produces bronchial spasm by reflex stimulation, through the intervention of the pneumogastric nerve. But is this the only way in which the lungs can be affected by what is put into the stomach? No. Although the pneumogastric nerve is the only single structure that has a distribution common to both organs, yet the venous system affords a very close and intimate bond of connexion between the stomach and the lungs; for any rapidly absorbable material introduced into the stomach is at once taken up by the venous radicles of the gastric mucous membrane, and within a few seconds, having passed through the liver and the right side of the heart, finds itself in the pulmonary circulation. In this way the blood in the lungs is liable to constant change in its composition, from admixture with it of the different materials thus taken up by the gastric veins; and, from the absence of secernent or elective power on the part of these veins, is ever at the mercy of the food. The chief parts of the normal results of healthy digestion, or the morbid results of depraved digestion, and that numerous class of bodies which are at once taken up without any change, are thus thrown directly upon the lungs. In the intestines we have an additional channel for the introduction of the contents of the alimentary canal into the pulmonary circulation; namely, the lacteal absorbents. It is, I believe, in this way, by the actual presence in the vessels of the lungs of the materials taken up

from the stomach and intestines, that the introduction of food into the alimentary canal frequently gives rise to bronchial spasm. A contaminate blood is the irritant, and excites the bronchial tubes to contract through the intervention of the pulmonary nervous system, just as the effluvium of hay or an irritating gas would. In one case the irritant affects the surface to which the nerves are distributed, in the other, the capillaries among which they lie. When I say a "contaminate" blood, I do not mean that the material present in it is necessarily peccant. I believe it may be perfectly normal, and yet produce asthma. I believe that digestion may be everything that it should be, and its results in no way different from that of a perfectly healthy person, and yet they shall, in an asthmatic, produce asthma the moment they arrive at the lungs. I believe this because some of the materials that give rise to asthma are such as undergo no change, but are at once absorbed, and must therefore necessarily be identical in the lungs of the sound man and in the lungs of the asthmatic, and also because many persons who are rendered asthmatic by taking food, exhibit no symptoms of deranged stomach-action whatever. One such case in particular, with which I am very familiar, is that of a lady who every day within a few minutes after commencing her dinner experiences that dry constricted straitness of breathing characteristic of asthma; even if she has her asthma at no other time, she will have it then; after lasting a quarter of an hour, or half an hour, it passes completely off. Now this lady's digestion is remarkably good, unusually powerful and rapid; she is free from the ordinary restrictions of diet that most people are obliged to acquiesce in—radishes, cucumbers, and other unwholesomes, agree with her perfectly well; and the production of the asthma does not depend upon the quality of the food, such things as I have mentioned do not seem to induce it, while she will often become asthmatic during the plainest meal. I believe in this, as in hay, ipecacuan, and other asthma, that the irritant differs not in the asthmatic and the healthy person, but that the essential difference is in the irritability of the pulmonary nervous system—that it resents that which it should not resent—that its morbid sensitiveness exalts that into a stimulant which should not be a stimulant, and that thus the pulmonary nervous system registers (as it were) on the bronchial tubes those changes in the constitution of the pulmonary blood of which it should be unconscious.

There are, however, certain articles of diet which, either from their being peculiarly offensive when materially present in the lungs, or apt to give rise to dyspepsia and its vitiated results, or specially irritating to the gastric portion of the vagus, are very apt to give rise to asthma. Such asthmatic articles of food are—cheese, nuts, almonds and raisins, and sweet things in general, salted meats, condiments, potted and preserved and highly seasoned things, fermented drinks, especially malt liquors and sweet wines. I think malt liquor, especially the stronger sort, with a good deal of carbonic acid gas in it, is perhaps the most asthmatic thing of any; next to that I should place raisins and nuts. I know the case of an asthmatic gentleman who cannot eat a dozen

raisins without feeling asthmatic. But there is great caprice about asthma in this respect, strongly marked idiosyncrasies in individual cases. Thus, in one case, a single glass of hock would invariably bring on an attack, though any other wine might be drunk with impunity; in another, Rhine and Bordeaux wines—hock and claret—were the only ones that could be drunk; a dinner at which they alone were partaken of was sure never to be followed by asthma, but if port or sherry were drunk, the asthma would infallibly come on within an hour or two. In another case the whole mischief of a dinner, its sting, lay in its tail, in the usual post-prandial coffee; if that vicious drink was declined, no harm came of the dinner, but if it was partaken of, on came the asthma.

But why, it may be asked, do I choose to adopt the opinion that these different alimentary materials produce asthma by their material presence in the blood of the lungs, consequent on their gastric absorption? Why is not the supposition, that they act as irritants to the gastric portion of the vagus sufficient for the explanation of all cases? I do not say positively that it is *not* sufficient; I do not say it is not the sole way in which all these articles of diet excite asthma. But there is in particular one circumstance that makes me think that some of these materials at least, and in some cases, act by their presence in the lungs themselves, and it is this—that they induce the asthma in just such time as they would reach the lungs in subsequent to their absorption; the interval between the taking the material and the supervention of the asthma will be long or short, according as the absorption of that particular material is immediate or deferred. Thus, in a case which once came under my notice, in which the taking of wine or any alcoholic drink was always followed by asthma, the asthma was immediate, within a minute or two; while in another case, in which the food producing the asthma was such as would furnish material for lacteal absorption, the asthma did not come on till about two hours after taking the food, that is, when the chyle would be beginning to reach the blood being poured into the lungs.

But we must not forget that asthmatics are very commonly dyspeptics, and often exhibit symptoms of perverted and capricious stomach action, that suggest to one's mind the belief that the innervation of the whole of the vagus is vitiated, its gastric as well as pulmonary portion, and that the dyspeptic and asthmatic symptoms are but parts of a whole. I could mention some very interesting cases illustrative of this, if space permitted.

What I would wish, then, to express on this subject is—that I believe it possible that asthma is sometimes produced by particular materials admixed with the blood in the lungs, and that therefore it is so far *humoral;* but that these particular materials—whether absorbed unchanged, as alcohol, ethers, and saline solutions, or the results of healthy digestion, or of perverted digestion—have nothing particular in them, but are the same as they would be in any non-asthmatic person, and that the essence of the disease in these cases, as well as in all others, consists in a morbid sensitiveness and irritability of the pulmonary nervous system.

Great and valuable light is often thrown on pathological questions by considering the laws of the physiology of the part concerned, for pathology is often but deranged physiology, and pathological aberrations in strict subservience to physiological laws. Let us, then, see if we can detect in the probable purpose of the muscular endowment of the air tubes, an explanation of any of the phenomena of asthma. What *is* the purpose of the muscular contractibility of the air passages?

This is a question that has been variously answered, and whose certain solution is beset with considerable difficulties. In one light we may look upon the bronchial system as the ramifying efferent ducts of the great conglomerate gland the lungs, of whose excretion, carbonic acid, it affords the means of out-draught. Now the ducts of all large glands are furnished with organic muscle—liver, pancreas, salivary glands, kidneys, ovaries, so that the existence of muscular elements in the walls of the bronchial tubes is in strict conformity with anatomical analogy. But when we pass on from anatomy to function all analogy ceases. The purpose of the muscular endowment of the ducts of glands in general is the expulsion of the secretion; little waves of vermicular contraction pass along them, always in a direction *from* the gland, and thus their contents are driven along towards the orifice. Such cannot be the purpose of the muscularity of bronchial tubes, for they are permanently and necessarily patulous; indeed, throughout their greater length a special arrangement is adopted by which their closure shall be effectually prevented, by means of the little flakes of cartilage scattered throughout their walls. In the larger tubes these are such and so placed that only a slight amount of contraction is possible; they are continued down, in increasing tenuity and scantiness, to tubes of a smallness of half a line in diameter, and wherever they exist perfect closure is impossible; so that in none except tubes of extreme minuteness can perfect closure take place. But beyond the point of the cessation of cartilage flakes muscle still exists; it has been demonstrated in tubes $\frac{1}{120}$th of an inch in diameter, and probably exists even in the ultimate lobular bronchiæ; here, of course, perfect closure can be effected.

What are the acts, then, of which the bronchial tubes, thus constituted, are the seat? Respiration and Cough. For we must consider cough a normal act. It is the constituted mechanism of expulsion of any particles of foreign matter which may at any time be introduced with the respired air, and against whose ingress the stricture function of the glottis so imperfectly provides. Indeed, the respiratory organs have no power of selection, no means of filtering the material on which they are every moment dependent for the exercise of their function; they are ever at the mercy of the air, and of any materials that may contaminate it. The power, therefore, of expelling any foreign or offending particle that may have found entrance, becomes a necessary appendage to respiration. Cough is, no doubt, often a phenomenon of disease, but it becomes pathological from the material on which it is exercised, and not from the essential nature of the act. If exercised on blood, pus, or excessive mucus, it is a symptom of disease; if on

some foreign particle that has found accidental ingress, it is strictly normal. It is no more pathological than sneezing is pathological, which a particle of dust, or a sunbeam, may cause at any time. What purpose, then, if any, has the muscular endowment of the air passages in relation to these two acts—respiration and cough?

It has been maintained by many that the bronchial tubes contract at each expiration, and so assist in the expulsion of the air. But the character of the fluid expelled—gaseous, and the method of its expulsion—quick, transient, and iterated, are neither such as the ordinary slow vermicular contraction of organic muscle would be appropriate for; it is impossible that the bronchial tubes can expel the expired air in the same way as the ducts of glands do their secretion, it is impossible that a wave of contraction can pass from extremities to trunk of the bronchial tree at each expiration; it would be a rapidity of transit entirely at variance with all that we know of the law of organic muscle contraction. Moreover, we know that uninterrupted patulence from glottis to air cell, both in inspiration and expiration, is an essential condition of normal breathing. If there is any bronchial contraction coincident with expiration, it must be a slight narrowing of all the tubes, which would act not as a special expiratory force, but would merely diminish the amount of residual air locked up in the air-passages at the end of expiration.

This slight contraction of the whole bronchial system at each expiration I am, for the following reasons, inclined to believe. I have long observed that rhonchus and sibilant râles are often only audible during expiration; that in inspiration they cease; that they are frequently confined to the end of an expiration, not becoming audible till the expiration is half performed; that the longer and deeper the expiration the louder they are; and that the inaudible respiration of persons in apparent good health, particularly the old, may be rendered wheezing by making them effect a prolonged expiration. I have remarked this, not only with the dry sounds of asthma, but the moist râles of bronchitis; the crepitous wheeze of senile bronchitis is often confined to the termination of the expirations. If you tell a person with unsound lungs to wheeze, he immediately effects a prolonged expiration, as if he knew that that was the way to produce a wheeze. Now the source of sound—the plug of mucus, or the inflamed tumid membrane—exists as much during inspiration as expiration: why, then, should the sound be present in the one and absent in the other? On what does it depend? Manifestly on the alternate contraction and dilatation of the air passages. The plug of mucus, or the tumid membrane, which does not narrow the tube sufficiently to give rise to a musical sound in inspiration, in expiration does. I do not know how we can explain these phenomena except by supposing that all the bronchial tubes undergo contraction during expiration, and thus magnify the effects of any sources of inequality in their calibre; for a plug of mucus, for example, that would form hardly any impediment to the passage of air in a wide and patulous tube, would form a considerable barrier, and throw the air into strong vibrations, if that tube

were in a state of contraction. Moreover, I have noticed (and this is a very curious fact) that when the breathing is inclined to be asthmatical the dyspnœa may be aggravated, and the asthmatic feeling very much increased, by a prolonged expiration; while, on the other hand, the spasm may be broken through, and the respiration for the time rendered perfectly free and easy, by taking a long, deep, full inspiration. In severe asthmatic breathing this cannot be done; but in the slight bronchial spasm that characterizes hay-asthma I have frequently witnessed it. It seems as if the deep inspiration overcame and broke through the contracted state of the air-tubes, which was not immediately re-established.

Now all this certainly looks as if expiration favoured, and inspiration opposed, contraction of the air-passages—in fact, I think it amounts to positive proof of it. But I am not prepared to say that the bronchial tubes undergo *distinct muscular contractions* at each expiration. I think it possible to explain the phenomena otherwise. I think it possible that the diminution of their calibre at expiration may be due to the constant and unvarying tendency of their muscular and elastic walls to contract. This tendency is antagonized and overcome during inspiration, by which the tubes, like the other contents of the chest, are forcibly distended; in expiration this tendency is no longer opposed, and, like the other contents of the chest, they collapse. Still the assumed active contraction of the bronchi is quite consistent with the phenomena, especially those which I mentioned with regard to asthma—that a long expiration, namely, deepens the spasm, while a full inspiration may temporarily annihilate it. This looks as if bronchial contraction and inspiration were incompatible, and could not coexist. Still, it would be possible to explain both these circumstances by supposing that prolonged expiration merely *suffered* the tube more completely to yield to the pre-existing asthmatic spasm, while the distension of expiration was too strong for it and overcame it.

In relation to cough, the opinion has been advanced that the muscularity of the bronchial tubes may, by diminishing their calibre, increase the rapidity of the rush of air driven through them by the act of coughing, and thus increase its expulsive power. If this contraction were general and extended to the smaller tubes, the reverse would be the case, for a smaller stream of air would be brought to bear upon the obstructing material. If, however, it is a circumscribed contraction, confined to the situation of the matter to be expelled, then it would be a veritable adjuvant, and the air would rush through the point of narrowing with increased rapidity, and therefore increased expulsive power, just as narrowing an outlet of water clears and deepens the channel. But the contraction being at the seat of the matter to be expelled, and there alone, is an essential condition to this increase of expulsive power. A little glottis is, as it were, formed there, and the material inevitably driven through it.

But there is a third purpose, I think, arising from this very danger of the access of deleterious matter to the lungs, more important than either of the other two to which I have referred, and which I believe to be *the* purpose, *par excellence*, of the muscularity of the air-tubes.

It is the guarding the delicate ultimate lung-structure, the shutting off from the air-cells, and preventing reaching them, any deleterious material that may have gained entrance through the glottis. After having passed the glottis, there is no other means by which the further progress of any foreign matter may be arrested; and its arrest ere it reaches the pulmonary structure seems as essential to the well-being of the lung as its subsequent expulsion by cough. We know, in fact, that very little of the foreign particles contaminating the air reaches the air-cells; that it is almost entirely arrested and expectorated; and we know, too, that the respiration of air charged with any irritating or noxious material, such as the smoke of burning pitch, &c., will immediately produce in many people the symptoms of bronchial stricture.

Another office that has been claimed for the muscularity of the bronchial tubes, is the regulation of the supply of air to those portions of the lung in which they terminate, in the same way as the muscularity of the arteries regulates the blood-supply to the capillaries. We can easily understand how, when one lung or one portion of a lung is injured, a relaxation of the bronchial tubes of the other portion would enable them to deliver to it a freer supply of air, and thus capacitate it better for doing double duty. On the other hand, we can imagine how, in certain violent inspiratory efforts, a narrowing of the bronchial tubes would limit the supply of air, and exercise a conservative and protective influence by preventing too great and sudden a distension.

But if we would form a correct idea of the purposes which are stored up in an organ, our attention must not be restricted to its working in health, but must embrace also those exceptional processes that arise in the course of disease—those latent, and, during health, inoperative powers kept in store, as it were, against the emergencies they are to meet—by the possession of which in its different organs, the body becomes not only a self-maintaining and self-regulating, but a self-correcting machine. Have the bronchial tubes any such exceptional office, adapting them to a pathological exigency? Such a one has been assigned them. It has been maintained that when the smaller bronchial tubes become filled with secretion, as they so commonly do in disease—with mucus, for instance, as in capillary bronchitis; or with muco-plastic matter, as in pneumonia—they empty themselves of it, and pass it on into the larger tubes by a peristaltic contraction. This would be the acquisition in disease of the same kind of action as the ducts of most glands have in health. There are two strong negative reasons in favour of this view. One is, that there is in these minute bronchial tubes an absence of that anatomical peculiarity that would prevent anything like a peristaltic contraction of the larger ones—namely, the flakes of cartilage. The other is, that these ultimate tubes are wanting in the ordinary mechanism of discharge by which the bronchial system keeps itself empty—the *vis à tergo*, namely, of cough; for, having behind them only so small a volume of air—that contained in the lobule or group of lobules in which they terminate—sufficient explosive force cannot be brought to

bear upon the mucus obstructing them—there is no air behind it to drive it forth. The larger the tubes the more effectual cough becomes, because the greater portion of lung with which they correspond, and the larger the volume of air on which the parietes of the chest exercise their pressure in the explosive expiration of cough. The ultimate bronchiæ, then, being deficient in this power of clearing themselves, we should naturally expect that some succedaneum would be provided, and this we recognise in the peristaltic contraction of which they are probably the seat. Were it not for this, one can hardly conceive by by what mechanism they would empty themselves of the materials by which, as shown by the sounds of respiration during life and post-mortem examination, they are so apt to become infarcted.

Now can we see in these real or probable purposes of the muscular contractility of the bronchial tubes any explanation of the phenomena of asthma, and any clue to the ultimate pathology of the disease? Yes, I think, most clearly. We see that the purpose of this muscular furniture of the bronchial tubes is that they should contract under certain circumstances, and on the application of certain stimuli; and seen by this light we recognise in asthma merely a morbid activity—an excess—of this natural endowment; the tubes fall into a state of contraction, with a proneness, a readiness, that is morbid; the slightest thing will throw them into a state of spasm, the irritability of the muscle is exalted, the contraction violent and protracted, that becomes a stimulus to contraction which should not be, and the nervous and muscular system of the lungs is brought within the range of sources of irritation applied to such distant parts as ordinarily in no way affect them. Any healthy man may have his bronchial tubes temporarily thrown into a state of asthmatic spasm by the inhalation of ammoniacal or carburetted or other irritating gases; but only by such materials, whose exclusion is necessary for the safety of the lung, will this natural asthma be brought about. A greater degree of bronchial sensibility is shown in those cases, by no means uncommon, of what is called "hay-asthma," in which the stimulus to bronchial spasm is the effluvium of hay; a still greater, in those cases much rarer, in which the emanations from ipecacuan powder will at once give rise to asthma; a still greater in that numerous class of cases of asthma in which the disease is called into activity by certain atmospheric peculiarities which are altogether inappreciable, as where an attack of asthma is inevitably brought on by going to a certain place, living in a certain house, sleeping in a certain room. All these cases fall strictly under what we may call the formula of health; they are physiological, they are instances of the contraction of a muscular tube in obedience to stimulus applied to the mucous membrane that lines that tube; the nervous system engaged is the *intrinsic* nervous system of the tubes, its own ganglia and perceptive and motor filaments, as in œsophageal deglutition or intestinal peristalsis; the error is merely a morbid exaltation of a normal irritability. But there are other cases in which the error is more than this, in which the nervous apparatus involved in the phenomena is abnormally extended; in which certain

outlying and distant parts of the nervous system are the recipients of the stimuli that give rise to the bronchial spasm, as in those cases to which I have referred, where an attack is induced by an error in diet, a loaded rectum, the application of cold to the instep, mental emotion; in which the gastric filaments of the pneumogastric nerve, the sympathetic, the cutaneous nerves of the foot, and the brain, are respectively the recipients of the stimulus that gives rise to the bronchial contraction. In the former class of cases the bronchial spasm takes place in obedience to the wrong stimulus applied to the right place; in the latter, place and stimulus are alike wrong; the relation of the asthma to its cause is in the one case immediate or primary, in the other, remote or secondary,—mediate, through the intervention of some part of the nervous system extrinsic to the lungs.

In what, then, does the peculiarity of the asthmatic essentially consist? Manifestly, in a morbid proclivity of the musculo-nervous system of his bronchial tubes to be thrown into a state of activity; the stimulus may be either immediately or remotely applied, but in either case would not normally be attended by any such result. There is no peculiarity in the stimulus, the air breathed is the same to the asthmatic and the non-asthmatic, the ipecacuan powder, the hay effluvium, is the same in both; nor, probably, is there any peculiarity in the irritability of the bronchial muscle; the peculiarity is confined to the link that connects these two—the nervous system, and consists in its perverted sensibility, in its receiving and transmitting on to the muscle, as a stimulus to contraction, that of which it should take no cognizance. In those cases where the spasm is produced by some irritant applied to the air-passages themselves, this perverted irritability is confined to the bronchial nervous system. The exact seat of the perverted nervous action in those other cases where the stimulus is remote, is more doubtful. Take, for instance, that case in which cold water thrown on the instep immediately produced asthmatic dyspnœa. Was the cerebro-spinal nervous system in fault here? Did it transmit to the pulmonary a morbid stimulation, or was the fault, as in the other cases, confined to the pulmonary nervous system—to its being morbidly affected by a nervous impression perfectly normal? These are questions that in the present state of our knowledge it would be difficult or impossible to pronounce upon. At any rate, it is clear that the vice in asthma consists, not in the production of any special irritant, but in the irritability of the part irritated.

These considerations, I think, tend to rationalize our notions of asthma, and to impart at once an interest and an order to its phenomena.

ART. II.

On the Theory of Inflammation. By JAMES HINTON, M.R.C.S.

INFLAMMATION, being the most frequent form of disturbance in the animal body, has received from pathologists the largest amount of study. By observation and experiment its phenomena have been traced from first to last. Yet the word conveys to us still but an indefinite meaning; the relation which the changes implied by it bear to each other has not been distinctly grasped. The senses have contributed their part, but the mental element is defective. We still wait for that true knowledge which consists in the recognition of order and mutual dependence; and our efforts must continue until we are able to place before our intellectual sense the observed phenomena in a rational and necessary sequence.

Nor is there in such an attempt anything unreasonable. The links of necessary causation must exist, and a right knowledge of them must be simpler and more conformable to reason than hypotheses constructed in ignorance. We seek the relation in which certain observed processes stand to each other, the rational bond between them. In a word, we require a dynamic view of inflammation. Some progress in this direction has indeed been made in the proposition now so generally held, that inflammation is "an altered nutrition." Unquestionably this is so far good. It recognizes in inflammation a *process*, and excludes therefore the idea, which is so apt to suggest itself to us in relation to all that is not understood, of a specific entity. But this expression can hardly be said to advance us far on the road to a positive knowledge. If we may on the one hand affirm it to be true, must we not on the other admit it to be a truism? What is the amount of information it conveys to us which we did not previously possess? It tells us that inflammation is a diseased or perverted state of life, but are we not apt to think that it tells us much more? Does not that unknown term "nutrition" stand in our thoughts for some definite addition to our knowledge? Does it present itself to us so clearly as it should do that if the meaning of nutrition be so large, and we know so little of its nature, it is but a form of words to say that inflammation is an altered state of it?

All writers on inflammation have recognised in it processes of two opposite characters and tendencies. Mr. Paget classifies them into those that are productive, and those that are destructive, and the distinction is broadly obvious. Into the ordinary conception of nutrition itself indeed both these processes enter; it is regarded as including two opposite actions or series of changes—growth and decay. But this oppositeness of action is even more marked in inflammation than in health. In an inflamed part we may see a structure decomposing, not in invisible molecules, or by mere interstitial removal of its elements, but dying in large masses, while all around it the evidences of *vital* action, of the impetus towards growth, are seen in more than ordinary energy. Is there any intimate relation between these oppo-

site actions; may inflammation consist in either alone; or, if both be essential, what is their connexion?

That an increase of both processes, the decay and the vital action, is necessary to constitute inflammation, appears when we consider the distinctive characters of that affection. It differs from mere increased decay, as primary gangrene or atrophy, on the one hand; and from mere increase of vital action—hypertrophy, repair, or development—upon the other. Its peculiar characters involve at once an abnormal increase of destruction and of growth.

If, then, both these changes be essential to inflammation, can there be traced between them any other connexion than that of co-existence? Are they related as cause and effect? Which is the starting-point of the morbid process?

I answer: they are related as cause and effect; the increased decomposition is the starting-point; the increased vital action is secondary and dependent.

The first proof of this position is found in the nature of the causes by which inflammation is induced. All of these, it has often been remarked, are such as clearly tend to lower the vital power or to produce actual destruction of the parts on which they act. In every case in which the origin of inflammation is distinctly traced, the starting-point is found to be in fact an anti-vital change.

And this practical evidence is reinforced by the most cogent theoretical considerations. Can we represent to our thoughts any clear idea of a primary abnormal increase of the *vital* or formative action that should be inseparable, as inflammation is, from a concurrent increase of decay? And this increased decay not such as attends and is subservient to increased growth, but of so disproportionate an amount as almost always to result in a lessened vitality of the affected part. Is it not a contradiction that an approximation to death should be the result of an increased life? It is not inquired now how such a primary increase of the formative action should arise, and especially in such circumstances of debility and depression as most favour inflammation, because that subject will be considered hereafter in tracing the relation between inflammation and adventitious growths; but there is a direct bearing on the question in the fact that inflammation arises in tumours then first when decay begins in them. It is incompatible with the increased formative action which produces them; it is a constant attendant on their disintegration.

Connecting thus the two series of changes, destructive and formative, as cause and effect, both may be understood. For the increased formative action some cause is demanded, some additional and locally acting force to which it may be ascribed. This demand is fulfilled by the increased decomposition, which is a known source of force, and which is itself sufficiently accounted for by the tendency of all organized substances to undergo decay. The abnormal decomposition is referrible to known and sufficient causes, and itself supplies a cause for the abnormally increased activity of the formative process. For not only is decomposition of the tissues (a change belonging to the class of

chemical actions) a recognised source of *force* as such, and thus capable of acting as a stimulus upon the vital activity of adjacent tissues, but it is shown by well-known facts to be immediately concerned in the production of the formative action. Such facts are the liquefaction of certain portions of the embryo as conditions for the development of other portions; the decomposition of the food which forms the first stage of digestion; and especially the immediate dependence of the nutrition of any organ upon its functional activity.

Inflammation indeed stands thus but as an exaggerated instance of this normal relation of decomposition and growth: it is strictly correlated to the ordinary processes of life; an abnormal or excessive functional or decomposing change, producing a similar excess of the reparative action. It may seem strange indeed how so natural an interpretation of the facts should have escaped the sagacity of those observers who have especially noticed the intimate connexion between functional activity and inflammation, and who have remarked that excessive function of an organ leads to or becomes inflammatory action by such continuous gradation that the line cannot be drawn between them.

Inflammation is excessive function, with or without qualitative perversions: *common* in the absence of such perversions, *specific* when they exist. The term "function" is here used to signify that disintegrating change of which the functional activity is an indication. In attributing inflammation to an excess in this respect, nothing is assumed but a known tendency, the chemical affinities, which may always be presumed to act when not prevented by opposing force, or absence of the requisite conditions; and which are therefore necessarily brought into play by all that diminishes the perfection of the vital state. From the operation of these forces all the main phenomena of inflammation may be traced in a consequent series, and no recourse is necessary, as upon the hypothesis of a directly increased vital action, to mysterious, or at least to unknown, powers.

The twofold nature of the processes concerned in inflammation has been one chief source of the difficulty that has invested the subject; these processes not being seen in their true relation, nor recognised as corresponding, in respect to that relation, to the healthy life. Two opposite views have been maintained by different writers, each with great support from observation, yet each failing to supply a theory of the affection that could be accepted as complete, or as applicable to all cases. On the one hand is the theory of "increased action;" on the other, that of "debility," or diminished vital force. Each reposing on one portion of the phenomena, with an insufficient recognition of the other, and embarrassed, therefore, instead of aided, by half of the facts with which it had to deal; each capable of a most plausible demonstration, yet leaving in the mind a painful consciousness that the problem was not solved, nor the true nature of the disease revealed. The old theory of increased action* demanded as its complement the

* For a most ingenious argument in favour of this view, see a paper by Dr. Cappie, On the Nature of Inflammation: Edinburgh Medical and Surgical Journal, No. 81, p. 58.

modern one of debility or diminished action; but the latter, though more philosophical, equally fails to express the whole truth, and had it existed first, would not less certainly have been supplemented and supplanted by the one whose place it has usurped. If inflammation be in all cases merely diminished action, "depression of the vital force," what is the distinction between sthenic and asthenic inflammations? why should stimuli be in some cases useful, in others injurious? Would not the term, "diseases of debility," become then a mere pleonasm, while yet we cannot but feel that it does express an actual and most important distinction between classes of disease which may be both alike inflammatory? And are not greater heat, more rapid circulation, a more vivid sensitiveness, among the indications of a higher life by which the warm-blooded animals are elevated above the cold-blooded? Shall we, to make a theory consistent, permit contradictory interpretations of identical phenomena?

Let me not be misunderstood. I do not deny that inflammation is, in one sense, always a disease of debility; that is, its starting-point is an anti-vital change, it originates in decay; but it includes not less an opposite class of actions, the downward process generates an upward one; decomposition adds intensity to life.

The inflammatory process, then, is an affection primarily due, as all functional processes are, to a disintegrating change which generates a formative process that would not else exist. Thus viewed, inflammation may not only be better understood in itself, but may be brought into definite and intelligible relations with a wide circle of kindred phenomena, mutually giving and receiving light. And first, as to its own nature, it is found to bear a distinct and decisive character. It may be defined. The boundary which separates it alike from health and from other morbid processes is distinct and legible. From health it is distinguished in this, that it is an excess or perversion of the *functional* activity, with its consequences; the decomposition which is normal in function exceeds in inflammation that amount which is compatible with the integrity of the tissues.

And from other local diseases it is clearly marked by these characteristics, of involving a twofold action, and of starting from a decomposition. Tumours may present the twofold action of growth and decay, but the growth in their case has precedence. Hypertrophy presents increased formation only; atrophy, diminished formation, and probably diminished energy of decomposition also. Degeneration, properly so called, if agreeing with inflammation in having increased decomposition for its starting-point, differs from it in the absence of the vital reaction; as also does primary gangrene, though the latter is a cause of inflammation in the surrounding parts. It seems to me that the difficulty, on which so much stress has been laid, of indicating precise lines of demarcation between inflammation and other affections, does not exist if the case be rightly conceived. That various abnormal processes may coexist is true, but there is no necessary confusion among them. Where a local decomposition, carried beyond the bounds of the normal functional activity, has brought in its train an abnormal formative action, in however slight a degree, in whatever condition of

the system, or with whatever other morbid processes it may be mixed up, there has been inflammation. The relation of the forms of action concerned in the inflammatory process is well seen in the phenomena attending suppuration. For in the formation of pus-cells there appears to be a true growth; and we may conceive that the force arising from the increased decomposition which has previously been operating upon the solid textures, producing in them the heat and redness and swelling which are characteristic of inflammation, operates after the effusion partly on the effused fluid. So that while the dynamical process remains the same, the "symptoms" begin to subside.

Again, if inflammation start from increased decomposition, and all exaggeration of the normal proportion of that process tend to give rise to it, then its extreme frequency is sufficiently accounted for. It must be that inflammation should result from every form of irritation, should complicate every other disease, should arise the more readily the more the vital powers are depressed, should attend all injuries, should affect all structures, should know no limitations of age or circumstance; that it should be, in short, the great disease, and the chief subject of the healing art. The producing cause of inflammation is one that is in constant operation; the tendency to it is involved in the very existence of a living body. Life is a state of constant *tension*, any relaxing of which results of necessity in that excessive decomposition which initiates the inflamed condition. It needs not any extraneous agent to sustain it. As is the spark to gunpowder, or the electric shock to a mixture of oxygen and hydrogen, so is its "exciting cause" to inflammation. Take away, or suspend by any means, the controlling force which holds in organic relations the elements of the living frame, and that anti-vital change takes place, that new arrangement approximating to the inorganic state, which the familiar affinities of those elements tend always to produce. Thus is inflammation, as it were, the sword of Damocles suspended over the head of every living thing. Even as death for ever threatens life; for inflammation involves a partial dying of the part affected, and the reaction of the living frame against it.

Thus it conforms itself to the radical idea of a disease; that of a defect of life. For it was a fatal objection to the old doctrine of increased action, that it assumed, as the essence of a disease, excess of life. All disease deviates from health primarily by defect, it is a sinking, not an elevation; in so far as any organism suffers disease, it has approached to death. Yet, in another sense, there is in inflammation increased action; it is not a mere absence of the vital power, as atrophy perhaps may be, it is action opposed to it. The organizing process must have been performed, or inflammation cannot be. It is like the running down of a watch, which implies that it must first have been wound up.

But is it true that decomposing actions in the body do originate, or intensify, actions of an opposite character? or is this only an apparent, and not a real, relation? Has this conception so much basis, in fact, apart from inflammation, that it may legitimately be used as a guide in the theory of that disease? It is a wide question that is thus

suggested, but it is one that is fairly within the scope of observation. Virtually, it amounts to this: Is chemical action one among the forces by which the organizing processes are instituted and maintained, under the conditions appropriate to them; or if not itself one of those forces, is it a source of them? For this inquiry the way has been perfectly prepared by the researches which have established the dependence of the organic state upon the operation of force *ab extra*. There is clearly no theoretical or *à priori* reason that it should not be so. For chemical force takes its place in the chain of inorganic forces, mutually producing and produced. There are no characters which separate it from the rest, or should forbid it to have its share in the organizing agency so freely ascribed to the light and heat with which it is interchangeable; or if the apparent opposition between chemical and vital processes should be objected, two observations may be made in reply. First, that heat and light show themselves in certain aspects opposed to life. Is not heat unequivocally a determining cause of vital action, yet what is more destructive of vitality than a temperature raised too high? Nay, is not cold itself often an agent invigorating to the vital processes; yet what is more opposed to life than cold? And light, too, may operate against vitality. The direct rays of the sun will paralyse the retina, or passing as it were into heat, when concentrated with a lens, will burn the textures. Over-stimulation by ordinary light injures the eye or withers plants. And secondly, if chemical action be so directly opposed to vital, as is implied in the objection, then must the two forms of action be similar in kind though opposite in direction; and nothing is more familiar to us than the production by a given action of an action opposite to itself. Does not the contraction of a substance in cooling produce expansion in the substances around, the fall of one scale of a balance the elevation of the other? Is not every motion in a limited space (if it be not a vacuum) of necessity two equal and opposite motions? What else, in truth, is the conception of a vibration but that of an action producing action of an opposite kind? As when a tense string deflected from the straight line is let go, its motion towards the central line reproduces the deflection.

I conclude, therefore, that there is no reason why chemical change should not have its part with other forms of action in determining the operation of the formative force, if observation afford evidence that it is so; and in this statement that particular form of chemical action concerned in the decomposition of the tissues is of course included.

Now, that chemical change does stand in this relation to the organizing process is indicated by very numerous facts, of which those that follow are only examples. The albumen of the seed partly decomposes with exhalation of carbonic acid, as the embryo germinates; decomposing organic substances are the seats in which fungi and animalculæ are developed; the increased organic action produced by light in the leaves of plants is preceded by a decomposition in those leaves.*

* See Draper On the Forces concerned in the Organization of Plants.

In fermentation, the yeast sporule grows while the liquid decomposes; and in this case the organic development cannot be obtained without the decomposition, while the decomposition may take place, although more slowly, without the organic development. To these instances we may add those before alluded to in respect to the animal body—the decomposition which takes place in the first stage of digestion; the breaking-up of portions of the substance of the developing embryo (the relation of which to the development of other parts has been noticed by Mr. Newport); and the part borne by functional activity, which means active decomposition, in effecting not only the maintenance but the increased nutrition of the organs.

Such facts as these justify us in placing decomposition in organic tissues among the circumstances which give rise to the organizing process; and their force is greatly increased by the evidence afforded by the phenomena of inflammation itself. For in this affection, whatever there may be of additional formative action points to a primary action of a decomposing character as its source, the origin of the entire series of inflammatory changes being always traceable to causes which overthrow the vital equilibrium and operate injuriously to the organic state.*

For a full appreciation of the bearing of the phenomena of inflammation on this argument, it only needs to be remembered that a disturbance of the vital condition, or lowering of the vital force, is not the inducing a merely passive condition in the part affected, but that there necessarily arises under those circumstances an active change, although not a vital one in the strict sense of the term; a decomposition of the tissues which the vital condition warded off and restrained. To diminish vitality is to permit a change more or less intense in the chemical constitution of the body. A heavy body sustained by any force, falls and produces action when that sustaining force is removed or weakened. So after death the body decomposes; it is in a truly active state, though not a vital one—an active state which can only be prevented by means which operate to forbid the play of the elementary affinities within it. Such is the "increased action" in which inflammation commences; from such action, all the increase of the vital energies which may be displayed in its course directly or indirectly draws its origin.

Still, it may be said, the conception of inflammation as a chain of effects, commencing with an excess of decomposing action in the part which is its seat, is unsatisfactory, and that in two respects. First, that the conception of excess or defect is too impalpable and abstract; the standard to which a reference is implied is not sufficiently defined. The "normal state," or equilibrium, itself perpetually oscillates within wide limits, and how can disease be defined by a reference to health, when health is definable only by a reference to disease? And secondly, that the inflammatory process presents many characters other than those of quantity, which involve diversities of kind or

* See especially Mr. Paget's Lectures on Surgical Pathology, vol. i. p. 437.

mode of action, and cannot be formulated as differences of degree alone.

The first of these objections is more plausible than valid. The idea of excess or defect is perpetually had recourse to in other cases, and found not too indefinite even for rigid science. A watch may go too fast or too slow; the spring may be elastic in excess, or in defect. The steam in a boiler may be expansive in excess, or the resistance be defective. Nor is it true that health is to be defined only by a reference to disease; for the functions and uses of a living body are as definite as those of any mechanical contrivance. There is a standard, known by experience, to which the balanced processes of growth and decay should conform, and deviations from which of any considerable amount manifest themselves by precise and definite results.

The second objection has more weight, and indicates another of the causes that have made the theory of inflammation so difficult.

The phenomenon is complex, and demands analysis. The various conditions to which the term is applied refuse to be brought under any single definition which is not so vague as to be almost unmeaning, as that of an abnormal nutrition, for example. But since all of them in common do present symptoms which we describe as those of inflammation, it is impossible to narrow the meaning of the term by limiting the cases to which it is applied. Only one course is open, but that is the same which is adopted in all like cases; the points of agreement, being abstracted, may receive a common name. Now the points in which all cases of inflammation agree are those which have been mentioned, of an increased formative action consequent upon an increased decay. To these conditions, therefore, the name of inflammation should be confined. Whatever other circumstances—whether of abnormal nutrition, or of any other kind—may be present in addition, that name of inflammation should have no reference to them. Many of them doubtless are causes of inflammation, such as the morbid diatheses, or poisoned states of the blood; but between them and inflammation itself no confusion should be allowed. The gouty, or rheumatic, or scrofulous diathesis, may be an "abnormal nutrition" (perhaps such conditions are better designated by that expression than inflammation is); but as they are perfectly separable from inflammation, so is inflammation, even when occurring with them, or as their consequence, perfectly distinguishable from them. Little progress, I venture to submit, can be made towards clear conceptions on these subjects until this distinction is recognised, and the different elements of the morbid process in what are termed specific or unhealthy inflammations are held apart, and receive their separate investigation. The constitutional morbid condition is one thing; the inflammatory action is another.

But though the connexion of inflammation with various diseased conditions has embarrassed the interpretation of its phenomena, by leading men to mix together in their thoughts elements that required to be distinguished, yet it affords an insight, hardly else to be obtained, into the use and meaning of that process in the animal economy. For

if the decomposition of the tissues in inflammation be a source of increase in respect to the vitalizing action, an immediate utility becomes evident in it. In diseased conditions of the system, the vital power is depressed; in the inflammations to which they give origin there is a source of increase of the vital power. Certain textures fail in their vitality owing to the defective vitality of the whole, and that failure is attended with chemical processes in them, which generate in their reaction an increased energy of vitality. In a word, inflammation, destructive as it seems, is in one point of view strictly a conservative and remedial process. In respect to the individual, it is the sacrifice of a less for a greater good. The benefit of it is often very evident, as when a blister induces healing of an indolent ulcer, or mechanical irritation the union of old-standing fractures. It is indeed from such cases as these that the theory of "increased vital action" draws its chief support; for as a fact, such increase is in these cases undeniable. But though less obviously, yet not less truly, I conceive, is the reactive process in inflammation in every case a salutary, that is, a saving or restorative, process. That we do not see it so, is that we do not sufficiently perceive the elements involved in the case. We do not carry our thoughts back to the loss or defect of the vital power which necessitates it, and to which it stands in the relation of a remedy or amelioration. Doubtless it is an evil; so is a forming callus, or a granulating wound. But these are less evils than a useless limb or a torpid sore, and so is inflammation a less evil than the mere decay and loss which would be without it. It is ever to be remembered that the symptoms of increased activity in the inflammatory process can never go beyond their cause, can never exceed the defect of vitality of which they are at once the effect and the sign. How violent soever or injurious in their results, the evil is not in them, but in that approximation to death for which they are the divinely appointed and only remedy. True, the results are often disastrous, the materials effused in inflamed organs may interfere with essential functions, or the excitement of the general system may exhaust the powers. But this is because the loss of vitality has existed in a great degree, or has affected a texture of primary importance. An organ that has suffered inflammation is a damaged organ, but it is a better one than if it had not inflamed: a constitution may thereby be weakened, but it might otherwise have sustained a severer injury. So far as is possible, inflammation restores a life that has been lost: it adds to vitality, not detracts from it; loss of vitality is its starting-point, but not its essence. I do not deny, indeed, that the effects of the inflammatory reaction may be injurious, and in a secondary way, as by mechanical pressure or otherwise, may give rise to evils serious or even fatal: nor that it may be wise in many cases to seek to moderate or subdue it. These are questions which experience must decide; they do not affect the physiological significance of the process.

And this aspect of inflammation becomes the more evident when we view it in relation to the other processes which constitute organic life. I have said it is an exaggeration of the *functional* activity, and

with some of the functions it corresponds not only in being a decomposition followed by nutritive action, but also in this, that a certain amount of the force, generated by the decomposition, is given off from the organic to the inorganic world. The heat of inflammation answers in this respect to the mechanical force of muscular contraction. But the function to which inflammation seems most nearly to approximate is that of secretion. Almost it appears as if one might speak of it without violence as a new secretion. To this idea, indeed, Marshall Hall may perhaps be said to have lent the sanction of his great authority, adopting the name of "excito-secretory" for inflammatory action produced by eccentric irritation, such as dentition, or the application of cold to the surface. Almost we might conceive the very same process to be secretion in an organ supplied with ducts, and inflammation in one in which ducts are not present: the secreting glands to be normally in a state which were inflammation in any other organ. Nor is it otherwise than favourable to this conception, that when the function of some of the secreting glands is hindered, other parts perform a compensatory action through the medium of inflammation. The urea which should pass off by the kidneys may find exit in the fluid of a pleurisy.

Doubtless, between the processes of inflammation and secretion there are many and important differences, but the question is whether there be not also an interesting and instructive likeness. Not least among such points of likeness may be the vitalizing, organizing power exerted on the blood by the secreting glands, or some of them, and this by virtue of retrograde changes involved in the process of secretion.*

So far, secretion and inflammation would agree as an increased vital action produced by a decomposing change; in the one case normal, in the other abnormal, but in both the decomposition being due to diminution or withdrawal of the controlling force. And as the natural secretions are rendered necessary by the normal life, are the results and complements of it, without which it could not be maintained, so may not specific inflammations be new secretions rendered necessary by that altered life which constitutes the morbid diathesis? As secretion is to life in the healthy state, so is inflammation to life in disease.

But there are other events in the natural life of various organisms to which the inflammatory process bears an analogy. Such are, for example, the reproductive processes of some of the lowest animals, as excited by cold or injury. Mr. Paget has remarked respecting the production of organized material in inflammation, that it is of large amount, but of the lowest grade. Now, a large amount of material of the lowest organization is produced in the gemmation of the polypes, which we know to result from some of the causes which give rise to inflammation in the higher animals. Does not inflammatory

* See Bernard's Experiments on the Effect of Secretion in rendering the Blood red instead of black as it issues from the Glands. The physiological doctrine, however, is entirely independent of these experiments. Dr. Prout says of excretion, "this function operates by denuding the matters excreted of their vitality which is retained, and separating the excrementitious matters in the form of common chemical compounds."

new production answer to an abortive gemmation? Especially does this appear when we extend our consideration to the case of repair, for between the gemmation of a hydra when wounded, and the granulation of a wound in man, is there not an obvious parallel?

Nor can I pass from this subject without again adverting to the phenomena of embryonic development. When we see one portion of the germ deliquescing and other portions developing, as if at their expense, can we avoid recognising in it a similarity to that which is the essential part in inflammation? Is it not as if, in inflammation, the system, under the pressure of adverse circumstances, threw itself back, as it were, upon the mode of existence proper to the embryo? As if, to retain as much as possible of perfectness under conditions threatening to destroy it, the law of its first formation came again into operation? The process which develops the life of the germ comes in to remedy defect of life in the completed animal. Not, indeed, by any special alteration of the laws of its being, but by the operation of the universal conditions of organic existence. For the processes of life in germ and adult are the same; different to our imperfect view they may appear, but the essential identity is made manifest in disease. The generation of life from death, organization from decay, striking to sense in germ-life and in inflammation-life, is patent to the reason equally in the life of maturity and health. It is the law of life. No new thing is presented to us in inflammation. The embryonic powers come forth in disease to meet the hostile agencies, only because they are at work unseen in all the operations of the vital force.*

And if inflammation be thus parallel to the processes of health, no less may its relation be seen to other morbid conditions. Of these it may suffice to select for the comparison the class of tumours. Differing in all other respects, these two diseases appear to possess in common but the one element of increased formation, yet, if what has been advanced respecting inflammation be well grounded, it affords a sufficient basis for the establishment of an intimate connexion between them. For if the increased formation in inflammation has its origin in increased decomposition, the same, it would appear, must be the case with tumours. Let it only be granted that such an origin is possible, and the evidence in favour of it is abundant. If all the known causes of adventitious growths be analysed, they will be found to correspond very closely to those of inflammation. They are causes of irritation, things that operate antagonistically to the vital power. Whether local or general, they have this character in common. How often the development of a tumour follows a blow, for example. It has indeed

* Dr. W. Addison has observed the resemblance of the morphological conditions in inflammation to those of the embryonic state. (On Healthy and Diseased Structure, &c.) If it should occur to any one as an objection to this view that by inflammation the vital integrity is lowered and not elevated, it will be sufficient to remind him that an injury to, and loss of, the vital integrity, is the sole occasion of inflammation. The inflammatory action seldom or never wholly repairs this loss, but its tendency is in that direction. Such repair is its object, its final cause or use in respect to the organic body. A man is not ill because he has an inflammation, but he has an inflammation because he is ill or injured. In reference to this view of the subject I have been greatly benefited by the perusal of an unpublished paper, entitled 'The Philosophy of Disease.'

been frequently remarked that it appears almost a matter of accident whether a given injury shall produce an inflammation or a tumour. All the evidence, therefore, which assigns an anti-vital starting-point for inflammation applies with equal force to tumours. Nor does the increased formation in the one case furnish any opposing evidence that would not bear equally upon the other. The distinction, then, between tumours and inflammation is not that the one disease is primarily of the formative process, the other of the decomposition. In this respect they do but appear to differ. They are both increased formation, due to increased decomposition; that is, to a diminution of the control maintained in the living state over the chemical affinities existing in the body. But they differ in the extent and intensity of this decomposing process; in inflammation it amounts to a true destruction, in part, of the vital condition, with a giving off of force to the inorganic world; in tumours it produces only an increased local activity of the organizing process. So far these affections resemble each other; they have this *dynamical* correspondence. But into other questions relating to tumours of course I do not enter. A condition that appears like a gradation between inflammation properly so called, and the growth of a tumour, may be seen in the increased formation of bone from chronic inflammation.*

It remains to consider the particular symptoms and terminations of inflammation, in so far as they bear upon the view suggested of its nature. But of these it is not necessary to speak at length. For, in the first place, the insufficiency of microscopic observations to furnish any clue to the essential character of the inflammatory process has been proved, and indeed admitted, long ago. Opposite theories dispose of them equally well, and the most minute investigations respecting them do plainly leave us entirely in the dark. In truth, these phenomena are to be interpreted by a sound theory derived from other and more appropriate facts, and not the theory to be framed on suppositions about the meaning of these phenomena. Very important and suggestive it is to know the particulars respecting contraction or dilatation of the arteries, the stasis of the blood, the aggregation of the corpuscles, and the nervous or other conditions associated with these, when we know to what essential changes, as respects the forces concerned in the life of the organism, they are to be referred. But we may gaze on such appearances for ever, and remain merely in blind wonder, or blinder theories of mechanical obstruction, paralysis, or mysteriously altered qualities. This is but a caricature of science.

And it is also undesirable to say much about these local phenomena, because we know so little. It is almost impossible to attempt to explain them without assumptions which go beyond our knowledge, and therefore, without creating hypotheses which are neither necessary nor useful. To me it seems sufficient to say at present that, under given circumstances, such conditions of the vessels and the blood have

* As indicative of the close connexion which has been felt to exist between inflammation and tumours, I may refer to Mr. Simon's representation of malignant disease as an *excretory process*—a "new secretion," we might say, the very conception which has suggested itself so strongly in respect to inflammation.

been observed. The facts must be valuable, but as yet they are not available for use. This however we know, that neither any state of the vessels, nor of the nervous system, can be primary or even essential elements in the inflammatory process. For a condition identical with inflammation occurs in plants. What else is the "increased formation" occasioned by the deposition in them of the larvæ of insects? The same cause which in the leaves of plants occasions swelling, hardness, and excessive formation of abnormal structure, produces in the animal textures heat, redness, swelling, pain, and the formation in excess of lowly organized tissue. Would it not be unreasonable to refuse to recognise identity of condition? In the plant, then, we see inflammation in its purest and simplest form, and so understand at once the secondary part which must belong to any changes affecting the specifically animal structures.

But if the conditions of the circulation be of little moment in respect to the essential nature of inflammation, they are of the greatest importance in respect to its progress and terminations. Constriction or dilatation of the vessels, stagnation or altered qualities of the blood, merely consequences though they be of dynamical changes wholly independent of them, may nevertheless be the chief agents in determining the course and results of the inflammatory process. The accumulation and stagnation of the blood in a part may be a cause of sloughing or of gangrene; its excess, with or without co-existent alterations in its quality, may prevent the restoration of the normal vitality, or may give rise to effusions of various kinds. So it may become a matter practically of the utmost importance, to control or remove the accumulation of blood in an inflamed organ. The issue of life or death may depend on it. But these therapeutical questions are so far distinct from that which has been the subject of this paper, that they are better treated independently. I would remark only, that if inflammation be, as I have sought to show, a twofold process of increased decomposition and increased formation, having its source in a diminution of the vital control over the coerced chemical affinities in the living textures, a general conception of the appropriate treatment is easily deducible therefrom. If it be possible, let the vital power be restored, and the suspended control reinstated; all our efforts should be directed most strenuously to this end, to prevent or diminish that failure of the vital tension from which the active symptoms of inflammation spring. But if this be not possible, then let the resulting changes be so regulated as may be best adapted to conserve at once the sufferer's general strength and the integrity of the affected organ. If the action be threatening from its violence, let those means be adopted which check decomposition or divert the flow of blood. But ever and above all, let two things be remembered: first, that the process of inflammation, as it meets our view, is not wholly an evil; that the *formative* process in it is the witness of, and the remedy for, an injury to the system unseen and too often unthought of by us. If we could remove all these symptoms, there would still remain that condition which has necessitated them; a worse evil, a more serious disease, in

combating which we should have deprived ourselves of our only ally, in having set aside Nature's only remedy. There would still remain that diminution and loss of vital power which no art of ours could then supply ; a death in life from which we might well shrink in impotent dismay. Let it be remembered ever, that where the cause of inflammation in failing vitality exists, there inflammation will be. No power of ours can prevent it, nor could it be anything but most disastrous if it did. All her resources, all her life, will Nature pour into the gulf of a local inflammation rather than suffer the deteriorated organ to fail of its support. The quick sympathy compels all the living powers to that work, and sooner should the veins be drained of blood, and the most vital functions droop and fail, than the weakened member not receive its larger share.

And, secondly : In cases of constitutional inflammation, the morbid condition of the system is a cause continually operating to produce that lowered vitality on which the symptoms of inflammation depend, and the removal of that diseased state is the means whereby those symptoms must be averted.

In conclusion I remark that inflammation is rightly enough represented as an altered nutrition. I have endeavoured not to controvert this view, but only to add somewhat to its definitiveness and value. As a formative or vital process, dependent on a decomposing or chemical one, it corresponds to the clearest conception of nutrition that we can gather from the phenomena of life in all its forms. Inflammation is the same process that constitutes all nutrition, but taking place under conditions other than those which are natural to, or best for, the individual organism in which it is excited. As an abnormal nutrition, it not only is illustrated by the other phenomena of life, but serves also to illustrate them. The process of nutrition receives elucidation from the comparison as well as gives it. For that which is found to be the essential character of inflammation, must be not less essentially the character of nutrition. This is perhaps the chief advantage that results from the establishment of the parallel between them, that all the knowledge which is obtained by a study of the phenomena of inflammation, more definite and susceptible of rigorous investigation as they are, may receive a direct application to the more extensive and difficult problem of nutrition.

PART FOURTH.

Chronicle of Medical Science.

HALF-YEARLY REPORT ON PHYSIOLOGY.

By Hermann Weber, M.D.
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I. General Physiology.

Brown-Séquard: *Laws relative to the Dynamic Phenomena of the Animal Economy.* (Brown-Séquard's Journal de la Physiologie, tome i. p. 7, 1858.)

Brown-Séquard places in front of his new Journal of Physiology a series of observations regarding various functions and phenomena of the animal economy, which he designates as physiological "laws." Some of these, as the author himself states, have been pointed out already by other physiologists. The twelve observations published in the present number may be comprised in the following general law:—The energy of the faculty possessed by animal tissues to produce nervous action, muscular contraction, the discharge of the galvanic apparatus of fishes, the galvanic current of certain organs, and several other dynamic phenomena, appears to be in direct proportion to the normal reparative process, and in inverse proportion to the energy and duration of the existence of these phenomena.

II. Food and Digestion.

1. Hammond: *Experimental Researches relative to the Nutritive Value and Physiological Effects of Albumen, Starch, and Gum, when exclusively used as Food.* (Philadelphia, 1857.)
2. Corvisart: *On the Digestion of Azotized Substances by the Pancreatic Juice.* (L'Union Médicale, vol. xii., No. 21, 1856.)
3. Bérard and Colin: *On the Effects of the Extirpation of the Pancreas.* (L'Union Médicale, vol. xii., No. 8, 1858.)

Hammond performed on himself, with great self-denial, a series of experiments on the nutritive value of albumen, starch, and gum. He commenced with ascertaining the usual condition of his system, and of the several excretions under the influence of mixed diet, and afterwards subjected himself, retaining in other respects the same manner of living, exclusively to one of the articles of food above named. The researches on the effect of *albumen* were continued during ten days. The author's conclusions are: 1. That albumen may be assimilated into the system in such quantity as to furnish a sufficiency both of nitrogen and carbon to the organism; 2. That under the use of an exclusively albuminous diet, the nitrogenous constituents of the urine are increased over the ordinary average amount, though not in proportion to the

quantity of albumen absorbed into the circulation; 3. That either some other means than the urine exist for the elimination of nitrogen from the system, or the excess (over two-thirds) is retained in the organism, even when the body is rapidly decreasing in weight; 4. That the continued use of albumen increases the proportion of this substance (and of fibrin) in the blood, and in a short time causes it to appear in the urine; 5. That while pure albumen cannot be regarded as of itself adequate to supply the several wants of the system, there is no reason why, when associated with suitable inorganic matters, it should not support both life and health. With regard to *starch*, employed likewise during ten days, Hammond arrived at the following inferences: 1. It can be assimilated in more than sufficient quantity to sustain the respiratory function; 2. Under its use the nitrogenous constituents of the urine are much reduced in amount, even below what would probably occur during inanition, and, although starch is not capable of nourishing the tissues, it is yet serviceable, apart from its heat-producing power, in retarding their destructive metamorphosis; 3. The continued use of highly amylaceous food causes the appearance of sugar in the urine; 4. Under such diet the nitrogenous constituents of the blood are diminished, and the carbonaceous increased. The experiments with *gum* were continued only during four days, as the feeling of hunger, debility, febrile excitement, and abdominal pain, became at the end of that time so great, that a change of diet was imperative. The author concludes: 1. That gum is altogether incapable of assimilation, and therefore possesses no calorifacient or nutritive power whatever; but is, on the contrary, a source of irritation to the digestive organs; 2. That, therefore, the solids of the urine are entirely derived from the waste of the tissues of the body, and the carbon exhaled by the lungs from the consumption of its fat.

Corvisart has continued his researches on the action of the pancreatic juice on azotized substances. In addition to the results previously related,* the author states that the quantity of albuminose (peptone) formed, by the process of digestion, from a certain quantity of proteinaceous food, varies much according to the quality used; that, for instance, albumen yields only half of the amount which casein yields.

Bérard and Colin having extirpated the pancreas of a considerable number of animals of various families, draw from their experiments the inference that in herbivorous ruminants, in carnivorous animals, in the pig, which is omnivorous, and in birds, the pancreatic fluid is not necessary either for the digestion or for the absorption of fatty materials.

III. Blood, Respiration, Animal Heat.

1. Heidenhain: *On the Physiology of the Blood.* (Archiv f. Phys. Heilkunde; Jahrgang, 1857, p. 507.)
2. Brown-Séquard: *Experimental Researches on the Physiological Properties of the Red and Dark Blood.* (Journ. de la Physiol., vol. i., p. 95 and 353, 1858.)
3. Bernard: *On the Varieties in the Colour of the Venous Blood of Glandular Organs, according to their state of functional activity or rest.* (Journ. de la Physiol., vol. i., p. 283, 1858.)
4. Brown-Séquard: *On the Modification of the Blood-globules of Mammalia in the Blood-vessels of Birds, and of the Blood-globules of Birds in the Blood-vessels of Mammalia.* (Journ. de la Physiol., vol. i., p. 173, 1858.)

* Cf. this Journal, vol. xxxix. p. 240. 1857.

5. Hammond: *On the Injection of Urea and other Substances into the Blood.* (North-American Medico-Chir. Review, vol. ii., p. 287, 1858.)
6. Aubert and Tschischwitz: *Experiments on the Arrest of Action of the Diaphragm, &c.* (See sub. v.)
7. Ludwig and Spiess: *Comparison of the Warmth of the Saliva of the Submaxillary Gland and Blood of the Carotid.* (Sitzungsbericht der Wiener Akademie, Iuliheft, 1857.)
8. Martins: *On the Temperature of the Palmipedes of the North of Europe.* (Journ. de la Physiol., vol. i., p. 10, 1858.)
9. Brown-Sequard: *Note on the Temperature of some Longipennate Palmipedes.* (Journ. de la Physiol., l. c., p. 42.)

Heidenhain employed Welcker's method* in calculating the total amount of blood in several species of mammalia. After having exposed the fallacies of the other methods, he discusses the preference and also the disadvantages of Welcker's method, for which discussion we must refer to the essay itself. According to the author's experiments, the quantity of blood bears to weight of body in rabbits, as the mean of six cases, the proportion of 1 : 18·13, the minimum being 1 : 20·78, the maximum 1 : 14·92; in dogs, as the mean of five cases, the proportion of 1 : 13·53, the minimum being 1 : 15·09, the maximum 1 : 12·33; in a fat guinea-pig, weighing 676·5 grm., the proportion was 1 : 25·01. If we compare these figures with those given by Valentin—viz., 1 : 4·50 for dogs and 1 : 6·20 for rabbits, the difference is certainly very striking; they are, however, much more in accordance with those obtained by Welcker himself†—viz., 1 : 12·5 for mice; 1 : 11·8 for a young sparrow; and with that calculated by Bischoff for an adult man in health—viz., 1 : 13.‡ It may be further observed that the variations in the same species, with regard to the amount of blood, are, according to Welcker's result, much greater than according to Valentin's, who found as the minimum for dogs 1 : 4·93, as the maximum 1 : 4·31.

Brown-Séquard purposes to demonstrate, in a series of communications, the physiological properties of the red and of the dark blood, and especially to show that the properties and functions of these two kinds of blood depend principally on the oxygen and carbonic acid which they contain. We can scarcely give more than the author's inferences; for the arguments and experiments we must refer to the original. 1. The blood, he contends, possesses two distinct physiological properties—viz., that of nutrition, or of the production of the vital properties of the tissues; and that of stimulation of the organs and tissues endowed with vital properties. Thus, the author maintains that arterial blood owes to its oxygen the power of producing the vital properties of the nervous and contractile organs and tissues, to its carbonic acid the power of stimulating certain organs, as the heart, and perhaps also the medulla oblongata. Venous blood, on the contrary, does not possess the faculty of nourishing the brain, but is more able than the arterial blood to stimulate the heart and the medulla oblongata, and still more so the pneumogastric nerves, the spinal marrow, the uterus, &c. 2. The essential difference between the arterial and venous blood, with respect to their physiological properties, is caused only by the different quantities of oxygen and carbonic acid contained in the two kinds of blood. Experiments made with venous blood charged with oxygen, and with arterial blood charged with carbonic acid, on muscles, as also on the brain and spinal marrow, have led the author to this

* Conf. this Journal, No. xxxvii., p. 244, 1857; and also Prag. Vierteljahrsschrift, vol. xliv., pp. 11–81, 1854.

† Prag. Vierteljahrsschrift, loc. cit., p. 79.

‡ Conf. this Journal, No. xxxvii., p. 244, 1857.

inference. 3. All the contractile tissues can, after having completely lost their vital properties, recover them under the influence of oxygenized blood injected into their arteries. 4 and 5. The brain and all the nervous tissues can, after having completely lost their vital properties, regain them under the influence of blood charged with oxygen; 6. The longer muscular organs retain their vital properties after death—i.e., after the cessation of the circulation—the longer they remain able to recover them again, under the influence of oxygenized blood, after they had completely lost them. In general, it may be said, this space of time is longest when the temperature of the contractile organs and of the surrounding air is low, and longer in adult than in new-born animals. 7. The longer the nerves and nervous centres preserve their vital properties after the cessation of respiration or circulation, the longer they remain able to regain them under the influence of oxygenized blood; 8. In limbs separated from the body the muscular irritability may be under the influence of oxygenized blood, augmented to such a degree that irritation produces more effect in them than it had done in their normal condition; 9. Muscular irritability appears to be able to be maintained for an indefinite time in such limbs if one injects blood charged with oxygen.

Bernard has discovered—first, on the blood of the veins of the kidneys; later, on that of the veins of other glands—that this is sometimes as red as arterial blood, at other times as dark as the darkest venous blood. Further observations and experiments have convinced this author that the blood passing from the glands is red while the organs are performing their function; that it is dark while they are in a state of rest. It is evident from these facts, that the signification *red* and *dark blood* ought not to be considered identical with *arterial* and *venous blood*. Three interesting experiments are annexed to this communication. The first shows that the venous blood from the submaxillary gland becomes red, flows in increased quantity, and in distinctly jerking motion, when the nerve supplying the gland is galvanized. At the same time it was observed that the red venous blood lost, by standing, sooner its redness than the arterial taken from an adjacent artery of the same animal. From the third experiment we see, that tying the sympathetic branch going to the submaxillary gland causes the blood to become red and flow in large quantity, while no saliva is excreted; that galvanizing the sympathetic branch renders the blood dark, diminishes the quantity, and makes it even cease to flow. Through the same experiment we further learn that the galvanizing of the filament derived from the lingualis effects an abundant secretion of saliva, together with an increased flow of red blood.

Brown-Séquard confirms Magendie's observation, that the blood-globules of birds, when injected into veins of mammalia, disappear from the blood of these animals within one hour's time. He promises further information regarding the changes which the oval blood-globules undergo before disappearing. The author contradicts, however, Magendie's assertion, that the blood-globules of mammalia disappear almost immediately from the blood of birds, as he found them in large quantity on the day after transfusion, in much reduced proportion after a few days, and some isolated round globules even in the third week. It will be remembered that Marfels and Moleschott have traced the globules of sheep's blood in the blood of frogs several weeks after the injection.*

With the view to test Frerich's theory of uræmic intoxication, Hammond performed two series of experiments on dogs. In the first series he injected into the veins of sound animals sixty grains of each of the following substances —urea, urea and vesical mucus, carbonate of ammonia, nitrate of potash and

* Moleschott's Untersuchungen zu Naturlehre, vol. i. pp. 52–60. 1856.

sulphate of potash; in the second series he injected the same substances, after having previously extirpated the kidneys. From these experiments the author concludes:—That urea (simple or combined with vesical mucus), carbonate of ammonia, and sulphate of potash, when injected into the bloodvessels of sound animals, do not cause death. 2. That nitrate of potash, when thus introduced, is speedily fatal. 3. That death ensues from the injection of any of the above-named substances into the circulation of animals whose kidneys have been previously extirpated. 4. That in neither case does urea, when introduced directly into the circulation, undergo conversion into carbonate of ammonia.

Ludwig and A. Spiess found that the temperature of the saliva excreted under the influence of electric irritation of the nerve is about 1° C. (i.e., 1·8° F.) higher than that of the blood in the carotid close to the salivary gland. The experiments were made by means of a thermo-multiplicator, for the description of which we refer to the original. The result just mentioned proves, at all events, that the act of secretion is one of the sources of the warmth for the organism.

Martins availed himself of a journey to the Faroe Islands and to Spitzbergen to examine the temperature of various families of the order Palmipedes (Uria, Porcellaria, Larus, Anas, &c.). In looking over the table giving the average temperature of the various families and species of palmipedes, living in the north of Europe, we are struck by the low temperature of the *Procellaria glacialis*, being only 38·76° Cent. (i.e., about 101·77° F.), especially when we compare it with that of *Anser cygnoides* 42·84° (i.e., about 109·11° F.). This great difference of about 7·34° F. in animals nearly related must astonish still more, if we think of the energetic swimming and flying powers of the procellaria glacialis.

Brown-Séquard confirms the fact, that in general the temperature of sea birds, and particularly that of the genus *procellaria*, is lower than that of other birds. This physiologist offers the hypothesis, that the long fasting to which sea birds are frequently exposed, may in part explain this phenomenon.

IV. Secretion; Excretion; Metamorphosis of Matter.

1. Funcke: *Contribution to the Knowledge of Cutaneous Transpiration.* (Moleschott's Beiträge, vol. iv., p. 56, 1858.)
2. Mosler: *Researches on the Passage of Substances from the Blood into the Bile.* (Virchow's Archiv, vol. xiii., p. 29, 1858.)
3. Figuier: *On the Formation of Sugar after Death in the Liver of Animals.* (L'Union Méd., vol. xi., No. 79, 1857.)
4. Becquerel: *On the Non-existence of Albumen in Normal Urine, and the Insufficiency of Chloroform as a Reagent on Albumen.* (L'Union Méd., vol. xi., No. 144, 1857.)
5. Gigon: *On the Inferiority of Phosphoric Acid as a reagent on Albumen, and on the Reality of the Normal Albuminuria.* (L'Union. Méd., vol. xii., Nos. 12 and 13, 1858.)
6. Kuhne and Hallwachs: *On the Formation of Hippuric Acid after the Ingestion of Benzoic Acid.* (Virchow's Archiv, vol. xii., p. 386, 1857.)
7. Kuhne: *On the Metamorphosis of Succinic Acid.* (Virchow's Archiv, vol. xii., p. 396.)

Funcke communicates valuable researches on the physiology of cutaneous transpiration. The method of obtaining the excretion was that used by

Schotten, viz., to wrap up the arm in a wide sleeve of India-rubber, and cause an increased excretion by various degrees of muscular action under varying external influences. The quantity of the excretion was found to be very variable, as well in the same person under various external influences, as also in different persons under the same external influences. The author calculated the quantity of sudoriparous excretion within an hour to vary between 53·040 and 815·337 grms., that of solids between 0·923 and 6·967 grms. The proportion of solids dissolved in the fluid (i.e., exclusive of epithelium), varied from 0·696 to 2·559 per cent., and in general the law was confirmed, "that the relative amount of solids exhibits an inverse proportion to the amount of excretion." With regard to the solids alone, the proportion of inorganic salts increased in the same ratio as the total amount of solids decreased. The most important result of Funcke's experiments, however, is the corroboration of the normal existence of urea in considerable proportion in the healthy cutaneous transpiration, a fact which had been denied by Schottin, who, it will be remembered, found it in so large a quantity on the skin of cholera patients.

Mosler performed some experiments with the view to determine which of the substances introduced into the blood may be traced in the bile. After having produced biliary fistulas in dogs, the author injected various substances into the blood. 1. By injection of lukewarm water into the crural vein, he proved that albumen does not pass so rapidly into the bile as into the urine; that, however, dilution of the blood causes an analogous alteration in the secretion of the bile to that described by Kierulf with regard to the secretion of the urine.* 2. Injection of grape-sugar showed that the quantity of grape-sugar in the blood, sufficient to effect its passage into the bile, must be still greater than that causing its admixture to the urine. Cane-sugar was found to pass more easily into the bile than grape-sugar, an injection of 40 grms. of the former being sufficient, of 70 grms. of the latter. 3. The introduction of even a moderate quantity of iodide of potassium into the blood leads to the entrance of this substance into the bile, from which however it soon disappears again. 4. Nitrate of potash introduced, as also the former, through the mouth, is soon discovered in the urine but not in the bile. 5. Sulphate of copper, given by means of a pill, was in one instance distinctly traced in the bile. 6. Calomel did not enter so readily into the bile, nor did it effect such an increase in the quantity secreted as is generally supposed. 7. Sulphate of quina was never found in the bile. 8. The same was the case with benzoic acid. 9. After the administration of oil of turpentine, the bile assumed a peculiar resinous smell, which, however, was not identical with the violet odour of the urine.

Figuier communicates experiments, the result of which, according to the author's opinion, disproves the formation of sugar after death in the liver of animals. He explains the opposite results obtained by Bérard and others, by the supposition that the livers had not been sufficiently washed out. The livers of three horses, after having been washed out during two hours and a half, did not yield a trace of sugar after the lapse of twenty hours. In three other examinations, the livers, after a stream of water having been passed through during an hour and a-half, exhibited still a notable amount of glucose, which was not found augmented after these organs had been left for twenty-four hours. Figuier promises to prove that Bernard's glycogenic substance is nothing but albuminose.

In contradiction to Gigon's assertion, that normal urine contains albumen, and that chloroform is the best test for it,† A. Becquerel states:—1. That

* Conf. this Journal, No. xxv., p. 265, 1854.
† L'Union Médicale, Oct. 1857.

not only albumen, but also other organic substances, and principally mucus, form an emulsion with chloroform, and that it is the presence of mucus, and not of albumen, in normal urines, which causes the production of an emulsiform precipitate by the addition of chloroform. 2. That normal urines contain no trace of albumen. 3. That chloroform is a very untrustworthy test for albumen.

Gigon, on the other side, repeats his former assertion, that chloroform manifests under all circumstances and without exception the presence of the smallest traces of albumen, that it precipitates it even when only one part is contained in 40,000 of fluid, while in a much less diluted solution no precipitation takes place by boiling, addition of nitric acid, pyrophosphoric acid, creosote, tannic acid, or any other reagent hitherto used for the purpose. With regard to pyrophosphoric acid, especially recommended by Becquerel as the most delicate reagent, Gigon remarks, that it does not show the albumen when the solution is more diluted than one to two thousand. The author gives then the result of the examination of two specimens of normal urine, of which the one yielded for twenty-four hours the quantity of 2·6 grms., the other 1·71 grm.

Kühne and Hallwachs endeavoured to find in which part of the system the transformation of benzoic acid into hippuric acid takes place. Injection of a solution of benzoic acid into the veins of dogs was not followed by excretion of hippuric acid through the kidneys; prevention of the entrance of bile into the digestive tube, by means of biliary fistula, did not prevent the transformation of benzoic acid when it was injected into the stomach; prevention of the formation of bile in the liver, through a ligature round its vessels, caused prevention of the transformation of benzoic acid; injection into the veins of dogs of a solution of benzoic acid mixed with the constituents of the bile, was followed by excretion of hippuric acid with the urine. The authors infer from these results, that the transformation does not take place in the alimentary canal, nor in the whole of the vascular system, but in the vessels of the liver; therefore in blood mixed with the constituents of bile, and especially at the expense of the glycocholic acid, the glycin of which unites with benzoic acid to form hippuric acid by giving off two equivalents of water.

Kühne confirms, by experiments made on himself, the observation of Buchheim* and Piotrowsky, that the ingestion of succinic acid is followed by the excretion of hippuric acid in considerable amount through the kidneys.

V. Nervous System.

1. Schiff: *On the Function of the Posterior Fasciculi of the Spinal Marrow.* (Moleschott's Beiträge, vol. iv., p. 84, 1858.)
2. Brown-Séquard: *New Researches on the Physiology of the Spinal Marrow.* (Brown-Séquard's Journal de la Physiologie, vol. i., p. 139, 1858.)
3. Brown-Séquard: *Influence of either Lateral Half of the Spinal Marrow on the corresponding Half of the Brain and Face.* (Brown-Séquard's Journal, l. c., p. 241.)
4. Brown-Séquard: *Causes of Death after Extirpation of the Vital Point.* (Brown-Séquard's Journal, l. c., p. 217.)
5. Aubert and Tschischwitz: *On the Arrest of the Diaphragm through the Pneumogastric Nerves in the Contraction and Relaxation.* (Moleschott's Beiträge, vol. iii., p. 272, 1857.)

* Archiv für Physiol. Heilkunde, p. 142, Jahrgang 1857.

M. Schiff records some important points on which he is at variance with Brown-Séquard. 1. While the latter denies that the white posterior columns convey sensitive impressions to the brain, Schiff is led by his experiments to the inference that the white posterior columns, and these alone, conduct the impressions of touch; while the perception of common sensation—i.e., pain through stronger mechanical, chemical, or thermal agencies, cannot be produced by the posterior columns alone. 2. The grey substance conducts the impressions of common sensation—pain caused by strong pressure, burning, vulneration, &c. The perception of mere touch is not conveyed by the grey substance. 3. After the section of one half of the spinal marrow, or of both posterior columns, simple touching is not felt any more, but stronger impressions on the respective parts of the body are felt as pain. True hyperæsthesia, as contended by Brown-Séquard, does therefore, according to these inferences, not exist.

Brown-Séquard, on the other side, adheres to his former assertions. He further relates experiments performed with the view to explain cases of paraplegia from disease of the spinal marrow with persistence of sensibility. The result of these experiments tends to show that every small portion of the conducting part of a lateral half of the spinal marrow contains conducting elements for the sensitive impressions coming from all points of the opposite side of the body situated below that small portion of the spinal marrow; and that impressions from any part of one lateral half of the body are transmitted to the brain by conducting elements distributed over all portions of the conducting part of the opposite lateral half of the spinal marrow.

The same author communicates that section of either half of the spinal marrow in the lumbar region is frequently followed, after some time, by convulsive winking of the eyelids, as also by twitchings of the other muscles of the face on the same side, by increased secretion of mucus from the corresponding half of the nose, sometimes also by disturbed nutrition of the eye of that side. He infers from these observations, that through the section of the spinal marrow an impression is propagated to the corresponding hemisphere in a manner different from that in which sensitive impressions are transmitted, as these pass to the opposite side.

Brown-Séquard states, that extirpation of the small portion of grey substance on the point of the calamus scriptorius, which Flourens called "point vital," does not necessarily produce immediate death; that the latter, if it takes place, is induced by the sudden arrest of the actions of the heart, which depends on the irritation of the medulla oblongata; that after the section of the pneumogastric nerves, the extirpation of the vital point does not produce the sudden arrest of the heart's actions; that the stoppage of the respiratory movements sometimes observed, is not the effect of the removal of this small portion of grey substance, but of the irritation of the spinal marrow acting like the galvanization of the pneumogastric nerves; that respiration and circulation may continue for days after the extirpation of the portion in question; that this is, therefore, not the prime mover of the respiratory mechanism, nor the centre of the "vital force."

The numerous experiments of Aubert and Tschischwitz performed on rabbits lead the authors to the following inferences:—1. That very weak streams of electricity cause in general a predominance of the contractions of the diaphragm, accompanied by either very frequent and small, or by rare and deep inspirations. 2. That streams of moderate strength produce the arrest of the diaphragm in the state of contraction—i.e., inspiration. 3. That strong streams lead to arrest of the diaphragm in the state of relaxation—i.e., expiration;

that they cause at the same time relaxation of the heart. 4. That the whole diaphragm is influenced, even if only one pneumogastric nerve is irritated. It will be observed that these results tend to explain the opposite assertions of other experimenters—Traube, Lindner, Kölliker, Müller, and Snellen having found this arrest to take place in the state of contraction; Eckhard in the state of relaxation.

VI. Senses.

1. Graefe: *Remarks on Various Subjects.* (Gräfe's Archiv, vol. ii., Abth. ii., p. 299; and Canstatt, l. c., p. 11.)
2. Helmholtz. (Physiological Optics, 1856; and Canstatt, l. c., p. 55.)
3. Schultze: *On the Mode of Termination of the Olfactory Nerves.* (Monatssch. d. Berlin Akad., Nov. 1856; and Schmidt's Jahrb., vol. xciv., p. 169.)

Gräfe describes a case of importance for the doctrine of accommodation—namely, a complete paralysis of all six muscles of the eyeball without diminution of the faculty of accommodation. In another case Gräfe observed a spasmodic condition of the apparatus of accommodation, finding it, after an injury to the eye, constantly in the state suited for near objects, with inability to see distinctly distant objects.

Helmholtz gives, in his 'Physiological Optics,' a description of the eye which contains new information on several points—as the diameter of the curvature of the cornea, the lens, the distance of both surfaces of the lens from the vertex of the cornea, &c. We are, however, prevented from entering on these subjects.

Schultze's researches are performed on the representatives of all classes of vertebrate animals. They lead to a result which is not quite in accordance with that obtained by Ecker and Eckhard.* The author shows that the terminating fibrils of the olfactory nerve are in no anatomical connexion with the epithelial cells; that they penetrate, however, between the cells to the free surface of the mucous membrane; offering thus a striking analogy with the termination of the optic and acoustic nerves.

VII. Organs of Locomotion.

1. Deiters: *De Incremento Musculorum.* (Dissert. Inaug. Bonnæ, 1856; and Canstatt, l. c., p. 105.)
2. Auerbach: *On the Tonus Muscularis.* (Bericht. d. Schles. Gesellsch., 1856; and Canstatt, l. c., p. 118.)
3. Heidenhain: *On the Tonicity of Muscles.* (Physiolog. Studies, Berlin, 1856; and Canstatt, l. c., p. 118.)
4. Heidenhain: *Restoration of the Exhausted Irritability of Muscles by Continuous Galvanic Streams.* (l. c., and Canstatt, l. c., p. 118.)
5. Matteucci: *Researches on the Physical and Chemical Phenomena of Muscular Contraction.* (Comt. Rend., vol. xlii. and vol. xliii.; and Canstatt, l. c., p. 16.)
6. Valentin: *The Action of Contracted Muscles on the Surrounding Air.* (Archiv f. Physiol. Heilkunde, Jahrgang 1857, p. 285.)
7. Matteucci: *On the Conditions which Influence the Duration of the Muscular Contraction after Death.* (Compt. Rend., vol. xliii.; and Canstatt, l. c., p. 16.)

* Beiträge zu Anat. und Phys. Giessen, 1855.

8. Brown-Séquard: *On the Laws of Muscular Irritability, Cadaveric Rigidity, and Putrefaction.* (Compt. Rend., vol. xlv., Oct. 1857.)
9. Brown-Séquard: *Limits of the Possible Return of Cadaveric Rigidity, after having been Destroyed by Elongation of the Muscles.* (Brown-Séquard's Journal de la Phys., vol. i., p. 281, 1858.)
10. Budge: *On the Action of the Intercostal Muscles.* (Archiv f. Physiol. Heilkunde, Jahrg. 1857, p. 63.)

Deiters, who made his researches under the guidance of Budge, arrived at the conclusions that the growth of muscles takes place by increase in the thickness of the primitive fibrils; that by this circumstance alone the increase of volume may be accounted for; that the supposition of the formation of new fibrils between the existing ones is unproved.

Auerbach and Heidenhain directed their researches to the question, whether there does exist a *tonus* (tonicity) of transversely striated muscular fibres or not. Both arrived at negative results.

Auerbach measured the tension of the muscles of the calf of rabbits. Dissection of the sciatic nerve, ligature of the abdominal aorta, narcosis by means of chloroform and other agents likely to produce an alteration in the condition of the motor nerves, effected no change in the tension of the muscles. Auerbach denies, from this, the existence of a tonus depending on the active influence of the spinal marrow. The normal involuntary tension of the muscle is ascribed to the elasticity of its substance.

Heidenhain experimented on the muscles of frogs and rabbits. He attached a weight to the lower isolated extremity of the muscle, and measured the degree of extension on a scale allowing, by means of a perspective, the discerning of a change amounting to $\frac{1}{50}$th millimetre. As dissection of the motor nerve supplying the muscle under examination did not produce any increase of the extension, the author denies the existence of a tonus due to the action of the nerve.

The same author has made the observation, that a muscle tired to exhaustion may be restored again by a continuous electric stream of some intensity to such a degree that the muscle regains its irritability and contractile power. The stream must be passed through for fifteen to thirty seconds without interruption.

Matteucci has occupied himself with the investigation of the respiration of muscles. He found that the muscle in the state of contraction, compared with that in the state of rest, gives off more than twice as much carbonic acid, and absorbs more than the double quantity of oxygen. The author also witnessed exhalation of nitrogen and increase of temperature by 0·9° Fahr. Matteucci institutes a comparison between the amount of mechanical work performed by the contraction and the products of combustion produced, and finds that the former corresponds to the surplus of carbon consumed during the contraction.

In a further publication Matteucci treats of the conditions most favourable to the preservation of the muscular irritability. He finds them in the free access of air, or (if the muscle is kept in a closed space) in the presence of an alkali, to absorb the carbonic acid formed.

Valentin has continued his researches on the action of the muscle on the surrounding atmosphere;* he has constructed for this purpose a particular apparatus, for the description of which we must refer to the essay itself. 1. Frogs confined in this apparatus absorbed during the hot summer days an increased amount of oxygen, giving off at the same time only a normal amount of carbonic

* Conf. this Journal, No. xxxv., p. 229, 1856.

acid. In the cool days of autumn this peculiarity was not observed. 2. When tetanic contractions of the muscle of a frog killed by opium or strychnia were produced by mechanical irritation, the proportions of oxygen absorbed and carbonic acid given off became much increased; the volume of oxygen absorbed was less in proportion than that of carbonic acid given off, while in the state of rest the opposite is the rule. 3. The frog or frogs' muscle exhausted by the tetanic contractions manifested the same phenomena in a diminished degree. 5. The exhausted muscle recovering its irritability during a period of rest, returned by degrees to the normal proportion in the absorption of oxygen, and in the giving off of carbonic acid. The contracted and exhausted muscles therefore show a greater liability to decomposition, than the fresh or refreshed muscles in state of rest. 6. Galvanic irritation leads to changes very similar to those produced by mechanical irritation. 7. These phenomena of contraction were observed to be most intense when small periods of contraction were allowed to alternate with proportionate intervals of rest. 8. Poisoning by means of opium or strychnia causes the muscle of the frog, even when in the state of rest, to give off an abnormally large amount of carbonic acid, and to absorb more oxygen, but not quite in proportion to the carbonic acid exhaled.

Brown-Séquard calls to mind several well known observations by Fontana, Haighton, Sir Astley Cooper, T. Reid, Flourens, Bernard, and himself, which he considers as sufficient proof for the correctness of Haller's view regarding the independence of muscular irritability. A series of experiments, in which convulsions confined to one side were excited shortly before death, corroborates the theory that every muscular contraction causes a diminution of the irritability by producing a change which, if death occurs soon after it, hastens the appearance of cadaveric rigidity and putrefaction; that, on the contrary, by the influence of rest and of oxygenated blood, the irritability of muscles separated from the nervous centres becomes increased, that it lasts longer after death, that such muscles do not pass so soon into the state of cadaveric rigidity and commencing putrefaction.

Brown-Séquard's examination of Nysten's assertion, that cadaveric rigidity, when once overcome by sufficient force, is not again developed, has led to the result that this assertion is correct, when the experiment is made a sufficiently long time after the cadaveric rigidity has commenced, but that the latter may be developed again when the forcible elongation of the muscles had taken place soon after the commencement of the rigidity. The author's experiments further show, that the sooner after death the rigidity commences, the sooner the power of its re-establishment after forcible overcoming is lost, which circumstance depends on the irritability of the muscle at the time of death. The limit of the reproduction varies between one hour and twelve hours.

Budge's inferences regarding the functions of the intercostal muscles differ in some points from those of other physiologists. 1. All the three species, viz., the intercostales externi, interni, and intercartilaginei, are, according to Budge, muscles of inspiration. 2. The interni act principally by raising the ribs. 3. The posterior portion of the externi, which extends from the vertebræ to the angulus, and is not covered by the interni, produces a greater vaulting of the ribs. 4. The anterior portion of the musculi externi has two functions, first, that of effecting, like the posterior portion, an increased vaulting; secondly, that of assisting the intercostales interni in raising the ribs. 5. The musculi intercartilaginei raise the cartilages of the ribs.

VIII. Supra-renal Capsules.

1. Brown-Séquard: *New Experimental Researches on the Physiology of the Supra-renal Capsules.* (Compt. Rend. de l'Acad. de Paris, tome xliii.; and Canstatt, l.c., p. 99.)
2. Gratiolet: *Note on the Effects following the Extirpation of the Supra-renal Capsules.* (Compt. Rend., tome xliii.; and Canstatt, l. c., p. 100.)
3. Philippeaux: *Note on the Effects of the Extirpation of the Supra-renal Capsules on Albino Rats.* (Compt. Rend., tome xliii.; and Canstatt, p. 100.)
4. Vulpian: *Note on some Reactions peculiar to the Substance of the Supra-renal Capsules.* (Compt. Rend., Sept. 1856; and Canstatt, l. c., p. 176.)
5. Virchow: *The Chemistry of the Supra-renal Capsules.* (Virchow's Archiv, vol. xii., p. 481, 1857.)

In a former number (No. xxxvii., p. 254) we have chronicled the result of Brown-Séquard's first series of experiments on the supra-renal capsules. The result of a second series induces the author to adhere to his former inferences; he especially endeavours to prove that the phenomena ascribed by him to the extirpation of the supra-renal capsules, are not due to the injury done to the neighbouring tissues.

Gratiolet, on the other side, states that he repeatedly extirpated the left supra-renal capsule in guinea-pigs, without giving rise to any serious symptoms, while the additional removal of the right capsule was always followed by death on the third day, and the simultaneous extirpation of both capsules by death within forty-eight hours. Inflammation of the liver and peritoneum was met with in all the animals that had died after the operation, and it is to these causes that Gratiolet ascribes the fatal termination.

Philippeaux operated on white rats, some of which survived the extirpation of both capsules, when the second capsule was not removed until the wound from the first operation was healed. As the author did not observe any morbid symptoms in these rats, he infers that death, when it follows the extirpation, is caused by the lesion of other organs.

Vulpian describes some peculiar reactions which he obtained when examining the juice expressed from the medullary substance of the supra-renal capsules. The fluid is neutral, or very slightly acid. The salts of oxide of iron and the chloride of iron produce a greyish coloration, which may assume sometimes a blackish, sometimes a greenish or bluish, hue. A watery solution of iodine causes the appearance of a distinctly carmine colour. Potash, soda, ammonia, and baryta, as also chlorine and bromine, have a similar effect, when added in a very small quantity, while the addition of a larger proportion makes the colour disappear. The influence of the light of the sun seems to favour, and the process of boiling not to prevent, these reactions. The substance effecting them is soluble in water, alcohol, and ether. Vulpian obtained these results from the capsules of all the mammalia examined, as also from those of the duck and gulls, while the examination of other organs never yielded similar phenomena.

Virchow corroborates Vulpian's observations. He states that the capsules of the horse and of other large animals are especially suitable for the manifestation of these reactions, as the medullary and cortical substance can be easily separated from each other. The reactions above mentioned are, according to this author, not dependent on the morphological elements, but on the intercellular fluid. The reaction of the watery solution of iodine appears to him

the most characteristic. As new, we may mention Virchow's discovery of a large quantity of leucin in the medullary substance of the capsules. He found also a large proportion of fat and of myelin. From the present knowledge of the chemical constitution of the capsules, Virchow is inclined to consider them more as glandular than as nervous organs, although he has found sympathetic ganglia within their tissue.

IX. Generation and Sexual Phenomena.

1. Siebold, C. Th. v.: *True Parthenogenesis in Butterflies and Bees.* (Leipzig, 1856; and Canstatt, l. c., p. 146.)
2. Coste: *General and Special History of the Development of Organized Bodies.* (Comp. Rend. de l'Acad., tome xliii., Août, 1856; and Canstatt, l. c., p. 147.)
3. Fick: *On the Vas Deferens.* (Müller's Archiv, 1856, p. 473; and Canstatt, l. c., p. 146.)
4. Oldham: *History of Two Cases of Hernia of the Ovaries.* (Med. Times and Gazette, Nov. 7th, 1857; from the Proceedings of the Royal Society.)
5. Savory: *Effect upon the Mother of Poisoning the Fœtus.* (London, 1858.)

Siebold's work contains a review of the older publications on *parthenogenesis*, and describes this curious phenomenon as it offers itself in the bee and in the silkworm. The queen-bee—i.e., a female bee with perfectly developed sexual organs—is, according to Dzierzon, fecundated by a drone. The semen is retained in the receptaculum seminis of the queen. The eggs of the queen which are to be developed into working-bees—i. e., female bees with imperfectly developed sexual organs, or into a queen-bee, require impregnation, while those which are to give rise to drones are not to be impregnated. The queen, while placing the eggs into the cells, feels with the lower part of the abdomen whether she has to fill a wider or a narrower cell, and accordingly induces impregnation by causing the coming forth of a particle of semen from the receptaculum. Siebold corroborates this statement, and has, in addition, ascertained the entrance of spermatoza, through the mikropyle, into the interior of the ovum. In the silkworm (*bombyx mori*), as well male as female individuals may be developed out of unimpregnated eggs; while in the solenobia the unimpregnated eggs can produce only female individuals.

Coste endeavoured to learn, by means of experiments, in which part of the genital apparatus the impregnation of the eggs takes place. When he had hens that laid their eggs regularly every other day at a certain hour, say at noon, he knew that eighteen hours after the laying of the egg—i. e., at about six A.M. next morning—a new egg became detached from the ovary. Coste then arranged that the fecundation could not take place until two or three hours after this detachment of the egg. The result in such a case was, that the first egg which had met with the seminal fluid in the tube was always sterile, while the five or six following eggs were impregnated. From this experience it would follow that the impregnation of the hen's eggs takes place either on the ovarium or at the very entrance of the tube. Similar observations were occasionally made on mammalia, although with them such experiments are not easily practised, as the females generally refuse coition as soon as the egg has left the ovary. From the successful experiments, however, Coste considers himself entitled to this inference, that in mammalia, too, the impregnation takes place on the ovary, or at the commencement of the tube.

Fick mentions that the *vas deferens* of the dog does not possess the same qualities as that of the rabbit. While in the latter vermicular movements

are easily excited, these do not appear in the vas deferens of the dog by the direct application of a stimulus, as electricity. The author inclines to the view that the contraction of the vas deferens of the dog, and probably also of man, is not analogous to that of striated or smooth muscles, but to that of the arteries.

Although Oldham's communication belongs to another department, yet one of his cases is of great physiological interest, as it furnishes decided evidence of an *ovarian menstrual act*. The subject of this observation is a woman, now about twenty-five years of age, in whom frequent careful physical examination failed to detect any trace of a vagina or uterus, and the conclusion arrived at is, that these central pelvic organs have not been developed. The ovaries are situated close to the external abdominal ring on either side. When in their usual state, their size is that of a walnut, but at periods they become considerably enlarged. "For the first three years" (that is, from the nineteenth to the twenty-second year), the author says, "the right ovarium was exclusively enlarged, and the intervals were not so regularly marked, varying between three and six weeks, excepting for the first year, when they were much longer, occasionally extending to three months. For the last two years the left ovarium has been far more frequently affected, the right one remaining quiescent; occasionally both are painful and tumid, but even then one more than the other. The intervals are now pretty regularly three weeks."—"The accession of a menstrual period is sometimes suddenly felt. She will go to bed well, and in the morning the ovary will be swollen. More commonly, however, it is very gradual, augmenting in volume for four days, then remaining stationary for three, then gradually declining; the whole process before the ovary is reduced, generally lasting ten or twelve days. On separating the ovary, when at the height of its swelling, from the tissues surrounding it, it appears scarcely, if at all, less than double its usual volume; its outline is clearly defined, and it is plain that the whole, and not merely a part of the organ, is involved."

Although pathological experience had made it probable* that morbid conditions of the fœtus may be communicated to the mother, yet the principal physiologists appeared to consider until very lately that the connexions between mother and fœtus may lead to transmission of morbid tendencies from the former to the latter, but not *vice versâ*. Savory's experiments, performed by means of a solution of acetate of strychnia injected with all possible care into the abdominal cavity of the fœtus (of dogs, rabbits, and cats), while still in connexion with the mother, through the cord, prove distinctly that the mother may be poisoned through the fœtus in utero; and the result of these experiments is therefore also favourable to the view that "the fœtus in utero may inoculate the maternal with the peculiarities of the paternal organism."†

* A. Harvey, in the Monthly Journal of Medical Science for October, 1849, and September, 1850; and T. Hutchinson, in the Medical Times and Gazette for December, 1856, and January, 1857.

† Harvey, loc. cit.

HALF-YEARLY REPORT ON MATERIA MEDICA AND THERAPEUTICS.

By Robert Hunter Semple, M.D.
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I. *Experiments with Atropia in Epilepsy.* By Dr. Max Maresch. (Zeitschrift der k. k. Gesellsschaft. der Aertze zu Wien, Feb. 1858.)

The great number of epileptic cases introduced, in complication with insanity, into lunatic asylums, has led to numerous experiments with various remedies, but hitherto without very favourable results. Dr. Maresch, who is the physician to the Imperial Lunatic Asylum in Vienna, has employed atropia in epilepsy in the case of some of the lunatic patients in that establishment. The preparation employed was pure atropia from Merk's laboratory in Darmstadt, dissolved in the proportion of 1 grain of atropia to 500 drops of spirit, and the dose was 5, 10, and 12 drops every day in the morning or evening, and continued for a month. The cases treated were very severe, and complicated with maniacal and suicidal insanity, and the results therefore were not uniformly favourable. In fact, the writer states that out of eight cases which were treated by atropia, there were only three in which the epileptic convulsions disappeared. But he remarks, that in the asylum to which he is attached, only those cases of epilepsy are received which are complicated with mental derangement, and that he has been unable to extend his experiments to cases of pure epilepsy of recent origin.

We understand that Dr. Sieveking has recently tried the effects of a solution of sulphate of atropia upon an epileptic patient as well as upon himself. In his own case, a hundredth part of a grain produced brief vertigo, followed by dryness of the throat of several hours' duration; vision not being affected. On the following day he suffered from nervous depression, which was the main symptom very urgently complained of by the epileptic patient, who took one hundreth of a grain on three successive days.

II. *Observations on the Beneficial Effects of Pepsine in the obstinate Vomiting of Pregnancy.* By Dr. L. Gros. (Bulletin Général de Thérapeutique, Feb. 15, 1858.)

In the great majority of cases the vomiting of pregnancy may safely be left to the influence of time; but there are some cases in which females are scarcely able to retain in their digestive system a sufficient amount of nourishment to support their existence, and are therefore reduced to the last degree of emaciation. In some, also, the shocks occasioned by this obstinate and repeated vomiting become the source of abortions, which might have been prevented by moderating the activity of the morbid phenomenon. A very remarkable case was related in 1856, by M. Teissier, Professor of Clinical Medicine at Lyons, showing the immediately beneficial effect of a dose of pepsine in a case of vomiting during pregnancy. In this case the symptoms resisted all the ordinary methods which were employed, and the patient was unable to retain in her stomach any substance whatever. Under these circumstances, the patient was brought to M. Teissier, who found her in the following condition: The vomiting had continued for two months, and she was at the end of the fourth month of her pregnancy; she presented the appearance of a skeleton, having the aspect and the cough of a phthisical subject; the pulse was 140, and M. Teissier thought at first that the case was one of pulmonary tubercle. Finding that all treatment had been hitherto inefficacious, and that the lady was

actually dying of inanition, he was seriously meditating upon the propriety of inducing abortion as a means of saving her life; but as a last resource before operating, he determined to employ pepsine. He accordingly prescribed one gramme, to be divided into two doses, and taken every day in a spoonful of broth. At the very first dose the broth was retained, and from that moment the vomiting never returned. On the third day the lady ate some chicken, and then some beef-steak. The treatment was continued in the same manner for three weeks, and at the end of that time the cure was complete; the emaciation was replaced by *embonpoint*, the fever and the cough ceased with the vomiting, and at the end of the ninth month the lady was safely delivered.

Dr. Gros then relates six other cases in which the pepsine was employed with the same success, and he thinks himself warranted in concluding that pepsine undoubtedly produces good effects in the vomiting which attends pregnancy. He explains the results by supposing that, although in the first instance the vomiting is due only to the sympathy existing between the uterus and the stomach, yet subsequently the stomach itself becomes affected, as is proved by the fact that in the beginning of pregnancy the vomiting occurs only in the morning or the evening; but in aggravated cases it supervenes after every meal, and all alimentary matters are rejected. In such cases, therefore, when the stomach has taken on a morbid habit, and exhibits an alteration of secretion, the pepsine appears to be really indicated; although in a merely sympathetic action between the uterus and stomach it would be difficult to explain the efficacy of its action.

III. *On the Local Application of Belladonna with Mercurial Ointment in the Treatment of Croup.* By Dr. SHELTON, of New York. (American Journal of the Medical Sciences, April, 1858.)

Three years ago the idea occurred to Dr. Shelton that the local application of a combination of belladonna with mercurial ointment might be serviceable, in addition to the ordinary treatment adopted in croup, and he accordingly employed an ointment composed of two drachms of extract of belladonna and six of mild mercurial ointment, rubbing it freely every two hours into the sound skin over the trachea. This medication was so far successful that he recommended a trial to his professional friends, one of whom, in treating a case of croup, applied the above-mentioned ointment, though not to the sound skin, as Dr. Shelton had recommended, but to a blistered surface. It turned out that this deviation from the original plan was a great improvement, for the child on whom the ointment was applied, and in whom the symptoms were of the most aggravated character, was wonderfully relieved in a very short time, and rapidly recovered. Dr. Shelton now took advantage of the hint thus given to him, and in his subsequent cases he first blistered the throat, and then applied the ointment to the blistered surface. In the first case thus treated the patient became delirious, with dilated pupils, and the other symptoms denoting poisoning by belladonna; but these effects gradually passed off, and the recovery was soon completed. In another case, which was that of a child two years old, the same treatment was adopted, and with similar results, except that the poisonous effects of belladonna were not nearly so well marked. Dr. Shelton observes, that the district in which he practices is a marshy and malarious one, peculiarly favourable to the production of intermittent diseases and throat affections, and that he has therefore seen a great number of fatal cases of croup, for which he intended on several occasions to employ tracheotomy, but he now proposes the treatment by belladonna and mercurial ointment as a substitute for that formidable operation. He also suggests the internal employment of

belladonna as an adjunct to calomel and ipecacuanha and other ordinary remedies in the treatment of croup, but he has not yet tried it himself in this manner.

IV. *On Glycerine as a Local Application in Pseudo-Membranous Croup.* By Dr. MAYER. (American Journal of the Medical Sciences, April, 1858.)

Dr. Mayer was induced to employ glycerine in pseudo-membranous croup, from observing the relief obtained by its being snuffed up or injected into the nostrils in some cases of ozæna, in which hard concretions form in the nasal fossæ. Glycerine is remarkable for its extraordinary power of adhesion, extension, and penetration, especially when applied to a mucous membrane, and therefore it is not necessary to apply it to the laryngeal surface by direct application, for its mere apposition in any quantity to the rima glottidis, or the parts nearly adjacent, is followed by immediate entrance into the cavity, without provoking to any extent the spasm and violent convulsive cough which are always caused by the forcible introduction of a probe or sponge into the orifice. In pseudo-membranous croup the larynx is lined by an excretion somewhat analogous to that thrown out in ozæna, and it seems probable that the introduction of the glycerine into the laryngeal cavity may loosen the membrane and facilitate its expulsion by coughing or vomiting. Dr. Mayer has employed the glycerine in severe cases in conjunction with other treatment, and indeed he does not propose this remedy as a substitute for, but as an addition to, other means; and he has found the application to be followed by a manifest improvement of the cough, and relief of the dyspnœa and general distress. The article employed was Price's Candle Company's glycerine, and it was applied by pressing down and drawing forward the tongue with the finger, and squeezing out the contents of a long and thick camel's-hair brush dipped in the liquid over the chink of the glottis, or as near it as possible, concluding the operation by swabbing the whole throat. Dr. Mayer thinks that this mode of treatment is applicable to several other diseased states of the respiratory passages.

V. *On the Treatment of Obstinate Intermittent Fevers by Cold Water, and by the adoption of the most Simple and Ready Means.* By Dr. DAUVERGNE, Physician of the Hospital of Manosque. (Bulletin Général de Thérapeutique, Feb. 28, 1858.)

Dr. Dauvergne, remarking that some writers believed in the possibility of curing obstinate intermittent fevers by sea-baths—namely, by the specific chemical powers of this kind of water—advances his own opinion, that the action was due only to the impression of cold produced, and to the direction given to the organic movements. Twenty-seven cases have been collected by Dr. Dauvergne in proof of the efficacy of water in the cure of these fevers; 26 of them attacked with intermittent fevers of all kinds, but especially obstinate and chronic, were cured with surprising certainty and rapidity. The only case which failed was that of a soldier, who became puffy and œdematous after a douche; and as the weather was cold, the sulphate of quinine was given, and effected a cure. The cases of treatment by water are recorded by Dr. Dauvergne, who seems to have adopted a plan somewhat similar to those pursued in the hydropathic establishments. The strength of the patients was supported by generous diet.

[The bloated and œdematous condition of one of the patients thus treated, and his subsequent cure by sulphate of quinine, seem to prove that the water-treatment cannot be adopted without great risk; and although some of the

other cases may have been improved, in a hot region like Africa, by the application of water to the surface, there is no proof whatever that the same treatment can be safely adopted in other countries, or that it can supersede quinine in the management of intermittent fevers. The exhibition of sulphate of quinine is attended with no danger, and this cannot be said of the water-treatment.—Rep.]

VI. *On Quinium, a New Preparation of Cinchona.* (Bulletin Général de Thérapeutique, Nov. 30, 1858.)

The high price and the scarcity of the South American cinchonas, and the consequent difficulty of obtaining sufficient supplies of the sulphate of quina, have induced physicians to recommend again the use of the powdered bark of all kinds of cinchona, which contains both quinine and cinchonine; but the powder is always uncertain in its operation, from the variable proportion of the alkaloids which it contains. To obviate this inconvenience, MM. Delondre and A. Lebarraque have composed an extract which faithfully represents all the useful principles of cinchona. On the recommendation of the Academy of Medicine, the French Minister of Agriculture and Commerce has approved the formula for the composition of this extract, which is called *quinium*, and is prepared in the following manner. Those barks are employed the composition of which is known, and they are mixed together in such proportions that the quinine and the cinchonine exist in the relation of two of the former to one of the latter. The barks are then bruised, and the powder is mixed with half its weight of lime slacked with water; the mixture is then treated with boiling alcohol till all the alkaloids are taken up; the greater part of the alcohol is recovered by distillation, and the evaporation is completed. The residue is the alcoholic extract of cinchona free from lime. Four grammes and fifty centigrammes of this extract should give, by the known processes, one gramme of the sulphate of quinine and fifty centigrammes of sulphate of cinchonine. The quinium may be used in pills or in wine. M. Bouchardat, in drawing a comparison between the utility of sulphate of quinine and that of quinium, considers that the sulphate of quinine is still unrivalled in its power of cutting short the paroxysms of a recent intermittent fever, but that in the treatment of a chronic fever, the quinium is possessed of superior powers. In the fever countries, therefore, in the midst of the endemic causes which give rise to intermittents, the advantages of quinium are remarkably manifest.

VII. *Practical Remarks upon the Use of Uva Ursi, considered specially as an Obstetric Agent.* By Dr. de Beauvais. (Bulletin Général de Thérapeutique, Jan. 30, 1858.)

The uva ursi certainly possesses the power of causing contraction of the fibres of the bladder as well as of those of the uterus, and Dr. de Beauvais has seen a case in which a single dose produced a most painful vesical tenesmus; and he saw another case of the same kind in a man labouring under piles, complicated with blennorrhœa and varices of the neck of the bladder. A case also occurred of paralysis of the bladder, following hysteria, in which the use of uva ursi succeeded in restoring the contractility of the bladder. From this excito-motor property possessed by the uva ursi, it has been thought that it might be used effectually in certain cases of atony of the uterus, in which the ergot of rye is commonly employed. Its properties are not so dangerous as those of strychnia or of ergot of rye, and moreover it is not disagreeable in taste; in fact, it smells and tastes something like tea. The mode of administration, in simple labours which are retarded by weakness of

the pains, atony of the uterus, and nervous exhaustion, consists in giving every hour a gramme of the leaves of uva ursi, infused in a teacupful of boiling water. It may be allowed to cool, and may be sweetened. But if it be desirable to produce a rapid effect a decoction should be preferred, and the administration should take place at short intervals. Dr. de Beauvais thinks that uva ursi may be placed, on the one hand, in the rank of the astringent tonics, by reason of the gallic acid and tannin which it contains; but that, on the other hand, it deserves to be placed by the side of the ergot of rye, as a proper excitant of the muscular system of organic life.

VIII. *On the Employment of Bromide of Potassium in the Treatment of Spermatorrhœa.* (Bulletin Général de Thérapeutique, Jan. 15, 1858.)

The sedative effects of the bromide of potassium upon the generative organs of man have led some practitioners to test its efficacy in the morbid erections accompanying blennorrhagia and in spermatorrhœa. M. Binet, an interne of the French hospitals, has recorded three cases of spermatorrhœa which appear to have been considerably relieved by the administration of the bromide. The editor of the 'Bulletin de Thérapeutique,' however, remarks that M. Binet's cases are instances only of amelioration, and that the evidence of the curative effect of the bromide in this disease is still deficient. The sedative effect of the salt on the sexual organs may indeed be one of the causes of impotence, so that its use should be avoided in chronic cases of spermatorrhœa, or in patients who are of advanced age.

IX. *Cases of Acute Rheumatism Treated by Iodide of Potassium.* (Bulletin Général de Thérapeutique, April 15, 1858.)

Three cases of acute rheumatism are recorded by M. Tillot as having been treated at the Hôpital Lariboisière, in Paris, by the iodide of potassium. The first case alone appears to have been cured entirely under the use of this remedy; the other two cases were only somewhat improved. These results, therefore, do not confirm the opinion formed of the efficacy of iodide of potassium in acute rheumatism by some practitioners, especially Dr. Weber, of Germany, and Dr. Campbell, of Dublin.

X. *On a New Method of Preparing the Phosphate of Lime of Bones.* By M. Dannecy. (Bulletin Général de Thérapeutique, March 15, 1858.)

On the ground of the necessity which exists for the administration of phosphate of lime in the molecular state which is best adapted for its incorporation into the living organs, M. Dannecy, of Bordeaux, proposes the following preparation of this salt: Beef-bones, washed and powdered, common water, and pure carbonate of potash or soda, are boiled together for an hour, when a perfectly homogeneous substance is formed; this substance is thrown upon a paper filter, and the alkaline liquid flows out; the mass is washed at several intervals with hot water; it is then dried and passed through a silk sieve, when a powder of excessive tenacity is obtained, soft to the touch, and of a mobility equal to that of lycopodium. This powder contains all the natural elements of bones, but without the gelatine which holds them together; the disaggregation attains its utmost limit, and is truly molecular. The phosphate of lime of bones thus prepared is easily kept in suspension in potions and in cod-liver oil, and it is easily moulded into different pharmaceutical forms, as pastiles, pills, &c.

XI. *On the Injection of a Solution of Chlorate of Soda into the Bronchi of Children affected with Croup.* (Bulletin Général de Thérapeutique, March 15, 1858.)

In consequence of the troublesome cough which often supervenes after the operation of tracheotomy in croup (notwithstanding the great care recommended and practised by M. Trousseau), M. Barthez has injected into the bronchi of the little patients a few drops of the chlorate of soda. He prefers the soda to the potash-salt in consequence of the greater solubility of the false membranes in a solution of the former. Some cases have been thus treated in the wards of the Hospital of Ste. Eugénie, and it is stated that their condition was improved by these injections. The instillation of water and nitrate of silver is not a new practice, having been already adopted by M. Troussean; but the question remains, whether the employment of the chlorate of soda adds to the efficacy of the operation.

XII. *On the Use of Biniodide of Mercury, in Combination with the Rays of the Sun, for the Cure of Goître.* By F. J. Mouat, M.D. (Indian Annals of Medical Science, April, 1857.)

Dr. Mouat states that in certain parts of India goître is so prevalent that one individual in ten may be said, in general terms, to be afflicted with this disorder. The application of an ointment of biniodide of mercury was found very efficacious in the treatment, the ointment being prepared by adding the finely powdered biniodide to melted lard or mutton suet. This ointment is applied, about an hour after sunrise, to the goître, by means of an ivory spatula, and is rubbed in for about ten minutes; the patient then sits with his goître held well up to the sun, as long as he can bear it. The ointment will probably produce a blistering effect, although no pustules are raised on the skin; and in the course of the day it should be gently rubbed in again, and the patient sent home with orders not to touch it with his hands, but to allow it to be gradually absorbed. This treatment is said to be quite sufficient for an ordinary cure, but a second application is sometimes necessary in very bad cases. Dr. Mouat states that in the year 1855 no less than 500 or 600 persons were sometimes treated in a single day, and it is estimated that altogether about 60,000 patients have been so treated. The cases now are far less numerous than formerly, and in fact the disorder is becoming extinguished. Dr. Mouat believes that the rays of the sun have some influence upon the cure, either by some chemical action on the ointment, or by causing its more rapid absorption, and he expresses his intention of treating one of his cases by night before a large fire, and comparing the results with those obtained by inunction under the rays of the sun.

XIII. *On the Therapeutic Applications of the Neem or Margosa Tree.* By Edward J. Waring, Esq., Madras Medical Service. (Indian Annals of Medical Science, April, 1857.)

The margosa tree is the Aradirachta Indica, a plant of the natural order of Meliaceæ, is about twenty feet high, and flowering in the hot season, and is found in all parts of the peninsula of India, and also in Ceylon, Ava, and Java. The only part hitherto analysed is the bark, in which a principle named sulphate of aradirine is said to have been discovered. All parts of the tree are bitter, but more especially the bark, the bitterness being accompanied with some astringency. The neem tree has long been held in repute by the natives of India; the bark has been regarded as an excellent tonic and anti-

periodic, the seeds and oil obtained from the pulp of the fruit are considered as anthelmintic; the bark of the root is emmenagogue; the gum, which exudes plentifully, is said to be aphrodisiac; and the leaves, in the form of poultice, are locally applied to ulcers and cutaneous diseases. Mr. Waring adduces proofs of the efficacy of the different parts of this plant in the diseases referred to, from the testimony of native and European authors, and he invites the attention of medical officers practising in India to experimental investigations of the results of its use.

XIV. *Observations on Saoria as a Remedy for Tapeworm.* By Dr. E. Strohl. (L'Union Médicale, Dec. 29, 1857.)

Dr. Strohl published in 1854 a memoir upon the principal *tæniafuges*. This work was undertaken to make known some remedies of this nature sent from Abyssinia by M. Schimper. This country abounds in tænia, and there are very few inhabitants who are not troubled with the parasite. It seems, however, that nature has placed the remedy close to the disease, and that Abyssinia possesses very powerful anthelmintics. One remedy which exercises a mild and certain operation over tapeworm is the *saoria*, or fruit of the *mæsa picta*. Dr. Strohl administered it to thirteen persons, some of whom were sickly women, and one was a child two years old; and after the expulsion of one worm, no other appeared in any of the cases. The new observations made upon this substance confirm Dr. Strohl in his previous favourable opinion of the remedy, and the results of his experience prove to him, 1st. That saoria is a more certain tæniafuge than the remedies of the same class indigenous to France, but that the constancy of its operation is not yet proved; it appears to kill the worm. 2nd. Its action is mild, seldom accompanied with disagreeable effects, and it is not difficult to swallow. 3rd. It may be administered without fear and easily, to young children, to women, and generally to persons of deteriorated constitution and with a weakened digestive canal. 4th. These different properties secure its superiority over the tæniafuges indigenous to France. 5th. It is preferable to kousso, in consequence of its milder action, although a tænicide remedy, and of its being much more common than kousso, from the low price at which it might be obtained. Its preservation, which is easier and longer, is also an advantage over this last medicine, and over the fern. 6th. Time alone can determine whether its action is radical, or only palliative. Dr. Strohl's mode of administration consists in mixing the saoria with some aromatic infusion, and the medium dose for an adult is twenty to thirty grammes. It usually acts as a purgative, and this action may be promoted by giving, in addition, some castor oil.

XV. *On a Double Salt of Nitrate of Silver and Soda, acting as a Caustic for Modifying the Condition of Tissues.* (Bulletin Général de Thérapeutique, Dec. 30, 1858.)

Most of the modifications of caustic agents have hitherto had for their object the augmentation of their caustic powers; but Dr. Brun, in the 'Gazette Médicale' of Lyons, has proposed an opposite result, and, in the formation of the double nitrate of silver and soda, he believes that he has found a substance possessing the *modifying* properties of lunar caustic without its destructive effects. The double salt is prepared by dissolving, on the one hand, in a sufficient quantity of distilled water, a portion of nitrate of silver, and, on the other hand, a portion of nitrate of soda,* in the same quantity of water.

* A similar combination of nitrate of silver with nitrate of potash was suggested by Mr. Spencer Wells some years ago, and made by Mr. Bastick.—Ed.

The two solutions are then mixed and evaporated to dryness; and the solid mass is afterwards fused and poured into moulds. A cylinder is thus obtained, representing a double salt of nitrate of silver and soda; it presents a crystalline and radiated fracture, and is very soluble in water and boiling alcohol. This double salt has been used for touching aphthæ in the mouth, for smearing the glans penis in a case of balanitis, and for touching a local inflammation of the lower eyelid; and it is stated that none of these applications was followed by severe pains—as after the employment of lunar caustic; that all the mucous surfaces were sufficiently modified by the action of the double salt, and that the cure appeared more rapid. This salt appears also to melt more easily without swelling when exposed to heat, and this circumstance will render it more convenient than the nitrate of silver alone, for application to the urinary passages, in the diseases of which the modifying method renders important services. It is said that its employment never presents any danger, and that it cannot cause any fear for the result of the disease.

XVI. *On Fumigations in the Treatment of Chronic Bronchitis, with a Description of a new Fumigating Apparatus.* By Dr. Louis Mandl. (Bulletin Général de Thérapeutique, Nov. 30, 1857.)

Dr. Mandl commences his remarks by observing, that an accurate diagnosis of the disease must be made before having recourse to the fumigating treatment; for that such a plan cannot be expected to cure cases of bronchitis which are symptomatic of diseases of the heart, of hydrothorax, or of syphilis; for in such cases, although fumigations may improve the state of the bronchi and diminish the sufferings of the patient, yet they will not effect a radical cure in the mucous membranes, which are altered in consequence of an affection which is seated externally to the lungs. These fumigations, also, are of no service, and are even injurious, in tubercular bronchitis, a state which ought not to be confounded, however, with chronic pneumonia at the apex of the lung, although the symptoms furnished by auscultation and percussion in these two cases are almost identical. The author restricts himself to cases of *chronic idiopathic bronchitis*, differing by well-known symptoms from dilatation of the bronchi and from emphysema. Of these cases of chronic bronchitis there are three varieties, as described by Laennec, namely, *pituitous catarrh* or *bronchorrhœa*, *mucous chronic catarrh* or *chronic catarrh* properly so called, and *dry catarrh*. The dry catarrh, according to Dr. Mandl, is peculiarly amenable to local treatment, which should consist of the employment of hot fumigations made by the help of the apparatus which he describes. The fumigating liquid consists of a mixture of fifty grammes of acetic acid, five grammes of creosote, and five hundred grammes of water. The patient uses these fumigations several times a day, at least three times, and on every occasion for twenty or twenty-five minutes; and the liquid is renewed at each fumigation, the quantity of the liquid being progressively augmented. The duration of the treatment is variable, the nature of the disease and its intensity causing it to last from one up to two or three months. The effects produced upon the patient are the augmentation of the appetite, and the diminution of the expectoration and its change of colour, for it changes from a greenish colour to a pale yellow or greyish-white. At the same time the respiration becomes free, the wasting is arrested, the strength and stoutness reappear, and the moral aspect of the patient undergoes a sensible amelioration. It is necessary to suspend the acid fumigations, and to replace them by emollient ones (of marsh-mallows, for instance) whenever in the course of the treatment an acute bronchitis supervenes. The chronic mucous catarrh does not yield by any means so certainly as the dry bronchitis to acid fumigations. In such cases, Dr. Mandl uses emetics

in conjunction with fumigations, and repeats them as often as the abundance of the râles indicates their use. The fumigating apparatus employed by Dr. Mandl consists of a glass globe, supported upon a stand, and having two apertures, one for a tube to enter the mouth of the patient, and another aperture communicating with the atmosphere. The breathing tube is of vulcanized india-rubber. The fumigating fluid is introduced into the glass globe, and is heated by means of a spirit-lamp. In breathing the vapour, the patient should close his lips hermetically around the tube, and respiration through the nose should be prevented, either by stopping up the nostrils by cotton, or by compressing the nose with an apparatus constructed for the purpose. The fumigation may be prolonged from fifteen to twenty minutes twice or thrice a day, according to the circumstances of the case. The best time in the morning is the period between rising and breakfast; in the evening, the hour before going to bed; and in the day, the space between luncheon and dinner.

All volatile liquids and substances may be employed in fumigations, according to the medical indications of each case; but soothing fumigations made with common water, or with marsh-mallow water, are sufficient in the acute affections of the respiratory passages.

XVII. *On the Use of Chlorate of Potash in the Treatment of Chronic Gingivitis, with or without Alveolo-dental Pyorrhœa.* By M. J. V. Laborde. (Bulletin Général de Thérapeutique, April 15, 1858.)

Alveolo-dental pyorrhœa is characterized by the flowing of a purulent matter across the insertion of the teeth, a matter which appears to be formed by the inflamed alveolo-dental membrane; and by an ulcerated condition of the tissue of the gum itself at its border, where it joins the crown of the tooth. But in respect of treatment, two special pathogenic circumstances involve two well-marked distinctions in the appearance, the progress, and the duration of the disease, and consequently its greater or less resistance to the therapeutic measures which are employed to oppose it. In the one case, the origin of the disease is to be traced to bad teeth, and then the affection is very much circumscribed; in the other case, the teeth are generally healthy, but the whole of the alveolar border of the gums may be attacked, and exhibit an ulcerated condition. The latter case appears to belong to a general cause, such as the want of cleanliness, joined to an idiosyncrasy probably of a scorbutic character, for the gums are generally anæmiated, flabby, fungous, and bleeding. It is in such a condition that the chlorate of potash exhibits undoubted efficacy.

The author relates 9 cases of chronic gingivitis, 8 of which were accompanied by alveolo-dental pyorrhœa, and in which chlorate of potash was employed internally or externally, or in both methods combined. In 6 cases the cure was complete, in 2 it failed, and 1 case was cured with difficulty after a long treatment. He concludes that the chlorate possesses a curative action over chronic gingivitis and alveolo-dental pyorrhœa; that the cases which receive most benefit are those which are connected with general pathogenic or diathetic influences; and that the efficacy of the salt appears to reside specially, and almost exclusively, in its topical application.

XVIII. *On the Thermal Establishment of Royat, near the Puy de Dôme.* By Dr. Homolle. (L'Union Médicale, May 29, 1858.)

Dr. Homolle, in visiting the Puy de Dôme in 1856, was struck by the remarkable conditions presented by the baths, formed in 1852, at the outlet of

the valley which leads to Royat; and he again visited the spot last year, and has made a full report upon the waters. The mineral waters of Royat appear to have been known from a remote antiquity, two of the baths discovered in 1843 being supposed to belong to the Gallo-Roman era. The discovery of these ancient baths was soon followed by that of an abundant mineral spring, and in 1845 some new baths were established, which were speedily found insufficient, and were replaced in 1854 by the present buildings. The establishment which now exists is said by Dr. Homolle to be among the most beautiful and complete in France.

The thermal spring of Royat furnishes a thousand quarts of water every minute. Its temperature is 35° Cent.; and the water is conveyed from its source to the bathing-rooms without any loss of carbonic acid; its great volume also allows a stream of mineral water to flow into the bath during the whole period of immersion, maintaining uniformity of temperature and a constant proportion of carbonic acid. The waters of Royat belong to the warm, acidulated, alkaline, and ferruginous classes. Less hot and rather less arsenical than those of Mont-Dore, these waters contain a much larger proportion of alkaline salts. The Royat water is limpid, colourless, and inodorous, and is strongly charged with carbonic acid; its taste is pungent and alkaline at first, becoming afterwards slightly ferruginous. It has been proved by analysis to contain carbonic acid, bicarbonate of soda and potash, sulphate of soda, phosphate of soda, chloride of sodium, bicarbonates of magnesia, lime, and iron, and silica. The chloride of sodium and the alkaline bicarbonates were in greatest proportion; and besides the mineral constituents, the quantity of which was accurately determined, traces were found of arseniate of soda, iodide and bromide of sodium, and a little alumina and manganese.

The following effects are produced on the system by these baths, according to Dr. Homolle. The sensation of warmth experienced at the moment of immersion is that of a temperate bath, but increasing progressively until it is accompanied by a slight pricking of the skin, which becomes of a scarlatinoid redness. If the nostrils are kept immediately above the water in the layer of air charged with carbonic acid, a certain embarrassment of respiration is produced, with giddiness, which soon cease when the head is raised. The urine, which is acid on entering the bath, progressively loses its acidity until it presents a well-marked alkalinity, if the bath is sufficiently prolonged (one hour), and taken in the morning before fatigue or eating. A feeling of comfort, of strength, and of muscular suppleness, constantly follows the use of the bath.

The thermal baths of Royat, as well as the other hot springs of the department of Puy de Dôme, are tonic, emmenagogue, and even slightly exciting. They are employed with success in chronic and apyretic diseases kept up by an atonic local or general condition, by rachitis, rheumatism, or the scrofulous diathesis. The simultaneous use of the temperate baths and of the waters taken internally, ameliorates or arrests chlorosis, anæmia, and the atony of convalescence. The hot baths and the inhaling-rooms (*salles d'aspiration*) are beneficial in chronic pulmonary catarrh, humid asthma, and phthisis in the first stage, when it is not complicated with hæmoptysis or fever. The hot baths, the mineral douches, and the vapour douches cure or ameliorate rheumatism. The baths and the temperate douches are of unquestionable utility in the congestions which succeed to sprains and dislocations, and in the muscular and tendinous stiffness which follows fractures, in deep abscesses, &c. Dr. Homolle adds that the beauty of the country and the mildness of the climate add to the efficacy of the waters, and permit their employment to be continued longer than in a great number of other thermal stations.

XIX. *On the Antagonism of Opium and Sulphate of Quinine.* (Gazette des Hôpitaux, May 29, 1858.)

In a late meeting of the Société Médicale des Hôpitaux, M. Gubler made an interesting communication on the antagonism of opium and sulphate of quinine. When attending at the hospital Beaujon, M. Gubler found a man affected with acute articular rheumatism who had been taking, without success, for several days, considerable doses of opium and sulphate of quinine. Finding that the physiological phenomena of the quinine were absolutely wanting, and that those of the opium were wanting also, M. Gubler began to suspect that these two medicines might neutralize one another, and he raised the dose of quinine at the same time that he diminished that of the opium. There was still no particular change. At last he discontinued the narcotic altogether, giving only the quinine, and then the peculiar action of the latter was manifested with great intensity. From that time the rheumatism improved in a marked degree. Other facts of an analogous nature, and a course of reasoning upon the subject, have led M. Gubler to conclude that opium may be definitely considered an antagonist or an antidote to sulphate of quinine. He also regards opium as causing a state of hyperæmia: and that probably its antagonist, sulphate of quinine, acts in an opposite manner—namely, in anæmiating and *decongestionizing*. The phenomena of quinic intoxication, hitherto attributed to cerebral congestion, would probably be due to another cause. This cause might be cerebral anæmia, the ordinary symptoms of which are giddiness, staggering, headache, singing in the ears, and deafness. What proves to M. Gubler the antagonistic action of opium, is the fact that subjects who take large doses of sulphate of quinine are particularly exposed to fainting, and that this salt prevents sleep. What tend besides to prove it, are the beneficial results obtained by the employment of this medicine in meningitis, or cerebral symptoms of a congestive nature, whether idiopathic or symptomatic.

M. Gubler thus gives the result of his researches, which, as will be seen, have an important bearing upon practice:—1. In opposition to opium, which exalts the organic actions, the sulphate of quinine acts upon the nervous system by condensing the strength in that quarter, so that it binds up the organic actions, which are sources of expenditure, and reduces as much as possible the afflux of blood to the congested parts. 2. This mode of action being admitted, the harmlessness of sulphate of quinine is explained in the cerebral symptoms of rheumatism—symptoms which this salt has been proved to have had no part in producing. 3. The employment of sulphate of quinine is indicated in all the inflammatory forms of cerebral rheumatism: opium being useful only in purely nervous disturbances, which are even exempt from febrile complication. 4. The sulphate of quinine and opium having an antagonistic action, ought not to be administered simultaneously. 5. These two agents may serve as antidotes to one another.

[While admitting the ingenuity of the above views, it is probable that physicians will not be disposed to allow the truth of all the premises on which they are founded.—Rep.]

XX. *Different Formulæ adopted in the Practice of the Medical Profession in Lyons.* (Bulletin Général de Thérapeutique, Feb. 15, 1858.)

The 'Gazette' of Lyons has published a series of preparations recommended by physicians of repute in that city, and which are said to have succeeded in certain well-marked cases. The following are some of these preparations:

Powder for the Convulsive Attacks of Hooping-Cough.—Bicarbonate of soda, seventy-five centigrammes; cochineal powder, seventy-five centigrammes; belladonna powder, fifteen centigrammes; and sugar in powder, eight grammes.

Mix, and divide into fifteen doses; two or three to be taken every day during the whole duration of the hooping-cough.

Doses for Intermittent or Remittent Spring Fevers.—Seignette salt, sixteen grammes; quina in powder, sixteen grammes: to be taken in a glass of warm water every morning for three days consecutively. It is remarkable that, in this formula, the purgative does not interfere with the antiperiodic effect.

Pills for Palpitations of the Heart, with Hypertrophy in an Early Stage.—Sugar of lead, two grammes; extract of digitalis, one gramme. Mix, and divide into twenty pills; one to be taken morning and evening, and the dose may be afterwards doubled.

Sedative Liniment for Eczema.—Oil of sweet almonds, ten grammes; glycerine, ten grammes; oxide of zinc, five grammes. To be employed in frictions when the acuteness of the eruption is over, or after the inflammation is calmed down, and the scales have been removed by meal-poultices. In chronic eczema, two to four grammes of sublimed sulphur may be added to the preceding liniment. This formula is particularly adapted for the eczema of the anus and the chaps of the breast.

Remedy for Habitual Headache.—Dr. Teissier, of Lyons, recommends fifty centigrammes of menyanthes to be infused for half an hour in a cup of boiling water, adding a tablespoonful of syrup of valerian. This quantity is to be taken two or three times a-day. Dr. Teissier has often ascertained the good effects of this very simple remedy, which had already been recommended by Tissot and Sainte-Marie, and which has improperly fallen completely into oblivion.

Pills for Acute Articular Rheumatism.—Dr. Bouchet gives the following pills, in conjunction with drinks containing nitre:—Extract of guaiacum, one gramme; extract of aconite, fifty centigrammes; sublimed calomel, ten centigrammes. Mix, and divide into ten pills, to be taken during the day, one every two hours. Dr. Bouchet has almost always seen the symptoms mend under this treatment from the fourth to the sixth day, movement becomes possible, and the disease is terminated from the twelfth to the fifteenth day.

XXI. *On the Value of Iodide of Iron.* (Journal des Cliniques des Hôpitaux de Paris, May 10, 1858.)

The iodide of iron is comparatively a new preparation, as it was only in 1834 that Dupasquier, of Lyons, distinguished no less as a chemist than as a physician, made some very interesting trials of this preparation in the treatment of pulmonary phthisis, and proposed a new method of preparing it. He then showed that he had performed several cures upon patients affected with crude pulmonary tubercles, and had considerably relieved others whose tubercles had begun to soften. In the hands of others, however, the use of iodide of iron has not been attended with uniformly good results, probably in consequence of the uncertain nature of the preparation; sometimes too much of the iodide was received into the stomach, and besides this, there was occasionally an excess of iodine present, which was converted in the system into hydriodic acid. In recent times, M. Gille has proposed to administer the iodide in the form of sugar-plums, and in the formula recommended it is said that the proportions of the iron and the iodine are preserved unaltered. M. Boinet records two cases of abscess of a very aggravated character cured by the internal use of iodide of iron, together with ioduretted injections into the sac of the abscess. The first case was that of a child, nine years old, in whom there was caries of the fourth, fifth, and sixth dorsal vertebræ; and there were two abscesses, one in the back, on a level with the diseased vertebræ, and the other, also on the right side, in the iliac region. The disease had continued for

several months without any amelioration, when, on the 8th of January, 1857, the abscess in the iliac fossa was punctured and injected with tincture of iodine; the patient was put upon good diet, and cod-liver oil was given internally, together with sugar-plums (*dragées*) of the iodide of iron. On the 17th of January both the abscesses were punctured and injected at the same time. This operation was subsequently repeated on several occasions, and under this treatment the fistulous openings caused by the punctures gradually dried up, and at last became completely cicatrized. The child improved in all respects, and under the use of the iodide of iron in sugar-plums, and the influence of strengthening food, became strong and well. The second case was that of a man, aged sixty-three, under the care of M. Malgaigne, at the Hôpital St. Louis, who had a large abscess below the crural ligament. M. Boinet punctured this abscess, which discharged more than three pints of a grumous pus. The opening was then injected with tincture of iodine; the abscess remained fistulous for some time, and allowed a small quantity of pus to flow out every day, but it soon closed. The iodide of iron was administered internally, and the patient was put upon a strengthening diet; and six weeks after the operation and the internal use of the iodide, the patient left the hospital quite well.

In a lecture on chlorosis by M. Gendrin, the lecturer recommends iron and manganese as the best remedies in this disease, and passes in review the different preparations of iron which are employed in medicine. One of the most useful preparations in cases of chlorosis complicated with scrofula, or only with a lymphatic temperament, is the iodide of iron. But, unfortunately, this salt is very easily decomposed, and in order to employ the syrup with advantage, it should be prepared at the very time when it is to be taken. The invention of M. Gille fulfils the objects of practitioners in recommending this medicine, for he envelopes the iodide with a layer of sugar, which altogether prevents the access of air. These sugar-plums have been preserved more than two years without any alteration of the iodide. M. Gendrin speaks strongly in favour of this preparation, the value of which consists not only in the indefinite preservation of the ferruginous salt, but also because it renders its administration easy and agreeable. The iodide of iron has the advantage over other preparations, of being well borne by the patient, an advantage which it undoubtedly owes to its great solubility.

M. Rostan, in a lecture on chlorosis, says that the iodide of iron possesses, in the promptitude of its curative action, a very manifest advantage over the other ferruginous compounds; the syrup of the iodide has not been used in medicine so much as it would have been if it had more stability; but the preparation of M. Gille renders the administration of this salt as easy as that of the oxide or the carbonate.

In phthisis, the iodide of iron has been found to act very beneficially, and its importance in this disease will be increased when it is preserved chemically pure, and given in suitable doses. Louis, Andral, and Bricheteau, have all used with success the syrup of the iodide of iron in pulmonary tubercle; and more lately Dr. Belouino has published some cures of this disease effected by the administration of the sugar-plums of M. Gille. "Recent observations," says Dr. Belouino, "have assigned to iodide of iron an important place among therapeutic agents, and it may be boldly placed among the best medicines which we possess. Formerly it was unworthy of confidence, because it was badly preserved and was easily decomposed, and consequently did not give always identical results. Physicians, in consequence, declined to make use of it. At present the iodide of iron—thanks to the laborious researches of M. Gille—is preserved in a state of perfect purity. I have had occasion to employ very often the preparations of this gentleman, and I have attained experimentally the conviction that the iodide of iron is an excellent medicine in cases of

anæmia, scrofula, rachitis, chlorosis, and often in certain cases of pulmonary phthisis in which the organism requires to be strongly fortified." Dr Belouino records two cases of phthisis which were cured by the administration of iodide of iron; one was the case of a lady, aged twenty-four; the other that of a child, aged five; in both, the existence of tubercular disease was ascertained, but it disappeared under the use of the iodide.

THERAPEUTICAL RECORD.

*Pills and Tincture of Digitalis Seed.**—M. Brossard, of Rouen, proposes as a substitute for digitaline, which cannot always be prepared, the seeds of digitalis, which will keep very well from one year to the next. A *gramme* of these seeds, made into a pill with a sufficient quantity of honey, makes a mass which, when divided into thirty pills, contains rather more than a milligramme of digitaline. He also prepares a tincture by steeping the seeds in alcohol for eight days; this tincture, prepared according to his directions, contains about one centigramme of digitaline in each gramme. M. Bouchardat, in commenting upon these preparations, denies that digitaline can be replaced by digitalis seeds; but he admits that the seeds of digitalis are less variable in their proportion of digitaline than the leaves, and that they also are more powerful as medicinal agents.

Caustic Glycerine for Lupus.†—The formula proposed by Dr. Hébra, of Vienna, consists of iodine, iodide of potassium, and glycerine. This topical application is laid on every two days with a camel-hair brush; it causes pain for more than two hours, but it has the great advantage of curing the lupus without producing unsightly scars.

Mode of Detecting the Smallest Quantities of Corrosive Sublimate mixed with Calomel.‡—The purity of calomel may be determined in a very simple manner. The test consists of iodide of potassium dissolved in distilled water; about fifty centigrammes of the calomel are made into a paste, with a drop or two of the test liquid, on a piece of glass. If the calomel is pure, it assumes a green colour; if it contains only a thousandth part of bichloride of mercury, red spots are produced.

On the Treatment of Iritis without Mercury.—Dr. H. W. Williams, of Boston, has published a paper, read before the Boston Society for Medical Observation, in which he proposes to treat iritis without the use of mercury; and he records 64 cases of that disease successfully treated, although they were of every degree of severity, including the idiopathic, traumatic, rheumatic, and syphilitic forms. Dr. Williams arrives at the conclusion that mercury and other antiphlogistic remedies are not indispensable in the treatment of iritis, although he does not assert that they are to be absolutely discarded. His plan consists in supporting the powers of the system, while the local circulation is relieved by moderate depletion, and the pain and irritation are soothed by sedatives. Leeches, therefore, are applied in some cases to the temple, and quina and iodide of potassium are given internally, together with Dover's powder, or the preparations of morphia. Solution of atropia is also dropped upon the eye, and warm mucilaginous fluids are also employed in the form of a lotion. The bowels are kept open by moderate purgatives. The results have been, according to Dr. Williams, very satisfactory in all cases,

* Bulletin Général de Thérapeutique, June 30th, 1857. † Ibid. ‡ Ibid.

except one which had been neglected previously, and three others which had been injured by homœopathic treatment. The eyes were perfectly restored, or if any adhesions were formed, they were so slight as not to impede vision.

*Of the preparation of Pastiles of Pepsin.**—In order to render pepsine an agreeable medicine for children, M. Corvisart had recommended a preparation of syrup of cherries with pepsin. But it was found by M. Berthé, that by the action of sugar upon pepsin, the latter was modified and transformed into glucose and lactic acid. The presence of water being the principal cause of this alteration, M. Berthé has adopted the form of pastiles. They are composed of gum arabic paste, with a few drops of essence of lemon, and when the mass is quite homogeneous, twenty-five centigrammes of pepsin are added to each pastile, which is very agreeable to the taste.

New Vienna Caustic.†—M. Dujardin, of Lille, having observed the inconveniences attending the use of the Vienna caustic, owing to the chemical reactions set up between the potash and the lime, which is never pure, and the composition of which is very various, has proposed to combine the potash with calcined magnesia, with clay dried at the fire, with fine dry sand, with impalpable powder of pumice stone; of these, he prefers the combination of caustic potash with clay. The preparations should be preserved in well-stopped bottles.

New Caustic Paste with Chloride of Zinc and Gluten.‡—M. Sommé has been led to the employment of gluten instead of flour in the formation of the caustic paste, by observing that the paste was more adhesive and easy to preserve in proportion as the flour which he employed was more rich in gluten. The gluten is obtained from the best wheaten flour, and the chloride of zinc and the gluten are mixed in the following manner. The chloride is placed in a porcelain capsule, and dissolved in alcohol with a gentle heat; then the gluten in powder is spread uniformly over the liquid mass and triturated, so as to incorporate the two substances completely together. This paste is very plastic and may remain for a long time exposed to the air without liquefying, and it may be handled with impunity, if there are no excoriations on the skin. It may be used in mass, in plates, and in cylinders, the latter form being applicable in the case of deep fistulæ.

Employment of Nitrate of Silver as an Abortive in Paronychia.§—Dr. Guinier, of Montpellier, proposes to employ the nitrate of silver in the treatment of whitlows, at a period when the disease is in a very early stage. He moistens lightly all the red and painful surface, and then passes over it slowly the extremity of a stick of nitrate of silver, and the operation is continued sufficiently long to be sure of the penetration of the caustic through the epidermis, which is indicated by the brownish colour of the latter. The skin is at first made brown, and after some hours takes a beautiful black tint, and from this moment the cure is complete. After some days the cauterized epidermis peels off.

On the preparation of Valerianate of Ammonia of definite composition.‖—This salt could never be obtained, up to a recent period, in a pure state and solid. In fact, in treatises on chemistry, the valerianate of ammonia is described as liquid and amorphous, and the persons who prepare chemical products have never been able to present it in a solid and crystalline state, pure, and of a constant composition. MM. Laboureur and Fontaine have attempted to supply

* Bulletin Général de Thérapeutique, Aug. 30th, 1857. † Ibid., May 30th.
‡ Ibid. § Ibid., Oct. 15th. ‖ Ibid., April 15th.

the deficiency. Their proceeding consists in preparing, in a pure state, valerianic acid and ammoniacal gas, and then uniting these bodies. In proportion as the combination proceeds, the salt crystallizes in a form apparently confused; but under the microscope prisms may be very well distinguished, having four terminal planes. The formula for preparing this salt is the following:—Take mono-hydrated and pure valerianic acid, dispose it in thin layers in a flat capsule, covered with a bell-glass completely closed. Pass into the bell-glass anhydrous ammoniacal gas to saturation of the valerianic acid, and preserve the valerianate of ammonia in small portions in well-stopped bottles.

*Treatment of Lead Colic by Alum and Sulphuric Acid.**—M. Briquet, physician to the Hospital of La Charité, records the results of the treatment of lead colic by alum and sulphuric acid upon fifty-seven patients. They each took every day sugar-and-water acidulated with sulphuric acid as a diet drink; in the course of the day, a potion of gum containing alum, taken by tablespoonfuls at a time, and in the evening, a pill containing extract of opium. Commencing from the first day of the treatment, they had a sulphur bath, renewed every two days, as long as the skin was covered with a black layer of sulphuret of lead. Sometimes, to yield to the entreaties of the patients, an injection was administered with decoction of senna or solution of sea-salt. Low diet was maintained as long as the appetite was not distinctly felt. All the patients recovered, and M. Briquet believes that although they did not recover quite so rapidly from the abdominal pains as those who were treated by purgatives, yet that the convalescence was shorter and less distressing, from the absence of after-symptoms.

The Thermal Springs of Luxeuil.†—M. Billout has lately called attention to the valuable properties of the springs of Luxeuil, which are said to rival in efficacy those of Plombières. They contain iron, arsenic, iodine, sulphur, and manganese in appreciable quantities, although some of the springs contain these elements only in very minute doses. Some of them possess the characters of the weak sodium-chlorides, and others present the properties of the thermal muds. The ferruginous springs of this place contain manganese as well as iron, and it would seem that there is some therapeutical as well as chemical analogy between these two metals.

Extemporaneous Preparation of Chlorine as a Disinfectant.‡—The chloride of lime, usually employed as a means of disengaging chlorine, has, besides its price, the inconvenience of being rather rapidly exhausted. M. Lambossy substitutes for it a cheap and simple preparation, consisting of common salt, red-lead, sulphuric acid, and cold water. The red-lead is mixed with the salt, and introduced into a bottle full of water. The sulphuric acid is added afterwards gradually, and shaken at intervals. By this process, sulphate of lead is formed and precipitated, and sulphate of soda and chlorine remain dissolved in the water. The chlorine, which gives the liquid a yellow colour, is disengaged as soon as the bottle is opened. To produce a more rapid disengagement, the liquid is poured into flat plates, so as to offer a large surface for evaporation.

On Santonate of Quinine and Cinchonine.§—It is known that santonine is a substance analogous to the fatty acids, allied in a certain degree to the ethereal oils, without acid reaction, but forming with bases, such as potash, soda, magnesia, and baryta, crystallizable salts. Such is the case with quinia and cin-

* Bulletin Général de Thérapeutique, Aug. 15th, 1857.
† L'Union Médicale, April 25th, 1857.
‡ Bulletin Général de Thérapeutique, July 15th, 1857. § Ibid.

chonia. M. Pavesi takes pure santonine and cinchonia, with animal charcoal and spirits of wine; the whole is boiled in a little alembic, which is kept closed for some minutes; the boiling solution is filtered through paper, and distilled at a moderate heat; it is then allowed to cool, and the santonate of cinchonia crystallizes. This is collected on a filter at ordinary temperatures, and the mother liquor, containing a certain quantity of the salt, may be used for other preparations. If the powdered salt is heated for some minutes in a retort containing water acidulated with a slight quantity of sulphuric acid, the santonine is precipitated.

*Treatment of Gonorrhœa by the Yellow Jessamine.**—Dr. John Douglas describes a case of gonorrhœa which was successfully treated by the yellow jessamine (*Gelseminum sempervirens*). The patient had been suffering for some months from improperly-treated gonorrhœa. A small handful of the root of the plant was put into a bottle of whisky, and the patient was ordered to take a tablespoonful of this tincture night and morning. The immediate effect was rather alarming, as the vision was impaired; but every symptom of gonorrhœa had ceased, and the cure was permanent. It appears that the flowers, roots, and the whole plant are narcotic, and that in South Carolina a saturated alcoholic tincture has long been used with marked success in rheumatism.

Treatment of Trismus Nascentium by Spirits of Turpentine.†—Professor H. L. Byrd has recorded an instance of the successful treatment of a case of trismus nascentium by spirits of turpentine, being the only case of recovery from this disease which he had witnessed out of more than thirty cases in a practice of sixteen years. The patient was a negro child eight days old, in whom the tetanic paroxysms were well-marked. Five drops of spirits of turpentine were ordered to be given every two hours with the same quantity of paregoric. The dose of turpentine was afterwards increased to ten drops, and an enema was occasionally employed, consisting of an ounce of flax-seed tea and forty drops of turpentine. The child eventually recovered, and Professor Byrd thinks that if the turpentine had been employed in larger doses at first, the disease would have yielded sooner.

Advantages of Glycerine over Mucilage in the formation of Washes and Injections.‡—In the formation of many metallic lotions with mucilage, a precipitate is formed, and an indefinite quantity of metal remains in solution; but M. Dannecy, of Bordeaux, proposes to employ glycerine as the solvent, which is free from the objection referred to. There is in France a common wash for the eyes, composed of borax and mucilage of quince seeds, but this solution is of undetermined strength, owing to mutual decomposition of the ingredients; and it is proposed to substitute glycerine, which undergoes no decomposition, and is as mild and soothing as the mucilage.

Of the Medical Properties of Phosphuretted Water and its Chemical Composition.§—When phosphorus is left for some time in water, a chemical action ensues, by which part of the phosphorus becomes dissolved, and the water becomes acid, and often phosphorescent. Struck with the physical and chemical characters presented by water in which phosphorus had been kept, M. Stanislas Martin was led to believe that it would possess some of the medical properties of phosphorus itself; and he accordingly administered it to some dogs and rabbits, and ascertained that its effects resembled those produced by the ethereal tincture of phosphorus taken internally, and by the oil of the same

* Charleston Medical Journal and Review, July, 1857. † Ibid.
‡ Bulletin Général de Thérapeutique, April 30th, 1858. § Ibid.

name applied in friction externally. Internally the phosphuretted water may be given in sugar, in simple or sugared water, or in an aromatic or mucilaginous infusion.

*Bicarbonate of Soda in Diphtheric Angina.**—Dr. Anglada employed bicarbonate of soda in the case of five persons affected with diphtheric sore throat. The mode of administration consisted in giving one gramme of the salt dissolved in a table-spoonful of common water every half hour, and in allowing as a drink a solution of fifteen grammes in a pint of sugared water. Four of the patients so treated were cured in the space of from thirty-six to forty-eight hours, the false membranes having completely disappeared in this short time. In the case of the fifth patient, a child of eighteen months old, the throat was only sponged with a saturated solution of the salt, and the diphtheric patches disappeared in less than forty-eight hours.

Ferruginous Soaps.†—MM. Tapin and Carrignon propose the employment of iron soaps in diseases in which ferruginous medication is indicated. They appeal to the perfect solubility of these soaps in fatty bodies and in essential oils, and to the favourable results which they have obtained in 50 cases. The ordinary dose is from four to six pills, containing two centigrammes of iron, taken in two periods and at meals. Afterwards only two pills are to be given, which prevent all relapse, and produce no effects of constipation or pain of stomach, which result from the use of other preparations of iron.

Iodo-tannic Syrup and Injections.‡—The aqueous solution of tannin possesses the property of transforming iodine into soluble hydriodic acid. A part of the tannin is then combined with this hydriodic acid, so as to render it insensible to starch, while the other part is altered and precipitated. M. Guilliermond has taken advantage of this property to compose an *iodo-tannic* syrup for internal use, and *iodo-tannic* injections for external use. The syrup has for its base the extract of rhatany and iodine, and the injections are made with iodine and quercitannin. The iodo-tannic syrup has no disagreeable taste, and patients may thus take considerable doses of iodine without fatigue or repugnance. The iodo-tannic injections are very useful in subacute or chronic blennorrhagia; they occasion no pain; they have an astringent action without any specific powers.

Chlorinated Lotions in Naso-pharyngeal Diphtheria.§—Dr. Charcellay relates the case of a child of four years old affected with naso-pharyngeal diphtheria, commencing in the nasal fossæ. Emetics were given at first, then chlorinated lotions were applied to the nasal fossæ and the pharynx. The symptoms were arrested by this topical application, and the cure was completed in twenty days.

On the Comparative Action of Chlorate and Iodate of Potash.‖—In a late discussion in a French medical society, M. Demarquay announced that he had made some comparative experiments upon the action of chlorate and iodate of potash in mercurial salivation and ulcerated stomatitis; and he found that although the first of these salts exercises a well-known and powerful effect upon the bucco-pharyngeal membrane, yet its operation is inferior to that produced by the iodate—one gramme of the iodate producing more prompt and satisfactory results than four grammes of the chlorate.

* L'Union Médicale, Dec. 22nd, 1857: from Travaux de la Soc. Méd. d'Indre et Loire.
† Ibid. ‡ Ibid., Dec. 29th. § Ibid. ‖ Ibid.

QUARTERLY REPORT ON PATHOLOGY AND MEDICINE.

By EDWARD H. SIEVEKING, M.D.
Fellow of the Royal College of Physicians, Physician to, and Lecturer on Materia Medica at, St. Mary's Hospital.

I. *Clinical Report on Pulmonary Tuberculosis; giving an abridged Account of Twenty-four Cases of Arrested Tuberculosis.* By AUSTIN FLINT, M.D. (American Journal of Medical Sciences, Jan. 1858.)—The change that has taken place of late years in the treatment of phthisis, the greater reliance upon hygienic measures, the avoidance of all debilitating and depleting agents, have entirely altered the prognosis that is now formed, at least in the early stages of tubercular deposition. Professor Flint brings strong evidence in favour of these views. In the paper before us, he analyses the histories of twenty-four cases of pulmonary phthisis which have fallen under his observation, and in which the presence of tubercle in the lungs was proved by physical exploration, as well as by general symptoms; while the arrest of the disease was shown by the improvement in the symptoms referable to the lungs, and in the restored tone of the body, as well as by a comparison of the results of physical examination before and after the arrest had taken place.

We gather from the description, that of the twenty-four cases five had manifestly advanced to softening; the remainder had not passed beyond the first stage. The first of the former class was clearly a case of cure by obliteration of the cavity; in the second and third the disease was non-progressive, lasting respectively twenty-seven and eighteen years; in the fourth there appears to have been a complete recovery, similar to the first, though the evidence of the existence of cavities is not equally positive; in the fifth there was an arrest, and a prolongation of life for about a year after the evidence of a cavity had appeared irrefragable; but this is the least satisfactory case of the collection, and, without the others, would scarcely have availed anything. We must refer our readers to the original for the details, but shall place before them a few of the observations with which the author concludes his paper.

Dr. Flint points out the tendency in a certain number of cases of pulmonary tubercle to spontaneous arrest, and seems to think that this arrest is more frequently than not interfered with by therapeutic proceedings, especially when of a lowering character. Hæmoptysis occurred in 18 out of the 24 cases; in 10 it occurred more than once; a fact that accords with the conclusion of Dr. Walshe, that hæmorrhage does not hasten the disease, but rather the reverse.

In 8 of the cases no medical treatment was adopted; in nearly all of the remaining 16 a change was made in the habits of life. This change, with a single exception, consisted in all instances in relinquishing partially or entirely sedentary pursuits, and giving proportionately more time to exercise in the open air. In none of the cases in which the disease was arrested, "without any measures of management," were the pursuits of the patients sedentary. After analysing the changes effected in the various cases, Dr. Flint observes that the facts confirm the correctness of the views of those who hold that laborious fatiguing exercise in the open air is best suited to promote the arrest of pulmonary tuberculosis. With his views regarding climate we are also disposed to agree—viz., that it exerts no special agency in determining the arrest of tuberculosis, but may favour this result indirectly by affording a better opportunity for exercise in the open air.

With regard to diet, the author urges a liberal allowance of nutritious food, and advocates a free use of wine, beer, or spirits; he finds that the latter do not produce their ordinary stimulant effects so readily in phthisis as in health; and lays down the rule, that if these effects are produced, the quantity is too large, but that any quantity within this limitation will be likely to be useful.

II. *On Congenital Contraction or Obliteration of the Pulmonary Artery.* By Professor HERMANN MEYER, in Zuric. (Archiv für Pathol., Anat., and Physiol., Band xii., Heft 6.)—A careful examination of 82 recorded cases, in which the pulmonary artery deviated materially in size or configuration from the normal condition, leads Professor Meyer to the following conclusion: That in all cases in which an imperfect septum ventriculorum and narrowing or obliteration of the pulmonary artery co-exist, the latter is always the primary malformation, and that its existence not only induces the patency of the septum ventriculorum, but also causes the other coincident malformations in regard to the origin of the aorta, the foramen ovale, the ductus botalli, and the bronchial arteries. The following is a brief summary of the case which has given rise to this investigation:

A girl, who had been cyanotic from her infancy, and always subject to dyspnœa on slight exertion, died at the age of fourteen years and three quarters. Her heart, when superficially examined, appeared normal, except in regard to the relation of the arterial trunks. The aorta appeared to be pushed over too much to the right, and its arch covered a part of the right lung; the anonyma was on the left side, while the right carotid and subclavian arteries were given off by the aorta. The pulmonary artery was remarkably narrow, only measuring fourteen millimetres (0·55 inch); it was almost buried in the subserous fat of the pericardium. The ductus botalli was open at its extremities, but obliterated in the middle. On opening the heart both left cavities were found to be very small and with feeble walls, especially those of the left ventricle. The mitral valve was normal in its relations to the size of the cavities; no arterial trunk was given off by the left ventricle; the right curtain of the mitral valve covered an opening at the upper part of the interventricular septum leading into the right ventricle. The right ventricle was thicker; the tricuspid and the right auricle, normal. The foramen ovale was still slightly patulous. The part of the right ventricle lying between the tricuspid and the septum formed a cavity which was separated from the rest of the cavity by a projecting trabecula; in this cavity three orifices were visible; posteriorly the abnormal communication with the left ventricle; more anteriorly and to the left the orifice of the aorta, disposed so that the upper edge of the perforation in the septum touched the lower adherent margin of that aortic valve which is not in relation with a coronary artery; anterior to the aortic orifice, and slightly below it, was a very small circular opening of about seven millimetres (0·27 inch) concealed among the trabecula carnea, and divided by a vertical columna, which closed the middle third of the opening, into an anterior and posterior portion. The upper half of the circumference and the surface of the intervening muscle were invested by an opaque endocardium. The orifice led into the upper part of the infundibular portion of the right ventricle, which was large enough to admit the tip of the finger; there were only two pulmonary semilunar valves, and there were a few cretaceous spots in the infundibulum. Without going into the author's analysis of recorded cases, to which we would refer those curious in such matters, we extract one or two remarks on the case itself. The open septum he regards as a malformation by arrest, the state of the infundibulum and the pulmonary artery as the result of a morbid process. The infundibulum, it appears, was separated from the body of the ventricle by a hard fibroid growth, which the author looks upon as the effect of inflammation of the muscle or the lining membrane. While we are ready to admit that in Professor Meyer's case intra-uterine inflammation may have been the pathological cause of the malformation, we remind our readers that the separation of the infundibulum from the sinus of the ventricle may be the result of arrested development.

III. *Fatal Pericarditis, caused by Lodgment of Artificial Teeth and Gold Plate in the Œsophagus.* By J. R. Buist, M.D. (Charleston Medical Journal and Review, Jan., 1858.)—A plate of gold containing teeth, about one and a half or two inches long and one inch broad, was swallowed by a Scotchman, aged forty, while at meal, on the 30th September. He felt some uneasiness at first, but it was not till the 10th of October that he suffered any serious pain. He now rapidly grew worse, and died on the 14th of October. The plate was found in the œsophagus, two inches and a half above the stomach, and had ulcerated through into the right and posterior part of the pericardium. The curious part of the story is, not that pericarditis ensued upon the presence of a foreign body occupying the position this did, but that a long time elapsed before the patient experienced any material inconvenience from the functions of the œsophagus being so seriously interfered with.

IV. *Acute Fatty Degeneration of the Muscular Tissue of the Heart in Pericarditis.* By Rud. Virchow. (Archiv für Pathol. Anat., Band xiii., Hefte 2 and 3.—Professor Virchow draws attention to the fact, that in certain forms of pericarditis fatty degeneration affects the muscular tissue of the heart as a direct result of the inflammation of the investing membrane; that the degenerative process advances from without inwards, and that by inducing weakness and paralysis of the heart, it materially augments the danger of the patient. This variety of fatty degeneration is to be distinguished from the chronic form of the disease, "which accompanies dilatation of the heart, and is also associated with debility, by the latter commencing at the endocardium, and generally in patches; the latter circumstance also serves to distinguish the second variety from the uniform fatty discoloration of the innermost muscular layers seen in old people." The following case gave rise to Professor Virchow's remarks on the subject:—A man, aged twenty, was admitted on the fifth day of acute articular rheumatism into the Charité; his pulse was unusually quick, there were profuse perspirations and great anxiety. No friction-sound appeared till the second day after admission; at this time a peculiar change had occurred in the pulse, which had lost its tension, while from every third to sixth beat there was a slight reduplication. From this time the anxiety of the patient and the feebleness of the pulse increased, delirium supervened, and death ensued, with extreme dyspnœa on the twelfth day after admission. The pericardium contained a thin sanguinolent fluid, in which no blood corpuscles were visible. The surface of the heart was covered with thick, strongly adherent layers of fibrin. A layer of from one to two lines in thickness of the muscular tissue adjacent to the pericardium presented a pale yellow and opaque appearance; the microscope showed it to have undergone complete fatty degeneration. No part of the heart was entirely free from this change, but it was much less towards the interior. Towards the surface there were layers of areolar tissue which passed into the thickened pericardium, and were full of closely packed dichotomous nuclear and cell growths. A similar state of things was found in a second case, where the autopsy revealed purulent pericarditis, which had not been suspected during life. The author has failed to discover it in other instances of pericarditis of a less severe character, or which only occurred as a complication of other diseases. Professor Virchow therefore concludes that it is the immediate consequence of the pericarditis.

V. *A Needle Implanted in the Septum of the Heart without causing Special Symptoms.* By M. Piorry. (L'Union Médicale, March 18, 1858.)—A man, aged fifty-four, a drunkard, was admitted into the Charité on February 10th, 1858, with symptoms of pneumonia. He soon recovered, and the examination of the heart during his illness showed no abnormal bruits or special symptoms. Erysipelas of the face supervened, and death ensued on the 20th of February.

The diseased lung was gorged with frothy mucus, the trachea full of sputa, the right side of the heart was dilated, and the liver very voluminous. In examining the heart a hard substance was felt, which proved to be a needle one millimetre in width, and five centimetres at least long. It was inserted in the interventricular septum: its ends were free, the point being directed towards the left ventricle. M. Piorry is of opinion that the needle was introduced through the intercostal spaces, that it was gradually drawn away from the surface by the movements of the heart, and thus became fixed in the septum. A fibrinous concretion was found on the ventricular pericardium where the needle had entered, and both ends of the needle were covered by a fibrinous layer.

VI. *Mechanical Obstruction of the Heart by Entozoa, causing Death.* By M. SCHUPPERT, M.D. (New Orleans Medical News, vol. v., No. 11.)—The subject of this notice is introduced to us as belonging to "the lower class of society," and is—a dog. For five days before his death he showed great debility, only being able to walk a short distance at a time, his appetite disappearing, and the thirst being great. The abdomen swelled, the urine became dark brown, the respiration laboured, and death ensued. Autopsy half an hour after death: lungs healthy; no fluid in pleura or pericardium. "The vena cava ascendens was cut near the auricle, and was filled with long white filaments, which proved to be worms. They not only occupied the superior portion of the cava, but further dissection discovered the right auricle to be filled with them, the right ventricle to be partially filled, and many of them reaching a considerable distance into the pulmonary artery. The right ventricle contained some coagulated blood, in which the worms were embedded. All the worms had their heads 'up stream,' or in the direction of the current of the blood towards the lungs. Although the autopsy was made very soon after death, the life of every entozoon was extinct." The coats of the ascending cava were inflamed. No material derangement was discovered in any other organ; nor were any worms found elsewhere.

The entozoa were from five to twelve inches long, and about one-sixteenth of an inch in diameter. They were round, elastic, colourless, and transparent. The upper extremity was of a conical form, and the caudal end pointed and filamentous. A canal was visible through the whole length of the parasite, which belonged to the genus *filaria*. Besides these large entozoa, a great number of minute filaria were discovered in the liver, moving about in the blood with great energy; these were only one-twentieth of a millimetre in length, and one three-thousandth to one five-thousandth of a millimetre in diameter.

VII. *On Gastritis Submucosa.* By Dr. H. WALLMANN, Chief Physician to the Joseph Academy at Vienna. (Zeitschr. der k. k. Gesellschaft. der Aerzte, xii. Monatsheft, 1857.) Dr. Wallmann relates an interesting case of this rare disease, of which the following are the prominent features:

Ivan S——, a private soldier, aged twenty-four, had enjoyed good health during his three years of service, but for three days before coming under treatment had felt unwell, and complained of pain in the region of the heart and stomach, with nausea, loss of appetite, and slight cough. He walked to the hospital, and his complaint was not thought to be serious; towards evening he became restless, and on rising from bed fell back suddenly and expired.

The autopsy revealed the presence of yellowish white flocculent fluid in the pelvic cavity, and fibrinous exudation investing the liver, spleen, and inferior surface of the diaphragm, and agglutinating the intestinal coils. "The stomach was slightly distended with gas; its serous coat dull and grey; the anterior surface of the fundus ventriculi here and there loosely covered with a

yellowish-white delicate membranous layer; the parietes of the stomach felt uniformly thickened, and on cutting into them near the pylorus, a large quantity of a yellowish-white thin fluid issued from between the thickened coats; they were six to eight lines in thickness; the thickest parts were at the pylorus, the cardiac region, and at the fundus; they were thinner at the posterior than the anterior wall of the pylorus and cardia. The stomach contained about six ounces of a mucous, flocculent, yellowish-grey, viscid fluid, with a few small friable masses. The mucous membrane throughout presented a congested appearance of a dirty-red colour, and especially at the fundus; it was perforated like a sieve, with numerous small circular openings, through which a yellowish-white liquid exuded on pressure. The mucous membrane itself was one line and a half thick, and unchanged, except by the holes. The submucous areolar tissue was everywhere distended and pultaceous, of a spongy texture, and in the vacuoles the above-mentioned fluid was accumulated. The muscular layer was thickened throughout, especially at the pylorus, where it formed a prominent ring, and much contracted the orifice; the muscular tissue was also hypertrophied along the larger curvature, and at the posterior wall, especially at the fundus, but less at the cardiac portion; it was of a greyish-red, here and there like brawn, and infiltrated with the above-mentioned fluid. The serous and subserous coats were thickened, opaque, and infiltrated with serum."

The other organs were carefully examined, but though some lesions were discovered, they do not require special mention. Dr. Wallmann concludes that the case was one of primary inflammation of the submucous cellular tissue of the stomach, with purulent exudation and secondary peritonitis. He quotes instances from Clauss, Andral, Olbers, and others.

VIII. *On the Relation existing between Diabetes Mellitus and Carbuncle.* By Dr. A. Wagner, Chief Physician to the Dantzic Hospital. (Archiv für Patholog. Anat., Band xii. Hefte 4 & 5.)—The coincidence of inflammatory and gangrenous affections of the skin with melituria has been observed by Prout, Marchal de Calvi, Landouzy, Dr. Goolden, and others. Dr. Wagner inquires into the relation the two affections bear to one another, and arrives at the following conclusions, which he supports partly by the records of his own practice, partly by analysis of other writers:

1. That inflammatory and gangrenous affections of the skin not unfrequently have been developed and have repeatedly occurred in persons who for a considerable time had laboured under chronic melituria.

2. That sugar does not appear to occur in the urine of persons affected with the skin diseases just mentioned in whom diabetes does not exist.

3. That in rare cases of persons previously in perfect health, attacked by extensive carbuncular disease which ran a tumultuous and fatal course, accompanied by septic symptoms, acute melituria has been observed.

In support of these views, Dr. Wagner brings forward an analysis of all the cases of inflammatory and gangrenous skin affections which had occurred to him for fourteen months, amounting to fifty-two. In none of them was any sugar found in the urine. They were as follows:—

	Males.	Females.
Carbuncle	5	3
Furuncle	9	6
Erysipelas	8	14
Erysipelas phlegmonodes .	2	5

Forty-eight of these recovered, four ended fatally.

Two interesting cases are given in detail, which bear out the writer's third argument, and in which saccharine urine appeared in the course of severe and

fatal carbuncular disease attacking gentlemen previously in perfect health, and exhibiting no sign of diabetes.

IX. *On Lead Colic.* By Dr. Briquet. (Archives Générales, Févr. et Mars, 1858.)—The author examines the various theories that have been promulgated regarding the seat of this disease, and relates the results of his own investigation, which it will be seen differ from the prevailing views. The main point of the latter is that he considers the site of the pain to be in the abdominal muscles; that it is a *myosalgia*. He finds that if palpation of the abdomen in persons affected with the disease be conducted so as to act only on the skin and the subjacent muscle, "so as, in a measure, to scratch the latter, and to respect the deeper-seated parts," the characteristic pain of colica pictonum is at once produced, the site of the pain always corresponding to the points at which it is felt in the disease. The position occupied by the patient when suffering from the disease is the next argument in favour of the muscular theory; he is doubled up and unable to lie flat; if the rectus is affected he is bent forward, if one of the oblique he is bent laterally. Thirdly, rest soothes and may alone suffice to arrest the pain; fourthly, the abdominal pain of lead colic is at times accompanied by a state of hyperæsthesia of the skin covering the affected muscles—a condition also met with in hysterical persons labouring under an analogous condition of the superficial muscles; fifthly, the constipation has no relation to the pain; the constipation persists after the pain has been arrested; sixthly, if the pain in the muscle be arrested there will be no local or general suffering, and if the patient has no other symptoms than the disturbance in the primæ viæ, the functions will be speedily restored. Having established the views just enumerated to his own satisfaction, Dr. Briquet analyses 43 cases, in order to determine what muscles were chiefly affected; the whole abdominal parietes were affected in 6, the whole of the recti in 10; the right rectus in 5, the left rectus in 4, the two oblique in 6 cases. Other muscles were also found affected, but we have not space to reproduce the whole analysis.

The induced galvanic current being applied to 42 persons affected with lead colic, in the author's words, it invariably gave instant relief, however severe and extensive the pain had been; this Dr. Briquet regards as a further argument in favour of his propositions, because Faradization is found generally to relieve muscular hyperæsthesia, hysterical pains of the superficial muscles, or rheumatic pains. Twenty-four of the patients remained permanently cured after the first galvanization; 10 patients required a second application; 7 required three applications; and in 1 only four Faradizations were necessary. The current is applied as directly as possible to the muscles supposed to be affected, one pole, in the shape of moistened sponge, resting upon one point of the muscle, while the other, terminating in a metallic brush, is carried over the different parts of the surface.

X. *Memoir on a Partial Black or Blue Discoloration of the Skin of Females.* By Dr. Leroy de Mericourt. (Archives Générales de Médecine, Oct. 1857.)—The peculiar affection of the skin known as stearrhœa nigricans or cyanopathia cutanea, was first described by Mr. Teevan and Dr. Neligan.* Dr. de Méricourt collects the cases already observed, and adds five new ones which have fallen under his notice at Brest. These closely resemble those published by British authors in their general characters; and the view adopted by Dr. de Méricourt as to the bearing of the phenomenon is essentially the same as that put forward by Dr. Neligan. The following is an abstract of one of the cases most carefully watched by the French writer;—

* See British and Foreign Medico-Chirurgical Review, p. 248, July, 1855.

Marie X., aged twenty, of good constitution and nervo-sanguineous temperament, has always enjoyed good health, with the exception of icterus at the age of ten years; she has a vivid complexion, black hair, and greyish-brown eyes. Catamenia always regular and copious from the age of seventeen; only that an hour or two before the menses are established she has severe colic, which speedily ceases. Two months before coming under observation, without appreciable cause, three days after the cessation of the catamenia, which had been normal, she perceived a dark-blue tint on the lower eyelids; it resembled the circle produced by night-watching and fatigue; on the following day it became as dark as Indian-ink, and extended somewhat to the cheeks. With the exception of a sense of weight in the head, loss of appetite, and some nausea at the time at which the catamenia ceased, nothing irregular could be discerned. The sensibility of the eyelids was natural, they were soft to the touch. The colour became more intense in hot weather, during an effort, in emotions, or from fatigue; it was persistent during the night, but diminished a little towards morning. Examined by a lens, the colouring matter was found to be due to a multitude of small black grains scattered over the skin like an efflorescence; it accumulated especially in the furrows of the skin, and when wiped with a white handkerchief, the latter was blackened, but no wiping or washing restored the surface to its normal colour; the colour was a little softened, but speedily became as dark as ever. The ensuing catamenial period produced no change in the phenomena; the patient's general health was excellent. With the exception of the case just detailed, and of one in which the state of the uterine functions has not been recorded, all the instances collected by Dr. de Méricourt have exhibited uterine derangement. All occurred in young females of from sixteen to twenty-two years, before marriage; in eight out of nine, of which proper accounts have been preserved, amenorrhœa or dysmenorrhœa co-existed; nine cases occurred in maritime towns.

The author makes no specific suggestions with regard to treatment, but advises rather to abstain from the use of remedies than to run any risk of injuring the patient. He advocates the employment of all rational procedures necessary to restore the normal functions, if they are out of order, but agrees with Dr. Neligan as to the futility of employing local applications.

XI. *Carate, a Non-classified Disease of the Skin.* By G. Van Arcken, M.D. (American Medical Monthly, April, 1858.)—The name of this disease, of which the author has failed to find any description in dermatological works, is derived from *cara*, the Spanish for *face*, and *ate*, the Indian for *look;* the word, according to Dr. Van Arcken, meaning: look at his face. It occurs in New Granada and the northern parts of South America, and presents three varieties, the blue, the white, and the rose-coloured. The *blue* is the mildest, attacking persons of fifteen to twenty-five years of age, and consisting in the appearance of blue, round, or oval spots on the face. The spots coalesce, and extend down the neck on to the chest, where the ribs are often so distinctly marked as to cause the patient to resemble a zebra. The hands are a favourite seat of the disease, and again the lower end of the tibia. It sometimes appears on the glans penis, but the female organs of generation remain free. The *white* variety rarely occurs in the male; it is commonly accompanied by diseases of the ovaries and uterus; the colour is of a dead chalky-white, and attacks people from thirty to forty years. The *rose-coloured* variety is the worst kind, and frequently follows the white, in which case there appears on the chalky spots of the latter some very minute red spots, which gradually enlarge, until the whole assumes a pale red colour. Those affected with this disease are mostly Sambos, Mulattoes, and others of a dark complexion. It always commences on the hands, extends from them to the face and neck, and then down over the abdomen. It attacks both sexes equally. Dr. Van Arcken attributes

the disease to a combination of such influences as filth, exposure, syphilis, and insufficient and unwholesome food. Syphilis prevails so extensively, that scarcely one in a thousand inhabitants is free from it. It seems, therefore, to be a necessary complication of every disease occurring in those parts. Carate is regarded as incurable by the native practitioners; Dr. Van Arcken has found alterative mercurial treatment, iodides, and arsenic, successful remedies. The blue variety he cures in about eight weeks; the other two forms require a longer period; "but the complete cases, whether they be congenital or contracted afterwards, are better left alone." Carate is never fatal by itself.

XII. *On Elephantiasis as it exists in Travancore.* By EDWARD J. WARING, Esq. (Indian Annals of Medical Science, Jan., 1858.)—As physician to the Maha Rajah of Travancore, Mr. Waring, wishing to make the endemic diseases the object of his special inquiry, commenced his researches by an analysis of the cases of elephantiasis to which he had access. They were 945 in all. Of these, 273 were examined personally by Mr. Waring. The points of chief interest are as follows:

Sex.—There were 716 males, or 75·76 per cent., and 229 females, or 24·34 per cent.

Age.—139 occurred between the ages of five and twenty-five years, 729 between twenty-six and sixty years of age, and 77 after the age of sixty. Mr. Waring himself saw no patient affected with elephantiasis who was less than fifteen years.

Duration.—From the table indicating the length of time during which the disease had existed, it follows that it has but little influence in abridging life. No less than 218 cases had lasted from twenty-one to fifty-five years.

The size of the limb is not proportionate to the duration of the disease. The enlargement of the limb varied much. As a means of comparison, the measurements were all taken round the ankle, and they indicated from 8 to 24 inches, the average of 340 cases being $11\frac{7}{12}$ inches. Generally, the greatest enlargement was below the knee. The increase of size depended upon the frequency of the febrile paroxysms. "A man who has a violent febrile paroxysm once a month would, *cæteris paribus*, have a larger leg in five years, than another person who has only a mild attack once annually would have after a period of fifty years."

Date of first appearance of disease.—Since childhood, 16; five years and under, 7; between six and ten years, 33; between eleven and fifteen, 111; between sixteen and twenty, 222. This was the maximum. The proclivity to the commencement of the disease then gradually diminished up to the seventieth year, after which time two cases commenced.

Part affected.—Right leg alone, or with other parts, 307, or 32·49 per cent.; left leg alone, or with other parts, 287, or 30·37 per cent. Both lower extremities alone, or with other parts, 344, or 36·40 per cent.; other parts, 7, or 0·74 per cent. The upper extremities were only affected in 4 cases. The author urges forcibly the necessity of distinguishing between leprosy and elephantiasis, or between elephantiasis Græcorum and elephantiasis Arabum, the former being tubercular leprosy, the latter the comparatively mild and local affection under consideration. In 919 cases in which attention was paid to this point, Mr. Waring found leprosy co-existent in 52, or 5·65 per cent.

An intimate relation exists between fever and elephantiasis. Fever had existed in 224 out of 226 cases examined with reference to this point. In 2 only had it been absent. Mr. Waring regards the fever as the primary, the elephantiasis as the secondary disease. The latter does not necessarily impair the procreative powers; upon the question of hereditary transmission, the author has obtained no conclusive evidence, either affirmatively or negatively. There is no proof of any particular kind of food giving rise to the disease;

contagion clearly is not an element in its production. Mr. Waring inquires into the influence of locality, water, and vegetation, in producing the affection, and in the northern districts of Travancore, Shertallay, where it is particularly rife, he satisfied himself that the water was a principal cause of the disease; "the sea-water penetrating through the porous sand renders the water saltish and brackish, and as the generality of these pools are surrounded by trees, it in addition soon becomes loaded with dead vegetable matter, which, undergoing decomposition, renders the water dark (almost black), and highly offensive to the taste and smell. Even boiling and filtering fail to deprive it of its unpleasantness."

Treatment.—The close relation between febrile paroxysms and elephantiasis induces Mr. Waring to place great reliance upon quinine, which, if properly administered, he regards as the sheet-anchor in checking the malady. He also advises the employment of iron, iodide of potassium, and cod-oil, according to the features of individual cases. Externally he finds iodized oil (cocoa-nut oil four parts, iodine one part, iodide of potassium one part), rubbed in night and morning, of use, the limb being afterwards enveloped in oil-silk. It is to be remarked, however, that Mr. Waring's observations seem to be suggestive, rather than the result of actual experience.

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E., London.

I. *On the Treatment of Trichiasis.* By M. Anagnostakis. (Annales d'Oculistique, tome xxxviii. pp. 1–14.)

So frequent is this affection in the East, that among 1500 patients presenting themselves at the ophthalmic hospital at Athens, 92 cases were met with; while in some of the minor towns of Greece it constitutes a tenth part of the whole number of affections of the eye. In Egypt the proportion is still higher, rising to 16 per cent., and of these 16 patients it is found in both eyes in 13, in 2 confined to the right, and in 1 to the left eye. The frequency of the disease in Greece is explained by that of glandular blepharitis, which is very common (occurring in 113 of the 1500 cases above named); but this is not the case in Egypt, where blepharadenitis is extremely rare. The trichiasis must in that country be rather attributed to the chronic inflammations of the conjunctiva which are so prevalent there; and in the majority of the cases uncomplicated with entropion, seen by the author at Alexandria and Cairo, more or less severe and repeated attacks of conjunctivitis had preceded the trichiasis, care being always taken by him not to confound this with ophthalmia consequent upon the trichiasis.

This great frequency of the disease has furnished the author with the opportunity of performing, within the space of two and a half years, 350 operations for its alleviation. In relation to its treatment, trichiasis may be divided into two principal varieties. 1. Sometimes the long eyelashes piercing the anterior lip of the free edge of the eyelid, become incurved downwards, so as to touch the globe of the eye by their extremities. This variety has its seat almost exclusively in the upper eyelid, and the contact of the eyelashes with the globe only takes place when the eyes are widely opened. 2. In the majority of cases, a more or less considerable number of hairs issue irregularly from behind the anterior lip of the same edge, lying in contact with the globe throughout their entire length. The treatment in the first of these cases need be only palliative, and may consist in merely cutting off the ends of the hairs

wherever they come into contact with the cornea. But the best plan is to restore their normal direction to these hairs by curling them. For this purpose, the author has had a small instrument made much resembling the ordinary curling-iron. The eye is covered by a piece of paper slightly wetted and cleft in its middle. Through this aperture the hairs are all passed, and then seized by the warm iron and curled upwards. This procedure is repeated until the normal form is attained.

In the second form, which is far more frequent and more serious, palliatives are not merely useless but mischievous, the thin, short hairs which succeed to depilation inducing far greater irritation than those which have been removed. A radical operation should consist in one of these methods:—1. The eversion of the eyelid; 2. The partial amputation of the integuments; or 3. The extirpation of the ciliary bulbs.

1. For the purpose of everting the superior eyelid in an effectual manner, the author proceeds by producing what he terms *partial tension*. The eyelid being stretched on a disk of ivory, he makes an incision with a convex bistoury parallel to, and about three millimetres from, its free edge. The incision should not penetrate deeper than the skin, and when this is too abundant a transverse fold should be excised with the scissors. An assistant forcibly stretching the upper lip of the wound, so as to expose the orbicularis, the bundles of muscle which cover the upper segment of the tarsal cartilage are carefully dissected and excised by the scissors. The tarsus is therefore covered at the height of this second wound only by cellular tissue and by a fibrous layer, which proceeds from the aponeurotic expansion of the levator and from the broad ligament. Finally, three or four sutures are passed, first, through the lower edge of the cutaneous wound, and then through the fibro-cellular layer covering the denuded portion of the cartilage, and, having been tied separately, these are left till they are discharged. It results from this operation that the portion of the tarsus which has been denuded gives rise to a solid cicatrix, which unites the cartilage to the lower edge of the cutaneous wound. The upper portion of the skin, which soon unites with the wound, remains sufficient, and still forms folds during winking, whilst the lower portion, attached above to the cartilage, and raised by the remaining fibres of the orbicularis, which here perform the office of a pulley, is forcibly stretched, and permanently everts the palpebral edge. The author has found this plan succeed in even the most difficult cases. Relapse can only take place when the cutaneous band is too broad to exert sufficient tension upon the eyelid; and in such a case, it is only necessary to excise a small transverse fold. In slight cases, the author performs a much simpler operation, excising first a transverse cutaneous fold immediately above the palpebral border, and next some of the subjacent muscular fibres, and then letting the wound heal by suppuration.

2. In partial trichiasis, the best procedure is the partial amputation of the integuments. This, however, by shortening the external lip of the palpebral border, favours the production of ectropion. To obviate this inconvenience, the author comprises the cilia to be destroyed between two long vertical incisions which diverge a little upwards; and after the resection of a portion of the cutaneous flap comprised between these two incisions, he draws it downwards to about half a line beyond the free edge of the eyelid, where it is maintained by two points of suture during twenty-four hours.

3. The destruction of the bulbs the author regards as a very difficult procedure to effect with precision, and in the attempting of which we may sometimes do more harm than good. At all events, it should be confined to partial trichiasis, and even here others besides the hairs in fault are generally destroyed.

All that has been said above relates to the upper lid. When the lower one is the seat of the affection, the ordinary excision of the skin will suffice. Great care is, however, required, so that ectropion be not produced; and where the

ectropion of the lachrymal point would be the consequence, it is better to remain content with the repeated extraction of the cilia, which in the lower lid are very slowly reproduced.

II. *On Hare-lip Operations.* By M. GUERSANT. (Gazette des Hôpitaux, 1858, No. 24.)

"The more operations for hare-lip I perform," M. Guersant observed, at the Society of Practical Medicine, "the more I am convinced,—1. That in operating for simple or double hare-lip, at any age, the result is almost always favourable; 2. That the operation for complicated hare-lip, at whatever age it may be undertaken, is only exceptionally successful; 3. That in the operation, whether for simple, double, or complicated hare-lip, we almost always fail when a complication supervenes in the shape of disease of the skin or other part." He added that he generally operated early, but waited until six weeks or two months after birth, being able by that time to judge whether the child is lively and well nourished, and also to have it vaccinated, and thus obviate a mischievous complication. In support of these opinions, it is to be observed, that some cases of simple hare-lip operations practised a few days after birth have failed because the infants unable to nourish themselves fell into a state of languor and died. Moreover, a considerable number of children die within the first two months, even without having undergone any operation. If this is true of the operation for simple hare-lip, it is still more so with regard to complicated hare-lip; for in the latter, the mortality is greater in the earliest period of life than it is in simple hare-lip. Moreover, having to undergo a more laborious operation, these infants lose more blood; and as in some of them the operation has to be completed at several intervals, it is better to wait till they have attained the age of a year, when they are better able to support it.

Great care must be taken both before and after the operation as to the presence of other children who have or have just had any of the diseases of infancy, as hooping-cough, scarlatina, measles, &c.; affections likely to cause the failure of this, as well as of all other operations performed on infants. A child, thirty-two months old, with a double hare-lip, had been vaccinated by M. Guersant a few days before he intended performing the operation, and was carried to the door of one of the wards in which scarlatina prevailed, in order to furnish lymph for some of the children in the ward. The next day it was operated upon on one side only; but two days after it exhibited scarlatina, union failed, and the child died.

For some time past, M. Guersant has employed separate points of suture, having found that they less easily cut through the lips of the solution of continuity than the twisted suture. In the above case, although union was prevented by the scarlatina, the two points of suture being divided at the end of a week, it was found that the lips of the wound had not been cut through by the double threads which were employed.

III. *On Salivation in Syphilitic Patients.* By PROFESSOR SIGMUND. (Wien Medicin. Wochenschr., 1858, Nos. 5 and 6.)

During the last fifteen years Professor Sigmund has treated 8983 syphilitic patients by means of mercury. All the various preparations have been tried, and it has been found that the number of cases of salivation have much decreased during the latter portion of the period, in consequence of the greater attention that has been paid to the cleansing of the patients' mouths with gargles, the due regulation of diet, &c.

1. *What is the proportion of cases in which salivation occurred?*—Of the 8983 mercurialized patients, 520 exhibited it—viz., 139 among 4792 hospital patients, and 381 among 4191 private patients. This far smaller number occurring in hospital practice without doubt arises from the greater care taken of the patients, and especially from the attention paid to the mouth; and it has been found that in spacious, well-ventilated wards, supplied with careful nurses, the occurrence of salivation may be nearly prevented altogether. At the time of the author's writing, 157 cases were under mercurial treatment, without salivation having occurred in any one of them.

2. *What mercurial preparation is most liable to induce rapid and severe salivation?*—The author marshals the different preparations, together with the numbers respectively treated and affected by these. Our limits do not admit of these details, and we can only observe that calomel affected the mouth more frequently and more rapidly than any other preparation; while, united with cinnabar, and used in fumigation, it rapidly gave rise to excessive salivation. Blue pill, and especially mercurial ointment, which is the author's favourite preparation, acted far more mildly. Not only was there a much less proportion of cases of salivation observed in hospital than in private practice, whatever the form of mercurial employed, but these were almost all mild cases, the examples of excessive salivation being almost all observed in private practice.

3. *What circumstances favour the production or the prevention of salivation?*—First among the individual circumstances is the attention paid to the state of the mouth and fauces, neglect in keeping this clean being the cause of 80 per cent. of the cases of salivation. Due attention paid to this point will, in nearly all cases, enable the mercurial treatment to be uninterruptedly continued. The preparation employed is an influential circumstance, as we have seen by the active operation of calomel. A loose, spongy, separated condition of the gums, and the presence of bad teeth, seem, *cæteris paribus*, to exert but slight influence. Anæmia from ague, chlorosis or tuberculosis, does not seem to favour salivation; and even the lesser degrees of scorbutus have not occasionally prevented the use of mercury. Among the general circumstances favouring the production of salivation may be mentioned impure or defective air, insufficient warmth, and damp; and where these are combined with neglect of the mouth, the proportion of attacks of salivation become immense. Such cases are not unfrequently met with in private, as they may also be in hospitals when these are overcrowded during epidemic visitations. As a means of cleansing the mouth every conceivable substance has been tried in the Vienna Hospital, and as the result of prolonged experience, alum, borax, chlorate of potass, tannin, or tincture of opium, are now only employed. Gargles composed of some of these must be used every hour, half or quarter hour, and the attendant must not merely direct the patient to use them, but see that he does so. We should not wait for the commencement of the salivation to employ these, but begin to use them at once as soon as we prescribe the mercurials. Internal medicines, without the use of gargles, are of no avail whatever; but in combination with these, chlorate of potass or soda, or iodide of potassium, are useful.

4. *Is salivation an essential sign of the curative influence of mercury?*—Of the 8983 syphilitic patients treated with mercury, 8463 exhibited no salivation, and yet were just as permanently cured as those in which this occurrence took place. The author's multiplied and prolonged experience, indeed, enables him positively to state that salivation is neither a sign nor a condition of cure, and should, whenever possible, be prevented.

IV. *On Supra-Pubic Puncture of the Bladder.* (Gazette des Hôpitaux, 1858, No. 59.)

A discussion upon this operation recently arose at the Paris Surgical Society, on the occasion of the presentation of a memoir by M. Fleury, in which he stated he had often performed the operation with success, and considered it a very easy one.—M. Boinet regretted that the author had not stated whether his patients suffered consecutively from the adhesion of the bladder to the abdominal wall at the seat of puncture, and the consequent impediment to the functions of the organ.—M. Chassaignac could not admit the ill consequences said to result from these adhesions—such, indeed, not being observed in the case of the high operation for lithotomy, after which much more considerable adhesions take place. He did not, however, admit that puncture of the bladder was the simple operation it was stated to be by M. Fleury. When the abdominal parietes are very thin and the bladder is much distended, few precautions are necessary; but in very fat or very muscular subjects serious difficulties may arise. We have then to employ a very long trocar, and to plunge it in very deeply; and there is danger of wounding the opposite side of the bladder —an accident which has occurred to M. Chassaignac himself.—M. Robert considered puncture of the bladder as preferable to forced catheterism, and has had recourse to it seven or eight times. He does not fear the accidents attributed to the retention of the metallic canula in the wound. The substitution of a gum-elastic catheter for this is sometimes very difficult, owing to the contraction or deviation of the track made by the canula, and the attempts may give rise to painful laceration and to infiltration. He therefore leaves in the canula for a fortnight, and then substitutes a caoutchouc tube. He observed, also, that the urine should not be allowed to run continuously from the canula. This should be plugged, and only opened every three or four hours, otherwise the bladder, contracting too readily upon itself, may abandon the canula.—M. Deguise could not understand how any difficulty could arise in introducing a catheter by the track of a canula that had remained *in situ* for eight days. For his own part, he introduced a catheter on the first day, and changed it on the third or fourth, and he had never found any difficulty in so doing. He considered the operation a very easy one, providing that a preliminary incision be made down to the linea alba. He employs also a straight, in preference to a curved canula, the latter being liable to injure the *bas-fond* of the bladder or the prostate. The trocar is to be introduced horizontally, and a gum-elastic catheter is to be passed into the canula immediately on the withdrawal of the stiletto, and to be fixed *in situ* when the canula has been slid away upon it.—M. Huguier maintained that there were conditions which rendered this a very difficult operation, and among these is particularly the ascension of the prostate and *bas-fond* of the bladder. When the bladder becomes much distended, it rises, like the uterus in pregnancy, above the superior aperture of the pelvis; and in complete retention the fluctuation felt through the rectum, so much spoken of by authors, cannot be felt, because the bladder becomes raised to a point beyond the access of the finger. Under these circumstances the prostate may be wounded, and that when in nowise enlarged. This occurred to Richerand, who was a great advocate of the operation, and very skilful in its performance. To avoid this accident the straight should be preferred to the curved trocar, and this should be passed horizontally above the pubis, instead of, as is usually directed, downwards and backwards. He seldom removes the canula before the seventh day, and has never known a straight instrument excite any irritation of the posterior wall of the bladder. He however takes the precaution of introducing a gum-elastic catheter into the canula and fixing it there, so that its smooth, rounded extremity, furnished with its lateral eyes, may project at least a centimetre beyond the vesical extremity of the canula.—M.

Giraldès could not agree with M. Huguier in admitting this ascension of the *bas-fond*, the ease with which the organ may be punctured by the rectum showing that the relation of the parts is not changed. This rectal operation has very often been performed by Mr. Cock, of Guy's Hospital, who finds it of much easier execution than the supra-pubic puncture.—M. Lenoir has been struck with the facility with which some surgeons decide upon the performance of this operation. For his own part, although attached to the hospitals for twenty years, he has never had recourse to it but twice. He thinks it should be reserved for extreme cases, when the rupture of the bladder seems imminent. Then the bladder is very distended, and its puncture presents no difficulty. He entirely rejects the opinion of M. Huguier as to the ascension of the prostate. This gland is solidly fixed, and if it sometimes extends above its ordinary level, it is only because it has become hypertrophied. It is possible that when it is greatly hypertrophied it might become punctured by a curved trocar; but M. Lenoir never exposes himself to such an accident because he never punctures the bladder for prostatic retentions. Such an obstacle being permanent, there is no chance of re-establishing the natural course of the urine, and the patient is exposed to the inconvenience of a hypogastric fistula for the rest of his life. In such a case, forced catheterism, by means of a conical instrument, is far preferable. It is an intra-urethral puncture through the prostate; and a false passage, an intra-prostatic fistula, is produced that will fulfil the functions of the prostatic portion of the urethra. The operation completed, the conical catheter should be replaced by one in gum-elastic—a practice much preferable to leaving in the metallic instrument.—M. Huguier added that although the anterior portion of the prostate is fixed by ligaments and aponeuroses which do not allow of its rising, its posterior part is in fact drawn up during distension of the bladder. The finger cannot then feel the globular or cylindrical fluctuation which has been described as one of the principal signs of retention; but this does not prevent a long and concave trocar, directed backwards and upwards, from reaching the bladder.—M. Chassaignac also admits this elevation of the posterior part of the prostate. He thinks the preliminary incision of the integuments, as recommended by M. Deguise, might give rise to infiltration; and he rejects puncture by the rectum as dangerous, because of the risk of penetrating into the recto-vesical cul-de-sac of the peritoneum.

V. *Case of Exsection of the Entire Radius.* By Dr. Carnochan. (American Journal of Medical Science, April, 1858.)

Dr. Carnochan, the first surgeon who exsected the entire ulna, now records the first example of exsection of the entire radius. The patient was a strumous Irish labourer, twenty years of age, in whom disease of the fore-arm was set up consecutively to a severe blow. Believing the osteitis was confined to the radius, Dr. Carnochan resolved upon the excision of this bone in preference to the performance of amputation. In the progress of the disease, the elbow-joint had become stiff, the fore-arm being flexed at an obtuse angle. Chloroform was administered, and the operation was performed with great care, especially at the lower part, in order to preserve uninjured the different tendons proceeding to the hand. The greatly-increased size of the bone rendered its detachment more difficult, and the bony union it had contracted with the humerus had to be destroyed with the chisel. The shaft of the bone itself was also divided across the middle, in order to render its detachment at each end easier. None of the nerves or arteries were injured, the interosseous artery and two muscular branches alone requiring ligatures. The patient was discharged cured at the end of eleven weeks, and returning afterwards for some other slight disease, he was found to have so regained the use of his arm and hand as to be able to

perform the duties of orderly in a surgical ward. The fore-arm remains flexed at a convenient angle, and the deformity is less than might be expected. The power of the hand is not quite so great as formerly, but its functions are otherwise uninjured. He can carry a bucketful of water without difficulty, and can write with ease. Flexion and extension at the wrist are performed readily, and the hand can be brought into the prone or supine, abducted or adducted condition. The radius removed was found to be much expanded over its whole extent, and weighed seven ounces and a half, the weight of a healthy radius being about two ounces.

VI. *On Imperforate Anus and Rectum.* By Dr. Gay. (Boston Medical and Surgical Journal, vol. lvii., pp. 397 and 415.)

Dr. Gay, on the occasion of relating a case of unsuccessful operation for imperforate anus, passed in review the principal cases on record. These—104 in number, he arranges under the following heads: — 1. Imperforate anus, 16 cases, all operated upon, with 2 recoveries and 14 deaths. 2. Imperforate rectum, 26 cases and 19 operations, with 4 recoveries. 3. Imperforate anus with abdominal opening of the rectum, 24 cases, 14 operations, with 9 recoveries. 4. Anus natural, and rectum with abnormal opening, 3 cases, 1 operation, with recovery. 5. Imperforate anus and rectum, 25 cases, 23 operations, with 8 recoveries. 6. Imperforate anus and absence of rectum, 8 cases, 4 operations, 1 recovery. 7. Ditto, with absence of colon also, 2 cases, no operation. Thus, in a total of 104 cases, there were 77 operated upon and 27 not operated upon. Recovery took place in 25, and death in 52 of the cases operated upon.

The general conclusion is, that as nothing can be expected from nature in the way of substantial relief, while several of the cases operated upon have furnished encouraging results, an operation should be recommended, unless other complications or conditions of the system contraindicate it. The undertaking this should not be delayed too long, lest laceration of the intestine be produced. In place of employing a trocar, the author prefers making a free incision with a sharp-pointed instrument guided by the finger. The opening should, when possible, be made sufficiently large at first, as the tendency to contraction becomes much greater in subsequent operations. Where the obstruction is hard and firm, it should all be removed, if this can be done safely, as it frequently resembles cicatricial tissue. The main difficulty and labour consists in the after-treatment, owing to the disposition of the parts to contract. During the operation, we should use the finger as a director, and when it can be done, dissect away with this the intestine from the neighbouring parts. A probe should also be introduced into the bladder.

VII. *Case of Dislocation and Reduction of the Crystalline Lens.* By M. Mahieux. (Moniteur des Hôpitaux, 1858. No. 41.)

The subject of this case, a farmer, aged sixty-five, had lost his right eye, twenty years since, from intense inflammation and evacuation of the humours. The left eye had preserved its functions, when, fifteen months since, without obvious cause, vision began to be sensibly disturbed, objects being no longer so clearly perceived, and a veil seeming to conceal their upper part. This state of things continued a year, unaccompanied by pain or inflammation, when without blow or violence of any kind, a condition of nyctalopia suddenly came on, objects feebly illuminated could alone be perceived, and these when placed low down could not be seen at all. Objects, too, perceived in one position of

the head were lost sight of in another. This condition had continued four months, obliging the patient to abandon his occupation, and of late it had been accompanied by congestion of the conjunctiva, giving rise to some amount of pain.

On examination, the lower part of the anterior chamber was found occupied by the opaque and quite moveable lens, its upper border rising a little above the centre of the pupil. The tremor of the iris characteristic of synchisis was also present. A lens that had remained so long without undergoing diminution in size was not likely to undergo absorption, but although its extraction would not be difficult, it would be a serious operation to risk in a man who had lost one eye, and who still saw enough to guide himself about. It was determined, therefore, to attempt the reduction of the lens, or rather its passage into the posterior chamber. The patient was laid on his back, and the sulphate of atropine was dropped several times into the eye, motion being imparted to the head from time to time, in order to direct the lens towards the centre of the iris. This gradually passed into the posterior chamber, and when the patient stood up, the upper part of the lens was alone perceived in the lower hemisphere. Next day, the patient had recovered his vision completely, except that there were some muscæ volitantes observed. A bright light now favoured, in place of preventing vision. The report only comes down to a fortnight after the reduction, when the pupil was found to be normal in its action, the iris continuing tremulent.

VIII. *On Hospital Gangrene.* By M. Maupin. (Mémoires de Médecine Militaire, tome xx., p. 368.)

This paper is the result of M. Maupin's observations upon hospital gangrene as it appeared in the French army of the East, in which it committed frightful ravages. In reference to the conditions under which it may become developed, M. Maupin observes that overcrowding of patients has usually been considered a necessary condition of the production of the disease; but that at the hospital into which the Russian Embassy was converted at Pera, and which united every condition of salubrity—and among these ample space—gangrene broke out amongst the wounded officers seven days after the arrival of eight others who had been severely wounded, the wounds of the preceding occupants having been slight. Still, the gravity of the disease will generally be found to be proportionate to the amount of vitiation the air has undergone. The disease may indeed arise even in the open air when there is a considerable agglomeration of wounded soldiers, examples of which occurred to the author in Algeria. Overcrowding may be only relative, and a given number of patients that in the time of peace may be advantageously treated in a hospital, will in the period of war give rise to hospital gangrene. Not only do the numbers of the patients, but the gravity of the cases and the constant succession of such cases, increase the hygienic exigencies. The rule is, then, that the wounded soldier requires space and air in proportion to the gravity of his wound; and when hospital gangrene is once set up in a ward, the dispersion of the subjects of it is a measure alike beneficial to themselves and the other patients. As long as the medium remains unchanged, the treatment is but tentative, and the results are uncertain.

In order properly to appreciate the instability of the results of treatment, we should bear in mind that, if epidemic hospital gangrene may be an essentially local affection, it is frequently during a campaign but the expression of a general modification of the economy, of a true intoxication, the energy of which, intimately dependent upon the salubrity of the locality, and the number and nature of the wounds treated therein, is increased or diminished, revived

or extinguished, with the increase or diminution of the number of the patients, with their agglomeration or dispersion. Means which in isolated cases of gangrene may act heroically—as the actual cautery, sulphuric acid, perchloride of iron, and, in milder cases, citric acid, carbon, iodine, &c., are in the epidemic form either powerless or only of temporary benefit. It is this which explains the differences of opinion that prevail on the treatment of this disease.

In respect to the local affection considered separately, we must recognise an acute and a chronic form, a distinction of importance as regards treatment. The acute form is denoted by the conversion of the tissues into a pulpous or putrilaginous magma, and which, as soon as the destructive process is arrested, becomes detached through a series of eliminatory processes. The ulcerative is the chronic form of the disease. The putrilaginous form is almost always primary, and is found almost exclusively in recent wounds; while the pulpous form, though often also primary, is also often met with in old wounds, and it does not pursue the same rapid and destructive course as the putrilaginous form. The ulcerative form is almost always consecutive to the two other forms, appearing especially in the case of old wounds, or after amputations these have necessitated. In the putrilaginous and pulpous forms the most appropriate measures are those which hasten the separation of the parts affected, and stimulate the reparative process. Of these, the actual cautery, sulphuric acid, and the perchloride of iron occupy the first rank. In a hospital in which there are at least 300 cases of hospital gangrene at the same time, the application of the actual cautery is made with difficulty, while, although it is just as painful as the other means, it does not adapt itself so readily to all the situations, forms, and depths. The sulphuric acid has been found by the author preferable to it and to the nitric acid, while other practitioners give the preference to the perchloride of iron. The eschars once separated, styrax and aromatic wine proved the best of topical applications; while, when cicatrization was delayed, or there was a tendency to substitute the ulcerative form, citric acid, camphor, cinchona and carbon, tincture of iodine or nitrate of silver, and, if necessary, nitric acid, imparted a new and favourable impulse to the cicatricial process. The formation of a crust or magma on the surface of the sore by means of some of the above powders has been found to encourage the healing process, and to much diminish the patient's sufferings. For the chronic form the perchloride of iron is better adapted than the sulphuric acid. But let the local measures employed be what they will, success will be impossible or transient, unless the general conditions and the necessity of removing the patient from the infected medium be borne in mind.

IX. *On the Treatment of Carbuncle.* By Dr. Gutzeit. (Medicin. Zeitung Russlands, 1858, No. 12.)

Dr. Gutzeit of Riga recommends the following ointment as the sole treatment of simple carbuncle—viz., half a drachm of opium mixed up with two ounces of white ointment, spread as thick as the back of a knife on linen rag, and applied to the tumour and its circumference three or four times daily. He says he cannot feel surprised at the incredulity with which his recommendation will be received; but having in numerous cases derived great benefit from the employment of this means, he must insist upon its utility. Relief takes place in some cases with remarkable rapidity, the dreadful pain becoming sometimes supportable after half an hour, while the various stages of the affection are expedited. The general symptoms, too, undergo a corresponding amendment. Moreover, the means is applicable to any stage of the affection, from the commencement of tumefaction to the separation of the eschar.

X. *Fracture from Muscular Contraction.* By Dr. Benedikt. (Wien Medecin. Wochenschrift, 1858, No. 12.)

Dr. Benedikt observes that in most of the cases of fracture produced by muscular contraction, pathological processes have rendered the bone liable to the production of solution of continuity. In such cases a slight contractile effort suffices to induce the fracture, and that repeatedly in the same bone. There still are cases, however, in which an entirely healthy bone may be fractured solely by muscular contraction. Two such were treated in the surgical division of the Wieden Hospital, near Vienna, during 1856. The first occurred in a healthy young weaver, sixteen years of age, while in the act of quickly raising his arm during snow-balling; an oblique fracture at the upper third of the right humerus, manifesting all the usual signs, having been produced solely by muscular contraction. After thirty-six days of treatment, the fragments were found to be completely united. The second case happened in the person of an engraver, aged twenty-eight, who had always enjoyed good health. The accident occurred while he raised his arm in order to throw a stone, the humerus being fractured transversely at about its middle. Reparation was effected in the usual time.

XI. *On Caoutchouc Bandages in Fractures.* By Dr. Edward Zeis. (Deutsche Klinik, 1858, No. 14.)

Dr. Zeis had already reported highly in favour of these bandages, and two years' additional employment of them in the Dresden Hospital has only confirmed his good opinion. They are especially useful in the œdematous swellings left after ulcers, in varix, and in general whenever pressure is advisable—as when the gravitation of pus is desired to be prevented, and uncertainty of progression remains long after subluxations. They succeed better than any other description of bandage; for if the swelling, under the influence of their gentle pressure, subsides, they still remain closely applied, while, when it increases, they gradually yield.

After several years' trial of the gypsum bandage in fracture, Dr. Zeis has come to the conviction that the praise bestowed upon it is undeserved. It is beautiful in appearance, and many prize it because it allows of the patient going about. The advantage of this going about on crutches, with the limb suspended, is very questionable; the foot hanging down becomes swelled, and the pressure of the unyielding bandage soon obliges the patient to return to bed. Moreover, however firmly this bandage be applied, it always becomes looser on drying, so that two fingers can be inserted between it and the limb. Hence, the cures by its agency are not satisfactory. It is also removed with great difficulty. The author having broken his own arm, has experienced the sensation both of a too tight and a too loose bandage; the first is insupportable, and the other is constantly reminding one of its uselessness, and of the danger of displacement. The elastic bandage alone can be so adapted as to produce neither of these effects; and when once put on properly, it may be safely left on without renewal longer than any other. Whenever the author has renewed it after a week or a fortnight, he has had to regret meddling with it, so well had it continued applied.

As to the mode of application, a piece of damp linen is first applied over the bare skin, so as to lie nowhere in folds, and in the case of excoriations, these being first covered with mild ointment; and over this is laid a layer of soft wadding or cotton. The splints may be the same as in other modes of treatment, those of gutta percha being preferred in all those parts in which the thickness of the limb suddenly diminishes. In some cases the splints have

been kept confined by strips of caoutchouc four inches broad, first moistened in warm water. Usually, however, the caoutchouc bandage is applied next the splints. Dr. Zeis has never known his patients complain of undue warmth, produced by the obstruction of transpiration.

XII. *Case of Sudden Death after the Operation for Hare-lip.* By Dr. Volkmann. (Monatsschrift für Geburtskunde, Band xi., p. 353.)

An unusually strong and powerful boy, a twelvemonth old, was operated upon by Dr. Volkmann, at two different periods, on account of hare-lip, with double fissure of the jaw and hard palate. On the first occasion, the coaptation of the bony parts was commenced, and the result of the operation and of four weeks' after-treatment being favourable, it was determined to proceed to the union of the rudiments of the lip. During the application of the last needle the child became suddenly of a blue colour, and respiration ceased. Examination showed that neither mucus nor blood obstructed the pharynx; but as the child spasmodically closed the mouth, and seemed about to expire, all the sutures were at once removed. The child now took a deep breath, and recovered itself. As its condition four and a-half hours afterwards was found quite satisfactory, the uniting the parts was again undertaken with great circumspection. No ill consequence followed, and the child lay with its mouth wide open, and freely breathing. Dr. Volkmann then left his patient, but in less than three hours was sent for, as suffocation was again impending. He was on the spot two minutes after, and removed the sutures, but the child was dead, and all attempts at his recovery proved fruitless. Dr. Volkmann, in communicating this case to the Berlin Midwifery Society, referring to Busch's opinion that those children accustomed to breathe with closed mouth owing to the wide nasal fissure, continue from habit to do the same after the operation, and thus induce suffocative paroxysms. Dr. Gurlt had observed several similar cases; and Dr. Biesel drew attention to Langenbeck's precaution of always stationing an experienced nurse near the little patient, with the direction, immediately on the appearance of any difficulty in respiration, to open the mouth and press the tongue down with a spatula.

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D. Lond.

PHYSICIAN TO THE ROYAL MATERNITY CHARITY.

I. Physiology and Pathology of the Unimpregnated Female.

1. *On a peculiar Discoloration of the Skin in Females.* By Professor Banks. (Dublin Quarterly Journal, May, 1858.)
2. *Anatomical and Anatomo-Pathological Studies on the Statics of the Uterus.* By Dr. F. A. Aran. (Arch. Gén. de Méd., February and March, 1858.)
3. *Apparatus for Ligature of Uterine Polypi.* By Dr. Dittel. (Wochenbl. der k. k. Gesellssch. der Aerzte zu Wien, July, 1857.)
4. *On the Use of Purgative Enemata in the Treatment of Uterine Catarrh.* By Dr. Aran. (L'Union Méd., May, 1858.)
5. *On the Linear Ecrasement of the Cervix Uteri.* By Dr. Breslau, of Munich. (Monatsschr. f. Geburtsk., January, 1858.)

1. Dr. Banks has contributed another case of that singular black discoloration of the skin which Dr. Neligan some time back described under the name of

"Stearrhœa Nigricans," and by Dr. Billard (Arch. Gén. de Méd., 1831) "Cyanopathie Cutanée." The memoir of Dr. Banks may also be referred to as being an excellent epitome of the history of this curious affection. The following is a sketch of the case observed by himself:

An unmarried female, aged twenty-three, was admitted into the Richmond Lunatic Asylum on the 17th of Sept., 1853. She had experienced a fearful mental shock from being suddenly shown the hanging body of her lover, who had destroyed himself in a fit of insanity. She became maniacal, with suicidal tendency. Soon after admission she had an attack of erysipelas of the face. After several months of amenorrhœa, she became the subject of periodical hæmoptysis, evidently vicarious. When seen by Dr. Banks she exhibited a remarkable discoloration around the eyes, but chiefly beneath the nose; the parts looked as if they had been painted with Indian ink, or rather with Prussian blue. This has existed since the cessation of the catamenia, but it becomes more vivid during the periodical hæmoptysis. In the midst of her insane preoccupations, she is at all times most sensitive upon the subject of this discoloration.

Dr. Banks thinks the cases that have been described cannot be included under one nosological appellation. The most general feature appears to be some disturbance in the menstrual function.

2. The researches of Dr. Aran into the statics and means of suspension of the uterus form an important contribution towards the settlement of this vexed question. He observes that modern anatomists maintain that the uterus is kept in position by several ligaments—namely, anterior, or *vesico-uterine;* lateral superior, or *round;* lateral proper, or *broad;* posterior, or *recto-uterine*, ligaments. Latterly, Virchow has insisted with much force and reason on the part performed in the suspension of the uterus by the adhesion of the uterine neck to the bladder. Our space will not allow of the fulness of detail required for anatomical description. We can but summarize the author's conclusions. The posterior ligaments do not stop at the middle of the sacrum. Diverging over the sides of the rectum, they terminate in very fine filaments, which lose themselves in the sub-peritoneal cellular tissue from the middle of the concavity of the sacrum, as far as the body of the last, and even the penultimate lumbar vertebræ. If the uterus is lifted by its fundus upwards and forwards, the posterior ligaments are seen to be stretched, and become prominent under the peritoneum. If the uterus is depressed from above downwards, or if it is attempted to drag it down by the neck towards the vulva, the posterior ligament is not stretched at first; it is only when the rectification of the curve described by it is made that the ligament begins to stretch, and strongly resists traction. If the neck of the uterus is pushed backwards in the vagina, or the fundus is strongly depressed forwards, the posterior ligament is relaxed; but if the neck is drawn strongly forwards, or the fundus pushed backwards, the ligament is immediately stretched, and starts up under the serous membrane. There cannot, therefore, be a doubt as to the use of the posterior ligament. It serves to oppose, up to a certain point, the descent of the uterus, and to preserve the uterine neck in the posterior part of the pelvis.

The use of the so-called *broad ligaments* is merely to give passage to the vessels to hold the round ligaments, Fallopian tubes, and ovaries; and to maintain by lateral support the uterus in the centre of the pelvic cavity.

The disposition and uses of the *round ligaments* deserve to fix attention. The round ligament is nothing more than an emanation from the proper tissue of the uterus. If the external inguinal ring is exposed, and the uterus moved, its movements are seen to be communicated to the uterine cord, the fibres of which may thus be traced into the cellular tissue of the external genital organs. If the cord is pulled upon from the ring, the uterus is partly rotated

on the corresponding side and bent forward. If both cords are pulled upon at the same time, the fundus of the uterus follows the diagonal of the two opposing forces, and is brought directly forwards. In this manner the most complete anteversion can be produced. These cords do not, then, suspend the uterus in the pelvis, they help to maintain the fundus forwards, and especially to limit its movements in the antero-posterior direction.

M. Aran attaches little importance to the supporting power assigned to the vagina. He agrees that the experiment of Stoltz, who observed that the uterus kept its position when the vagina was cut away, is decisive. Without discrediting the utility of the operations undertaken for the relief of prolapsus, he questions whether these, while acting on the vagina, have not often also extended their action to the utero-sacral ligaments and cellular tissue surrounding the vagina.

M. Aran cites with assentation, the observations of Paul Dubois, Forget, and Virchow, on the connexion between the uterine neck and the neck of the bladder. The uterus participates necessarily in all the changes experienced by the bladder. But he does not agree with Virchow, in regarding this adhesion as the principal means of suspension of the uterus. What gives especial importance to this adhesion, is the fact, that at this point is also inserted the vagina, whose dartoid layer is lost in the cellular tissue which binds the uterus to the bladder, at the same time that it runs into the superficial longitudinal fibres of the uterus. Nearly at the same level, behind, the dartoid fibres of the vagina confound their insertions with those of the posterior ligament. Hence there results for the uterine neck, a true ring, comprising in front the adhesions of the *bas-fond* of the bladder to the uterus, the insertion of the vagina on the neck; behind, the posterior ligament, the insertion of the vagina upon the neck. This point of union, M. Aran proposes to call the *axis of suspension* of the uterus, in opposition to what has been described as the *axis of the organ*, and which is only its *axis of inclination*, or the properly called axis of suspension. It is upon this point that all the forward, backward, and side-to-side movements of the body of the uterus are effected. Moveable in every other part, this point is truly that upon which the uterus reposes.

In the second part of his memoir, Dr. Aran proceeds to give the result of researches into the frequency of deviations of the body of the uterus, and their causes. His conclusions are mainly drawn from the post-portem examinations of 37 women—a number too small to secure against error. Anteflexion and antecurvature were found in half the cases, and this frequency is peculiar to youth and multiparity. Of 21 women who had had children, 7 only presented anteflexion. In reference to what M. Cruveilhier has called the *indifferent* position of the uterus, M. Aran says that in 5 cases he observed the greatest facility of displacement, so that when the body was laid upon the back, and the intestines lifted up, the uterus immediately fell back into the concavity of the sacrum. As to the causes of anteflexion, the author cites Huguier, who has shown that in fœtal life the body of the uterus is a mere small membranous sac, with thin walls, greatly contrasting with the more developed and rigid cervix, and therefore easily bent upon this latter by weight of the abdominal organs, the anterior flexion being determined by the sus-pubic cords. He concludes that anteflexion is congenital. Thus, he says, under the influence of sexual congress and pregnancy, there is a tendency to the removal of this anteflexion. He shows, by measurements of the neck and body at different ages in virgins and multiparæ, that there is a progressive alteration in the relative length of these two component parts of the uterus; that the body enlarges at the expense of the cervix. This tends to diminish the angle formed by the body with the neck, and to convert anteflexion into antecurvature. Pregnancy especially tends to straiten the uterus. Dr. Aran's

final deduction is, seeing the physiological origin of anteflexion, that mechanical means of rectification are uncalled-for and dangerous.

3. Dr. Dittel proposes to add another weapon to the cumbrous armamentarium provided against uterine polypi. It is especially designed in order to combine, what is not seldom wanted, a grasping and down-dragging power, with the apparatus for strangulation. The complete instrument nearly resembles the ordinary obstetric forceps. But one blade, which is made to separate from its handle, is pierced at the extreme end by a small hole for the transmission of a loop of silk—the ligature; and has at its lower end a screw-nut, also perforated, to receive the ligature brought down. The loop is first passed over the tumour, the ends are passed through the screw-nut, which, being turned, tightens the ligature; the blade then fixed in its handle is brought in apposition with the other blade. The two, when locked, grasp the tumour, and serve to bring it away, when the neck is cut through. [The difficulty of passing the loop round the pedicle of the tumour must be greater than with Gooch's double canula. The only advantage seems to be to obviate the inconveniences sometimes encountered with large polypi, of extracting them from the vagina after separation.—Rep.]

4. Dr. Aran reports that he has obtained the most satisfactory results from the use of purgative enemata in cases of uterine catarrh. He insists that the cases selected for the application of this remedy be cases of chronic leucorrhœa without active inflammatory complication, and that the discharge proceed from the uterus.

The mode of administration recommended is, every day or every other day, according to the effect produced, at bedtime, to use a lavement of tepid water to clear out the bowel, and then an enema prepared of aloes and soap, of each five to ten grammes in one hundred grammes of boiling water. This is mostly retained until the following morning. In four or six days, says Mr. Aran, the uterine discharge is commonly arrested.

5. Dr. Breslau records 4 cases in which he removed all or part of the cervix uteri by the écraseur of Chassaignac. In 3 cases there was carcinoma, and in 1 chronic hypertrophy and induration. Of the 3 first, both lips were removed in 2 cases; in the third, only the anterior lip was removed. Two of the patients with cancer were quite well at date of report, without trace of local relapse. One case was operated in April, 1856; the other in September, 1856. In the third cancerous case all the disease could not be removed; the indication sought was to restrain the bleeding. A temporary amendment followed; but a fatal result is anticipated. The case of hypertrophy was quite cured, and remained well seven months after operation. In all the cases the bleeding was very trifling. In 2 cases an accident occurred very suggestive of caution. In tightening the écraseur-chain a piece of the anterior vaginal wall was caught and pinched off. In one case a hole was made, through which a piece of intestine protruded, which is not reported to have closed.

II. Physiology and Pathology of the Pregnant and Puerperal Female.

1. *On Certain Points in the Anatomy and Physiology of the Mucous Membrane and Epithelium of the Uterus during Pregnancy.* By Dr. Ch. Robin. (Dr. Brown-Séquard's Journal de la Physiologie, Jan. 1858.)
2. *On the Glycosuria of Puerperal, Suckling, and Pregnant Women.* By Dr. Riedel. (Monatsschr. f. Geburtsk., Jan. 1858.)

Dr. Ch. Robin's memoir on the uterine mucous membrane contains several points which we feel it a duty to extract. He observes that the general belief, that the *inter-utero-placental decidua* is carried away by the placenta, as is the rest of the decidua by the chorion, is not altogether correct. The chorion carries away with it the decidua uterina and decidua reflexa, which adhere together and to itself. The internal aspect of the muscular coat is left, lined by the new substituted mucous membrane which has already begun to grow. This forms a thin, soft, rosy layer, moulding itself on muscular fibres, and leaving them sometimes quite bare, when torn, and at any rate visible by transparency. The free surface of this developing mucous membrane looks a little irregularly villous when examined under water. But in women dying seven or eight days after labour, it has already assumed a smooth surface, although its thickness is very little increased; it is somewhat shining, as if glutinous, and as yet too soft to be separated by dissection from the underlying coat. It is also seen that the placenta carries away with it the *circular sinus* which surrounds it, and which is slightly exterior to it, rather than *inter-utero* placentary. It carries away at the same time scarcely the half of the *serotina*, or *inter-utero-placentary decidua*, because it tears through about the middle the sinuses which traverse this membrane. Nevertheless it retains in this thin portion of serotina some portions of sinuses still entire, especially in the neighbourhood of the *circular sinus*, and in the sulci which separate the cotyledons. Thus the placenta, when it separates itself from the uterus, does not carry away the entire *inter-utero-placentary decidua*, as it does in the case of the remaining *decidua vera*. It leaves the greater part adhering to the uterus. The placenta exhibits the surface of its cotyledons covered by a greyish semi-transparent soft membrane, varying a little in thickness in different subjects. Sometimes this membrane is smooth—sometimes rough. It does not present in its thickness any vessels comparable to the circular sinus, nor consequently to the sinuses of the serotina. This layer or membrane detached from the serotina by the placenta is represented by the thickened epithelium of the *inter-utero-placentary* portion of the uterine mucous membrane. Thus it is chiefly constituted of epithelial cells which have undergone a considerable hypertrophy of the body and nucleus, as well as odd deformations. [These cells of the maternal membrane are figured in Dr. Barnes's first memoir, On Fatty Degeneration of the Placenta, 'Medico-Chirurgical Transactions,' vol. xxxiv.] The membrane also includes amorphous matter and different molecular granulations. It may further be observed, that at the periphery of the placenta, in approaching the decidua, it is continuous with that portion of this membrane which adheres to the chorion rather than with that surface of the decidua which is just detached from the uterus. The existence of this membrane, which is constant, demonstrates a series of very important facts. Thus the placentary villosities do not plunge freely and directly into the large sanguineous sinuses of the serotina. The ramifications of the villi traversed by the fœtal blood are separated from the blood of these sinuses by the thickness of this membrane, and by the very thin membrane of the sinuses of the serotina.

The internal surface of the uterus at the seat of the placenta after the separation of this organ retains, as has been said, the decidua, with the exception of the thin superficial (epithelial) layer, carried away by the placenta. Indeed, this portion of the uterine mucous membrane, remaining rich in vessels dilated into sinuses, has not become deciduous, at least in a direct manner. This is owing to the circumstances that its vessels are continuous with those of the muscular wall, and that being vascular, it has continued to nourish itself. This peculiarity strikes the more because we are always astonished at seeing, on dissection, how slightly the proper tissue of the mucous membrane which forms this membrane is compared, with the diameter of the large sinuses between which it is placed in the shape of thin layers or bands of separation. This

tissue is reddish, deeper than the proper decidua. Lastly, and this fact is important, there has not been produced between the serotina and the muscular coat of the uterus a new thin substitution-mucous membrane, such as exists between this latter and the decidua vera. It is only little by little, during the lochial discharge, that it exfoliates and is eliminated. Then only is the serotina replaced by the thin substitution-mucous membrane.

In the woman dead some days after delivery, the remains of the inter-utero-placentary decidua are found, forming a layer much thickened by the contraction of the uterus, which throws it up into a smaller area. From the projecting irregular borders of this membrane is continued the new thin mucous membrane which clothes the rest of the uterus. This last is rosy, generally smooth, a little shining, except when blood or bloody or purulent mucus covers it. On the contrary, the thick layer of the serotina is rough, as if tuberculous, and irregularly mamelonated. It is easily scraped off; its colour is reddish or greyish, verging upon black; sometimes on the summit of the elevations of the surface it assumes a grey tint, from true mortification. It is not rare to perceive at the surface of this layer some vascular orifices, plugged by fibrinous coagula. If traced by scissors, we are soon led into the sinuses of the muscular coat. We are struck by the areolar cavernous aspect given to this layer by the numerous anastomoses of these large vessels, when opened. In autopsies of puerperal fever, this rough, blackish, pultaceous layer has been mistaken for remains of placenta adhering to the uterus, and in process of decomposition.

Second Part. On the modifications undergone by the epithelium of the body of the uterus during gestation.—The epithelium of the uterine neck preserves, as is known, its cylindrical state, losing only the vibratile cilia on the most of its cells. It is not so with the epithelium of the cavity of the body. Having examined several gravid uteri of 2, 2½, 3, 5, 6, and 7½ months, I have ascertained that this epithelium passes gradually from the cylindrical, or rather prismatic form, to the pavimentous condition. There is no fact to prove that it is the prismatic cells which directly assume the pavement-form. Everything shows that a certain time after fecundation the epithelium of the cavity of the uterus exfoliates cell by cell, or by small shreds, then what replaces it is an epithelium, pavimentous, of large cells. These have a spherical or slightly ovoid nucleus, about the size of a blood-globule. Deep yellow granulations almost completely fill the mass of the cell. Many cells are wanting in nuclei, and are uniformly filled with these yellow fatty granulations. This condition of the cells is met with from the sixth week to the second month, both on the decidua vera and reflexa. From two months and a half there are seen in addition many other cells, much larger, and especially much longer. Their nucleus is larger, they are pale, little granular. These large elongated irregularly-shaped cells go on increasing in relation to the first according to the advance of gestation, and are far the more numerous near the term.

2. Dr. Riedel has tested the propositions of Dr. Blot relative to the physiological occurrence of sugar in the urine of puerperal, suckling, and pregnant women. Dr. Riedel examined the urine of eleven puerperal women, and three pregnant. The urine of the first class was taken at periods varying from twelve hours to twelve days after delivery. Two of these women were not suckling. Of the three pregnant women, one was in the sixth month, the other two apparently in the last month. The tests employed were boiling with liquor potassæ, and Trommer's. The result was negative in all cases.

III. Labour.

1. *Two Cases of Infants born Asphyxiated, Reanimated by Marshall Hall's Method.* By Smith Warner, M.D., Ga. (Charleston Medical Journal, Jan. 1858.)
2. *Difficult Labour from Membranous Closure and Rigidity of Os Uteri—Incisions.* By Professor Hohl. (Monatsschrift für Geburtsk.)
3. *Several Cases of Placenta Prævia Centralis, treated after my Method.* By Dr. Cohen. (Monatsschrift für Geburtsk. Nov. 1857.)

1. Dr. Warner, of Lumpkin, Ga., U. S., records two cases of asphyxiated infants restored by the Marshall Hall method. In the first case it is reported that the cord was pulseless, but it is not shown that respiration was *in*excitable. The rotations were successful in thirty minutes. In the second case, blowing on the chest and dashing cold water proved useless. The respiration was therefore *in*excitable. Rotation was quite successful at the end of thirty-five or forty minutes.

2. A woman, aged thirty-seven, very large, but of relaxed habit, who had aborted once at four months, and again at six months, had reached the normal time of gestation. Pains came on with great violence towards the morning of the 28th July, lasting till midday, when the waters broke, and after a short pause the pains returned with still greater violence. It was now found that the uterus was strongly inclined at the fundus to the right side, and contracted regularly at each pain. It was remarkable that in the interior wall, right under the navel, was an apple-sized swelling of harder consistence than the uterine wall, apparently a fibroid. The parts of the child were plainly felt, the fœtal pulse and placental rush were heard on the left side. The soft passages were dry and unyielding. The os uteri gave passage to the finger. This continued all through the night, the head only being driven a little lower down. On the following day, no advance was perceived; but it appeared as if a thin membrane were stretched between the two lips of the os uteri, preventing its expansion. This was therefore divided, and immediately the os opened somewhat. Its edges remained very sharp. This condition remained throughout the day, notwithstanding resort to cramp-allaying remedies, and warm vapour-baths to the vagina. The head continued to be forced lower down, and the lower segment of the uterus was yet more strongly distended. On the morning of the third day, intense pain appeared on touching the upper part of the uterus; and the hitherto tranquil pulse began to rise. It appeared that longer continuance of labour was undesirable. Fresh incisions were therefore made in the whole circumference of the os, with the slight good effect of leading to a dilatation of the size of a dollar. But there progress stopped. The child died, and began to putrify; gases were developed, and escaped during pains from the uterus. Fresh dilatations were therefore made in three directions, three hours after which a further expansion of the os was manifest. Thirty-six hours later the forceps was applied, and the head delivered. The mother fell ill with phlebitis of the left crural vein, which, however, subsided, and she recovered. [In this country, the labour would have been shortened by forty-eight hours at least by the resort to craniotomy, which, as the child was dead, was an operation eschewed without adequate reason in the present instance.—Reporter.]

3. Dr. Cohen relates several cases of placenta prævia centralis, treated by the method of partial detachment, and conversion into placenta lateralis. This he does in reply to an expression of regret uttered by Dr. Barnes, in December, 1855, that his memoir detailing his views embraced no clinical illustrations.

Dr. Cohen's first recorded case occurred in December, 1856. Madame T—— was delivered on that day, having expected delivery at the end of January. The first flooding appeared in the last week of the sixth month, six weeks before delivery. From this time, about every eight or fourteen days, profuse floodings returned, but were quickly suppressed by recourse to horizontal position. On the 1st December, watery discharges took place (it could not be ascertained that these were amniotic). On the 3rd December, slight pains came on. On the 7th stronger pains, and more copious bleedings. At nine P.M., the os uteri was found slightly dilated, centrally covered by placenta, pains inconsiderable, pulse small, face anæmic; no movements of fœtus felt for some time. It was found that the smaller flap of placenta was attached to left wall of uterus; the mass, therefore, lay on the right side. The smaller left flap was detached by the finger. The bleeding was immediately and definitively arrested. The arm and cord were presenting. The os dilated more and more, and the pains increased. After two hours and a half, the child was turned, and easily delivered. The child had long been dead: there were marks of decomposition. The patient did well.

2. Madame P—— was delivered, on the 13th January, 1857, at the normal term. Flooding broke out suddenly at three P.M., no bleeding having occurred before. The flooding was profuse, with faintings. First seen at five P.M. The os was but slightly opened. Plug inserted. The bleeding, however, ceased for a short time. Soon strong pains came. Plug removed. The os was now more dilated. The smaller flap was on the right side, the mass on the left. The smaller flap was detached by the finger. The bleeding entirely ceased. The labour proceeded naturally, as in ordinary head-presentation, without a drop of blood. At ten P.M., a dead child was born. No fœtal movements had been felt all day. Puerperal week normal.

3. Madame M——, delivered on the 27th January, 1857, within fourteen days of last month of gestation. In the first week of the eighth month—that is, six weeks before delivery—floodings occurred several times. From the end of the eighth month—that is, fourteen days before delivery—floodings were renewed several times in a day, but not in great quantity, and were lessened by horizontal posture. Five days before labour, violent flooding, accompanied by slight pains, so as to require a plug. The os was closed, the cervix not obliterated. Flooding ceased for three days, then patient complained of strong pains. The plug was removed; but Dr. Cohen thinks it would have been better to leave it until the pains should expel it, for the bleeding stopped, and there was no internal hæmorrhage. The cervix was now obliterated, and the os open to the size of a shilling. No fœtal movements felt throughout, nor fœtal pulsation heard. The bleeding returned more strongly after the plug was removed. The smaller flap of placenta lay on the right. The os was now softer. This flap was detached by the finger. The bleeding instantly ceased. The os uteri, however, contracted rather than expanded. It is to be remarked that Madame M—— had all her life been subject to spasmodic hysterical affections. Not urged by flooding, and satisfied that the child was dead, Dr. Cohen felt he might safely wait. Twelve hours before delivery, the greater flap of the placenta was detached spontaneously, and the whole placenta was driven by the pains into the vagina. The os uteri dilated tediously under nervous symptoms peculiar to the patient's constitution. In twelve hours, delivery was effected by turning, the child's shoulder presenting. The patient did well.

[It is to be remarked—1st. That in all these cases hæmorrhage took place although the child was dead; so that these instances may be added to others recorded by the Reporter, to negative the explanation offered, that when the

flooding in placenta prævia ceases spontaneously, it is because the child is dead. 2ndly. That, concurrently with the artificial partial detachment of the placenta and the arrest of flooding, decided uterine contraction set in. 3rdly. That, the flooding being arrested, time was gained for the normal progress of the labour, so as to admit in two cases of turning and the passage of the child, without use of dangerous force. 4thly. That in the last case the placenta was wholly cast off and expelled before the child; but that the cessation of the hæmorrhage dated from the *partial* cervical detachment; but for the accurate observation of Dr. Cohen, this case might be cited as a proof of the efficacy of total detachment in arresting hæmorrhage, as many similar cases have been, which, if rigidly analysed, would simply prove the hæmostatic agency of cervical detachment and contraction. 5thly. That there appears a fallacy in Dr. Cohen's theory and practice, in limiting the detachment to the smaller flap only. It is a matter of common experience that lateral-cervical placenta, where there is no smaller flap passing over the os—that is, where the whole placenta is attached to one side only, but coming down to the edge of the os, precisely in the condition brought about by the operation of Dr. Cohen—we still have flooding, dangerous flooding. It follows, therefore, that this operation may be expected to prove ineffectual; and probably would have proved so in the present cases but for the advent of strong contraction. The Reporter submits that for these reasons, and others detailed in his work 'On Placenta Prævia,' the operation recommended by him of detaching *all* that portion of placenta which adheres within the cervico-orificial zone, is the proper one to be performed.—Rep.]

MEDICAL INTELLIGENCE.

The Inauguration of Jenner's Statue.

The 17th of May, 1858, is an era in British science, for on this day, for the first time, the same honour has been publicly paid to a man of science and a philanthropist which has hitherto been thought the exclusive due of military and political eminence. The statue of Jenner, in spite of the opposition of some who profess to march in the vanguard of civilization, now occupies one of the most prominent positions in the metropolis of Great Britain; a sign to the people of what persevering energy, combined with scientific sagacity, may accomplish to the advancement of human happiness; a proof to the labourer in the cause of science and humanity that, if he achieves something worthy of his country's gratitude, that country henceforward may reward him as she has hitherto rewarded the heroes of another stamp. It was a noble sight to see the assemblage of peers, of distinguished commoners, and of the most eminent in the medical profession, which, on the 17th of May, united, at the Royal College of Physicians, under the presidency of His Royal Highness the Prince Consort, to do honour to the great name of Jenner. It was touching to hear men who form a link between the last generation and the present one, like the illustrious Marquis of Lansdowne and our professional brother, Dr. Pettigrew, speak of their personal reminiscences of the man; the former the first nobleman to recognise his merit, and to come forward in his support—the mover in the House of Commons of the resolution to confer a grant of 20,000*l.* upon Jenner, not as a reward, but as an indemnification; the latter, one of the personal friends of Jenner, and one of the oldest vaccinators in the kingdom. It is unnecessary for us in this place to urge the importance of vaccination, but we would fain hope, with the Royal President of the meeting, that its result may be to arouse the attention of the public to the merits of the great

discoverer of vaccination, and that it may be the means of preventing that neglect of vaccination owing to which, in this country, no less than 5000 persons still fall victims to small-pox annually.

A Contribution to the Curiosities of Medical Literature.

The French Society of Surgery has recently been subjected to a mystification which, to all but the persons immediately interested, may appear somewhat amusing, were it not that ethical principles are involved which forbid the rising smile. A work on stricture of the urethra was presented to the Société de Chirurgie in December, 1856,* in which the author, Dr. Pró, claims the merit of having made, at the suggestion of M. Malgaigne, extensive personal investigations on the subject, especially in the pathological museums of London. What he terms the *travail spécial, qui forme pour ainsi dire le corps du mémoire,* occupying about 25 out of the 122 pages of the work, is a verbatim translation of a part of Mr. Henry Thompson's well-known work,† which received the Jacksonian prize for the year 1852. No acknowledgment of the source from which Dr. Pró has obtained such valuable assistance is given, and the translation is so well done, that we can understand how the referees of this society, who reported well on the memoir, and who could not be familiar with Mr. Thompson's work, should have been deceived into the belief that the production before them was a *bonâ fide* work of the professed author. It appears that Dr. Pró is not a Frenchman, and we should imagine that he must be unacquainted with the facilities of intercourse, personal and literary, which exist throughout Europe, but especially between France and Great Britain, otherwise he could not have ventured upon such an act of plagiarism. The assurance of subsequent detection would have been a sufficient check upon him. We much regret, for the sake of the distinguished men who formed the commission to which the book was referred for examination, that they should have been led to regard the work as the result of Dr. Pró's labours, whereas the most important parts belong to Mr. Henry Thompson. We can well conceive their annoyance on the circumstances being pointed out to them. At the same time, it is proper that such an act of literary piracy should be loudly proclaimed, and that a well-earned reputation should not suffer through a maudlin sentimentality in favour of the culprit. We understand that steps have already been taken to prevent the recommendation of the Commission from taking effect—viz., that the author should be made a corresponding member of the Société de Chirurgie.

The Medical Officers in Lucknow.

Those who have followed the story of our reverses and of our successes in India, cannot fail to have noticed the uniform devotion and gallantry of the medical officers. It affords us particular pleasure to extract from the General Orders issued by the Governor-General of India after the liberation of General Inglis and his brave fellow-sufferers, the following testimony to the distinguished services given by our professional brethren during that trying siege:

"The medical officers of the garrison are well entitled to the cordial thanks of the Government of India. The attention, skill, and energy evinced by

* Mémoire sur l'Anatomie Pathologique des Rétrécissements de l'Urèthre. Par José Pró, Docteur en Médecine de la Faculté de Paris. Paris, 1856. pp. 122.

† The Pathology and Treatment of the Urethra, both in the Male and Female. By Henry Thompson, F.R.C.S. London, 1854.

Superintending-Surgeon Scott; Assistant-Surgeon Boyd, her Majesty's 32nd Foot; Assistant-Surgeon Bird, of the Artillery; Surgeon Campbell, 7th Light Cavalry; Surgeon Brydon, 71st Native Infantry; Surgeon Ogilvie, Sanitary Commissioner; Assistant-Surgeon Fayrer; Assistant-Surgeon Partridge, 2nd Oude Irregulars; Assistant-Surgeons Greenhow and Darby, and of Mr. Apothecary Thompson, are spoken of in high terms by Brigadier Inglis.

"To Dr. Brydon especially the Governor-General in Council would address his hearty congratulations. This officer, after passing through the Cabul campaign of 1841–2, was included in the illustrious garrison who maintained their position in Jellalabad. He may now, as one of the heroes of Lucknow, claim to have witnessed and taken part in an achievement even more conspicuous, as an example of the invincible energy and enduring courage of British soldiers."

BOOKS RECEIVED FOR REVIEW.

The Continental Review, No. 3, Vol. I.

Urethro-vaginal and Vesico-vaginal Fistules. By N. Bozeman, M.D. Montgomery, U.S., 1857. (Reprint.)

Gazette Médicale d'Orient, March and April, 1858.

Clinical Lectures on the Principles and Practice of Medicine. By John Hughes Bennett, M.D. Second Edition. Edinburgh, 1858. pp. 958.

Transactions of the National Association for the Promotion of Social Science. Inaugural Addresses and Select Papers. London, 1858. pp. 608.

Evil Results of Over-feeding Cattle. By Fred. J. Gant, M.R.C.S. London, 1858. pp. 39.

Transactions of the Odontological Society of London, 1856–57. London, 1858. pp. 154.

The Human Mind in its Relations with the Brain and Nervous System. By Daniel Noble, M.D. London, 1858. pp. 158.

On Dislocations and Fractures. By Joseph Maclise, F.R.C.S. Fascic. III., IV. London, 1858.

On Certain Irregularities of the Teeth. By Charles Gaine. Bath. (No date.)

Lehrbuch der Allgemeinen Chirurgie. Von C. F. Lohmeyer, Dr. Med. Lahn, 1858. pp. 253.

Materia Medica, or Pharmacology and Therapeutics. By William Tully, M.D. Vol. I. Parts 1 and 2. pp. 1534.

The Assurance Magazine, April, 1858.

The Nature and Treatment of Cancer. By George Southam, F.R.C.S. London, 1858. pp. 47. (Reprint.)

The Indian Annals of Medical Science. Jan. 1858.

Seventh Annual Report of the Wilts County Asylum, Devizes, for the year 1857.

Das Wesen und die Entstehung der Spondylolisthesis. Von Dr. Wilhelm Lambl. Würzburg, 1857. (Reprint.)

Exencephalitische Protuberanzen am Schädeldach. Von Dr. Wilhelm Lambl. (Reprint.)

The Medical Practitioners' Bill. Explained in a Speech delivered by the Rt. Hon. W. Cowper, M.P. London, 1858.

Die Complicirten Luxationen. Von Dr. Albert Schinzinger. Lahr, 1858.

Cyclus Organisch Verbundener Lehrbücher Sämmtlicher Medicinischen Wissenschaften. Schiff, Lehrbuch der Physiologie. 1 Heft.

Suggestions and Instructions in reference to—1. Sites. 2. Construction and Arrangement of Buildings. 3. Plans of Lunatic Asylums. Edinburgh, 1858. (Lunacy Board.)

Tenth Report of the Somerset County Lunatic Asylum. Wells, 1858.

On Spontaneous Gangrene from Arteritis, and the Causes of Coagulation of the Blood in Diseases of the Bloodvessels. By Joseph Lister, F.R.C.S. (Reprint.)

Essays, Scientific, Political, and Speculative. By Herbert Spencer. Reprinted chiefly from the Quarterly Reviews. London, 1858. pp. 435.

Silver Sutures in Surgery. The Anniversary Discourse before the New York Academy of Medicine. By J. M. Sims, M.D. New York, 1858. pp. 79.

Untersuchungen zur Näheren Kenntniss des Baues der Quergestreiften Muskelfaser. Von Alex. Rollert. Wien, 1857. (Reprint.)

Osservazione Chimiche sulle Reazioni per le quali la Cristallina si dovrebbe Distinguere dall' Albumina. Fatte dal Dottore, M.C. de Vintschgau. Wien, 1857. (Reprint.)

Untersuchungen über das Cylinder Epithelium der Darmzotten. Von J. Brettauer und S. Steinach. Wien, 1857. (Reprint.)

Medical Jurisprudence. By Alfred S. Taylor, M.D., F.R.S. Sixth Edit. London, 1858. pp. 976.

A Letter to the Rector of Exeter on some proposed Changes in the Residence required by the University for Degrees in Medicine. By G. W. Child, B.M. Oxford, 1858. pp. 19.

An Essay on Physiological Psychology. By Robert Dunn, F.R.C.S. London, 1858.

The Painless Extirpation of Cancerous Growths by Congelation and Caustics. By James Arnott, M.D. London, 1858. pp. 28.

A Manual of Psychological Medicine. By John Charles Bucknill, M.D., and by Daniel Tuke, M.D. London, 1858. pp. 562.

Handbook of Chemistry: Theoretical, Practical, and Technical. By F. A. Abel, and C. L. Bloxam. Second Edition. London, 1858. pp. 785.

The Institutes of Medicine. By Martyn Paine, M.D. New York, 1858. pp. 1095.

The Unity of Medicine. The Corruptions and Divisions, as by Law established in England and Wales. By a Fellow of the Royal College of Surgeons. London, 1858. pp. 154.

An Experimental Inquiry into the Effects upon the Mother of Poisoning the Fœtus. By W. S. Savory. London, 1858.

Local Government. A Bill to amend the Public Health Act, 1848. Ordered by the House of Commons to be printed. 1858.

A Few additional Suggestions with a view to the Improvement of Hospitals. By Mr. John Roberton. 1858. (Reprint.)

The Spread of Cholera by Personal Communication, as seen in the Crimean Campaign. By A. W. P. Pinkerton, M.D. Edinburgh, 1858. (Reprint.)

On the Sanitary Condition of the British Army. By W. A. Guy, M.B., F.R.C.P. London, 1858. (Reprint.)

The Influence of Sewer Emanations. By T. H. Barker, M.D. London, 1858. pp. 19.

Medical Opinion in the Parish Will Case. By Pliny Earle, M.D. New York, 1857. pp. 50. (Reprint.)

Report of the Pennsylvania Hospital for the Insane, for the year 1857. By F. S. Kirkbride, M.D. Philadelphia, 1858. pp. 48.

Minutes of the Proceedings of the Quarantine Convention held at Philadelphia, 1857. Philadelphia, 1857. pp. 60.

Electro-Chemistry, with Positive Results and Notes. By Charles Chalmers, late of Merchiston Academy. London, 1858. pp. 100.

American Medical Monthly, April, 1858.

Letter from Mr. Hussey to Dr. Child, on the proposed scheme of Mr. Pearson.

De l'Emploi des Eaux de Vichy dans les Affections Chroniques de l'Uterus. Par le Dr. Willemin. Paris, 1857. pp. 247.

Lehrbuch der Geburtshülfe. Von Dr. Otto Spiegelberg. Lahr, 1858. pp. 376.

A Manual of Obstetrics, Theoretical and Practical. By W. Tyler Smith, M.D. Illustrated. London, 1858. pp. 628.

On Medicine and Medical Education. Three Lectures. By W. T. Gairdner, M.D. Edinburgh, 1858. pp. 130.

Hygiene, or Health as depending upon the Conditions of the Atmosphere, Foods and Drinks, Motion and Rest. By James H. Pickford, M.D. London, 1858. pp. 290.

The American Journal of Insanity. April, 1858.

L'Avenir Médical de l'Iodure de fer. Journal des Cliniques des Hôpitaux de Paris. Mai, 1858.

Essay on the Pathology of the Blood and its containing Vessels. By Thomas A. Wise, M.D. Edinburgh, 1858. pp. 388.

An Essay on Wasting Palsy. By Wm. Roberts, M.D. London, 1858. pp. 210.

Observations on Naval Hygiene and Scurvy. By Alexander Armstrong, M.D. London, 1858. pp. 117.

The Diagnosis of Surgical Cancer. By J. Z. Laurence, F.R.C.S. London, 1858. pp. 128.

Ophthalmic Hospital Reports. April, 1858.

On Elephantiasis as it exists at Travancore. By E. J. Waring, Esq. (Reprint.)

The Pacific Medical Journal. April, 1858.

North American Medico-Chirurgical Review. May, 1858.

Charleston Medical Journal, May, 1858.

Second Report of the Commissioners of Her Majesty's Customs on the Customs. London, 1858. (By D. M'William.)

Four Letters to Sir James Clark, on Administrative Reform. By Alex. Harvey, M.D. London, 1858. pp. 80.

Kritische Geschichte des Ursprungs der Pathologie f. der Behandlung des Syphilis. Von Dr. Friedrich Alexander Simon. Erster Theil. Hamburgh, 1857. pp. 261.

On Localized Galvanism applied to the Treatment of Paralysis and Muscular Contractions. By Richard Moore Lawrance, M.D. London, 1858. pp. 164.

ERRATUM.—In the April Number of the 'British and Foreign Medico-Chirurgical Review,' at p. 383, line 29 from the top, *for* Mr. Hodgson, *read* Mr. Thompson.

THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

OCTOBER, 1858.

PART FIRST.

Analytical and Critical Reviews.

Review I.

1. *Lehrbuch der Gerichtlichen Psychologie.* Von Dr. Karl Wilhelm Ideler, Professor der Medicin, und Lehrer der Psychiatrischen Klinik, an der Friedrich-Wilhelms Universität, Dirigirendem Arzte der Irren-Abtheilung in der Charité, &c.—*Berlin*, 1857. 8vo, pp. 348.

Manual of Forensic Psychology. By Dr. Karl Wilhelm Ideler, Professor of Medicine, and Lecturer on Clinical Psychiatrics in the Friedrich-Wilhelms University, Medical Director of the Division for the Insane in the Charité, &c.—*Berlin*, 1857. 8vo, pp. 348.

2. *The Medical and Legal Relations of Madness, showing a Cellular Theory of Mind, and of Nerve Force, and also of Vegetative Vital Force.* By Joshua Burgess, M.D.—*London*, 1858. 8vo, pp. 283.

3. *Generalberetning fra Gaustad Sindssygeasyl for Aaret* 1856. Ved Ole Sandberg, Direktor.—*Christiania*, 1857. pp. 43.

General Report of the Gaustad Lunatic Asylum for the Year 1856. By Ole Sandberg, Director.—*Christiania*, 1857.

4. *An Act for the Regulation of the Care and Treatment of Lunatics, and for the Provision, Maintenance, and Regulation of Lunatic Asylums in Scotland.* 20th and 21st Victoria, Cap. lxxi. 1857.

Under no aspect of the subject of Insanity, is it one which it is easy to approach without a feeling of awe and reverence. The mind, which must shrink from the thought of the possibility of its own ruin, when

constrained to contemplate the destruction of the intelligence in others, is subdued and softened by the sense of so great a calamity: and those sympathies would indeed be dull, or deeply hidden, which were not touched or aroused by the consideration of the realities of the topic, as that pride would be truly obdurate that derived from it no admonition to patience and humbleness. It is no theme for the rash intermeddling of inexperienced men, who, without either opportunity or perseverance to gather true instruction on its proper field, know humanity only on its surface, and have not followed it, with the practical physician, closely and anxiously into its remotest recesses: and who, therefore, must remain alike incompetent to judge of the intrinsic nature of a mental malady in itself, or of the specialities, as well as the immense importance, of its relations to the family and to society.

The experience of the lunatic asylum is a true and valuable experience, but, while we admit that it is singularly precious, it is to aver nevertheless that in itself it is not enough, because it falls short of the character which we have just indicated. The physician of such an institution does not see the sufferer at a sufficiently early period to appreciate those causes and conditions which have produced or favoured the aberration; he has observed the growing malady as it is affected by all the ordinary associations of life, its domestic influences, and its customary duties, and as it reacts upon these; and, when the patient is restored to society, he does not trace him in his resumption of his former habits, and watch him as he either intertwines himself with these thoroughly, or holds partially aloof with a still doubtful equanimity, awakening the suspicion of a danger only lurking, and not yet wholly subdued. It is a one-sided experience which knows the insane merely among the insane, and which cannot study him in the various phases and grades of his disorder, among the passions, the affections, and the reciprocal obligations of the mode of existence which the individual has been compelled to relinquish, and to which it is desired to procure his restoration. Besides, there can be no true or secure conceptions of either pathology or treatment, where the relations of insanity to general disease, and the relations of general disease to insanity, have not been first studied during a more or less prolonged familiarity with the range of general practice, and with the varied lessons which it conveys. Yet we cannot always free the mere specialist of the asylum from the charge of evincing a proneness to arrogate to himself a too exclusive competence on all questions relating to mental disorder, and to ignore the claims of those who, living beyond his seclusion, and practically familiar with the wide and general basis of medical science, can learn to know the disease and the world besides.

It would be indeed unfortunate if the ordinary practitioner, charged as he is, in the immensely greater proportion of cases, with their sole treatment during the most interesting period of their development, and under that early stage in which they are the most susceptible of a cure, should not be the best fitted to judge of the causes of the disorder, and thence the most competent to suggest the means for its removal, including among the principal of these the possible necessity,

always a distressing one, for restraint in an asylum. True, the superintendent of an extensive establishment for the insane sees ultimately the larger aggregate of cases, but he sees them only in certain of their stages, and under the control of modifying circumstances of a peculiar character, which necessarily limit, and may even warp, his means of judgment, involving him in the danger of biassed views, and depriving these of the larger number of correctives. It is neither a justifiable nor a safe notion, that the asylum, under all circumstances, is the sole and indispensable resource for the management of mental disorder; nor does the regulation of the asylum embrace the entire field of duty. The dictator of this peculiar commonwealth, when he is to extend his authority elsewhere, has not only much to learn, but even much to unlearn, and many habits of thought and action of which he is to divest himself. The generally experienced physician will justly respect the high worth and talents of many of these specialists, but it is not necessary that he should thence submit to a discredit of his own pretensions. He will not readily forget that Pinel, the great master who gave first in France a humane and rational direction to the treatment of insanity, was no exclusive specialist, but a physician in general practice; and that it was to the author of the 'Nosographie Philosophique' that we are indebted for the 'Traité de l'Aliénation Mentale.' Nor will he forget that our own Prichard, the author of the best systematic treatise on insanity in our language, joined also to his extensive attainments otherwise, the like wide basis for a general knowledge of the science of medicine. It is not the extent merely, but the extent in relation to the quality, of the experience, which teaches; and it is less the scope than the capacity of observation, that turns the lesson to profit. Many are thus entitled to say with Hecquet:—"Je sais qu'il y en a qui voient beaucoup de malades, mais je vois peut-être plus de maladies."

Impressed with the vast interest and importance of the theme of insanity generally, we feel that our interest deepens when we are compelled to survey it under the phases presented to us by the description of treatise now before us. The lunatic, as a sufferer in the abstract, had already an irresistible claim upon our sympathies: but the lunatic, as implicated in, or stripped of his individual and civil rights, or as prone to acts involving him in questions of criminal accountability, has an infinitely stronger title to our commiseration; and the idea of the possibility of judging him harshly, or upon any bare plea of legal or social expediency, seems to be ready to recoil in condemnation upon ourselves, if we could suppose ourselves to be its abettors. We know that the balance must be evenly held; that to extend the shield from the guardianship of the truly insane to the protection of the merely vicious, would be a dangerous error, producing, like all errors, its mischievous reaction; and that the utmost caution must be used lest charity should mislead justice, for the worth and dignity of the latter does not lie in its power, but in its truth. We are aware, of course, that this degree of caution has not been uniformly exercised by all our writers on forensic psychology; and that, on the contrary, an originally

humane and partially just idea has been sometimes fostered into a crotchet, till its undue obtrusion has provoked an antagonism between law and medicine little to the credit or advantage of either. In any remarks suggested to us during a perusal of either of our authors, on the subject of the medico-legal relations of insanity, or on points arising incidentally from the consideration of their theme, our leading anxiety will be to avoid this error.

Professor Ideler has been long known as an able writer on a variety of topics connected with disease of the mind. In his previous publications he has always distinguished himself as a strong and sound reasoner, and we once more gladly recognise this characteristic in the volume before us. His treatise presents us with a comprehensive view of a subject often abstruse and difficult, and involving, were it for this reason only, numerous points of contest. Beginning with the discussion of the topic of moral liberty, he reviews this in its relations to the intellect and to the affections, as well as to the will, and examines the influences to which it is naturally subjected through the various states of the living organization, and the extrinsic conditions of existence. His primary ideas on the subject having thus moulded themselves into the requisite precision, he next extends his inquiry to the consideration of the principle of moral freedom as lying at the basis of all our notions of what constitutes the essence of crime, and of the just application of penal law; examining into the limits and degrees of accountability, and into the competence of physicians to determine as to these, along with the necessary appreciation of the motives of action. Having secured this as the first part of his groundwork, he now surveys the different faculties of our moral nature, passing before him in succession the consciousness, the power of conception, the emotions, affections, and the passions, and treating the operations of the more energetic of these in detail, as well in their relations to each other reciprocally, as to the faculties of the intelligence, and to the influences of the social system. The modifying effects of physical disease are then considered, and general rules are supplied for estimating the bearing of these upon the condition of moral freedom. After this cautious preparation, the author approaches more closely the examination of the special disorders of the mind, first rapidly surveying the general doctrines of insanity, and then reviewing separately its various forms of monomania, mania, melancholia, incoherence, and amentia, attaching to each the consideration of its effects upon the accountability of the individual. Some observations on simulated insanity, and a sketch of what he terms the more doubtful affections of the mind, including homicidal monomania, mania transitoria, dipsomania, amentia occulta, pyromania, kleptomania, and suicide, complete the outline of his interesting treatise.

A work so carefully elaborated has not been an easy labour, and we must even own that its assiduous perusal, demanding as it does a continuously earnest attention and reflection, and supplying interminable materials of suggestion, requires no insignificant effort of the intelligence. For him that desires a treatise which he that runs may read,

the volume of Professor Ideler will present comparatively few attractions. The author is not one of those who seize a favourite dogma, and, complacently veiling it under a tissue of assumptions, force it upon us and upon the public as the reasoned exposition of an incontrovertible principle. His views, on the contrary, are always elicited with care, and stated with becoming moderation. He acknowledges the right of society to be protected in its peace and safety against every undue enforcement of the plea of criminal irresponsibility, and he respects in the magistrate those scruples which occasionally hesitate to acknowledge the results of a disease, where they may seem entitled to recognise only a vice or a crime. But he is not the less the strenuous assertor of the mournful privilege of true insanity to secure immunity from punishment; while, the better to ensure it, he is zealous in his attempts to demonstrate upon what grounds alone this immunity can be justly vindicated, and to discriminate the conditions under which there ought to be no pretence to claim its protection. In short, he is one who knows what is that proper position, to which we have already alluded, of the forensic psychologist, in relation to the dispensation of justice; and he has no wish to arrogate to himself the functions of the judge and of the Court, but rather, in seeking to reconcile his respect for constituted authority with his sense of a peculiar duty, takes common experience for his guide, and finds evidently no difficulty in the effort. It is here, of course, impossible for us to enter largely into any general discussion of the multiform topics embraced in Professor Ideler's treatise. In glancing over them we have been willing, therefore, rather to select one as a more especial subject for comment; and adopting this view, none seemed to us more important, or more eligible otherwise from the interest which it has been recently exciting in this country, than that of the medico-legal relations of the so-called dipsomania, or inveterate proneness to states of drunkenness, to which the author has devoted a judicious chapter. In the tenor of our remarks we shall be found to differ less from Professor Ideler in essential particulars, than in the mode of development of our views, and the occasionally greater amplification of certain portions of them.

The vice of habitual drunkenness, we need hardly remark, continued for long to be regarded everywhere as the subject of merely police regulation; and the crimes, for the most part of violence, to which it gave rise, came under the common category of offences to be repressed by the arm of the law, under no other limits than the ordinary rules of justice. Some of these police regulations were sufficiently severe, indicating beyond doubt a sense of the magnitude of the evil, but still more that harshness which was then the too prevailing characteristic of our codes. Yet the history of the conditions of society, until a very recent period, shows how vain was this attempt to restrain by legislative rigour what was not controlled by morals. The Church and the State fulminated everywhere their reprobation of the gross and prevailing excess, but the bolts fell harmless when they were wielded through the means of a judge who sat mellow on the bench, or of a priest who hiccuped in the pulpit. Our Scotch friends are not, we

observe, universally satisfied with a recent law, shutting the taverns daily after eleven at night, and throughout the whole of the Sundays, yet in the old Catholic times, prescriptively entitled merry, we find it more rigorously ordained :—

"That na man in Burgh be founden in Tavernes of wine, ail, or beir, efter the straike of nine houres, and the bell that sall be rungin in the said Burgh. The quhilkis founden, the Alderman and Baillies sall put them in the Kingis prison : The quhilk, gif they do not, they sall pay for ilk time that they be founden culpabill before the Chamerlane fyftie schillinges."*

We thus observe here already a manifest indication, that the magistrate was suspected of a bias towards undue leniency in the repression of a fault, which had commonly, in all likelihood, something more than his sympathies. In Sweden, drunkenness was punished on the three first occasions by a gradually augmented fine ; on a fourth by a loss of political privilege ; on a fifth by a short imprisonment in the House of Correction ; and on a sixth, the imprisonment was extended to no less than a year. In several of the German states, the vice was denounced habitually, under threats of heavy penalties. Yet so thoroughly, nevertheless, did the propensity remain rooted everywhere in the social habits of the times, that even the popular jests regarding it have a kind of cosmopolitan diffusion. The Scottish clergyman of the last century, who, preaching against drunkenness, did not fail to find a plausible reason for a dram in the morning, to be repeated in the forenoon, with another before dinner, and another after dinner, and another at night, yet could not tolerate the perpetual drinking to which some of his parishioners were addicted, is well represented elsewhere by the German suffragan bishop, of whose sermon Goethe offers a specimen in his 'Reise am Rhein, Mayn und Neckar.' The worthy prelate, cautioning his hearers against excess and its results, which he painted in strong colours, reminded them that there were few to whom had been vouchsafed, as to him, the power of drinking eight quarts without ever having been once chargeable with abuse of his friends or household, or neglect of his sacred duties. It was this thorough intertwining of convivial recklessness among the habits of the people, that set at defiance all attempts at a serious repression; and so wide and recent was the contagion, that Mr. Gunning, the Senior Esquire Bedell at Cambridge at the close of the last century, was even then able to tell us, in his gossiping reminiscences, that intemperance was still so completely the besetting sin of the day as to induce him to record as something remarkable, that there was one of his University friends whom he had never seen drunk. How public opinion, however, in one of its stronger revulsions, has dealt with this since, and how different is now the vantage-ground of the social reformer, it is scarcely necessary for us to indicate.

It appears paradoxical, yet it is not really inconsistent, that, with the recent gradual improvement in the manners of the nations of Europe in relation to habits of drinking, there has been a coincident relaxation in the severity of our penal laws ; and that not merely

* Act of King James I., parl. xiii. (An. 1436), c. 144.

generally, but with especial reference to crimes committed under the influence of intoxication. Where justice is weak, it is always vindictive; and, at the former period, it became aroused and struck fiercely at intervals, as if to atone for the natural results of its ordinary remissness. We may judge that it was a true detestation of the vice of drunkenness which induced Pittacus, six centuries before Christ, to attach, as we are told, a double penalty to all crimes committed during intoxication; because the ancient Greeks, like the Romans, were really remarkable for sobriety, and this description of excess, though well known to them, was by no means usual, and still less was held in honour. The maxim of the old lawgiver, however, seems to have passed over into many of our modern codes, where there was not the same substantial basis in the habits of the people to sustain its authority, though there was indeed far more to justify its necessity. But there was a prevalent heedlessness of human life during the middle ages, which has only recently been everywhere extensively mitigated; and the same rough spirit which turned easily, in all classes, the debauch into the broil, and the broil into the serious injury of person or destruction of life, dictated the measure of retaliation, and exacted a penalty which was sometimes little commensurate with the intrinsic quality of the offence. Yet, even in the sixteenth century, the principle in law, that a crime was not punishable when committed under the deprivation of reason, began, in many of the states of Germany, to have its modified applications to offences during, and the result of, intoxication. Long before then, though less directly enunciated, it had evinced its influence in other countries. Thus, in Spain, so early as the middle of the thirteenth century, in the famous code of the *Siete Partidas*, the work of King Alonzo IX., surnamed the Wise, we find it enacted, while murder generally was punishable by death, that if any one should so intoxicate himself, that he should kill another through violence committed during his drunkenness, as the death was through his own fault, the perpetrator was to be banished into an island for a period of five years.* In this country, however, the rigid letter of the law still tells us, though the application of the rule has neither been always thoroughly uniform nor without hesitation, that no person has any privilege of excuse through this voluntarily contracted madness. In the penal code of France, the state of drunkenness of the accused is not mentioned as modifying the accountability for crime; but there has been a growing disposition on the part of juries to interpret, as extending its merciful influence over this class of offences, the sixty-fourth clause of the code, which determines that there is no imputation of crime where the perpetrator was in a state of dementia at the time of the action, or where he was constrained by a force which he could not resist. In like manner, the fortieth clause of the penal code of Prussia enacts, that no crime is imputable where the perpetrator was insane or imbecile at the time of the event;† and here, too, there has been an increasing disposition to

* Las Siete Partidas del Sabio Rey Don Alonzo el Nono, Ley v. Tit. viii. Partida viii.

† Strafgesetzbuch für die Preussischen Staaten (1856), Theil i. tit. iv. § 40.

include under this category all offences committed during a state of intoxication. The code of Austria, on the other hand, is more exact and determinate in its provisions on this subject. According to the third clause of its second section, no crime is imputable when the act was committed during a state of advanced intoxication, not designedly premeditated, or during any other condition of mental disturbance in which the individual is not conscious of the nature of the action; and this is irrespective of actual insanity, which is provided for in the two preceding clauses.* But, with now all the deeper justice, the drunkenness itself, where it has led to an act of violence, is here punishable as an offence (§ 236): and this punishment is fixed at an imprisonment of from one to three months; while, where the individual was aware, from previous experience, of his usually furious tendencies when intoxicated, the imprisonment was to be more rigorous in itself, and became extended, for especially flagrant instances, to a period of half a year. In Würtemberg the qualification proceeds even further, and declares a diminished responsibility for those lower grades of intoxication, where accountability could not be held to have become extinguished, yet where the individual was unable to perceive the full tendency of the danger and criminality of his conduct.

But even this degree of leniency, extraordinary, or even unintelligible, as we suspect it must appear to most among ourselves, has not sufficed for many of the medical psychologists of the Continent. Not content with the concession that the actual state of intoxication was to be accepted as a condition of temporary insanity, and its acts to be sheltered, to a wide extent, under the corresponding immunity, they have insisted that the inveterate drunkard should be regarded as habitually under the influence of an insane impulse, which it was impossible for him to resist, and for the results of which, therefore, he was not responsible; while a few have even proceeded so far as to pronounce him, even in his sober intervals, as so manifestly of a biassed or perverted understanding as to have ceased, solely because a drunkard, to be an accountable being. It has been customary to assign the initiative of this doctrine, though scarcely with justice, to the treatise of Brühl-Cramer, a physician practising in Moscow towards the commencement of this century. But it seems to us rather to have derived its origin from a disquisition, 'De Amentia Vinolenta,' by Platner, a professor at Leipzig, which was published in 1809, and which has been comparatively little remembered. In the views of Platner we find clearly indicated,† and referred, as his title implies, to a basis of disordered mental condition, those dogmas which have since expanded with others into greater breadth, and acquired greater prominence. The object of Brühl-Cramer was to show rather a physical than a mental basis for the appetite for intoxicating liquors: and even in this he was anticipated by Salvatori, a physician also resident in Moscow, whose observations were published in that city in 1817; while those of Brühl-Cramer did not appear till two years afterwards, at Berlin, and

* Das Strafgesetz für das Kaiserthum Oesterreich (1853), Theil i. § 2.

† Platner, Questiones Medicinæ Forensis (ed. Choulant), p. 266.

then were singularly silent as to any anticipation of his leading opinions by another so closely contemporary, and in his own immediate field of practice.

Salvatori, assuming a malady which he entitles ebriosity, terms it a proper disease, hitherto excluded from nosological systems, and divides it into a continuous, a remittent, and an intermittent form. His descriptions of the various modifications of the drunkard's condition and progress are full of truth; and it seems easy to recognise their impress on all subsequent writers, although their source, with which we have only chanced to become directly acquainted during the last few years, has been suffered to sink generally into an unmerited oblivion. His treatment, for which he boasted the extraordinary success of 28 cures, without relapse, in 50 cases, was by an infusion of wild thyme, also similarly recommended by Linnæus and Murray, to which he added saffron and cardamom.* The silence of Brühl-Cramer, with reference to Salvatori, surprises us the more, that he appears to have been his colleague in the Physico-Medical Society of Moscow, before which body the observations of Salvatori were first read, and in whose Transactions they had afterwards a place. Brühl-Cramer, like his precursor, introduces his remarks by proclaiming the novelty of his theme as a nosological entity, and he assigns, as the main proximate cause of the physical ailment he seeks to constitute, a peculiar state of irritation of the brain and nervous system, chiefly localized, however, in the region of the stomach.† Often embarrassed in the attempt to render his views consistent, and entangled in the difficulty of separating the moral and social relations of the vice from those which are merely physical and individual, the treatise of Brühl-Cramer contains, nevertheless, much able and sound disquisition; and if his portraitures have less of truth and energy than those of Salvatori, they are aided at least by a greater abundance of collateral illustration, and are not unworthy, in many respects, of the credit with which they have been received. Still it is to the prevailing correctness of his premises that we must assign the chief merit; for his deductions often appear to us singularly warped. The idea of a mental affection, sustained by Platner, and that of a physical disease, alleged by Salvatori and Brühl-Cramer, have never since been separated from each other, or from the vice to which they have been referred: and the impulse towards habitual or periodical intoxication has been explained with a leaning sometimes to one of these views, and sometimes to the other; yet generally, and as to both, more under the further influence of the materialistic opinions of a Condillac and a Cabanis than has been always either felt or acknowledged.

Modified subsequently by the views of Esquirol, Heinroth, and Hoffbauer, the dogma of a drunken insanity, in its widest acceptation, has received its most strenuous support from Henke and Friedreich;

* Salvatori, Commentatio Pathologica de Ebriositate continua, remittente, et intermittente: In Comment. Soc. Phys. Med. apud Universitat. Mosquens. institut. (1817) vol. ii. part i. pp. 260, 290.

† Brühl-Cramer, Ueber die Trunksucht (Berlin, 1819), p. 23.

has since been gradually diffusing itself, though not everywhere with equal acquiescence, among the medical psychologists of the Continent; and has been at last pressing its influence strongly on those of our own country. The designation of a dipsomania, or oinomania, admitted to a great degree merely figuratively by many, but by others literally, has been simultaneously so largely adopted as to have aided at once to diffuse the notion, and to insinuate its acceptance. A step of great importance in this country was that in 1844, of the English Lunacy Commissioners, who, at that time, leaving it to be inferred that they regarded the habitual drunkard as a moral monomaniac, urged the justice, as well as the expediency, of his detention in our lunatic asylums.* Able and intelligent medical superintendents of such asylums, most of them utterly beyond all imputation of motives of self-interest, were yet naturally not wholly unbiassed from their position, or unprejudiced from the consideration of the special class of drinkers, usually violent and unmanageable elsewhere, who were customarily submitted to their charge. These, therefore, joined warmly in sustaining the propriety of this recommendation; and physicians enjoying and well meriting the public confidence, impressed with the lamentable social evils of intemperance, more or less willingly yielded their assent. In Ireland, the Lunacy Commissioners for that country have pursued a similar course. Yet with all this, and with the growing urgency for the expression of a more direct legal sanction, extensively and recently displayed, a survey of the opinions enunciated has failed to convince us that these are either so consistent with each other, or reciprocally with the measures which they severally inculcate, as to impress us with the necessary sense of the due stability of the opinions themselves.

It is manifestly necessary, before the magistrate can be reasonably required to interpose his authority, unless by a kind of legal compromise, for the seclusion of the inveterate drunkard among the insane, and still more before any warrantable demand to that effect can be made for direct legislative interference, that it should be upon some thoroughly determinate principle as to what were the positive relations of the propensity, whether truly to a mere vice, or to a real mental derangement. We must not make the exception, or even any considerable class of exceptions, usurp the place of the general rule; just as, on the other hand, we must not be too tenacious in maintaining the general rule before the special instance. It may be occasionally expedient that the inveterate drunkard should be forced into the discipline of the lunatic asylum: but no bare question of expediency, however specious and seductive, can long be tolerated as laying claim to respect and authority; because, to ensure this, there must be a clear and absolute union between what are here the merely convertible terms of truth in science and justice in law. What may be the proper means, otherwise, for the repression of the social calamity of intemperance, for the reform of its victims, and for the restraint of its mischief, is not here the point for consideration. If the drunkard, solely as a drunkard, be not insane, no makeshift of medical or legal in-

* Report of Metropolitan Commissioners in Lunacy (1844) pp. 170, 175.

tervention can place him with propriety in an establishment for lunatics. If he be insane, and, once more, solely as a drunkard, and that insanity be held to be unequivocal enough to entitle the law to strip him of his personal liberty, and consign him to the subjection and the society adapted to what is now his just position, we must be prepared deliberately to contemplate what ought to be the ulterior and legitimate deduction from this, as to the modification or extinction of his civil rights and his criminal accountability. The topic, in all its tendencies, is too important to render its treatment a matter of indifference; and we shall advance ourselves little in this, if it should be ultimately proved that we have but misinterpreted or ignored a truth that we might elude a difficulty.

We are not sure that there has not been a considerable warping of views on this subject, through the accidental influence of a name. The designation of *dipsomania* or *oinomania*, we repeat, as applied to the propensity of the inveterate drinker, has been adopted by many as an appellation attached with justice to a state which they honestly asserted to depend upon an actual insanity; but it has passed, doubtless, largely into almost popular usage, under an acceptation denoting merely a strong predilection, as we speak of the mania for gambling, or for books, or music. Yet its adoption, even to this extent, has unquestionably favoured the insinuation of its deeper import, and men have glided easily from its metaphorical to its literal acceptance, or have hesitated and alternated between them. But there is a misfortune in admitting a term which thus to many implies a foregone conclusion; which unquestionably comprehends too much, even if it can be justified in part; and which has, besides, been only indifferently constructed. It is not to characterize the condition in question, to term it a dipsomania, for it is no mania for thirst; or an oinomania, for wine is comparatively rarely the beverage lusted after: if it be a mania at all, it is a *methomania*, or a mania for intoxication. To employ a new or unaccustomed phraseology in science, is an evil only subordinate to the retention of an old one conveying an inaccurately limited or a deceptive meaning. In the following remarks, therefore, if we should venture to introduce a new designation, it shall be with the endeavour that it should appear as the development of a conviction, and as bound rather to a fact than a hypothesis.

There are certain fundamental truths with reference to the contemplation of a connexion between insanity and drunkenness, which are too obvious to escape notice or to admit cavil. Thus, no one will dispute that the actual fit of intoxication is a state in itself of unquestionable, though transitory, insanity, always manifest, and often exceedingly violent. The experience, again, of every day tells us plainly, that this state frequently acquires the proneness to its own repetition, with more or less regularity and continuousness, or that its production degenerates into a habit. It is not less undeniable, that the frequent recurrence of this state leads finally, in a great proportion of cases, to a less transient, though still only a temporary form of aberration, strikingly peculiar, and of great intensity, known as the

delirium tremens. Lastly, there remains a large proportion of cases, where the deterioration passes, after a multiplicity of similar and successive shocks, into a state of fixed and permanent mental derangement. In these facts we have a direct condition, a repetitionary tendency, a constituted habit, and an induced insanity; with a consecutive insanity, more remote in its relations, though scarcely less absolute, and differing in fact from other forms of mental disease rather in its cause than in its essence. Hence a nomenclature might naturally suggest itself, the nature of the foundation of which protects it from challenge. In the direct condition we have, as a root, a *methia* (*μέθη, ebrietas*): in the tendency to repetition, a *methilepsia* (*λῆψις, prehensio, paroxysmus*), which may be either only occasional (*m. vaga*), or may assume a form of generally indefinite periodicity (*m. periodica, seu alternans*): in the confirmed habit we have a *methexia* (*ἕξις, habitus*): while in the delirium tremens we have a true *methexiphrenesia* (*φρένησις, mentis delirium*), or a *methexomania*, either term being applicable to a mental disorder the direct result of the habit of intoxication. The remotely consecutive insanity has been termed a *vesania ebriosa;* but perhaps, with a still more rigid propriety, might be designated a *vesania ebriosorum.* The name, however, is scarcely wanted, unless we are to proceed beyond, to a classification and denomination of mental diseases generally, according to their causes; thus taking an opposite course from that which seeks, we fear even less philosophically, to establish essential distinctions in their diversities of tendency, whether towards one predominating idea, or towards another. Indeed, it is only to the terms *methexia* and *methexiphrenesia*, with the conditions which they indicate, that we attach any special importance; partly for reasons that are already apparent, but still more on the score of an accuracy of application, simply enunciating a fact, and involving neither dogma nor foregone conclusion. But we do not pretend, meanwhile, to obtrude the use of any of these designations. We are convinced of their accuracy, but it is better to leave others to judge of their necessity.

Admitting, then, the fit of intoxication as an evanescent paroxysm of induced insanity, and the delirium tremens as its frequently produced, more intense, and enduring development, we have still a considerable, though decreasing, number of further examples in which the insanity, unless intercepted by the results of physical ailment, assumes a condition of permanence; the least equivocal of its ultimate forms being a state of imbecility or dementia. We must not forget, on the other hand, that in a different and far more limited category of cases, because restricted to those already insane, or evincing a proneness to insanity from other and independent causes and tendencies, and by no means predominant among these,* we find that the craving for ardent spirits follows or associates itself with a prior mental disease. Yet perhaps there may be, at least in some of these cases, no further

* Certainly not so commonly as was alleged by Rabelais, whom we may cite here as a physician, when he tells us: "Encores ne veidz ie oncques fol, et si en ay veu pour plus de dix-mille francz, qui ne beust vouluntiers et a longs traictz."—Pant. lib. iii. c. 45.

connexion between the insanity and the drunkenness, than that the insane are not necessarily divested of those physical appetites which may have in them, as in every one else, an independent existence and origin; and which have thus only become coincident with a mental aberration, rendering them certainly less susceptible of control, but towards which otherwise they do not stand in any directly causal relation. Undoubtedly, at all events, it is not in any indulgence of an appetite, but in the opposite state of a wilful abstinence from a necessary aliment, that we are ordinarily to look for the clearest proof of a connexion with mental disorder. A well ascertained rank of priority on the part of the indications of insanity will determine much in these questions; while even the existence of any peculiar eccentricity, or marked flightiness and instability of character, though not amounting to insanity, yet as denoting a tendency thitherward, will justly weigh with us in establishing a criterion.

In so far, then, yet no farther, we may admit often a reasonable, though still not a thoroughly proved, or at any time an absolutely irrefragable, relation between insanity as a cause, and habits of intoxication as an effect. But the concession, even far more unhesitatingly, of the truth of this, does not assuredly constrain us to proceed, with the many whose views we have already noted, to the extreme of regarding all cases of inveterate drunkenness, considered in themselves, as the results or the proofs of insanity. On the contrary, no assumption so violent in its first aspect is required, according to our clear conviction, to explain the ordinary position of the habitual or inveterate drinker, whom it has been our fortune, or our duty, to have watched and compared many times, with intense interest, from the first of his slight and convivial indulgences, to his last and lowest degradation. Obviously the history of his progress, if we are to arrive at sound inferences, can no more be freed from this description of safe and philosophic induction than any other theme of scientific inquiry. Indeed, he is but a cursory or an indifferent observer, to whom the different grades of the drunkard's career, and their transition into each other, are not sufficiently apparent, if thus traced in their sequence. It is in reconciling the two extremes of his first mirth and his last misery that we are startled into a difficulty; for no other extremes can seem so far apart, that are yet so closely bound in the chain of causation. The commonplace lesson that is conveyed by Petrarch, when he cautions a friend against a feeling of security with reference to this vice, is one which we may not the less cordially accept from his genial spirit, that it would tend to repel us from the lips of an ascetic:

"If an individual," he says, "considers himself unchallengeable now, because he has not yet reached the verge of excess, let him reflect that there is more in taking the first step, even though a moderate one, from the limit of discretion and sobriety, than, when he has once begun to slip, to fall to the bottom."*

We cannot wonder, to pursue here a somewhat hackneyed theme, that where the primary effects of intoxicating fluids, in their best aspect,

* Epist. Variæ, Lib. iii. Ep. 9.

have once been experienced, there should again be some longing to be placed under the charm of their influence. But this influence is not sought at first for itself merely. Independent of a natural or usual inclination to escape at intervals the restraints and anxieties of labour, or to compensate its fatigues, or to yield to any other pretext for amusement or relaxation, it is not our least amiable qualities otherwise which impel us often towards the enjoyments of conviviality. The leaning of man towards man, the cheerful interchange of opinion, the opportunity of forming and increasing intimacy, the customary attendants on festivity, ever graceful and alluring, with always the flow of soul, if less uniformly the feast of reason, suggest, as our daily experience tells us, additional inducements enough to entice the facile, and there still appears nothing to repel the prudent. There are flowers strewn in the way, and lights are sparkling on them; and men pursue the path for its own sake, not because it conducts them further.

Yet in all this we know there is danger: not to every one, certainly, for we are no admirers of *teetotal* exaggeration, but to many. As, by a repetition of certain actions or operations, the body acquires a greater and greater facility in their performance, and a stronger and stronger proneness to their renewal, so it is with our moral or mental faculties and operations also; while, on the other hand, under the neglect or inertion of either, there ensues an equally marked tendency towards enervation or torpor. The mental habitudes are growing, if men do not give heed to them, or are too indolent or feeble to check them, which change into a weariness what was begun as a pastime. The dissipation of thought annuls the power of thought, or the capacity for sustaining it, unless with long intervening pauses. In the vacuity of these the mind shrinks from itself, and longs for excitement, which it seeks and renews where custom has shaped its inclination; and so, by an unceasing interchange of natural action and reaction, as the physical propensity strengthens, the mental distraction increases, till the energies of the mind neither permit themselves the leisure, nor are capable of the effort, for any earnest or persevering exertion, and offer to the cravings of the depressed and languid frame the pretext only of a resistance. "The pain arising from the contemplation of a life mis-spent," it has been truly said by one* who felt the misery of his own position, "is often the cause of continuance in misdoing, even after the temptations which first misled have lost their power, and when the sophisms which have long deluded appear in their true deformity." Reproaches fall henceforward on a temperament too unstrung to bear them, and they soon lose their authority, or must be silenced. Fits of remorse still intervene, or the constitutional effects of the debauch compel its interruption, and thus give space for keen reflection; but a moral position lost is not easily regained, and the feeling of hopelessness as to the effort sooner or later rises to over-rule the will, and restore the mastery to the appetite. Thus alternating, but usually with ever shorter intervals of intellectual freedom; now

* Hartley Coleridge, in letter to his father: Works, vol. i. p. xciii.

in darkness, and now emerging into light, but the light ever becoming less clear and inspiriting, the process of mental degradation passes on. What wonder then, if irritable, unstable, without force of purpose or of comprehension, the individual becomes aimless in his pursuits, or, riveted to his habits, dreams of duties only to recoil before them?

But, in this prone and natural sequence, there is no vestige of a primary insanity; unless we are to extend our notions of insanity to that limit, which would leave one-half the world to lock up the other. Unquestionably, the conduct of the drinker is irrational, because it prefers the presence of one set of sensations, and the escape from others, with the gratification which ensues, to that which ought intrinsically far to outweigh the consideration of either. But, under this view, and often with far more stringent force of reason, every great vice is only another name for a great folly; while, if the pitiful barter is made, it is made knowingly, and with the capacity for a full, and even, in a certain sense, deliberate consciousness of its nature and its results. Too little attention has been paid here to the distinguishing peculiarity, that, of all excesses, the excess in intoxicating drinks is the only one the distress following which finds its relief, from the obvious relations of the nervous system, in the repetition of itself; while the vice, moreover, possesses a vantage-ground over every other in the power which it confers of stilling the conscience at pleasure. In this way, then, are prepared the open facilities of the drunkard's descent. Besides, there is a gradation in this descent to which we must assign its due importance. The position of each day is nearly that, but it is not that, of the day preceding, and the day after finds it again modified; and so at length, as indeed with every kind of vice or criminality, a point is reached, and reached easily, which was inconceivable at first, but which is perfectly consistent when we mark the stages of the progress, looking back from each to that which last preceded it. Let us, then, humbly leave all idea of perfect wisdom to the Godhead; and, content to discover human springs for human actions, let us not affect a necessity to attribute to derangement, that which shows no other proof of derangement, than the mere persistence in the indulgence of an appetite, where that appetite suggests a present solace, and therefore a present motive, however unseemly its nature, or pernicious and degrading its tendencies, and at whatever cost of falsehood and meanness promoted or concealed. Thus rationally considered, every new act of the drinker is the natural and last development of the whole previous tenor of his existence, and it is nothing more.

But, could we admit the craving for intoxicating liquors and their results as, *per se*, and in a logical sense, an insanity, under what head is it to be classed? With some it is accounted a monomania, definable as a madness on a single object, relating chiefly to the passions or affections; but surely here it is neither passion nor affection, but simply an appetite, that is primarily and indispensably involved. Others have ranked it under the more comprehensive head of Moral Insanity; but in this form, to some of questionable scientific title, it

is the misdirected will, misname it as one may, that is predominant, while, in the drinker, the will is in subjection, and a physical appetite has the mastery. Nor could it be otherwise than a poor psychology which confounded the appetite with the will, these being not only essentially different, but customarily opposed to each other—the appetite, to use the words of Hooker, as the will's solicitor, and the will as the appetite's controller. But in truth, the so-called Moral Insanity of Prichard, if analysed carefully, and the term applied in its most allowable sense, is merely, on the one hand, a permanently lower development, or, on the other, a temporarily less advanced stage, of proper insanity; which necessarily always first manifests itself through the moral relations, because it is mainly through these that the individual connects himself with his social condition, and acts immediately upon those around him, yet certainly, from the very essence of the disorder, never without an implication of the intellect. Beyond this very limited qualification, it is impossible, unless in merely arbitrary speculation, to separate the affections of the moral from those of the intellectual faculties, and to view either as independent sources of action, instead of, what they really are, only modes of relation of the one sole and indivisible mind, and inconceivable unless intertwined in their operations. No one has termed the drinker's propensity mania, or ordinary madness, for there is no general disturbance or raving; or incoherence, for there is no want of apprehension, or of just sequence in the ideas. If, again, even in moral insanity, it is the consciousness rather than the judgment which remains unimpaired, here both are untouched. Hallucinations or illusions are alike out of the question.

If we are still to cling to the notion of an insane propensity to drinking, possibly the nearest approach to what might be thus accounted is shown in the paroxysms of that periodic drunkenness, in which the individual exchanges (*methilepsia alternans*) more or less lengthened periods of sobriety for sudden digressions into the deepest excess. But distinctions of this kind are sometimes forced into more than their real value and importance: and it must be admitted that even here there must have been necessarily a previous familiarity with the nature of the agent which has induced its adoption; and that it is almost universally but a prior trained habit, interrupted only by the physical effect of the debauch, and held possibly in abeyance for a while afterwards by a moral consciousness of transgression, that sooner or later in such cases resumes its influence, when the first of these disappears and the second becomes blunted. If the periodic craving have followed upon a severe injury of the head, as is observed to occur in a remarkable, though, of course, limited class of instances, the boundary of insanity, in its sphere of somatic, and therefore most palpable causes, is evidently still more closely approached. As to hereditary drunkenness, which appears to occur often, yet, with so common a vice, in unquestionably a comparatively narrow minority of instances, it may frequently be as much, and is usually far more, a proof of hereditary temperament, rather than of inheritance of dis-

ordered intellect, conditions which few will be willing to confound with each other; while it would be unjust, besides, to disregard here the habitually prominent results of defective or perverted training, whether as withheld by neglect or incompetence, or conducted in a vicious direction. It is thus, in reality, that drunkenness in kindred appears far more frequently as a collateral than as a transmitted defect, and we have seen whole families destroyed by it, where the original error of the parents was not insobriety, but passiveness and over-indulgence. Where there have been more positive faults, the result is naturally the more certain.

The habit of physical excess, then, with its necessary counterpart of the production of mental neglect and remissness, but not, at least primarily, of insanity, and the reciprocal actions of these, with the resulting depravations, appear to be unquestionably as naturally associated, as consequent in their growth, and to depend upon as obvious and rigid an order of causation, as is manifest in relation to any other of the mixed phenomena of the organism. Thus considered, the proneness to intoxication is a vice for which man, still endowed with his judgment and his consciousness, is, in the great bulk of instances, completely responsible; and this responsibility, whether in justice or in prudence, and in his own behoof, we should do nothing to shake. An opposite view has led us to hear far too much of the want of power to resist the propensity, and far too little of the want of effort; while it seems forgotten that, on all matters apart from what concerns his gratification, the will of the drinker is often peremptory enough. So invincible is the impulse, we are told, that drunkards have been heard to declare that, were hell yawning at their feet, and alcoholic liquors before them, and they were warned that to partake was to be instantly hurled down, they could not forbear. The expressions here are reported by Macnish, and we have ourselves heard others scarcely less impious. Yet we can discern, it may be inferred, in this so-called impulse, only another, and a less accurate, form of expression for a gross appetite. If it be not this within the body, it must be something imaginary beyond the body, for whose basis of action, to collate the microcosm with the macrocosm, we are as much at a loss to assign a resting-place as Archimedes, when he sought a fulcrum for the lever with which he fancied he might move the world. Besides, a metaphor or a hyperbole is not a proof; and whoever knows the character of the drunkard in his sober moments, and we cannot shift the question here from the propensity to the actual condition of intoxication, knows how completely the reverse of everything like a spirit of determination is the truth. The inveterate drinker is essentially a coward. It is simply because he cannot face a present inconvenience, that, sometimes with prayers and tears, or with threats if he recognises the feebleness of the power that curbs him, though generally for long with the possession of the easiest facilities, he turns for its relief. That he is conscious of incurring the risk of a remote evil weighs little, because he knows it may yet be deferred, and believes or hopes it may be prevented; for some vague fancy or desire of a coming

reform is always appearing, if always, because without any proper consideration of the means, retreating before him. Thus the illusion of amendment, instead of serving as a safeguard, increases his seduction. He lapsed easily while he could say:

> "——illud malum aderat, istuc aberat longius.
> Illud erat præsens, huic erant dieculæ."

Bring retribution at hand, however, and make it visible, rapid, and certain, and far less than the horrors of hell will strike the cup instantly from his grasp.

We have entered upon this somewhat detailed analysis of the character and progress of the inveterate drinker, because it evolves distinctions and inferences that are of real importance. It is not alone, that to attribute all intense drunkenness to mental alienation, becomes, in a certain degree, and however unintentionally, or with however doubtful a satisfaction to the drunkard, a vindication of drunkenness. There is truth in the apposite lesson which we derive here from him who could read the human heart more deeply than any before or since his time, and who therefore speaks for all times, when he tells us in *Lear:*

"This is the excellent foppery of the world, that, when we are sick in fortune (often the surfeit of our behaviour) we make guilty of our disasters the sun, the moon, and the stars: as if we were villains by necessity; fools by heavenly compulsion; knaves, thieves, and treachers by spherical predominance; drunkards, liars, and adulterers by an enforced obedience of planetary influence; and all that we are evil in by a divine thrusting on: an admirable evasion."

Yet, while admitting the evils of this, we should be unwilling to be justly accused of passing into an opposite extreme. Wherever there is actual insanity, however produced,—and we have shown that it may precede drunkenness, and will follow it,—there is annihilation of crime. But, to shake the accountability for even the mere drunkenness, we must have other proofs of insanity than the propensity itself; while, if we are to make the concession of a condition of insanity, to the extent, and in the absolute way, in which some have demanded it of us, it must be with a consideration of the consequences of the admission, whether as to the rights or obligations of the individual or those of society, which has not always been cautiously instituted.

Let us follow, with this view, the results of the opinion of those who are now the many, who believe that the inveterate drinker, as such, is truly insane, or the victim of an insane impulse; and who extend this conviction so far as to declare him a fit subject for seclusion in a lunatic asylum.* It seems manifest that a state, which is held thus sufficient to justify the privation of personal liberty on such a plea, and under such a form of restraint, ought not to leave the individual in possession simultaneously of his legal rights or responsibilities; be-

* Not every physician of an asylum, however, entertains this view. We learn, for example, from a notice in the Nederlandsch Tijdschrift voor Geneeskunde for 1857 (p. 581), that Dr. Roell, the zealous and esteemed superintendent of the institution at Dordrecht, complains in his Report, that its doors should be opened so easily to the drunkard, whose pernicious propensity he thinks fitter to be dealt with in a prison than in an asylum, the arrangements of which he desires to see reserved for a class of inmates more deserving of sympathy.

cause that which is enough to deprive him of the one, ought of necessity to denude him of the other also. He cannot be any longer, in common reason, held accountable for crime; for the man who is admitted to be so far insane as to be legally placed under constraint, cannot, at the same time, be legally responsible for actions the possible result of his freedom. Hence a complete subversion of our whole criminal policy with reference to the habitual drunkard; or, on the other hand, in order to be consistent, the necessity for a retrogression in the whole of our judicial views as to the relations of mental disorder to crime. If the habitual drunkard be improperly placed in seclusion as an insane person, the question falls; if properly so placed, we cannot fairly avoid the other horn of the dilemma, and he is stripped, along with his freedom, of his liabilities also. It is true that we have seen an opposite view maintained by the able physician of an extensive lunatic asylum, even so far as to refer without reprobation to the conviction, in 1812, of Thomas Bowler, an epileptic maniac, for shooting at another,* although the panel had been found insane only a month previously by a Commission of Lunacy; but, in adducing such a proof that partial insanity does not exculpate from guilt in every instance, and bringing it to bear on the question of the propriety of confining the drunkard in an asylum, while holding him still burdened with his responsibilities, we could have wished that the usually highly judicious writer had known or remembered the poignant remark of Baron Alderson on this identical case, when cited before him at the trial of Oxford, by the then Attorney-General: "Bowler, I believe, was executed, and very barbarous it was."

It follows with the like necessity, that the drunkard, if so insane, simply as a drunkard, as to be justifiably confined in an asylum, must resign along with his personal immunities, his civil rights also. He is not likely to have the opportunity for many negotiations while so circumstanced; for few honourable minds would willingly enter as parties into any business transactions with an individual in a position thus stamped as more than equivocal. But he even ought not, conformably to reason, to be permitted what would now be the unjust and hazardous privilege of disposing of his property by will; and any testamentary document, made while so situated, should be considered void, unless its provisions, regarded in themselves, were so completely reconcileable with the natural course of succession, or the proper disposition of the party's effects, as to recommend themselves, on that score alone, to the sanction of a court of law. Here again, while it is admitted as a principle, that if a will were executed by a person confined in an asylum, the presumption would be that the deed was invalid on the plea of insanity, unless the contrary could be proved, it has been averred, on the other hand, that many disputed wills have nevertheless been sustained, although made by persons who were not equivocally or hypothetically, but undoubtedly and confessedly, to a certain extent insane at the time at which they were executed. As a marked illustration of this, reference is made to the case of Cartwright

* Shelford, Law concerning Lunatics, p. 590, second edition, 1847.

v. Cartwright and others, where the testatrix wrote her will, and with her own hands, loosened from their ligatures for the purpose, without, as alleged, even any collateral circumstances to indicate the fact of the existence of a lucid interval; yet where Sir William Wynne decided for the validity* of the document. But the grounds for this decision were clearly those we have noted above. The writing was a perfectly steady and correct performance, and entirely consistent with the attachments, impressions, and habits of the writer when in a sane state; under the absence of which conditions the deed would unquestionably have had little chance of being sustained, while their presence leaves us in doubt as to what could have been the real necessity for the ligatures at the time. It is thus the character of a will, and not the position of the testator, which very properly influences a court in their judgment as to its validity; and they would be unreasonable themselves, were they to annul just and rational provisions merely because they appeared to proceed from an irrational source, and thus to inflict a wrong under what would be the fiction of avoiding an injustice. That there is here the necessity for a very rigid test, it is needless to add; and to such a test the will of a drunkard, confined on the plea of insanity, would be justly subjected.

Yet, if the doctrine of the drunkard being the victim of an insane impulse have, under its civil aspects, its serious implications, it may bring its occasional advantages also, and in a way which we suspect few of its abettors in this country have anticipated. We have a remarkable example of this in the Report, for 1855, of the Royal Board of Health for Denmark. A revenue-officer, dismissed for his fits of drunkenness, occurring irregularly and with constantly increasing frequency, applied to the heads of his department for the usual retiring allowance of invalided officers, on the plea of his irresponsibility, through the domination over his will by an incontrollable impulse. The Revenue department consulted the Board of Health as to the reasonableness of this claim, and were answered,* by what seems to us a curious instance of arguing within a circle, that periodic drunkenness can so weaken the power of the will, that it ought to be classed with the other mental and physical disabilities qualifying for a pension, the solace of which the claimant was thus held entitled to receive. Such, however, is in fact the kind of predicament into which the force of a foregone conclusion must necessarily, in due time, lead all those who cling to the notion, that the propensity to drunkenness rests on an insane impulse; and thus, with its adequate diffusion, we may possibly see the day, when our sots shall be regularly pensioned like our deserving invalids and our heroes. And this is but one of a throng of embarrassing complications, more or less easy to foresee, which must inevitably rise in every direction through the same influence.

It will be observed that our question hitherto has been, not whether the inveterate drunkard should or should not be subjected to restraint at all, but whether he should be subjected to that restraint as

* Shelford, op. cit., p. 382.

* Det kong. Sundhedskollegiums Aarsberetning for 1855, p. 330. Kjöbenhavn, 1856.

an insane person, and therefore in a lunatic asylum. To place the drunkard in seclusion, whether by his relatives with the aim of weaning him from his habit, or by the police to free the public from a nuisance, is neither a new notion nor a new practice. The latter, as we have in part shown already, is legal everywhere, though naturally with but an inadequate and transitory efficacy; but the power to effect the former compulsorily is more generally wanting, and it is the perception of this want, growing with our change of manners, and our deeper consciousness and sterner condemnation of the evils of drunken excess, that has manifestly promoted the diffusion of the notion which we have combated, and which has doubtless been the more favoured, because it presented not rarely to the physician and the magistrate a means of evasion from a difficulty, otherwise too recently felt by society with sufficient keenness to have earlier induced the demand for a direct law for its remedy. This law, authorizing, instead of tacitly allowing by a kind of sufferance, as at present, the confinement of the drunkard in places specially allotted for his seclusion and treatment, but apart from the asylums for the ordinarily insane, has been even advocated by certain of those who insist upon the idea of the insane nature of the propensity. If we differ from them when they pronounce the drinker as *ipso facto* insane, we agree with them when they differ from themselves, by requiring for the so-called insane a retreat separate from the lunatic asylum; neither do we advocate now for the first time the institution, under legal sanction, of such a resource for a prolonged suasion and coercion of the drunkard, in order to renovate his health, moral as well as physical, and effect his cure.* But even here we contemplate a difficulty, though we consider it one which ought to be encountered, and which may with prudence be obviated.

By and by, we trust, the want will be less perceptibly felt; but we must own that we place our confidence as to this, far less in the dominance of a provision of law than in the weight of a public opinion. Of the feebleness of the one, when placed at feud with the other, or when destitute of its sympathies, we had long ample illustration in the subsistence of the practice of duelling, and in the constantly recurring opposition between the verdicts of juries and the denunciations of the laws in those instances which had led to fatal results. But now, veering round on the side of reason, public opinion has abrogated, at least in this country, what the menaces of justice could not shake. The duellist, who faced easily the risk of being charged with murder, dares not encounter the certainty of being accused of folly, and contempt has effectually accomplished that before which rigour failed. Excess at table, a virtue with our forefathers, is now gliding into a similar reprobation; and here, too, should our progress in civilization have the fortune to proceed onwards in sufficient strength and purity, while the contagion of the good example extends, the lower classes will discover their interests as the upper and middle classes lose their indifference. It is in this, and in nothing irrespective of this, and

* The notion, in fact, is clearly indicated in the old Roman law. See the fragment of Ulpian, Digest., Lib. xxvii. Tit. x. § 1.

least of all in legislative enactments, that we place our hopes for the repression of drunkenness. Meanwhile, the community has certainly a right to look upon the confirmed drunkard, at least during his paroxysms of actual intoxication, as always more or less insane and dangerous to its safety, and it is besides entitled to escape the nuisance and indecency of his conduct. Thus, passing beyond, but not ignoring the question of the guardianship of his property, and the due protection of family and social interests, it is only another word for a form of the wisest humanity, to claim a power for the public to place him, when all other methods appear to have failed, under peremptory personal restraint in some appropriate institution; and that for the requisite period of detention, whether in part to hide his reproach, or to check his mischief, or, still better, to effect his cure. But we must not rest at this. Society will bring other influences to bear upon him; and it is indirectly, and through the sober, that it will have its chief success in curing the drunken.

Entertaining these views, it is no extensive or formidable apparatus that we contemplate as requisite for the purpose. If the want of a place of seclusion for the inveterate drunkard be really felt by the public, private enterprise will not fail to supply it to the adequate extent; and if the want be not felt, there will be found manifold ways of rendering a constrained provision inoperative. All that is required is, that a direct legislative sanction should be extended generally, and on its just grounds, to that which has hitherto been practised partially, and through the suasive force of a feeling of expediency, sustained though that have been sometimes, and especially recently, by what we believe to be a psychological fallacy. The propensity to drunkenness, where there exists no independent and truer indication of mental derangement, being a vice, and not an insanity, the committal to such places of seclusion will rest with the magistracy; and in them also will be vested the duty of the superintendence of their discipline and management, and the limitation of the period of detention. The acts of the drunkard, during freedom, whether of crime or misdemeanour, will belong, as hitherto, to the cognisance of the police, and to the domain of penal law; while, however his accountability may be modified for the time by the delirium of intoxication, for the state of the drunkenness itself he remains infallibly responsible. It seemed to us a most objectionable proposal, when the Scottish Lunacy Commissioners, not content with recommending the drinker to be first placed in an asylum, a recourse in which there have been many to sustain them, proceeded also further to advise that, after a certain time of coercion, he should be allowed to leave on probation, for a period to be subsequently determined; and that, during this period, the warrant should remain in force, so that the individual could be at once re-admitted, should it prove necessary, without fresh certificates.* Without fresh certificates! Let us hope that in this land of jealous watch over freedom, and the administration of equal laws, it will long remain impossible for any man to be torn from his home and liberty, when once

* Report of Scottish Lunacy Commission, p. 243. 1857.

restored to them, and thrust anew among lunatics, to feel himself a sane man on the morrow, without at least the decent forms and proofs which would be required for the re-committal of a ticket-of-leave convict. It would demand from us a higher idea of the value of the special suitableness of the kind of occupation and training to be obtained in such an institution; of the tendencies of the impressions and associations unavoidable while within its sphere; of the recollections infixed afterwards; and of the fitness of all this as a preparation for a kindly re-entrance upon the duties of life, to induce us to abate a single formality of precaution, that a power so momentous should not be arbitrarily abused. Even with the undeniably insane, the asylum, though a precious advantage, is still a painful necessity, which is to be warily, if not too tardily, approached; and we can well recollect the time, when that kind of philanthropy, the most prevalent of all, which proceeds upon an impulse, was as loud in its outcry against all possibility of unwarrantable seclusion as it is now silent on that score, and clamorous for the widest conceivable extension of the definition of a fitting object for restraint. But nowhere have we more need of reason than where we are to deal with the question of the want of it.

We now abandon the further consideration of the medico-legal relations of drunkenness with some reluctance, for the topic yet presents many deeply interesting points for our scrutiny; but the ramifications of these are too extensive for our present opportunity, though their importance may invite us, on some future occasion, to renew their examination. Of the other publications, the titles of which are placed at the head of our remarks, along with that of Professor Ideler, and bearing a more or less special relation to the subject of insanity, we propose only to take a cursory notice. The work of Dr. Burgess possesses many good materials, but they are by no means always satisfactorily turned to account; while we are sometimes not a little startled by the singularity of his views and statements, and wonder under what experience they could have been gathered. The Report of the New Asylum at Gaustad, near Christiania, which the Norwegians have recently erected at a great cost, we consider to be a model for this description of document, from its clear, precise, and relevant details, and therefore highly creditable to Mr. Sandberg, the director of the institution. We allude to it, in connexion with the new Lunacy Act for Scotland, because it affords us the opportunity of expressing our hope, that no effort will be spared by our Scottish fellow-subjects in order that the provisions of that Act may be carried out upon no narrow views of imitation, but that the good may be gleaned for their humane enterprise from a field as wide as the domain of human charity, wherever exercised. To accomplish this adequately, will require the aid of minds fitted for the task by knowledge of the world, at home and abroad; by an intimate and essentially practical acquaintance with the relations of disease generally, and of insanity especially; by the possession and energetic use of opportunities previously enjoyed for diligent study of all questions involved, under their different aspects, and not exclusively from the side of the asylum, which ignores their social, and therefore most important and truest, relations; and by that prudent capacity

which is able, thus comprehensively tutored, to separate the practicable from the impracticable, the beneficial from the nugatory, and thus neither waste blindly the means of the public, nor trifle with the exigencies of the unfortunate insane. We must recollect here, that all the learning of a physician who has not practised can extend no further than to the terminology of the science. The chartered asylums already existing in Scotland bear justly a high reputation; and it may be truly said of more than one of them, and in a variety of respects, that they have rather led than followed the march of modern improvement. But, beyond these, a fresh effort is to be made, the agents of which must remember that they are enabled to lay their basis on a higher level of experience than has been attainable heretofore; and that it is their duty to master this in the spirit of those who neither dread nor love innovation, but only seek to bring it, and all else, to the test of an intelligence patiently and thoroughly schooled in the world of realities. For our own parts, we shall look with great interest on their proceedings, and shall watch anxiously for the proofs of discrimination in the results. To assume the continuous superintendence of such a charge, is to challenge the confidence of the public on a point regarding which it is, or ought to be, the most deeply sensitive; because in no other is there a power conferred to probe so extensively, and, in a certain sense, so arbitrarily, the most tender recesses of the family life. It is one, therefore, which it would be a guilt to enter upon with temerity, as it would be a callous nature which could perform it with indifference; but it is one in which success would be a genuine triumph, and towards that, if achieved, we shall not, in due season, fail to be prompt in our tribute of commendation.

Let us conclude with the suggestion of a scheme, which, if we desire to apply it more especially to the formation of a place of seclusion for the lunatic, might also be largely modified, so as to be set apart as a fit retreat for the inveterate drunkard. Revolving such subjects in our mind, we have had our individual fancies as to the most eligible arrangements for establishments for the insane; and, above all, we have thought that the idea of the village-asylum has never had rendered to it the justice it deserves, and of which it is capable. We do not speak of such an asylum-village as that at Gheel, because we are convinced, with many competent observers,* that its merits have been over-rated, or at least that its defects have been considered too leniently. We are not willing, besides, to reduce the advantages of a tutored and systematized management to the level of that of the peasant or villager, comparatively uninstructed, and less uniformly open to superintendence and control. Nor can we believe that a system, which has been slowly

* Take, for instance, the following extract from the account of an intelligent visitor, Dr. Ludwig Dahl, in his Report of a journey made at the charge of the Swedish Government: "The first sound I heard, as I approached Gheel, was the clanking of chains, which, extended from leg to leg, hindered by their shortness the patient from wandering too far from his residence. The first story I heard, of life at Gheel, was of two insane young women, who, at a market festival at which they were present, had been rendered pregnant by some soldiers. Both facts serve to prove how difficult it is, under such circumstances, to unite the desirable freedom with the necessary superintendence."—Beretning, &c. Norsk Magazin for Lægevidenskaben, 1858. p. 7.

grafted in Belgium upon the basis of an old superstition, could have been created even there as a recent development; while, in this country, we are still more confident that neither our habits nor ideas are favourable to the trial, and that what could be attained in opposition to these would be at too great a cost and difficulty to be compensated by any success likely to follow.

The asylum-village which we contemplate is a *cure* as well as a *care*-village, and is one, the whole arrangements of which would remain an absolute system of graduated government and discipline, not differing in principle, but merely in mechanism and details, from our existing establishments. Our ideal of its site presents it in a shallow and moderately wooded valley, sloping on three of its sides with gentle acclivities, and opening towards the south-east upon a view, if possible, of the sea, or of an expansive lake in the near vicinity, towards which it contributes an unfailing brook. The little brook should supply a shallow fish-pond, with one or two rustic fountains, overshaded by a few old trees. Our village should have its central green, with its alleys of lime-trees; its cottages for the tractable and quiet among the poorer patients, with their attached flower-plots; its scattered villas, neat, but without prodigality of ornament, with their gardens and orchards, for the wealthier residents; and its workshops to supply the wants of the settlement, or to present available resources for occupation: while a central resort, for recreation and amusement under shelter, fitted for different tastes and classes, would worthily take the place of the rural tavern. Conveniently located with reference to the whole, the superintendent's house would represent with some dignity the dwelling of the surgeon of the hamlet. Neither should the modest church be wanting, placed in its own appropriate seclusion, and with its lowly spire peering from behind a screen of foliage, which would serve also to shelter the little parsonage, to be occupied by the asylum chaplain. A school, adapted for certain forms of instruction, with the schoolmaster's cottage and garden, would be fitting accompaniments for the same vicinity. A little apart from the rest, and surrounded by its proper wall, we should have our representative of the village manor-house; built substantially and plainly, yet not inelegantly, nor divested of all comely appendages; but inexpensively, like all the rest, and after the fashion of those prudent proprietors who held their ostentation within the bounds of their resources, and kept an overplus for works of kindness. But our retreat is no happy valley of Rasselas; and our manor-house would be the hospital for its peculiar community, in which would be guarded the more noisy, violent, and filthy of the residents, whose presence we cannot ignore, because their unhappy condition would necessarily render them the most interesting portion of our care. The village should be surrounded by a farm of a hundred, or a hundred and fifty tolerably fertile acres, with the requisite offices, the whole included within an adequate enclosure; and strewed here and there should lie the dwellings of the steward, the gardeners, and others of the workmen, like sentinels on the watch; while the regular attendants would be distributed among the patients according to circumstances, the cottages being so con-

structed as to present ready communication with each other, and afford facilities, by distribution of speaking-tubes and openness of access, or otherwise, for a due and prompt superintendence.

Such an institution, with its rural and village attributes, and, in a wide degree, with its accordant occupations, rivalries, interests, and amusements, we are convinced, would be no impracticable project; and the primary cost of its complete establishment, with the purchase of the land, and the addition of every desirable appurtenance, adapted to a population of three hundred patients, with a full complement of officials, would, at a rough estimate, not exceed 150*l.* for each patient, so that a judicious management might render it safely remunerative, and at an even unusually moderate scale of charges.* An establishment, however, for half this number of patients would, if not in an economic, at least in a scientific and curative point of view, be infinitely preferable; for any attempt to individualize cases with the slightest pretension to adequacy, among the masses now usually congregated under a scanty medical staff in our leading asylums, is but a ridiculous pretence, the futility of which forms one of the most prominent defects in their constitution. It is evident that, after the completion of the buildings of our *cure-village*, the free-stone for which we should hope to be supplied within the limits of the property, much of the subsequent labour in the original laying out of the precincts, walks, gardens, &c., might be advantageously accomplished by the patients, under a qualified guidance, and this independent of the succession of appropriate and genial occupations to be secured to them afterwards. Will any of our three Lunacy Boards thank us for our suggestion?

Review II.

1. *Elements of Pathological Anatomy.* By Samuel D. Gross, M.D., Professor of Surgery in the Jefferson Medical College of Philadelphia, and formerly Professor of Pathological Anatomy in the Medical Department of the Cincinnati College. Third Edition. Illustrated by Three Hundred and Forty-two Engravings on Wood. —*Philadelphia*, 1857. pp. 771.
2. *Human Histology in its Relations to Descriptive Anatomy, Physiology, and Pathology.* By E. R. Peaslee, A.M., M.D., Professor of Physiology and Pathology in the New York Medical College; of Anatomy in Dartmouth College; and of Surgery in the Medical School of Maine. With Four Hundred and Thirty-four Illustrations on Wood.—*Philadelphia*, 1857. pp. 616.
3. *Die Kunstgerechte Entfernung der Eingeweide des Menschlichen Körpers; Ein Leitfaden für Wissenschaftliche Leichenöffnungn.* Von G. Valentin, Prof. in Bern.—*Frankfurt-am-Maine*, 1857. pp. 44.

On the Scientific Mode of Removing the Viscera in Performing Autopsies. By Professor Valentin.

* At the extensive asylum of Slesvig, a new division for the females has been built from savings produced chiefly by the well-managed agricultural labour of the patients.—Dahl, Norsk Magazin for Lægevidenskaben, 1857. p. 391.

IN taking up a treatise on Pathological Anatomy, we feel ourselves in a special manner confronted by the science of our calling. We come to learn what disease does, we seek to know what disease is. It is no more a matter of detecting and weighing signs, of finding indications to direct our therapeutic efforts, of administering remedies whose working we dimly or not at all comprehend. The "befund" of the necropsy in the majority of instances is the dictum from which there is no appeal, the final proof to which the accomplished diagnostician confidently refers for a fulfilment of his predictions, and without which our French brethren of an earlier period used (if we may trust *on dit*) scarcely to consider a case complete. But, we need not say it, the knowledge of the material changes wrought by disease is not the only aim of pathological anatomy; beyond this, indeed, morbid anatomy does not go, but the complete science has a far wider scope. Who can look at a pleura full of fluid, a contracted or tattered valve, a liver far gone in fibroid change, or a cancerous tumour, and feel no inward questionings as to how these things came to be? Deep planted in the human breast is the tendency to inquire into causes. So wrote Virgil,

> "Felix qui potuit rerum cognoscere causas,
> Atque metus omnes et inexorabile fatum
> Subjecit pedibus;"

and so in our own day one of the largest minds states it as his deliberate conviction, that "it appears to be required, both by the analogy of the most successful efforts of science in past times, and by the irrepressible speculative powers of the human mind, that we should attempt to discover both the *laws of phenomena and their causes.* In every department of science, when prosecuted far enough, these two great stages of investigation must succeed each other." Thus, then, our morbid anatomy of necessity leads us on to pathology, to inquire into the "story" of the causation of the material changes which the eye contemplates. Let it be said at once that here we must come short, must feel the exceeding imperfection of our knowledge. Morbid anatomy is well nigh as complete as its healthy counterpart; the diseased body, like the normal, has been minutely scanned and explored and described. But in physiology we have but dim and imperfect ideas of the various powers that move the vital machinery; and in pathology we have still less knowledge of these changed and perverted motors, or of the agencies that have so affected them. Painfully does the medical philosopher feel this, especially as his mental thirst is intensified by the sympathy he must have for human suffering, and the call ever sounding to him to relieve it if possible. The mere morbid anatomist may elaborately detail all the "insatiable variety" of form and arrangement which cancerous growths display; but the physician, whose heart and thoughts are with the living, craves with an impatient longing for one gleam of light to indicate the essential substratum of all these morbid erections, one ray to show what rational means he may adopt to nullify the fatal influence. It almost seems to him a poor trifling to describe so curiously all the things that are non-essential, and to leave wholly untouched, even by an effort, the vital point.

The author whose work stands first on our list seems by no means

to participate in these views. We quote a passage from the commencement of his volume, which expresses very plainly his abnegation of all deep inquiry, and prepares his readers for the kind of information they are to find in the succeeding pages :

"Of the essence of disease," says Dr. Gross, "very little is known; indeed, nothing at all; nor can the utmost ingenuity hope to remove the veil which still envelopes the subject, until the physiology and pathology of the vascular and nervous systems shall be better understood. The proximate cause of morbid action, and the immediate cause of life in the healthy state, are as inscrutable to the human mind as the cause of gravitation, of attraction, and repulsion. All we can boast of is, that we know something of their effects; beyond this, it is extremely problematical whether we shall ever be able to penetrate. With this, indeed, every philosophical inquirer after truth should be contented, remembering that the secrets of Nature are not easily detected, and that to God alone belongs the knowledge of the intrinsic property of things."

We do rather marvel that one belonging to a so eminently "go-ahead" nation should have given the above as his deliberate opinion. Have no steps been taken, no great advances made, in the knowledge of the physiology and pathology of the nervous and vascular systems? Have Bell, and Marshall Hall, and Reid, and Bernard won no ground for us? Is the experience of the past such as to discourage hope for the future? Though we may never know the immediate cause of life, yet surely we may be permitted to discover that which perverts the normal course of health, and we may discern in what direction the perversion takes place. The cause of gravitation, very probably, we shall never be able to reach; we can only assign it to the great First Cause, and consider it as an ultimate property of matter. But could the physiologist produce for vital phenomena so comprehensive and sufficient a theory as the astronomer can for cosmical, what a gain for science it were! True it is the secrets of Nature are not easily detected; but what then? Are we to sit down and refuse the search, or are we to gird our loins for more earnest and intelligent toil? Had Kepler and Newton reasoned as Dr. Gross, where had the theory of gravitation been, or what would have been achieved in any branch of knowledge requiring more than superficial observation? In answer to the last sentence of the above extract, we would quote the remarks of an excellent Scotch writer—"God denies nothing to well directed diligence." Certainly it is wonderful how much is given to clear-headed and honest labour.

We have no wish to enter into a controversy with the author upon all the points upon which we happen to differ from him, but we cannot pass over two of our author's propositions—the first of which he himself thinks may "be startling" to some; to us we confess both are. The first proposition affirms that, with few exceptions, "all organic diseases, whatever be their seat or extent, are the result of inflammatory action, either of an acute or chronic kind." The second proposition is, "that every inflammation, irritation, or morbid action, is originally of a local nature—that is, it always makes its impression in the first instance upon some particular part, texture, or organ.

After the malady has continued for a longer or shorter time, it may extend to and implicate other structures." The term organic in the first proposition is made to have a wide latitude, including "every temporary alteration which the tissues experience when in a state of disease." It does startle us that Dr. Gross should pronounce all the minor atrophic changes, the results of debility, anæmia, &c., fevers; the great family of scrofulous affections, the manifold degenerations, the whole class of growths, innocent and malignant, to constitute but a few exceptions to his first dogma. No doubt inflammation has a great deal to answer for, but we think decay and morbid growth have quite as much. Of inflammation we may presently say more in particular; but we wish to remark at present, that while we are debating as to the part which it plays in morbid change, we ought to form some tolerably exact idea as to what is to be called inflammation, and to which states the term is to be restricted. Our own view is that the *typical* case of inflammation, acute or chronic, is easy enough to recognise and decide upon, but that the inflammatory character gradually shades off, and is lost as we advance in the directions of morbid growth and degeneration. The acumen of the pathologist is exercised in determining to which of two types a given case comes nearest, what morbid element is most marked in it. Thus, we suppose fatty liver, fatty heart, arterial atheroma, pulmonary emphysema, anæmia, are organic changes which no one would put down to inflammation; they are typical instances of decay, of failure of the vital power of the several living structures. So, too, we suppose no one* would affirm that a scirrhous, or encephaloid, or fatty tumour, was the result of inflammation; the prominent feature in these is unquestionably that of morbid growth. But with regard to cirrhosis of the liver and granular disease of the kidney, there may be some room for doubt. To our mind, the first comes much nearer to morbid growth than to inflammatory action, and the second to degeneration. But in both there may be a certain dose of inflammatory *nisus*. The recollection of this caution to denominate any given instance according to the type of morbid action which it *most* approximates to, will prevent us falling into such errors as those of Broussais' followers, towards which we fear Dr. Gross has a decided leaning.

With regard to the second proposition, we demur quite as much as we do to the first. Does Dr. Gross mean to affirm that small-pox, scarlet-fever, influenza, gout, and rheumatism, are primarily local, and subsequently "extend their sphere of action so as to become general?" No doubt many diseases do proceed in this way, but of a surety quite as many do the reverse. At p. 40 the author reiterates strongly this dogma, and tells us point blank that "if this opinion shall ever be proved to be true, the whole class of febrile maladies, with its hundred varieties and subdivisions, will cease to have a place in our medical treatises." For this day we suspect, however, we shall have long to wait, and meanwhile Dr. Gross's teaching appears to us grievously unsound.

* When writing this we had not perused the section on scirrhus.

After these introductory statements we come to the subject of inflammation, and all its various attendant conditions or consequences, which are discussed in thirteen chapters. Having expressed his contempt for theories of inflammation (which, however, he takes some notice of at pp. 56–58), Dr. Gross begins with the phenomena. Taking the four Celsian marks as the usually accepted signs of inflammation, he observes that these

"Are not always present, nor are they the only circumstances which occur in inflammation in this disease (*sic*): in every case there is a perversion of the vital actions, attended with an altered state of the nutritive and secretory functions."

Again,

"If we regard the four phenomena—discoloration, heat, pain, and swelling as being essential to the process, it will be at once perceived that there can be but few inflammations; and we shall therefore be obliged, in describing diseases, to invoke other names, as irritation and fever, a blind adherence to which has unfortunately tended too much to retard the progress of pathological science."

These terms he would have "entirely discarded," and have "diseases named according to the tissues they implicate." It would seem from the above that an alteration or perversion of nutrition and secretion essentially constitutes, in Dr. Gross's eyes, inflammation, and the other ordinarily observed conditions are not necessary. On such a view it is difficult to say what is not inflammation. Yet, while thus advocating the supreme importance of the derangement of nutrition, he speaks at p. 57 slightingly of Dr. Hughes Bennett's theory, which ascribes the production of the redness, heat, pain, and swelling to abnormal nutrition, or in other words, recognises the deranged tissue-force as the chief agent in producing the visible phenomena. To our mind Dr. Gross's view is utterly vague and pointless; Dr. Bennett's is rational and appropriate to the facts. The former makes no distinction between the typical case and the numerous instances in which the characters are feebly marked, or are in various degrees replaced by others. In objecting to the term irritation, Dr. Gross denies that Mr. Travers or other writers have pointed out "anything concerning the essential character of the disease," or, "told us anything of the peculiar condition of the nervous and vascular systems which accompanies it." Prostration with excitement is the descriptive term used by Mr. Travers to express the characters of irritation, and we think it is undeniably correct. Indeed, we should have thought it quite sufficient to stand by the bedside of a case of delirium tremens, or delirium traumaticum, to satisfy oneself of this. One might, we think, without extraordinary difficulty, perceive that there was high exhausting excitement of the nervous, and prostration or enfeeblement of the circulating, system. The action of remedies, to say nothing of post-mortem examination, would convince most persons, we suppose, that in such cases at least inflammation was out of the question; and that amid all the show of increased activity in the nervous and vascular systems, there was profound debility and failure of functional power. The demonstrated effects of paralysis of the sympathetic upon the arteries does not appear

to be known to Dr. Gross, nor the almost equally certain influence of the medulla oblongata, and probably other nervous centres, upon the action of the heart. The humid contractions of the latter organ in a weakly person are anything but a sign of increased functional energy, but on the contrary are certainly indicative of debility. Hyperæsthesia, or increased excitability, is almost the sure concomitant of weakness, whatever be the part whose power is failing. An example which we think our author would do well to consider, because it is so free from any local complication, is that of feverish disorder occurring in consequence of over-exertion. A physician of our acquaintance has, on more than one occasion, after prolonged pedestrian exercise, had attacks of this kind, which were once regarded by those who saw him as the commencement of a typhoid fever. We have ourselves observed the same. Here the only pathological change which we can speak certainly of is exhaustion of nervous force. In ordinary cases this is confined to the cerebro-spinal nerves and centres, but in some few it involves the sympathetic, and then the phenomena of pyrexia, or fever, show themselves. As the nervous power is restored by rest, the pyrexial condition ceases. If Dr. Gross assumes a latent inflammation in such cases, have we not a right to say "de non existentibus et de non apparentibus eadem est ratio?" We would fain persuade him to study carefully Virchow's admirable chapter on "Fieber," in the 'Handbuch der Speciellen Pathologie und Therapie,' and then to consider whether the phenomena of febrile diseases are not much more intelligible on such a view than on his own.

At p. 45, remarking on the absence of the ordinary signs of inflammation, heat, pain, &c., Dr. Gross says:

> "In gastritis, the only manifestation of which is frequently irritability without heat, or pain, or uneasiness, or pressure, the digestive function is entirely suspended, gastric juice is no longer secreted, and the organ is oppressed by the mildest articles of food."

This passage gives us a considerable notion of what sort a good many of Dr. Gross's inflammations are. In England we should look on such symptoms as simply indicative of debility of the organ, or of disordered sensibility.

At pp. 49 *et seq.*, we have an attempt to analyse the inflammatory process. The author says:

> "The very first step in the process of inflammation is an altered sensibility of the part, produced by some hurtful agent, which the system makes an effort to dislodge. To effect this, the local impression is reflected upon the cerebro-spinal axis, and through this again upon the heart, which, being sympathetically incited to increased action, sends more blood to the part concerned than it is accustomed to receive, at the same time that the capillaries are perceptibly dilated. . . . . That these vessels do contract and dilate no one will dispute, for experiment has fully decided this point."

We must remark on this passage, 1st, That the system making an effort to dislodge a cause of irritation, is quite a myth, a teleological fancy, which should not be introduced in a "science of observation and induction;" 2, That it puzzles us to conceive how the heart is to

send more blood to one part than to another, to cause "preternatural determination of blood," in consequence of having its action accelerated; 3, That Bidder has shown that the capillaries possess no contractility, and do *not* contract and dilate, a point of which our own observations have quite convinced us; 4, That everybody, we believe, is agreed that the arteries are the regulators of the blood-current, allowing more or less afflux to a given part to take place.

The subject of congestion and discoloration is well treated, and there are some good remarks at p. 56 of the deceptive resemblances between these states and inflammatory redness. The so-called terminations of inflammation are correctly stated to be for the most part concomitant conditions of the process. Chapter II., respecting the effusion of semen, contains nothing of novelty, except a notable instance of our author's highly inflammatory diathesis. He argues that, because inflammatory congestion may have quite disappeared when copious serous effusion has taken place, therefore our ordinary dropsies, such as occur with obstructed portal vein, or cardiac alveolar alterations, are dependent on inflammation, and not simply on the mechanical obstruction. The passage (see pp. 62, 63) is too long to quote. We can only give our readers the conclusion, which Dr. Gross thinks is obvious, but which neither his facts nor his reasoning appear to our mind in the least to warrant: "The effusion of serosity, no matter in what part, organ, or region it occurs, is invariably the result of a process analogous to, if not strictly identical with, inflammation." We may remark incidentally, that it seems to us objectionable to alter, even by translation, familiar terms, and we really do not see what advantage is gained by calling the inferior vena cava, "the ascending hollow vein." A novice might imagine that all veins were not hollow.

Chapter III. describes Lymphization—a term meant to be parallel with suppuration, and which we are content to accept. The information given on this subject is fairly complete, and in particular the question of the vascularization of effused fibrin is well treated. After describing the outgrowth process, Dr. Gross affirms his belief in at least the possibility of the independent development of vessels. He says:

"In the pleura it has often occurred to me to see as many as three, four, five, and even six, layers of this substance (exuded lymph) arranged so as to intercept cavities of various sizes, filled with serum, plates of fibrin, pus, and even blood. In some of the cases the circumstances were such as to render it perfectly certain that the development of these adventitious membranes must have taken place with so much rapidity as to preclude the idea that their vascularization could be effected through the agency of the subjacent serous structure."

Mr. Paget's testimony is very strong in favour of the outgrowth view, and Mr. Simon's is also on the same side. Rokitansky, in the second edition of his first volume (on General Pathology), seems to have changed his views so as to incline much more to the outgrowth theory than he did in the first edition. Our own examinations incline us to agree with Dr. Gross and others; the notes of one we subjoin.

A healthy man died with double pneumonia; the surface of the right pleura was coated throughout with a thin layer of recent lymph, which formed here and there thickish yellow patches; many of these contained distinct red spots, none larger than a pin's head and some smaller. Close scrutiny of these spots showed positively that they were formed of circumscribed masses of blood-globules, lying imprisoned in a firm fibroid and partly corpuscular substance, which contained also much diffused oily matter. The blood-globules appeared normal; there were no pigment grains around them. Nothing was to be seen of any development of vessels, but vacuolæ had begun to form in the fibrinous substance. The blood-spots were principally situated on the attached surface of the exudation layer, but at various depths; they were all actually in the substance of this, and not on its surface. The development of new blood in exudation-blastema, if established, would be an important point with reference to the hypothesis maintained by some as to the part taken by the ductless and lymphatic glands in the formation of blood. It would show that blood might be produced independently of their influence.

Chapter IV., on Suppuration, we can cordially recommend as containing a large amount of good information, more than, perhaps, is to be met with in any other work. Dr. Gross affirms, perhaps correctly, that

"In what is called a cold abscess, the formation of which is sometimes the work of months, inflammation is just as much concerned as in a phlegmonous boil that is developed in two or three days. The only difference is, that in the one the process goes on slowly, almost imperceptibly, while in the other it proceeds rapidly."

The pus of these abscesses, he says, is essentially scrofulous, of thin whey-like consistence, intermixed with small caseous flakes. Mr. Paget gives a very similar account, but intimates that the pus has undergone degeneration. It seems to us open to question whether, in these *abscès froids*, the process is not one of slow effusion of fibrinous fluid, which corpusculates into puriform matter, and is more like to deposition of tubercle, or of bacony matter, than to the actions of inflammation. Certainly, hyperæmic afflux does not seem necessary.

In the chapter on Hæmorrhage it is assumed that blood-globules may escape through the same pores as give passage to fluids and gases, and thus hæmorrhage by exhalation is explained. This opinion, we suspect, will satisfy the minds of none who have ever looked at a capillary through the microscope. In the case referred to, we see no other possibility than that the consistence of the homogeneous membrane becomes changed, softened, gelatinous, so as to be easily penetrated. After a short chapter on Softening we come to Gangrene, which is treated of pretty fully. True to his hobby, Dr. Gross can form no other idea of the mode of action of ergot in producing gangrene, except that "it exerts its deleterious impression in the first instance upon the blood, and through it upon the capillaries, causing inflammation in them, followed by gangrene." The opinion first put forth by Mr. Simon, that the action of ergot in causing gangrene is of the same

kind as in causing uterine contraction, in both instances stimulating organic muscular fibre to contract, is to our mind a great deal more probable than that of the author. M. Trousseau, noticing the occasional occurrence of sphacelus in those peasants who use ergotized rye as food, says, "Sphacèle qui suivant toutes les apparences est causé par l'obliteration des vaisseaux artériels de la partie." The account given by Dr. Gross would lead us to believe that the epidemic prevalence of gangrene in various parts of Europe at different times was owing to the use of ergot. This view is entirely rejected by Trousseau, and apparently on sufficient evidence. Ulceration is, of course, in Dr. Gross's mind, essentially connected with inflammation, and we shall not think it worth while to argue the reverse; only we hope that if we ever have simple ulceration of the cornea, we shall not fall into the hands of one of his disciples.

Granulations the author regards as of "extremely complex structure." It rather appears to us they are of extremely simple structure, consisting solely of nascent or embryonic areolar tissue and bloodvessels with an epithelial investment.

The chapters on Cicatrization, Induration, Hypertrophy, and Atrophy, call for no particular remark.

Transformations are well described, and judicious intimations given that the change produced is often more of an incomplete than perfect kind. Dr. Gross is "disposed to think" that fatty transformation is "uniformly the result of a low and imperceptible grade of inflammatory irritation." We are quite disposed to agree with him as to its being imperceptible!

The account of Pneumatoses in Chapter XVI. is very full and interesting—more so than any other we know.

Chapter XVII. is devoted to a description of Polypes, and though there is not much to find fault with in it, but rather the reverse, we must take exception to the heading. Dr. Gross allows that it is not very appropriate, but thinks it would only occasion confusion to change it. The name might have been retained for occasional use if he had grouped the tumours to which it is applied under the head of fibro-cellular, as Mr. Paget has done, or tegumentary, after the example of Mr. Simon. That these growths are "liable to take on malignant disease," is a statement which few pathologists of the present day will be inclined to assent to. Dr. Gross seems to consider it no uncommon occurrence.

Chapter XVIII. gives a pretty full and exact account of hydatids. We feel surprised, however, to read that the proper hydatid membrane is exceedingly delicate, so as to yield under the slightest pressure of the finger. Its curly tendency and peculiar lamination are not at all noticed.

Chapter XIX., on Serous Cysts, treats of all the various kinds of these formations. The distinctive epithet would have been better omitted.

Chapter XX. treats of Heterologous Formations, intending thereby tubercle, melanosis, scirrhus, encephaloid, colloid, epithelial cancer,

We quite join the author in his suspicion that the term heterologous is "altogether ill-chosen and out of place," and we think it would have been much better if he had forborne from any such grouping. The subject of tubercle is discussed in about sixteen pages, which of course are mainly a compilation from the works of others. The only points which seem to require notice, and that we are sorry to say in the way of protest, are the statements "that tubercles are organized structures," and that they are "always of inflammatory origin," though of a very mild and imperceptible kind. As to the latter point, we can scarcely imagine any feebler array of arguments than those six set down at pp. 157, 158, which we shall not take up space to reproduce in full. *Ex uno disce omnes.* "In the experiments of Cruveilhier, Kay, and Saunders, well-characterized tubercles were produced in a very short time, simply by dropping mercury into the trachea." M. Andral, repeating these experiments, found, as might have been expected, that the metallic globules had simply become surrounded by purulent matter or false membrane. After this we have the case of the needle-grinders, miners, &c., who inhale irritating matters, cited, and it is stated that they are peculiarly prone to phthisis. The evidence adduced by Mr. Ancell from various sources makes it almost certain, however, that tubercles are not produced in these workers without the intervention of other causes.

As to the question of organization, Dr. Gross does not assert that tubercles are invariably organized—e.g., where the tuberculous matter is spread over the free surface of mucous membranes. In other situations, however, he is satisfied that they possess vessels of their own, and even thinks it quite philosophical to infer the existence of nerves and absorbents. "If under favourable circumstances plastic lymph is capable of generating its own vessels, why should not tubercle be? Because, Dr. Gross, tubercle is eminently *a*-plastic.

The section on Melanosis contains a tolerably full *résumé* of the facts known respecting it. The author seems to be satisfied that it is a separate and distinct affection, and not merely an accident which may attach to various growths. This idea causes him to consider it very remarkable that in a case of widely disseminated melanotic cancer which he met with, there existed numerous white and greyish tubercles in the midst of the black. To our mind there is nothing remarkable in it.

We proceed to Scirrhus, respecting which our author concludes,—

"First, that the deposit of scirrhus is preceded by inflammation; secondly, that it has a great predilection for the glandular viscera; thirdly, that it rarely occurs under the age of forty; fourthly, that the matter of which it consists, when first deposited, strongly resembles that of tubercle; and lastly, that this matter is deposited always into the cellular tissue of our organs, in such a manner as to transform their proper parenchymatous structure."

Not one of these conclusions can we consider free from objection. The third, which is nearest the truth, ought to have been modified by a reference to Mr. Paget's table, showing that in 98 cases out of 400, or nearly 1 in 4, the cancer was observed before the completion of the

fortieth year. The expression "transform," used in the last, is objectionable, though from the context we believe Dr. Gross to mean that the new growth causes wasting of the adjacent parts.

The sections on Encephaloid, Colloid, and Epithelial Cancer are not to be regarded as very complete, or as fairly representing the present state of our knowledge; still, they contain a good deal of what is most certain, and we do not observe any statement that we are disposed to quarrel with.

We have thus taken a cursory review of the first part of this volume, and regret that want of space precludes us from entering on the second, which is devoted to special pathological anatomy. We feel, however, bound to say, after looking through it carefully, that we are quite satisfied of its possessing great merit. It is full of descriptive detail, based evidently in great part on original observation, and abundantly illustrated by very excellent woodcuts. There is not much of minute investigation of diseased structure, but all the grosser and more manifest changes are very completely described. The work is quite a repertory of fact, and will almost bear a comparison with that of Rokitansky in this respect. The author evidently belongs to the older generation of pathologists, to those who loved to create a museum round them, and to whom a "beautiful specimen" was of more value than the development of a principle or the explanation of a morbid process. The work of these men is good and sound, though not of the highest order: it is rather preliminary than final.

Dr. Peaslee's compilation has for its aim:

"1. To give a connected view of the simple chemical elements, of the immediate principles, the simple structural elements, and the proper tissues entering into the composition of the fluids and the solids of the human body. 2. To associate with the structural elements and the tissues their function while in health, and the changes they undergo in disease."

In fulfilling his purpose, the author has laid under contribution most of the best works on the subjects he has to deal with, and he gives a list of these sources of information at the end of his volume. Woodcuts and engravings have been borrowed even more freely than the text, and we hardly think with sufficient acknowledgment. A great amount of information no doubt has been brought together, and a book manufactured which may certainly be useful to the student. We should, however, be glad to see American professors more given to producing, and American students more given to reading, original works.

The title of Dr. Valentin's little work involuntarily reminds us of a hospital patient who one day urgently requested us to give him some medicine "to *re*move his bowels!" We hope our readers and the author will pardon this descent from critical gravity, and we proceed to say what we think of the *leitfaden* presented to us. It is a systematic detail of the best methods of conducting post-mortem examinations, and may be perused, we doubt not, with much benefit by any who have had no great experience in that way, and have no careful prosector to guide them. We cannot but think, however, that

half-a-dozen practical lessons by Dr. Valentin or his compeers would convey very much more usable instruction than the perusal of the pamphlet. As somewhat old and cunning in this matter ourselves, we will venture one or two remarks. In removing the brain, Dr. Valentin directs to divide the tentorium along the edge of the petrous bone, but to leave it attached to the groove of the horizontal sinus. We prefer to divide it right round at once, so as to bring it away with the encephalic mass, because otherwise it holds the latter after it is turned out of the cranial cavity. Dr. Valentin does not notice the special difficulty which meets us in examining the heads of young children, where the dura mater cleaves so fast to the thin and weak bones, that it is next to impossible to separate it from them. In this case we adopt the proceeding of cutting through the dura mater after sawing through the skull, and then, without attempting to remove the upper segment, dividing the falx cerebri, and raising up the brain and turning it out into the still adherent skull-cap. With the aid of an assistant to divide the tentorium, this may be done very nicely. Our author insists that the hypophysis should be removed still attached to the infundibulum. We can scarcely conceive the possibility of doing this—at least, in the majority of cases—on account of the delicacy of the connexion, and we cannot see the advantage of it, unless the object were to make a perfect anatomical preparation.

REVIEW III.

1. *A Theoretical and Practical Treatise on Midwifery, including the Diseases of Pregnancy and Parturition, and the Attentions required by the Child from Birth to the Period of Weaning.* By P. CAZEAUX, Member of the Imperial Academy of Medicine, Adjunct Professor in the Faculty of Medicine of Paris, &c. &c. Second American (translated from the fifth French) Edition. By WM. R. BULLOCK, M.D. With 140 Illustrations.—*Philadelphia*, 1857. 8vo, pp. 992.
2. *The Principles and Practice of Obstetrics; including the Treatment of Chronic Inflammation of the Uterus, considered as a frequent Cause of Abortion.* By HENRY MILLER, M.D., Professor of Obstetric Medicine in the Medical Department of the University of Louisville. With Illustrations on Wood.—*Philadelphia*, 1858. 8vo, pp. 624.
3. *Elements of Practical Midwifery; or, Companion to the Lying-in Room.* By CHARLES WALLER, M.D., Obstetric Physician to, and Lecturer on Midwifery at, St. Thomas's Hospital. Fourth Edition, with Plates.—*London*, 1858. 12mo, pp. 193.

THE above treatises, of respectively French, American, and English origin, although differing widely in their scope and character, reflect some of the more striking peculiarities of the national literature to which they severally belong. Thus, the French work is distinguished

by extraordinary elaboration and redundancy of detail, great freedom of style, and a subdivision of the matters treated of to an almost microscopic minuteness. The American, laboured and uneuphonious in its diction, is essentially utilitarian in its aim, but crude and hasty in its generalizations, and not always sound in the practical teaching it enforces; whilst the English work illustrates a remarkable phase in the literature of this country at the present day—viz., the endeavour to render learning in every department of human inquiry as accessible and easy as possible, to smooth away all obstacles to its attainment, and present it in a light, easy, and epigrammatic form; and thus we have the art and science of midwifery, which in the French work extends over a thousand, and in the American over six hundred large octavo pages, condensed in the English into a small duodecimo volume not containing as many as two hundred.

To place, then, such a publication as this in comparison with the very elaborate treatises of the French and American writers, or to regard it as a correct exposition of the present state of obstetric science in this country, would be equally unjust to the author and ourselves. We have adverted to the prevailing influence under which it has probably been conceived and executed, and it is right to add that it is professedly intended as a manual for students and young practitioners only. In this point of view, however, we cannot speak of it in very high terms. It is extremely meagre and commonplace in its details, exhibits but little power of analysis or condensation on the part of its author, and, with the exception probably of the question of the extraction of the placenta in certain cases of placenta prævia, and that of the general employment of anæsthetics in midwifery, which are treated of rather fully, scarcely touches upon the many practical subjects which of late years have engaged the attention of English obstetricians.

On the other hand, the French and American treatises are extremely elaborate; the former, indeed, from the fulness and multiplicity of its details, might be almost regarded as an encyclopædia of, rather than as a treatise upon, midwifery; whilst both, from the high professional status of their authors, and the care expended in the preparation of their respective publications, may be regarded as fairly representing the present state of obstetric science in their respective countries. It is in this point of view that we shall proceed to consider them, and as such we shall avail ourselves of the practical views set forward in each to inquire how various subjects which of late have excited the interest of the profession in this country are regarded elsewhere.

Passing over the earlier chapters of both, which treat of the anatomy and physiology of the generative organs of the female, we find, in the sections devoted to the pathology of gestation, a subject treated of in each which of late has much occupied the attention of the profession —viz., the influence of inflammation and ulceration of the cervix uteri upon the course and phenomena of pregnancy; and as this is a question of much practical importance, and one upon which much difference of opinion still exists, we propose to examine it with the aid of the writings before us.

The views of Dr. Miller upon this subject appear to be identical with those which were promulgated in this country some twelve or fourteen years ago, and directly deduced from observations made by the speculum in some of the Parisian hospitals. He observes—

"That the most prolific cause of abortion consists in a diseased state of the gestative organ either in whole or in part; that when a portion only is affected, it is the cervix rather than the body; and that as to the particular disease itself, it is inflammation of the uterine mucous membrane, which may invade also the parenchyma of the organ." (p. 182.)

In support of this opinion, he however merely alleges that in his experience he had had many opportunities of verifying the frequent existence of inflammation with ulceration of the cervix uteri during pregnancy, and of satisfying himself that it is not an uncommon cause of abortion. We do not find any other evidence adduced in support of the truth of this doctrine derived from his personal observations, but he supports it by the following argument, which is moreover levelled at another supposed cause of abortion—viz., a morbidly irritable state of the uterus.

"Against inflammation of the uterine mucous membrane, either of the cervix or body, considered as a cause of abortion, no such objections lie, nor can it be questioned that it is an adequate cause, for it is in harmony with the influence of inflammatory affections of other mucous membranes on the functions of the organs which they line. Gastro-enteritis, for example, quickens and perverts the peristaltic motion of the alimentary canal, and leads to the precipitate ejection of its contents, not permitting the food to remain a sufficient length of time to be digested. In this case the food may be said to be prematurely expelled, just as the ovum is liable to be when the organ it inhabits is inflamed." (p. 184.)

Now, without stopping to criticise the singular analogy which is said to exist between the vomiting of gastritis and the process of abortion after the ovum has been some six weeks or two months in the uterine cavity, and disregarding the admitted fact that the state of the uterus during the healthiest gestation is one of so much vascular excitement as to be closely allied to inflammation, we would wish to direct attention to the circumstance, that throughout the whole of the chapter on this subject, Dr. Miller has not once attempted to establish the relationship of cause and effect between the existence of inflammation and ulceration of the cervix uteri, and the consequences which he has imputed to them; and so far he may be said to be in an analogous position to that of many members of the profession in this country some twelve or thirteen years ago, who assumed that these conditions furnished the key-stone to the diseases of the pregnant state, upon data very similar to those now relied upon by Dr. Miller, but which subsequent investigation has tended very much to invalidate.

On the other hand, the observations of M. Cazeaux possess far higher value, because specially directed to this end. He is not satisfied with noting the existence of these abnormities of the cervix, but he seeks to determine their pathological value and significance also—whether, for instance, they are really as injurious as they have been said to be—whether they are so strictly pathological as they have been

assumed to be—or whether they may not and do not co-exist with the healthiest and most favourable progress of gestation. Now the conclusions he has arrived at upon these points harmonize for the most part with those of the several English practitioners who have recently investigated this question, and stand in direct opposition to the views of Dr. Miller and those which were formerly accepted and believed in in this country. After describing the various abnormal appearances met with on the cervix uteri, both in primiparous and multiparous females, which strictly correspond with those which have been described as inflammation and ulceration of the cervix uteri, he thus expresses himself in regard to them:

"MM. Boys de Loury, Costilhes, Coffin, and Bennett, who have directed their attention more particularly to ulcerations occurring in the first half of gestation, have been so forcibly struck with their tendency to produce abortion and puerperal diseases, that they class them with the most common causes of miscarriage. Dr. Bennett goes so far as to call them the keystone of all the diseases of the pregnant female, and the most frequent cause of difficult labours, obstinate vomiting, moles, abortion, and hæmorrhage.

"Notwithstanding the smallness of their number, the observations which I have been able to make differ so completely with the results obtained by these gentlemen, that I was tempted to accuse them of some exaggeration. However, after having heard MM. Huguier, Gosselin, Danyeu, Cloquet, &c., proclaim the innocence of these ulcerations, I have no hesitation in saying that they have misconstrued the facts observed by them. Finally, we would add, that after having read their observations, there seemed reasons for inquiring whether in many cases syphilis may not have been the principal cause of the accidents, and in others whether the frequent introduction of the speculum, and the numerous cauterizations which had been practised, may not have played the most important part in the production of the abortions." (p. 369.)

Elsewhere Mons. Cazeaux expresses a doubt as to the pathological nature of many of the lesions of the cervix which have been described as inflammatory and ulcerative:

"Unless my observations have been for a long time subject to a series of singular coincidences, it is probable that what we have just described is the normal condition, and should not be regarded as pathological, but simply as a consequence of the progress of gestation. As the violet-red colour, the swelling, the softening, and the almost fungous condition of the walls of the neck, are peculiar to pregnancy, and in no wise interfere with its progress, so I regard the ulcerations as a consequence of a physiological process, extreme in degree, and of no greater importance than they." (p. 368.)

Lastly, as regards the value of topical treatment, so much relied upon by some for the cure of these conditions, he enters his decided protest against its utility, except when the ulceration is specific, or manifests a strong tendency to spread. Speaking generally of these lesions he observes:

"Especially am I convinced of their non-injurious character, and therefore regard all treatment employed against these ulcerations, even when fungoid, as much more hurtful than useful." (p. 368.)

Again:

"I think that, except in a few rare instances, marked by specificity of character, or strong tendency to spread—a tendency, by the way, which I have never observed—all local treatment should be refrained from." (p. 368–9.)

And lastly:

"The insufficiency of local treatment, and the mischievous effect which it may have upon the progress of gestation, should, it seems to me, in the present condition of science, lead us to dispense with it whenever the ulceration has no marked tendency to invade a large extent of the cervix." (p. 370.)

Such, then, are the conclusions arrived at by one of the highest obstetric authorities in France as to the importance of those morbid appearances of the cervix uteri which have been assumed by some to exercise so injurious an influence upon pregnancy and the female economy at large. Such are the opinions respecting them which are daily gaining ground in this country, and such, we are persuaded, are the conclusions which will be finally arrived at wherever the subject is investigated in a candid and philosophical manner. It is not a question as to the mere existence of these lesions of the cervix, which nobody denies, nor the utility of topical treatment under certain circumstances, which everybody admits; but whether their pathological nature and importance have not been greatly exaggerated, and whether topical measures have not been resorted to to an injurious and unjustifiable extent. It was in the capital of France that the existence of these lesions was first brought to light by the persevering and indiscriminate use of the speculum in female disorders. The discovery was soon made known to the profession of this country, and subsequently to that of America. We have seen that a considerable modification of opinion has taken place of late years as to the nature and importance of these lesions, in the first and second of these countries, and we cannot doubt that the same will ultimately prevail in the third.

A question of considerable pathological importance has of late years been very ably investigated in France by M. Grisolle—viz., the reciprocal influence of pregnancy and parturition upon the progress of phthisis, and his conclusions are given at length by M. Cazeaux. Contrary to the general belief, they tend to show that neither pregnancy nor delivery affects sensibly the progress of phthisis, nor does the latter sensibly disturb the course of the former; thus

"Of 17 cases collected by M. Grisolle, and 10 others furnished him by M. Louis, 24 were those of women attacked with the disease during pregnancy at periods not far removed from its commencement; the 3 others had reference to individuals who presented the rational signs of tuberculosis at the time of conception, but in whom the disease became well marked only at a later period. In none of these cases was the pulmonary affection arrested, nor did it fail to progress quite rapidly. The symptoms peculiar to tuberculosis, whether local or general, were developed with the same order, the same regularity, and the same constancy, as in the ordinary conditions of life. But, on the other hand, contrary to what might have been expected, the pregnant condition neither aggravated nor rendered more frequent the accidents of the disease. Bronchial hæmorrhage was noticed as being even rather less frequent than usual.

"The entire duration of phthisis in 13 women who were followed to the end, was rather shortened than otherwise. Thus, in all of them it lasted on an average nine months and a half, which is a figure more than a third less than that which expresses its duration for women at the same age, but not pregnant." (p. 363.)

So much, then, as to the supposed influence of pregnancy in sus-

pending phthisis. The facts collected by M. Grisolle serve further to show that, contrary to general belief, parturition does not tend sensibly to accelerate it:

"Thus 12 women in whom the disease had reached the second, and in most of them the third degree, at the time of delivery, resisted its inroads for four months on an average; and in all, the symptoms followed the progression that is usually observed. In 10 others in whom the affection was in the first degree, or at the beginning of the second, at the period of delivery, the pulmonary lesion was found in 3 to advance slowly; in 2 only did it exhibit a notable aggravation; whilst in 5, or one-half the number, there was a considerable amelioration both of the general health and local symptoms, without however encouraging the hope of a cure, or of a long suspension of the disease." (pp. 363–4.)

With regard to the question, Does phthisis exert an unfavourable influence upon the progress of gestation? it is remarked that, in this point of view, it may at least be regarded as much less serious than pneumonia:—Thus, of 22 women, only 3 aborted in the fourth and sixth months, 3 were delivered prematurely about the eighth month, whilst all the others reached their full time.

The observations of M. Grisolle are, it appears, supported by those of M. Dubrielle, of Bordeaux, who has published a very interesting memoir upon the same subject.

The varieties and complications of labour are treated of so differently in these treatises to what they are in most English text-books, that it is difficult to observe that order in their consideration which would accord best with the ideas of English readers. The subject, however, of puerperal hæmorrhage, which stands prominently forward in the early part of the American, has of late so much occupied professional attention, and has given rise to so many suggested innovations upon the more established practice of our forefathers, that we do not think we could select a better for analysis or criticism.

But before doing so, we feel called upon to notice a passage in the work of Dr. Miller which appears to us to convey an erroneous and unjust impression of the more immediate object and character of the well-known essay upon 'Uterine Hæmorrhage' of our countryman, the late Dr. Rigby, of Norwich. After observing that Levret had unquestionably shown that in cases in which the placenta is found over the mouth of the womb prior to delivery, it had grown and become rooted there, and necessarily gave rise to hæmorrhage by its disruption in advanced pregnancy or at the time of labour, he adds:

"By a singular coincidence, Dr. Edward Rigby, of Norwich, in England, came to the same conclusion, as the result of his own observations in a large number of cases of flooding, before he was aware of the researches of M. Levret. The fruit of his investigations was given to the world in his work, entitled 'An Essay on the Uterine Hæmorrhage which precedes the Delivery of the Full-grown Fœtus,' which gave him not only an European but world-wide reputation. There is no ground to impeach the veracity of Dr. Rigby (and God forbid that I should do it), yet the historical fact is that Levret's *Dissertation* was printed several years before Dr. Rigby's *Essay*, the first edition

of the *Essay* being published in 1776, whilst the *Dissertation* is contained in the third edition of Levret's works, published at Paris in 1766." (p. 237.)

Now, with reference to this passage, we would beg to observe that it was by no means the object of Dr. Rigby to claim for himself the discovery of the fact in question, nor in any part of his essay has he done so. On the other hand, he was well aware of the prior publication of Levret's Dissertation, quotes from it, and gives the author full credit for "having proved, from very satisfactory reasoning, that the placenta may be situated on the os uteri without having been previously separated from any other part of the uterus, and pushed down upon it." It was rather the object and great merit of Dr. Rigby to apply the knowledge of this fact to some practical account, and to show how, as a general rule or principle, cases of unavoidable hæmorrhage require turning and artificial delivery, whilst cases of accidental hæmorrhage do not. He was fully aware that certain cases of both varieties furnish an exception to this rule, and it does not in the least detract from the soundness of the principle he enunciated, that at the present day some ingenious writers have endeavoured to make the exception the rule, and the rule the exception. It is a sufficient vindication of the great merit of Dr. Rigby to assert that he was the first to draw those practical inferences from the fact in question upon which are founded the rules he ventured to lay down for the management of these cases, and which have ever since guided the practice of the greatest masters of the obstetric art in this country.

But of late years many innovations upon, or modifications of, this practice have been proposed, more particularly in regard to that which relates to the treatment of placenta prævia. Dr. Miller protests strongly against artificial delivery in these cases. Dr. Simpson proposes the detachment and removal of the placenta before the birth of the child in certain instances. Dr. Cohen, of Hamburgh, and Dr. Barnes, propose only its partial or cervical separation. M. Gendrin proposes the simple puncture of the membranes through the placenta, whilst Dr. Mackenzie has recently submitted to the Medico-Chirurgical Society of London a mode of treatment based upon the action of galvanism upon the contractile structure of the gravid uterus. To these different proposals we will briefly advert.

I. In support of Dr. Miller's protest against artificial delivery in these cases, he refers to the want of success which has attended it, as indicated by the statistical data collected by Dr. Simpson, Trask, and others, with reference to which he observes:

"It thus appears that about one-third of the mothers and two-thirds of the children have been lost after the operation of delivery by turning in placental presentations. This startling mortality, be it remembered, occurred under the most favourable circumstances, both as regards the condition of the patients and the skill of the operators, for most of the cases were in the hands of such obstetricians as Mauriceau, Portal, Giffard, Smellie, Rigby, Clarke, Collins, Lachapelle, the Ramsbothams, &c." (pp. 270–1.)

He then proceeds to show from similar statistical data that spontaneous delivery is by no means so rare as it was formerly supposed to

be, and is much more favourable to the mother, and scarcely less favourable to the child, than delivery by turning, considered as the authorized practice in placenta prævia cases generally; for, he observes,

"Only 2 mothers and 47 children were lost out of 66 cases of spontaneous expulsion of the child; or 1 in 33, instead of more than 1 in 3 for the mothers, and only a trifle more than two-thirds of the children, which is the ratio in regular deliveries by turning. Thus he believes nature interposes much more efficaciously in behalf of mothers in these perilous cases than art, and is scarcely less mindful of their children." (p. 276.)

Guided by these and other considerations, Dr. Miller proposes to do away with the operation of turning in placenta prævia, and offers the following substitute:

"The substitute for turning which I will venture to propose is a modification of the method of Puzos, and consists in originating expulsive contraction of the uterus by the tampon or plug, and then puncturing the membranes, relying on the tampon to control the flooding until the liquor amnii is evacuated. This is the only method of treatment of which I have any experience, and I have employed it with uniform success so far as the mother is concerned. This is strong testimony, but it must be modified by the confession that my experience in placenta prævia cases has not been large; yet I have encountered them sufficiently often to have acquired some acquaintanceship." (p. 288.)

Subsequently he observes,

"The supervension of labour—the evacuation of the liquor amnii—these in their order are the great bulwarks of a flooding woman, no matter where the placenta is implanted. It is a maxim in obstetrics, that a contracted uterus cannot bleed; it might, I think, be amended and enlarged, by adding that neither can a contracting uterus bleed when it is emptied of its waters, or at any rate, if it bleed, the hæmorrhage is no longer dangerous." (p. 289.)

To this last observation we would venture to remark, that any one who will turn to the chapter On a Peculiar Form of Hæmorrhage from the Uterus in Dr. Gooch's admirable work 'On Some of the most Important Diseases peculiar to Women,' will find it to be entirely refuted; and with regard to the practice itself, as well as that which it is intended to supersede, we would further remark, that the use of the tampon, as well as puncture of the membranes, in cases of placenta prævia, have been extensively resorted to in this country, but with so little success that no one would now think of adopting them as a final mode of treatment; so little, indeed, are they to be relied upon for securing the safety of the patient, that they are now only employed as the means to an end—as the means of controlling hæmorrhage and exciting a sufficient dilatation of the os uteri to admit of the introduction of the hand and the operation of turning, a proceeding which, in the opinion of the most experienced accoucheurs, is the only certain method of saving the patient in extreme cases.

But, further, we venture to question the accuracy of the conclusions drawn from mere statistical data for or against the operation of turning in these cases. All practical men, for instance, admit that under certain exceptional cases it may be dispensed with; whilst they also agree that when indicated it may be employed either too early or too late to insure success. Now, to throw all such cases together, regard-

less of the peculiar nature and circumstances of each, and to draw a general conclusion from a series of facts of so conflicting and discordant a character, appears to us to be anything but the right way of arriving at a knowledge of the real value of the practice in question. Nor does the argument founded upon the reputation and practical skill of the writers who furnished them seem more reliable, for such persons are notoriously often consulted in the most extreme cases, in which the proper time may have passed for resorting to the practice, and in which success has become unattainable by any possible proceeding. With regard to such persons, we should infinitely prefer to appeal to their general experience than to their statistics, and we have little doubt but that it would endorse the soundness of the principle enunciated by Rigby—viz., that if, in a case of flooding before delivery, the presentation is that of the membranes, and not that of the placenta, you should rupture the membranes, and allow the liquor amnii to escape, upon which the uterus will contract upon the child, and the hæmorrhage will cease; but if the placenta presents, *as a general rule* you must turn and deliver as soon as the parts are sufficiently relaxed to permit this to be done without violence.

II. The practice of detaching and bringing away the placenta in certain cases of placenta prævia, recommended by Professor Simpson, does not meet with the approval of M. Cazeaux, even when restricted to the narrowest limits claimed for it by the Professor—viz. 1. When the flooding has resisted the principal measures, and especially the evacuation of the waters. 2. When the slight dilatation or development of the cervix, or contraction of the pelvis, render turning or any mode of artificial delivery dangerous or impossible. 3. When the death or immaturity of the fœtus restricts the duty of the accoucheur to caring for the safety of the mother. He observes:—

"Even with this reservation we cannot approve of the advice of Dr. Simpson, for we think that when the flooding continues after the evacuation of the waters, and when the neck does not allow the hand to be introduced, there is some chance left of saving both mother and child by applying the tampon.

"We also think, that when an obstacle, dependent on the neck, the soft parts, or the pelvis, prevents the termination of the labour, the tampon may be applied with advantage until the dilatation of the neck allows of the intervention of art; for I cannot see in what way, under these circumstances, the extraction of the placenta could facilitate that of the fœtus, which Dr. Simpson recommends to be practised immediately afterwards. The obstacles which prevented earlier action exist none the less afterward. It is therefore only when caring very little for the life of the child, or in case of the death or non-viability of the latter, that one could undertake to separate and extract the placenta, if the hæmorrhage were dangerous, in order to spare the mother the pain of applying the tampon." (p. 710.)

III. To supply a more effective and available agent than the tampon for controlling hæmorrhage and developing the os uteri when rigid and undilated, in cases of placental hæmorrhage, is the more immediate object of the practice recently recommended by Dr. F. W. Mackenzie. Having, from a series of experimental researches, ascertained that a

sustained current of galvanism, passed longitudinally through the uterus from the upper portion of the spinal cord, exercised a remarkable influence in increasing the tonicity and contractility of the uterine fibre, and thereby controlling hæmorrhage and developing the os uteri, he was led to test the value of this proceeding in these cases, and has embodied the results of his investigations in the three following propositions:—1. That a sustained current of electricity of moderate intensity passed through the gravid uterus exercises a remarkable influence in increasing the tonicity and contractility of the uterine fibre. 2. That in such increased tonicity or contractility of the uterine fibre, so excited and sustained, we have a powerful and reliable means of moderating and controlling uterine hæmorrhage, whether of the accidental or unavoidable variety, and of simultaneously accelerating the dilatation of the os uteri, and the general progress of the labour. 3. That such sustained current of electricity may be continued for a lengthened period, when the object to be attained requires it, without any appreciable pain or inconvenience to the mother, or danger to the child.

IV. Lastly, with reference to the practice recommended by Drs. Cohen and Barnes, we have merely space to observe that it consists in detaching the placenta from the segment of the cervix to which the lesser portion adheres. This done, there is nothing, Dr. Cohen remarks, to prevent the os uteri from expanding and carrying the liberated portion of placenta over to the side where the bulk of the organ adheres. This is the extent of Dr. Cohen's recommendation; but should the hæmorrhage continue, Dr. Barnes recommends the detachment to be carried further by sweeping the finger round between the placenta and uterus on that side on which the main bulk of the organ adheres, by which means the whole of that part which had been seated within the cervical zone will have been detached.

We have thus indicated the principal innovations which have been made upon the rule of practice laid down by Dr. Rigby for the treatment of placenta prævia. How far, however, they are calculated to supersede it in certain cases, or to be merely auxiliary to it in general, are questions which time and further experience can alone determine.

The relations of albuminuria to puerperal convulsions have been very carefully investigated by M. Cazeaux, and are fully treated of in his treatise. He insists very strongly upon its importance in the etiology of the disease, but regards it rather as its predisposing than exciting cause.

"Happily," he observes, "it is by no means uncommon for pregnant women to have the urine highly charged with albumen, without presenting a single convulsive symptom. Thus, of 41 women with albuminous urine, observed by M. Blot, but 7 had convulsions; and of 20 mentioned by MM. de Villiers and Regnault, 11 only were affected with them." (p. 716.)

Nevertheless, he is of opinion that the organic conditions that produce albuminuria are certainly the most, if not the only ones, favourable to the production of eclampsia.

"If it be true, as M. Rayer thinks, that the compression exerted by the developed uterus upon the renal vein may eventually produce hyperæmia, and then an inflammation of the kidneys, we are able to understand the mode of action of all the circumstances capable of increasing this compression. Thus we can explain the possible effect of, 1, the extreme distension of the uterus, whether due to dropsy of the amnios, or to the presence of several children; 2, of a first pregnancy, in which the uterus is strongly applied to the posterior walls of the abdomen, in consequence of the resistance of the abdominal parietes; 3, why, according to the observations of M. P. Dubois, rachitis is often connected with eclampsia, since in women affected with this disease the small stature and limited space within the abdominal enclosure obstruct the development of the uterus, which, by reacting in its turn upon the surrounding parts, forms a greater mechanical obstacle to the regular fulfilment of all the functions, and to the venous circulation in particular.

"Whatever the cause may be, long-continued albuminuria necessarily occasions a notable diminution of the amount of albumen which enters into the normal composition of the blood. Hence it is extremely probable that this fluid, when thus altered, gives rise to a peculiar excitement of the cerebro-spinal centre, which becomes itself the direct cause of the convulsions, or at least, which is more frequently the case, renders it more susceptible of the excitements, which reach it either from without or from previously irritated internal organs. These excitements, which under any other circumstances would have no effect, become here so many determining causes of an attack of eclampsia." (p. 717.)

The most frequent and effective of these determining or exciting causes, it may be added, are respectively irritation of the nerves of the uterus, vagina, bladder, rectum, and stomach.

We are unable to follow our author through the various details of treatment he lays down, which are indeed based upon a consideration of the predisposing and exciting causes of the attack, and essentially in accordance with those which are well known, and have been long practised in this country. We may, however, add that he is strongly opposed to the employment of anæsthetic inhalations in the treatment of this disease. Our readers are probably aware that chloroform has been of late introduced as a remedy for puerperal convulsions, on the ground that inasmuch as it had the power of destroying the action of the muscles of animal life, it might act in the same way upon the involuntary and spasmodic contractions resulting from puerperal convulsions. With reference to this proposal, our author observes:

"That an attentive reading of almost all the observations which have been published upon the subject, leads him to reject inhalations of ether or chloroform in eclampsia as generally useless, or perhaps as dangerous." (p. 747.)

The subjects respectively of the induction of premature labour, the production of abortion, and the Cæsarean operation, are treated of by M. Cazeaux with great fulness and clearness, and we are glad to find that the opinions of French practitioners are gradually assimilating themselves to those which have been long entertained in this country as to their value and expediency. With regard to the first of these operations, our author lays it down as a rule, that it should not be undertaken before the end of the seventh month of pregnancy, and that the smallest pelvic diameter should not be less than two inches and

three quarters; besides such a degree of contraction of the pelvis, he, however, considers it applicable to various other cases. To those, for instance, of any serious disease occurring to females during the latter months of gestation, in connexion with or as a consequence of pregnancy, and for which depletion of the womb offers the best and sometimes only means of relief. Cases of abdominal tumours restricted to the following instances:

"1st. When any voluminous tumour whatever exists in the belly and incommodes the enlargement of the womb, or is itself exposed to such a compression as almost necessarily to lead to consecutive inflammation.

"2nd. When a tumour developed in the excavation is so fixed and adherent to the pelvic walls that it can neither be pushed above the superior strait nor drawn down beyond the vulva, provided its bulk is sufficient to prevent the expulsion of a fœtus at term." (p. 836.)

Cases in which the capacity of the abdominal cavity is so inconsiderable from the small stature of the individual as to be insufficient for the normal development of the uterus without greatly interfering with the function of respiration; and again, certain cases in which in previous pregnancies the child has died after reaching the eighth or ninth month of gestation.

For the purpose of producing premature labour, six means are proposed. 1st. Frictions made over the fundus, and titillations of the os uteri. 2nd. The detachment of the inferior segment of the ovum from the uterine wall. 3rd. Perforation of the membranes. 4th. The introduction of a foreign body into the cervix. 5th. Plugging up the vagina. 6th. Uterine douches. Of these he expresses a decided preference for the last, and observes that having for two years past found such great advantage in the use of Kiwisch's injections, he does not hesitate to recommend them almost exclusively.

The operation for the production of abortion is, in the opinion of our author, perfectly justifiable in certain cases, but should be restricted to the following:—Extreme contractions of the pelvis, in which the smallest diameter is less than two inches and a half; voluminous, immovable, and non-operable tumours of the excavation; extreme dropsy of the amnion; irredeemable displacements of the womb, and hæmorrhages which have resisted the employment of the most rational means for their suppression. As to the best means of bringing on abortion, he is of opinion that the choice lies between the puncture of the ovum, the introduction of prepared sponge within the cervix, and the uterine douches, the latter of which he has every reason to believe would prove efficient.

With regard to the Cæsarean operation, our author is opposed to it in all cases in which embryotomy is practicable, and in this respect we need scarcely observe that his views harmonize with those which very generally prevail in this country; whilst we may remark at the same time that they are at variance with those which almost universally prevail on the Continent. Thus he observes:

"Supposing the smallest diameter measures two inches and one eighth, and it has been positively determined that the child is still alive (for the question

is no longer doubtful when there is the least uncertainty on this point), two different measures are presented for our serious consideration—viz., embryotomy and the Cæsarian operation. All the French accoucheurs, including Dubois himself, are in favour of the latter, for he says: 'The Cæsarian operation is our only resource, and therefore it must be resorted to.' (Thèse, p. 71.) We are not ignorant of the importance of this question, and it requires a settled and positive conviction on our part to warrant us in deciding it differently from other French authors; but we are sustained by the almost unanimous opinion of the English practitioners, who believe that the child ought to be sacrificed whenever the delivery can be effected by embryotomy." (p. 861.)

With regard to the smallest dimensions of the pelvis which admit of embryotomy, our author considers that it is impracticable when the smallest diameter does not exceed two inches. In this, however, we are persuaded that he is mistaken, for in this country the operation has been successfully performed within these limits; he observes:

"Unhappily, the Cæsarian operation is the only practicable resource when the smallest diameter of the pelvis does not exceed two inches, for the extraction of a mutilated fœtus is then so slow, difficult, and painful, that while necessarily killing the child, the danger to the mother is as great as from the performance of hysterotomy." (p. 862.)

With regard to the stage of the labour which is most favourable for the performance of the operation, supposing it to have been fully determined upon, our author deduces from various statistics that the duration of the labour appears to have an unfavourable influence upon it only when it has continued beyond seventy-two hours; and in regard to the rupture of the membranes, that the operation is so much the more unfavourable for the mother, as a greater time has elapsed after it has taken place. Hence he concludes, that when labour has actually commenced, the operation should be proceeded with as soon as the os uteri is sufficiently dilated to permit the subsequent discharge of the lochia.

The last topic we are enabled to enter upon in connexion with these volumes, is the important question of the employment of anæsthetics in parturition, upon which our several authors entertain somewhat different opinions. As regards those of Dr. Miller, it may be sufficient to observe that he appears to recommend it indiscriminately in every form and variety of labour, and seems to be singularly indifferent to the fact that serious, if not fatal consequences may follow from its indiscriminate and injudicious employment. Impressed with the belief "that of all the boons that could be conferred upon parturient females, the greatest and most inestimable would be the annihilation of the pains and sufferings to which they are doomed, and that modern science has happily discovered such an antidote in the inhalation of the vapour of æther and chloroform" (p. 448), he enters into a laboured defence of its employment in all cases of labour. We regret, however, that we can neither acquiesce in this opinion, nor find anything in the arguments by which it is supported sufficiently original or conclusive to lay before our readers.

The views of Dr. Waller are condensed into the four following propositions:

"1st. That the inhalation of æther and chloroform has been followed by injurious and fatal results.

2nd. That in natural labours no reason exists for the employment of a remedy the efficacy of which is at least doubtful, and its action often hurtful.

3rd. That the action of the uterus is sometimes suspended, but the published accounts are so contradictory, that it is impossible to form an accurate judgment on this point.

4th. That the production of anæsthesia in operative midwifery is likely to be injurious rather than beneficial." (p. 152.)

Subsequently he observes that the only cases which in his opinion justify the inhalation of chloroform are, 1st. Those in which there is an unusual degree of nervous excitability and sensitiveness. 2ndly. Where there are severe muscular pains in the neighbourhood of the uterus interfering with its action; in both which cases he recommends very moderate inhalation. And 3rdly. He believes that in very difficult cases of turning, some benefit might *possibly* be attained, although in many instances a dangerous amount of force might (unless great care be taken) be employed in extracting the child when the patient is in a state of insensibility.

With regard to the opinions of M. Cazeaux, we may premise that his chapter "On the Use of Anæsthetics in Obstetrical Practice," is one of the most philosophical and instructive essays that has ever been published on the subject. Without entering into any personal disputation, he has endeavoured by an appeal to facts to attain a right knowledge of their effects upon the various phenomena of parturition, and to arrive at sound conclusions as to their safety, utility, and proper mode of administration. *As regards the effect of anæsthetics on the uterine contractions,* he is led to the conclusions—1st. That in the majority of instances the contractions are unaffected by the inhalation of chloroform. 2nd. When the anæsthesia is pushed too far, the labour is often suspended. 3rd. In certain individuals the same result may be produced by moderate doses of the agent, and that before the loss of sensibility and consciousness. *As regards their influence upon the contraction of the abdominal muscles,* after pointing out the difficulty of determining this point, he quotes the opinion of Channing, which is probably correct, that in imperfect etherization, the abdominal muscles contract in different degrees, but when the anæsthesia is complete, *the effort is only apparent. As regards their influence upon the resistance of the perineum,* he is of opinion that fresh observations are necessary to settle definitely this question, the evidence regarding it being of a conflicting nature.

Having premised these inquiries, and shown that the careful administration of chloroform exercises no injurious influence upon the health of the mother or child, he proceeds to inquire what are the cases in which it should be employed.

"This question," he observes, "is variously answered in different

countries. Dr. Simpson, and with him quite a large number of his countrymen, recommend it unhesitatingly in all labours, whether natural or difficult. In France, on the contrary, it is confined almost exclusively to cases of difficult parturition. We adopt unhesitatingly the latter position, and a few words will suffice to explain the motives of our preference.

"Whilst regarding the use of chloroform as devoid of danger in the majority of cases, we cannot entirely forget the misfortunes of certain surgeons who had nevertheless taken the best precautions to avoid them. Now, though it be allowable to subject a patient to some danger in order to spare him the intense suffering of an amputation or any other bloody operation, are we sufficiently authorized to do so when the regular accomplishment of a function is concerned? and after all, is the suffering of childbirth in simple cases so grave and terrible? Do we not see women delivered almost without pain? To speak only of what is most common, do they not often preserve their calmness and gaiety to the end of the labour? Do they not often complain of the repose afforded by the intervals between the pains, and ardently desire their return, in the conviction that each is a step toward delivery? . . . Finally, we may add, that supposing the physician to be devoid of all fear, he is obliged to remain constantly by the side of his patient to administer the agent personally, and to watch attentively the state of the pulse, of the breathing, and of the heart." (pp. 977–8.)

Having intimated that the more he reflects upon it, the more determined does he feel to exclude anæsthetics entirely from simple labour, M. Cazeaux proceeds to indicate the cases in which he thinks they may be especially useful, and which may be thus recapitulated: 1. In calming the extreme agitation and mental excitement which labour often produces in very nervous women. 2. In those cases in which labour appears to be suspended or much retarded by the pain occasioned by previous disease or such as may supervene during labour, as cramps, colic, vomiting, compression of the sciatic nerve, &c. 3. In cases of irregular or partial contractions of the uterus which, notwithstanding the intense and almost constant pain they occasion, have no effect in advancing the labour. 4. Spasmodic contraction, or rigidity of the cervix uteri. 5. Cases of eclampsia, restricted to those which appear to be manifestly due to the local irritation of an organ whose extreme sensibility had excited the reflex action of the spinal nerves. 6. Obstetrical operations which are productive of much pain, or necessitate quiescence on the part of the patient—such as turning, symphyseotomy, or the Cæsarean.

Whether these restrictions to the employment of chloroform in midwifery practice are judiciously imposed or not, or whether they might not be extended with advantage, are questions which we will not here venture to discuss, but that some limit should be placed upon its indiscriminate employment, is in our opinion most desirable. It is not a question as to mere danger to life, because we apprehend that with tolerable care this need not be compromised; nor are we

influenced by the fear that it is calculated to give rise to convulsions or mania; that puerperal diseases are more apt to follow its employment, or that the patient under its influence is likely to give utterance to gross or obscene expressions. What we contend for is, that chloroform should be ranged with and employed in obstetric practice as other potent and effective remedies—as opium, antimony, ergot, &c., to be used unhesitatingly whenever circumstances indicate a necessity, and equally withheld whenever circumstances do not indicate such necessity. What, for instance, could justify our resorting to it in a case of natural and easy labour, in which the sufferings of the patient are moderate and their duration limited? Whilst, on the other hand, why should it be withheld in any case in which her sufferings are severe or long-continued? The truth, however, is, the employment of anæsthetics hitherto in labour has not been regulated so much by the exigencies or requirements of the case, as by a certain deference to popular feeling or fashion; and in this respect medical men have been content to become followers rather than leaders. No sooner, indeed, was the world startled by the announcement that childbirth might be accomplished in oblivion to its pains, than medical men vied with one another in their eagerness to announce their readiness to administer it, and we have witnessed the sad spectacle of a fashionable accoucheur proclaiming at one time his belief that the use of anæsthetics was fraught with the greatest dangers to the physical and moral constitution of females; and at another, when popular feeling had set in against him, recanting his own words, and proclaiming an exaggerated eulogium in their favour. What we contend for is, that a certain amount of judgment and discrimination should be exercised in their employment, that the profession should not too readily pander to popular caprice in regard to them, but that a due estimate should be made of the circumstances of each case, and their exhibition regulated by the same principles which determine the use and administration of other powerful remedies. Such appears to us to be the view taken of them by M. Cazeaux, and in such view we yield our entire concurrence.

In conclusion, we have to express our warmest admiration of the treatise of M. Cazeaux. It is unquestionably a work of the highest excellence, rich in information, and perhaps fuller in details than any text-book with which we are acquainted. The author has not merely treated of every question which relates to the business of parturition, but he has done so with judgment and ability. He has not merely introduced into its pages a notice of the latest obstetric discoveries and views of practice which have been promulgated, but he has at the same time carefully scanned their respective merits; and, not led away by the speciousness of novelty, has sought by a rigid scrutiny of facts to determine the real value and importance of each.

On these grounds we consider his work as peculiarly valuable, not only as being a safer guide to practice, but as containing a body of information which may be usefully consulted with reference to the many questions which of late years have been litigated in connexion with midwifery and the diseases of parturition.

Review IV.

1. *Kraft und Stoff — Empirisch-Naturphilosophische Studien. In Allgemein-verständlicher Darstellung.* Von Dr. Louis Büchner. Fünfte Auflage.—*Frankfurt-am-Maine*, 1858. pp. 250.
Force and Matter—Studies in Empirical Natural Philosophy. By Dr. Louis Büchner.—*Frankfort-on-the-Maine*, 1858.
2. *Mind and Body: a Discourse on the Physiology of the Phrenical Action of the Cerebrum.* By Robert Jamieson, M.D., Medical Superintendent of the Royal Lunatic Asylum.—*Aberdeen*, 1858. pp. 24.
3. ΨΥΧΗ: *A Discourse on the Birth and Pilgrimage of Thought.* By Walter Cooper Dendy, formerly President of the Medical Society of London, &c.—*London*, 1853. pp. 144.
4. *The Human Mind in its Relations with the Brain and Nervous System.* By Daniel Noble, M.D., Visiting Physician to the Clifton Hall Retreat.—*London*, 1858. pp. 157.
5. *A Catechism of the Physiology and Philosophy of Body, Sense, and Mind, for use in Schools and Colleges, and in Private Study.* By T. Wharton Jones, F.R.S., &c.—*London*, 1858. pp. 124.
6. *On the Characteristics and Tendencies of the Philosophy of the Nineteenth Century.* Being Parts 2 and 3 of an Historical and Critical View of the Speculative Philosophy of Europe in the Nineteenth Century. By J. D. Morell, A.M.—*London*, 1847. Second Edition, Revised and Enlarged.

As an extreme exponent of the school of scientific "materialists" stands Dr. Büchner. His work has, it appears, excited almost as much attention in Germany as the 'Vestiges' did amongst ourselves. It has passed into a fifth edition within a comparatively short space of time, and, no doubt, will continue to be employed as a text-book for a period by a certain school of dilettanti and pseudo-scientific naturalists. But in our opinion the book has certainly not deserved the outcry which has been made about it, nor can it pretend to having offered us a modicum of *novelty* relative either to opinion or to fact. It is rather a popular than a scientific exposition of some of the more important laws of "force and matter," together with a discussion here and there of some great questions lying within the region of philosophical research. As in neither of its two departments—facts and opinions—are we indebted to the author for much we did not previously possess, it may reasonably be inquired how it is that Dr. Büchner's "Studies" have attained so soon to a fifth edition. We would offer the following as the probable solution of the difficulty. In the first place, in spite of what is said by the author to the contrary, we regard the book as written mainly for the public; by it, certainly, the work has been extensively read, and to the public the mass of its facts were in their detail, no doubt, new.

We do not imply that the "Studies" were expressly intended to be brought "before the forum of frying-pans and coffee-pots;" but we cannot help thinking, with Gutzkow, that they certainly had in view "the entertainment of the domestic hearth;" at any rate, this well-known writer has noticed Dr. Büchner's work in his popular journal for that purpose.*

Now, we can readily imagine much of Dr. Büchner's exposition to take by surprise a large body of the German people; and, if it accepts his opinions as well as his facts, no wonder that a condition was thought likely to be produced against which the pens of many literary men and others have protested. Hence the wide reception, as well as the severe criticism, of the work on 'Force and Matter.' It may create surprise in some of our readers to find us thus speak of "a large body of the German people," but let them peruse the following extract from the late work of Mr. Buckle† (a work, by the bye, all of whose opinions we are by no means ready to indorse), and they will have a clue to the difficulty:

"There is no nation in Europe in which we find so wide an interval between the highest minds and the lowest minds. The German philosophers possess a learning and a reach of thought which places them at the head of the civilized world. The German people are more superstitious, more prejudiced, and, notwithstanding the care the Government takes of their education, more really ignorant, and more unfit to guide themselves, than are the inhabitants either of France or England. This separation and divergence of the two classes is the natural result of that artificial stimulus which a century ago was administered to one of the classes, and which thus disturbed the normal proportions of society. Owing to this, the highest intellects have in Germany so outstripped the general progress of the nation, that there is no sympathy between the two parties, nor are there at present any means by which they may be brought into contact. Their great authors address themselves not to their country, but to each other. They are sure of a select and learned audience, and they use what in reality is a learned language; they turn their mother tongue into a dialect eloquent indeed and very powerful, but so difficult, so subtle, and so full of complicated inversions, that to their own lower classes it is utterly incomprehensible. From this there has arisen some of the most marked peculiarities of German literature; for, being deprived of ordinary readers, it is cut off from the influence of ordinary prejudice, and hence it has displayed a boldness of inquiry, a recklessness in the pursuit of truth, and a disregard of traditional opinions, which entitle it to the highest praise."‡

Since Karl Vogt, Jacob Moleschott, and others, as well as Dr. Büchner, have been severally preparing expositions similar to the one before us for the use of the "educated German public," we are certainly not surprised that both Protestant and Catholic journals should have editorially attacked this outburst of materialism, nor that the 'Allgemeine Zeitung' (January 24th and 25th, 1856) should have brought forward the great chemist of Giessen to show his countrymen that there really is some difference between "inorganic nature and organic life." That against his opponents Dr. Büchner should make

* Unterhalt am Häulichen Heerd, No. 57. 1855.
† History of Civilization in England. London, 1857.
‡ Op. cit., vol. i. p. 217.

violent reclamation, was perhaps to be expected, and the prefaces to his third and fourth editions show that he is at any rate a "smart" writer, not only in defence of himself but in attacking others.

Dr. Büchner was led to the production of his work, he tells us, from being convinced that the time had arrived when empiricism must banish "every form of supra-naturalism and idealism from participating in the explanation of natural occurrences," and that it must be seen "nature and experience are the pass-words of the time;" or, "as Virchow says, 'we must take things as they really are, and not as we think them to be.'" The ultimate victory of the "real-philosophy" over its rival is undoubted; "the force of its demonstration consists in *facts*, and not in incomprehensible and meaningless forms of speech. Against *facts* there can be no continuous fighting; it is of no use kicking against the pricks." The fundamental basis of the "empirical natural philosophy" is the admission of the existence of *force* and *matter*. A dualism is here affirmed, but the dualism is apparent only, for neither factor can exist separate from the other, as so existing, indeed, they are *unthinkable*, and the terms conveying them in their isolation are but empty abstractions. Both are everlasting and indestructible, ever have been, and ever will be; what disappears of either on one side reappears instantly on another. The manifestation alone is changed; all is but a *metamorphosis* of force and matter. This correlate is endless in space and time. To talk of its ever having been *created* is therefore ridiculous; to believe it formed out of nothing is childishly absurd; and to think of it as capable of being put an end to is as illogical as is the supposition of its creation. The laws of nature are eternal, unchangeable, and,

"According to our present state of knowledge of the system encompassing our earth, we must admit that the same matter and the same natural laws of and by which we here see ourselves formed and surrounded, also compose the universal and visible All, and that the same are everywhere in operation in a like way, and ruled by the same necessity as in our immediate presence." (p. 45.)

According to Dr. Büchner, the world has never been created, in the light of popular acceptation; there have been no distinct periods of creative acts, either as regard themselves, or which can be parallelized with the days of creation alluded to in the volume of the sacred law.

"All ideas concerning the operation of immediate, supernatural, or even unexplainable forces in the history of the development of the earth, have vanished into a complete nothing before the eye of modern science. With the same mathematic surety with which this science has measured the endless space of heaven, it penetrated backwards through the millions and millions of years (whose unlifted veil had enveloped the history of the globe so long for man in a mysterious darkness, giving rise to every form of religious and superstitious phantasy), and discovered the proof that this history has everywhere owed its origin only to the simplest and most natural occurrences, often determinable with the greatest scientific accuracy." (p. 57.)

But there was a time when the earth, as "a glowing ball of fire," was not only incapable of bringing forth living beings, but was positively inimical to the existence both of vegetable and animal organisms. As its gradual cooling and solidification, with deposit of surrounding

body of vapour, ensued, the surface of the earth became endowed with the possibility of supporting life; and with the appearance of water, and of a still further diminished temperature, the first development of organic life took place. From thence to the time when, with the occurrence of man upon the scene, life seems to have culminated in its manifestations of perfectibility, a long series of living forms of all descriptions has been traced as having appeared and as disappearing, as passing from the earth's surface to a geologic tomb within its crust. For the origination or primal production of such organisms there was not, according to Dr. Büchner, any necessity for a special or peculiar creative and upholding power.

"We need not such assistance; on the contrary, scientific facts unhesitatingly point out that the organic forms peopling the earth owe their origin and continuance to the combined operation of natural forces and of matter self-inherent in things themselves, and that the gradual change and development of the surface of the earth itself became the only, or at least the chief, cause for such progressive increase of living beings." (p. 72.)

In other words, these natural forces are such as are usually implied in the "theory of progressive development."

"Inappreciably the plant passes into the animal, the animal into man. In spite of all endeavours, we have been unable, up to the present time, to draw a satisfactory line of demarcation between the animal and the vegetable kingdoms—two apparently strongly-differentiated portions of organic existence—and there does not appear any evidence to show that we shall ever attain to its consummation. Just as little does there exist that insurmountable limit between man and the animal of which we are forced to hear so much, perhaps because the talkers are afraid their own understandings may, by such a comparison, lose importance." (p. 86.)

But, let us ask again, whence springs the *first* germ of organic vitality in this long and varied existence? Simply, replies Dr. Büchner (p. 76), from that special co-ordination of the ordinary chemical and physical forces of matter which happened to take place at a particular period of the history of the world. Organic cells thus originating, organic life is necessitously compelled to advance according to the unswerving laws of "progressive development," until at length

"An unbroken series of the most varied and multifarious transitions and analogies unites the whole animal kingdom together from its lowest to its highest unit. Even man, who in his spiritual pride thinks himself raised high above the whole animal world, is far removed from being an exception to this law. The Æthiopic race unites him, by a crowd of the most striking analogies, with the animal kingdom, in a very unmistakeable way. The long arms, the form of the foot, the fleshless calf, the long slender hands, the general lankness, the but slightly protuberant nose, the projecting teeth, the low retreating forehead, the narrow and posteriorly protuberant head, the short neck, the contracted pelvis, the pendulous belly, the want of beard, the colour of the skin, the disgusting odour, the uncleanliness, the making of grimaces whilst speaking, the clear shrill tone of voice, and the ape-like character of the whole being, are just so many characteristic signs which, in all the corporeal forms and relations of the negro, unmistakeably show the most decided approach to the monkey genus." (p. 75.)

"Without doubt man, in his earlier periods, approached in his whole cha-

racter nearer to animals than he does in his present condition, and the oldest excavated human skulls indicate rough, undeveloped, and animal-like forms." (p. 87.)

It is stated that very lately, in Neander-thale (between Düsseldorf and Elberfeld), a human skull of very early date had been found, exhibiting such affinities to a low type of evolution as are scarcely to be met with at the present day in the most brutalized of the human race. It possesses, according to Dr. Schaafhausen, an almost animal expression, recalling to mind the countenances of larger apes.

In the opinion of Dr. Büchner, the entire doctrine of "final causes" must be at once abandoned, for between morphology and teleology there is not a point of reconciliation—

"If the stag has long legs for running with, he has not received them in order that he might run swiftly, but he runs swiftly because he has long legs . . . . the mole has short shovel-like feet for digging with, if he had them not it never would have occurred to him to have burrowed in the earth." (p. 92.)

As *matter* and *force* are inseparable, unthinkable, from each other, so are *brain* and *mind*. There is no brain without thought, no thought without brain; destroy the latter, and mind has for ever gone too. The brain is not only the organ of thought and of all the higher mental activities, "but alone and exclusively the seat of the soul" (p. 140). Every thought is produced in the brain, every variety of sensation and of feeling is there alone brought about, and from there only proceeds every kind of volition and voluntary movement. There are no such things as "innate ideas," "ground forms of the understanding," nor intuitive conceptions of "the beautiful, the true, and the good." Nature knows nothing of aims nor of intentions coming to her extrinsically, and limiting her conditions. The latter, such as they are, have from first to last been organically developed within herself. We have innate capabilities (*anlagen*), "not as the result of innate spiritual qualities, ideas, or intuitions, but only as innate material dispositions towards the particular development of this or that spiritual quality based upon sensuous and empirical acquirements" (p. 181). The idea of *God* is of a purely human origin (p. 187), chiefly anthropomorphic, its different phases being modelled according to the particular human individualities observed to adopt them. Daily, and the most simple, observation and empiricism teach us, says the author, that—

"The spiritual effect ceases with the destruction of its material substratum, that the man dies—

> 'The times have been
> That when the brains were out the man would die,
> And there an end,'

says Shakspeare. No actual phenomenon exists, nor has ever existed, which permits us to believe or to assume that the soul of a dead person still exists; it is dead, never to return." (p. 195.)

Thus the soul dies, like the body; without it, indeed, it is as force, without matter—unthinkable, an abstraction. The soul of an animal differs but quantitatively, and not qualitatively, from that of man

taken for those indicative of common sensation. But probably the cases already observed with respect to this point are too few for any decisive conclusion. Dr. Carpenter, in his later editions, admits the probability of the inferior ganglia of the cerebellum—the corpora dentata—constituting the encephalic region of the muscular sense; but is there good reason for deeming this to be anything else than common sensation as resident in the muscles?" (p. 50.)

In his present work, Dr. Noble has more fully worked out his suggestions as to the "emotional sensibility" being fundamentally distinct from the "sensational sensibility," endeavouring to show that emotional sensibility produces its own actions upon the general system distinct from the movements which Dr. Carpenter denominates consensual, and that these different sensibilities have respectively separate ganglionic centres in the encephalon. In his sixth chapter, 'On the Seat of Consciousness,' the author combats two well-known and remarkable doctrines taught by the able author of the 'Human Physiology.' The first doctrine is that in which Dr. Carpenter affirms the cerebrum itself not to be the seat of consciousness; to use his own words—

"The cerebrum is the instrument of all those *psychical* operations which we include under the general term *intellectual*. . . . . It does not hence follow, however, that the cerebrum has such a direct relation to the mind, that the consciousness is immediately and necessarily affected by changes taking place in its own substance, and however startling the proposition may at first sight appear, that the organ of the intellectual operations is not itself endowed with consciousness, a careful consideration of the relations of the cerebrum to the sensory ganglia will tend to show that there is no *à priori* absurdity in such a notion."

The *sensory ganglia* are regarded by Dr. Carpenter as the seat of consciousness, "not merely for impressions on the organs of sense, but also for changes in the cortical substance of the cerebrum, so that until the latter have acted downwards upon the sensorium, we have no consciousness either of the formation of ideas or of any intellectual process of which these may be the subjects." Dr. Noble asks whether a "fundamental vice" does not attach to the whole argument brought forward by its well-known author?

"Whatever may be said regarding ideas that rest for their support upon sensible forms, *intelligence* in most of its phases cannot surely be deemed to be the simple reproduction of impressions received through the senses. However plausible may be the reasons by which it is contended that purely representative thought consists of *transformed sensations*, according to Condillac's theory, there can be no corresponding argument sustaining a like theory with reference to the higher manifestations of mind, including its more general and abstract operations. It has been seen that the representative sensible faculty primarily developes ideas by the presence of an object acting upon the organs of sense, and that these ideas will afterwards spring up independent of the object, either spontaneously or by some operation of the will. Still even here, as I have already maintained, the idea, upon close attention, is distinguishable from the sensation itself; and however anxious we may be to reduce every idea to some internal form of a representative character, we shall find in the depths of our consciousness numerous thoughts which can have no proper basis in sensible images. What is that faculty of thinking which seizes upon analogies, which

traces the relations of metaphysical ideas, which estimates the possible? Do not the ideas of unity, number, space, and causality, express things which are not sensible? 'We may ask those,' says Balmez, 'who hold that every idea is the image of an object, what sort of an image the idea of *not being* would form.' And yet this sensational theory is an inevitable postulate in the argument which limits the seat of consciousness to the sensory ganglia—an argument which practically nullifies all psychical function in the admitted organ of the intelligence." (p. 103.)

Now, without unhesitatingly yielding our assent to Dr. Carpenter's opinion, we must confess that *we* do not see that it absolutely necessitates all knowledge to consist of transformed sensations, or to originate alone through the sense perceptions. In the sensory ganglia (says the writer in question) is localized all consciousness—consciousness both of cognitions and of ideas. The origin of the former (he might continue) is to be sought in the sense perceptions, that of the latter in the original intuitions of the human mind having their primary organic correlates in the cortex of the hemispheric ganglia. That it is difficult to understand how, as regards the direct and presentative knowledge of the intuitional consciousness, ideas or the concepts of the reason (*Vernunft*), where subject stands face to face with object, and where it is itself both subject and object at the same time, there should be such a complexity and an intervention as is here implied in the repetition of organic correlates at different localities, we however admit. We would go (with a modification) so far with Dr. Noble in asking, "When we come to the origin of ideas and their manifold relations one with another, what imaginable antecedent can there be" to the consciousness of them, save only the ideas themselves? (Dr. Noble says, "To the thought, excepting some sensational phenomena?" But this latter Dr. Carpenter's theory does not assert, though it does not explain what the antecedent be.) And we would admit, with the former, that the doctrine in question has the appearance of practically nullifying all psychical function in the admitted organ of the intelligence.

One thing is clear to our minds—that physiologists who deal with psychology use the term "consciousness" often very vaguely, and that if the question before us is to be satisfactorily settled, it can only be after strictly determining the value of the terms employed in its discussion. The second doctrine we before alluded to is that called by its author "*unconscious cerebration*," and which is employed also to support the former one—that the cerebrum is not the seat of consciousness.

"Most persons (says Dr. Carpenter) who attend to their own mental operations, are aware that when they have been occupied for some time about a particular subject, and have then transferred their attention to some other, the first, when they return to the consideration of it, may be found to present an aspect very different from that which it possessed before it was put aside, notwithstanding that the mind has been so completely engrossed with the second subject as not to have been consciously directed towards the first in the interval. Now, a part of this change may depend upon the altered condition of the mind itself, such as we experience when we take up a subject in the

morning with all the vigour which we derive from the refreshment of sleep, and find no difficulty in overcoming difficulties and disentangling perplexities which checked our further progress the night before, when we were too weary to give more than a languid attention to the points to be made out, and could use no exertions in the search for these solutions. But this by no means accounts for the *entirely new development* which the subject is frequently found to have undergone when we return to it after a considerable interval—a development which cannot be reasonably explained in any other mode than by attributing it to the intermediate activity of the cerebrum, which has in this instance *automatically evoked the result without our consciousness.*"

As we were lately perusing the 'Life of Charlotte Brontè,' the following extract struck us as not inaptly illustrating this theory of "unconscious cerebration." Mrs. Gaskell, we must premise, had asked the gifted authoress of 'Jane Eyre' whether she had ever taken opium, as the description given of its effects in 'Vilette' was so exactly like what she had experienced—viz., vivid and exaggerated presence of objects of which the outlines were indistinct, or lost in golden mist. Miss Brontè replied:

"She had never to her knowledge taken a grain of it in any shape, but that she had followed the process she always adopted when she had to describe anything which had not fallen within her own experience. She had thought intensely on it for many and many a night before falling to sleep—wondering what it was like, or what it would be—till at length, sometimes after the progress of her story had been arrested at this one point for weeks, she wakened up in the morning with all clear before her, as if she had in reality gone through the experience, and then could describe it word for word as it had happened. I cannot [continues Mrs. Gaskell] account for this psychologically. I am sure that it was so, because she said it."*

The facts usually cited in support of this theory are of course admitted by Dr. Noble as being more or less within the well-recognised experience of us all; but he maintains that an explanation of them can be offered more in accordance with the recognised laws of thought than that which involves so occult an agency, and which regards "nerve-substance as elaborating and perfecting thought without thought." (p. 95.) Dr. Noble's solution of the matter may be gleaned from the following extracts:

"Although we ordinarily remember facts and mental processes very much in proportion as they have engaged the attention and a certain reflex consideration at any time, this rule is by no means absolute. Ideas and feelings once experienced may at any time revive in the consciousness, and yet not always be recognised as having previously had existence, particularly when at some former periods they have never been subjected by attention to a reflex mental process. Undoubtedly, under these latter circumstances, numberless thoughts and reasonings and ideas of external occurrences pass for ever from the consciousness, but this is far from being always the case—again and again will they return without any systematic identification. And are not most of the phenomena cited by Dr. Carpenter in support of his theory of unconscious cerebration explicable by these laws of spontaneous thought, according to which our mental operations are frequently unremembered when repeated?" (p. 96.)

* Op. cit., vol. ii. p. 301.

"This mental process may probably be with some accuracy designated involuntary and inattentive thinking, but not with justice *an unconscious action of the brain*. I am decidedly of opinion myself, that the explanation now offered of these well-known phenomena will more or less cover all the psychical processes that have been cited to establish a doctrine of unconscious cerebration." (p. 99.)

The views which have been advanced by Professor Laycock 'On the Reflex Functions of the Brain,' are also regarded as very doubtfully true by our author. To them there would appear, he says, to be this objection—"that it is not very obvious how the evidence of facts can be made to corroborate them or otherwise." Nevertheless, as it is clear that

"Numerous psychical phenomena are observable, of a quasi-automatic character, from the dominance of particular ideas or trains of thought, &c., it may probably be correct to regard them as the product of some sort of reflex action of the hemispherical ganglia." (p. 109.)

The WILL swaying and dominating over mental conditions of every kind cannot, as Dr. Noble observes, be conceived as mixed up specially with any particular ganglionic mass.

Interesting and important as are the several departments of philosophy and science touched upon in the preceding pages, we are not sure but that we may have trespassed somewhat upon the reader's patience. We shall therefore draw our observations to a close, simply expressing our high opinion of the support which *philosophy* has received from the hands of Mr. Morell. It is impossible that his several writings could be here dealt with in the manner which they so amply merit; but this should not prevent us from remarking, that no intelligent and thoughtful person, after having gone through his 'History of Speculative Philosophy,' could, we conceive, afford to laugh at metaphysics; and none having perused his 'Philosophy of Religion' could remain unimpressed by the lucid truth of its argument, or by the broad catholicity of its Christian teachings.

REVIEW V.

A Manual of Psychological Medicine, containing the History, Nosology, Description, Statistics, Diagnosis, Pathology, and Treatment of Insanity. With an Appendix of Cases. By J. C. BUCKNILL, M.D., Licentiate of the Royal College of Physicians, Fellow of University College, London, Fellow of the Medico-Chirurgical Society, Superintendent of the Devon County Lunatic Asylum, and Editor of the 'Asylum Journal of Medical Science;' and by DANIEL H. TUKE, M.D., Licentiate of the Royal College of Physicians, Lecturer on Psychological Medicine at the York School of Medicine, and Visiting Medical Officer to the York Retreat.—*London*, 1858. pp. 562.

IT is a matter of some surprise, considering the interest and importance

of the subject, the vast opportunities that of late years have been afforded by the increased number and extent of the institutions in which insanity could be studied, and the superior order of men who have been enlisted in the service, that so many years have been allowed to pass away without the production of a comprehensive manual of psychological medicine. In the whole range of medical literature there was no book more wanted, both by practitioners and students; but the responsibility of presenting such a work to the world seems to have been too great for even such experienced authors as the accomplished physicians to the Devon and the York Asylums for either of them to have undertaken the task alone without the mutual support of one another. They have both been industrious labourers in this much-neglected field, and have already greatly assisted to establish a literature on the subject. If it has been reserved for physicians connected with provincial asylums to supply their professional brethren with a guide, the want of which has been so long and so generally felt, it reflects great honour not only upon themselves, but their order, and helps to prove that advancing civilization and increasing facilities of communication are gradually removing the distinctions that used to be generally recognised between Provincial and London practitioners, and that the advancement of science in its highest departments may proceed quite as rapidly and satisfactorily away from the busy haunts of men as in the centres of intelligence and the seats of learning—that, in fact, wherever the great book of Nature is open to the learned and industrious student, there is much to be gleaned by careful observation and thoughtful labour; while there is reason to hope that discoveries equal in importance to those which have already conferred such benefits upon the world, may yet reward the diligent and patient inquirer after truth.

We congratulate our authors upon having so well sustained their high reputation by such a very valuable contribution to the literature of their country, for the work before us will be appreciated not only by the medical profession, but by all who take an interest in the advancement of science. It must be admitted that there is some disadvantage in dividing the treatment of such a subject between two individuals, and that the unity of design is better preserved by an independent author; but this disadvantage is to a great extent compensated for by the additional learning and enlarged experience which are brought to bear upon the subject.

In the words of the preface, the arrangement adopted by our authors is as follows:—The chapters on the History, Nosology, Description, and Statistics, are by Dr. Tuke; those on Diagnosis, Pathology and Treatment, with the Appendix of Cases, by Dr. Bucknill.

After an interesting sketch of the history of insanity among the nations of antiquity, the influence of civilization on the production of this malady is ably discussed, and the various fallacies pointed out which have tended to erroneous conclusions as to the proportion of

insane to the general population. There are so many considerations affecting an estimate of this kind which are constantly overlooked, that we continually hear of the increase of insanity as an established fact, although perhaps the authoritative assertion rests only on the circumstance that a large number of cases is brought to the knowledge of the individual so expressing an opinion. Our authors say:

"On no subject has there been more absurd and illogical reasoning, and more hasty generalization, than on the proportion of the insane to the population, whether in regard to various countries, or in regard to the same country at different periods of its history. The most obvious essentials for making correct comparisons are constantly disregarded, notwithstanding which, the most important inferences are drawn with the utmost complacency, and apparently in entire ignorance of the fallacy which underlies such loose and worthless calculations. Even up to the present time, and in scientific journals, we are presented with a list of the numbers of lunatics in various countries, the conclusion being drawn that such numbers represent correctly the liability to insanity in these countries, the difference sometimes ranging between 1 in 8000 and 1 in 30,000! Generally, the only basis for such calculations is the number of patients in lunatic asylums; yet it must be obvious that in consequence of the very different provision made for the insane in different countries, such a basis as this is utterly fallacious."

The laws, customs, relative mortality, and success of treatment, vary so much in different countries, that it is impossible to form any correct estimate of the comparative number of their insane populations, and although in our own country we may approximate somewhat nearer to the truth in such a calculation, we must even here look suspiciously at statistics when we find, as our authors remark, that

"In the short period of nineteen years, the estimated proportion of the insane in England rose from 1 in 7300 to 1 in 769—a difference which led to the belief in the frightful increase of insanity, but which by no means warranted such a conclusion. The knowledge of an evil and the existence of that evil are two widely different things. Insanity may or may not have increased, but our increased knowledge of its extent is no proof that it has."

Again, improved methods of treatment and consequent recoveries—a large proportion of which, as we shall see, are not permanent—whilst they increase the number of recurrent cases, would apparently increase the number of the insane.

"Civilization, with its attendant knowledge and education, creates social conditions and offers prizes dependent solely upon intense intellectual competition unparalleled in any former age, and of course unknown among barbarous nations, which of necessity involve *risks* (to employ no stronger term) which otherwise would not have existed. . . . In a highly-civilized community, the highest standard of intellectual attainment is constantly presented to the aspirations of its members, and minds without reference to calibre promiscuously enter the lists of an unequal contest."

But it is not only the higher faculties of man which are forced into unnatural activity under the influence of civilization. The passions are constantly subject to the same influence in the great battle of life,

and the lower propensities also keep pace in the struggle. The most refined enjoyments even have a tendency to excess, and so all the conditions of civilization, involving as they do increased cerebral action in one form or other, lead to premature decay, in the progress of which the proper balance of the mind is frequently disturbed. The great danger to which civilization predisposes the human mind arises as much from the irregular and partial nature of the excitement to which it is exposed, as from the laborious exercise of its faculties generally. In the race for pre-eminence, it is often particular faculties which are subjected to an excessive strain—others, from disuse or subordinate exercise fail to exert the salutary influence which their steady employment is calculated to have on the mental equilibrium. It seems essential to the proper development of mental manifestations, that all the functions of the brain should be kept in exercise equally; but if no regard is paid to this condition, and some faculties are urged to their utmost without any corresponding exertion of the others, there is danger that what has been uncontrolled will become uncontrollable, and that the individual will be hurried on in his impetuous course to the object of his ambition, unconscious that he is gradually, and often rapidly, losing the power to guide his actions and his conduct, and establishing the diseased condition which we call insanity. An individual may be exhausted by mental as well as by bodily labour, but in the former case it is the exquisitely delicate structure of the brain which is subjected to the perils of over-work, and which is so much more susceptible to injury than the ruder muscular structures which bear the brunt of physical labour, and are at the same time more easily renovated and restored. It is not contended that the increased intellectual exertion which is a condition of increasing civilization, as such, predisposes to insanity. If due regard be paid to the proper exercise of all the mental faculties and bodily functions, the healthy brain is capable to meet the exigencies of an altered state of society, and to undergo increased exertion without damage, provided it be not carried to excess, for then what would only be temporary exhaustion in other organs, may be in the case of the brain permanent injury of its structure. Insanity is comparatively rare amongst barbarous races and people of a low order of intelligence, and also among the Chinese and other opium-eaters of the East, where the mental faculties are not called into very active exercise; whilst in those nations claiming a higher degree of civilization, we find numerous asylums filled with insane persons, and these forming only a portion of the insane population, the actual number being always very difficult to estimate.

The history of the amelioration of the condition of the insane is in fact the history of the abolition of mechanical restraint, and is instructive, as showing the fallibility of sound minds, and the difficulty which even zealous philanthropists have experienced in discarding old prejudices and fears. In this, as in many other things, we look back upon the proceedings of our ancestors, and even of ourselves, with wonder that we should have so long continued in a course which, with our

present experience, we hold to have been unnecessary and too often cruel. We are also led to see that insane persons are not so different to the rest of the world as to render necessary the management and treatment which were adopted in the beginning of the present century; and if the advocates of the non-restraint system have carried their views too far in asserting that restraint is positively injurious in all cases, they have at least rendered this service to humanity, and to the insane especially, that they have shown how much may be done without it, and how few are the cases where its employment is really necessary or useful. We are inclined to think that the unconditional terms in which the disuse of mechanical restraint has been urged have been calculated to excite opposition, and that if, instead of demanding its total and immediate abolition, an appeal had been made to the reason of those who entertained opposite opinions, suggesting at the same time a candid comparison of experience as to the results of the different systems, there would have been less difficulty in bringing others to unlearn the lesson which had been taught by their ancestors, and adopted as a rule of faith by their cotemporaries and themselves. In institutions specially adapted to the treatment of the insane, it seems to be generally admitted that mechanical restraint *may* be altogether dispensed with; and if cases occasionally present themselves in which some modified form of restraint would be an advantage to the patient, the true question seems to be, not, is it right in this particular case, but, is it expedient, as affecting the principle in which insane thousands are interested? If in employing a remedy which we believe to have powerful curative properties we find that it is necessary only in exceptional cases, that its employment is liable to and has been attended with the grossest abuses, some degree of which it is almost impossible to prevent, we may fairly doubt whether the minor good compensates for the major evil, and whether we had not better dispense with a remedy which is open to such grave objection. We do not presume to condemn those who hold that they are not justified in discarding a means of treatment which they find efficacious, and the abuse of which they make it a point of conscientious duty to prevent. There is nothing wrong or improper in the judicious employment of mechanical restraint, but on account of the difficulty in controlling its employment and the consequent abuses to which its use is liable, it is thought better to dispense with it altogether. We know that it is stoutly held by many eminent men that there *is* something wrong in the employment of mechanical restraint, but in truth the whole treatment of insanity involves some degree of mechanical restraint, inasmuch as one of its most important elements is the exercise of some control over the patient.

For some purposes it will be convenient to have a classification of the various forms of mental disturbance which will be generally received, and accordingly the classification adopted by our authors is a modification of that suggested by Heinroth. It is threefold, and comprises—

DISORDERS OF THE MIND INVOLVING—

CLASS I. *The Intellect.*	FORMS OF INSANITY.
Order 1. Development incomplete	IDIOCY. IMBECILITY.
Order 2. Invasion of disease after development	DEMENTIA. MONOMANIA (Intellectual), including— Delusions. Illusions. Hallucinations.
CLASS II. *The Moral Sentiments.*	
Order 1. Development incomplete . . .	MORAL IDIOCY (?). MORAL IMBECILITY.
Order 2. Invasion of disease after development	MORAL INSANITY. MELANCHOLIA— 1. Religious. 2. Hypochondriacal. 3. Nostalgic. EXALTATION, regarding— 1. Religion. 2. Pride. 3. Vanity. 4. Ambition.
CLASS III. *The Propensities.*	
Order 1. General	MANIA (usually a disorder of all the faculties).
Order 2. Partial	HOMICIDAL MANIA. SUICIDAL MANIA. KLEPTOMANIA. EROTOMANIA. PYROMANIA. DIPSOMANIA.

We quite agree with our authors, that any classification must only be regarded as a chart by which we may shape our course, having only the prominent points marked, or partially delineated. If we regard it as anything more, it is calculated to lead us astray and to encourage false notions, for, after all, one form so readily and frequently changes to another, that we cannot regard the difference between them as essentially very great; and again, the same cause in different individuals will occasionally produce the very opposite forms of insanity. A remarkable instance occurs to our recollection of two women, who having recently lost their husbands, became insane about the same time, and both apparently from that cause. In the one case the mental disturbance took the form of a joyous mania, in the other of intense melancholy.

Idiocy naturally claims a place in a systematic description of the various forms of mental disease; and this frightful affliction has of late years attracted considerable attention and interest, because it has been found that certain individuals of this most unfortunate class, who have hitherto been regarded as little better than the lower animals, are capable of a certain amount of intellectual training, and may, by care-

ful management and patient attention, be reclaimed, and taught at least to conduct themselves with propriety; some to enjoy life unconscious of their mental deficiency; and a few to be useful in a limited way;—probably none but may be benefited to some extent, if it be only physically; many may be raised out of the lowest form of degradation; and a few restored to reason, or rather have their reason developed. Idiocy is a congenital deficiency which becomes the more conspicuous as the individual approaches the age when reason is manifested. Our authors distinguish idiocy from imbecility by saying, that "idiocy always is, imbecility is not necessarily, congenital; idiocy implies a less amount of intellectual power than imbecility."

All the various forms of mental disturbance may pass into dementia; a large proportion of the cases of melancholia which do not recover subside into a degree of dementia, the mental faculties in many becoming absolutely and finally obliterated.

"Some were maniacs the very intensity of whose mental operations appears to have exhausted their supply of cerebral power; and a too rapid succession of images, which ought to have been spread over a lifetime, has been compressed within the narrow limits of a few months. The very brilliancy of the flame has caused its premature extinction; the oil which should have sustained the lustre of an entire life has been lavishly consumed in the production of one splendid but useless conflagration. The ashes in the socket alone remain."

The signification attaching to the terms hallucination, delusion, and illusion, is so uncertain, that they are frequently used indiscriminately, and much confusion consequently arises from the same thing being called by different names by different persons. Hallucination is said to occur where objects appear to be present to the senses of hearing, sight, &c., which have no reality. In fact, if the reason of the patient is unable to recognise and correct the true nature of the perception, his mind is unsound. The expression "delusion," is so often employed in every-day life to signify a simple error of perception or memory, that counsel sometimes puzzle a witness by leading him to say that a person labouring under a delusion is necessarily insane, and then reminding him that we are all liable to entertain delusions as to simple matters of fact. It has been consequently a common thing to call that an insane delusion which the reason is unable to correct, as distinguished from a delusion which is simply an error of perception, and is readily recognised and corrected. Delusion, then, with which illusion is considered to be identical, is a perception which is supposed to have some foundation, but the object appears to the eyes in an entirely different form to that which it actually has, and sensations are produced by the false perception of objects. We should be disposed to give a more extended signification to the term "delusion" than our authors have adopted, and include under this head those cases in which persons have certain false notions and ideas which have no immediate reference to the senses, such as a belief that they are other than they are, and that their condition is quite different to what it really is. The concluding observations upon hallucinations and illusions are clearly put:

"Either may exist (the former rarely) in persons of sound mind; but in that

case they are discredited, in consequence of the exercise of reason and observation, or, if credited, they do not influence the actions. They are sometimes with difficulty distinguished the one from the other, and indeed often merge into or replace each other. . . . Either may be the cause of violent acts, and terminate in murder or suicide; their discovery, therefore, in criminal insanity is most important."

The lawyers refuse to admit the existence of what Dr. Prichard called "moral insanity," notwithstanding that every writer of authority has recognised and described this well-marked morbid condition, although perhaps the designation is open to question. The term "moral insanity" has been supposed to signify only an exaggerated state of the passions and propensities, rendering the individual incapable to guide his actions and control his conduct. This has looked so like an apology for crime on the part of physicians, that lawyers have ignored it altogether, although this is but one of the forms of that variety of insanity which Dr. Prichard recognised as existing without any aberration of the intellectual faculties. There are, in fact, as many forms of moral insanity as there are feelings and passions. These may be in excess—that is to say, in a state of excitement—without disturbance of the purely intellectual part of our mental constitution: they may also be in abeyance, producing melancholy more or less intense, still without intellectual aberration. Excess or deficiency of the feelings and passions may with as much propriety be regarded as morbid, as excess or deficiency in any of the ordinary functions of the body would without question be referred to disease. The fact that a particular form of this malady does occasionally lead to the commission of crime, and is naturally urged in extenuation of it, has created doubts of its existence as a diseased condition, and induced many to believe that it is simply a disregard of self-control, or a deliberate indulgence in vicious passions; but assuredly the frail nature of man predisposes all his faculties and functions to morbid action, and excess of joy or grief (both in themselves natural) are as much entitled to be considered disease when characterized by irrational conduct, as any other departure from the healthy performance of the brain's functions.

In treating of the diagnosis of insanity, the following is the description given of the manner in which an inquiry should be conducted into the mental condition of the patient:

"The physician passes from the observation of the signs to the active investigation of the mental state, by questioning and conversing with the patient. In most cases it is well to commence by drawing the patient into a conversation on the most ordinary and natural topics. These will serve to test his power of attention, and to establish some confidence between the parties. If the physician is quite without clue to the state of the patient's mind, he will do well to observe some order in his examination thereof. By so doing he will save time and trouble; and should the delusions be limited in number and extent, he will be more likely to avoid overlooking them. The delusions which are unconnected with the patient's individuality are few and unimportant. Hence it arises, that if the physician can induce the patient to enter regularly into a description of his own sentiments and opinions respecting himself, he will seldom be left long in the dark respecting the nature of the delusive ideas. This will especially be the case if the physician has the forethought and the

tact to lead the patient to talk about himself in his various relations to his property, his friends, and relatives—his business, his health, his ambition, and his religious hopes. If the physician will range and quarter the extent of his patient's mind as a well-trained pointer does a stubble-field, he will rarely allow any delusion to escape undetected. But if he wanders at random he may expend his labour upon fruitless inquiries. Any order is better than none; but the order of inquiry which would most readily suggest itself—namely, that of examining the state of the mental faculties one after the other—is not in practice the most successful. After testing the fundamental faculties—the attention, the memory, and recollection—and the judgment, which may be done by ordinary conversation on any subject, it will be well to give up the idea of any metaphysical or phrenological system of mind, and to conduct the further examination upon a plan laid down upon the active duties and relations of life. The patient may be led to give an account of his own powers of body and mind with reference to health, to exercise, diet, and study. Thousands of delusions are entertained by insane people upon these subjects. He may then be led to converse respecting his possessions, his means of livelihood, and his hopes of advancement in rank or property; such conversation will open up the delusions of pride, ambition, and acquisitiveness. He may then be led to converse of his near relatives and friends, and especially respecting his birth and parentage—stress being laid upon his belief whether his parents were his actual and real parents. This inquiry will tend to open up any delusions with respect to imaginary greatness, and any perverted emotions towards those who ought to be dear to him. The subject of religious opinion may then be introduced. The religious devotions and exercises which he practises may be inquired into with the reasonable expectation of finding insane delusions on a subject which touches the deepest sentiment of the soul. If the patient is an educated man, it will be right to converse with him upon politics and upon science. If he can stand the test of a discriminating inquiry on these and similar subjects, he certainly cannot be the subject of mania, and if he has any delusions, he must either retain the power of hiding them, or they must exist in some obscure corner of the brain, from which they are little likely to influence with any force the opinions, the feelings, or the conduct." (p. 709.)

Coinciding in the main with the preceding quotation, we cannot quite go along with the author in his conclusions upon an examination so conducted. We think the probabilities are that an experienced physician will detect the delusions which exist, but it must be borne in mind that the most expert have sometimes failed to discover what nevertheless certainly existed; and this may be the case when there is no settled purpose on the part of the patient to conceal his delusions, but simply because the probe has not reached the wound. Then, again, patients suffering from mania may, in the tranquil intervals which they sometimes experience, pass muster, though the inquiry be ever so discriminating. Neither can we agree that, because delusions may exist in some obscure corner of the brain, they are little likely to influence with any force the opinions, feelings, or conduct of the individual. It occasionally happens that the irrational conduct of a patient, which has always been regarded as eccentricity or temper, has been discovered to owe its origin to a delusion which had been carefully concealed, and had at last been detected by accident or betrayed inadvertently. We cannot think that the difficulty of finding a delusion is any proof of its harmlessness. Indeed, the most dangerous are frequently those which are secretly cherished until the insane object

at which they aim is accomplished. The distinction between eccentricity and insanity is often important, and occasionally very difficult to establish. The following remarks will very much assist in an inquiry of this nature:

"There appear to be two forms of eccentricity radically distinct. The one arises from an excess of what phrenologists call individuality. With little regard for the opinions of others, the eccentric man of this class strikes out a path for himself in all matters, both of opinion and of conduct. Such a man is often endowed with more than an average portion of good sense and of moral courage, although his sense is founded upon reasonings marked out by his own mind from propositions laid down by himself, and adverse to the common sense or conscience of those among whom his lot is cast; and his moral courage is displayed in adhesion to his own opinions, and in setting at naught the ill-founded ridicule of the world. Goldsmith gives an admirable sketch of this species of eccentricity in the character of Burchel. It may be safely affirmed that an eccentric man of this type is further removed from the chances of insanity than most of the sane people upon whose prejudices and fantasies he sets a remorseless foot. Such a man possesses the minimum of vanity, and is therefore not easily wounded by events which would overwhelm others with disgrace and chagrin. His intelligence is generally clear and untrammelled, and little liable to be made the sport of his passions. His emotions may be strong, but they are under control. He steers an independent course, far from the fleet of common minds under the convoy of recognised authority; and in the storms of life he battles vigorously against disaster, and resists shipwreck better than most men. . . . . The eccentric man of the second class deviates from the ways of his fellow men from weakness of judgment, from love of applause, and the desire of drawing upon himself the attention of others—from conduct ill-regulated, and influenced only by vacillating emotions—strong and weak according to the caprice of the hour. Men touched with imbecility are almost always eccentric; if the imbecility is secondary upon an attack of acute mental disease—that is, if it is strictly speaking slight dementia—they are always eccentric. . . . . This form of eccentricity is often nearly allied to insanity, and is often premonitory to it." (p. 312.)

There can be no doubt that eccentricity is often nothing more than a disregard of the opinions of others, and that this independence of feeling and action is often combined with rare intelligence and acute reasoning powers; nor is it so liable to degenerate into, or be associated with insanity, as that which is so well described by Dr. Bucknill as the second form of eccentricity. The distinction between these two forms is ably drawn, but it is a question whether we ought not to regard the purest form of eccentricity, when carried to extremes, as disease. Society will tolerate certain departures from its usages, even when considerable inconvenience and some annoyance are occasioned; but this must have its limits. We might not at the present day consider another Diogenes insane, if he insisted on establishing himself in his tub on the pestilential banks of the Thames, and defied the opinion of the world as he wallowed in its mire, and boasted of its pure and invigorating air; but if, carrying his notions of independence a step further, he maintained his right, not only to assume what garb he pleased, but to dispense with clothing altogether, and, emerging from his filthy den to bask on the sunny side of a crowded thoroughfare, we should be driven to the conclusion that there was no alternative but

to treat him as a madman, however well he might be able to argue for the propriety of his acts. There are, then, we think, certain cases in which great extravagance of conduct in itself amounts to insanity, and must be treated accordingly, notwithstanding the absence of delusion and the power to reason correctly.

There are few points in relation to insanity of more importance than the ability to detect simulation, and none are more ably treated in the work before us. Some very interesting cases are given which admirably illustrate the difficulties of this kind of inquiry, and the errors into which those who attempt to feign this malady commonly fall are pointed out with great clearness.

"The feigning madman in all ages has been apt to fall into the error of believing that conduct utterly outrageous and absurd is the peculiar characteristic of insanity. The absurd conduct of the real madman does not indicate a total subversion of the intelligence; it is not utterly at variance with the reasoning processes; but it is consistent either with certain delusive ideas, or with a certain perverted state of the emotions. In the great majority of cases, feigned insanity is detected by the part being over-acted in outrageousness and absurdity of conduct, and by the neglect of those changes in the emotions and propensities which form the more important part of real insanity. Sometimes mania is simulated. The man howls, raves, distorts his features and his postures, grovels on the ground, or rushes about his room, and commits numberless acts of violence and destructiveness. If he has had the opportunity of observing a few cases of real insanity, and if he is a good mimic, he may succeed in inducing a person who only watches him for a few minutes to believe that he is in the presence of a case of acute mania; but if the case is watched for a few hours or days, the deception becomes apparent. No muscular endurance, and no tenacity of purpose, will enable a sane man to keep up the resemblance of acute mania: nature soon becomes exhausted, and the would-be patient rests, and at length sleeps. The constant agitation, accompanied by symptoms of febrile disturbance, by rapid pulse, foul tongue, dry and harsh, or pallid, clammy skin, and the long-continued sleeplessness of acute mania, cannot be successfully imitated. The state of the skin alone will frequently be enough to unmask the pretender. If this is found to be healthy in feeling, and sweating from the exertion of voluntary excitement and effort, it will afford good grounds for suspicion. If after this the patient is found to sleep soundly and composedly, there will be little doubt that the suspicion is correct. Chronic mania may be imitated, and if this should be done by an accurate observer of its phenomena, who also happens to be an excellent mimic, it cannot be denied that the imitation may deceive the most skilful alienist. It is remarkable that two of the most perfect pictures of insanity presented to us in the plays of Shakspeare, are instances of feigned madness—namely, the madness of Hamlet, assumed to escape the machinations of his uncle; and that of Edgar, in *Lear*, assumed to escape the persecutions of his brother. These consummate representations of the phenomena of insanity are so perfect, that in their perusal we are insensibly led to forget they are feigned. In both instances, however, the deception was practised by educated gentlemen; and on the authority of the great dramatic psychologist, it may perhaps be accepted that the phenomena of insanity may be feigned by a skilful actor like Hamlet, so perfectly that no flaw can be detected in the representation. Fortunately for the credit of psychologists, insanity is rarely feigned except by ignorant and vulgar persons, who are quite unable to construct and to act out a consistent system of disordered mind. It must be remembered that all the functions of every case of insanity form a consistent whole, which it requires as much

intelligence to conceive and represent, as it does to conceive and represent any dramatic character." (p. 336.)

Some remarks of Dr. Snell on this subject are quoted. He observes:

"Common people have not the slightest rational idea of insanity. They believe that all mental manifestations are completely altered in it, and that an insane person knows nothing—he ceases to read, to write, and to reckon, and that all his conditions and relations are completely reversed. Hence it happens that all uninformed people find it difficult to acknowledge actual insanity."

But it is not only "common," "uninformed," and "uneducated" people who find it difficult to recognise and acknowledge actual insanity. Some, in consequence of prepossessions of which they cannot divest themselves, are unable to understand how a person can be insane who talks rationally upon any subject; others are willing to give such latitude to eccentricity, to extreme religious opinions, belief, and practices, and to what are called the liberties of the subject—in more senses than one—that it is impossible to convince them that certain faculties of the mind may be disturbed, and insanity exist, while other faculties maintain their normal condition, and the patient is enabled to conduct himself with propriety, and to manifest great intelligence and reasoning power. The well-educated and well-informed classes fall into the same error, and it is shared by a large number of medical men of experience in other departments of medicine, who have comparatively little opportunity to study the subject of mental disease. There are also many who are as unwilling as others are unable to recognise in the persons of their relatives a malady whose existence may have an important bearing upon their own future. And again, it is not probable that, as a rule, insane persons would acknowledge that they were unable to take care of themselves, although there are many exceptions to this rule. The consequence of all this is, that many cases are allowed to become confirmed; the opportunity to arrest the disease, which perhaps only occurs in the early stage, is lost in discussing doubts and scruples, and in determining the true nature of the case; and then come the unavailing regrets that no efficient steps were taken to save the unfortunate sufferer when there was yet a chance of restoring his reason. The idea of controlling the actions and conduct of another, however unreasonable, is often thought so grave a matter, that patients who are only suffering from the premonitory symptoms of insanity—which are not recognised as such—are allowed to dissipate their property and ruin their families before any check is interposed to save them from the effects of their own helplessness. In ordinary illness, or in cases of injury from accident, we at once adopt proper treatment and restrictions, if need be; why should disease of that exquisitely delicate organ, the brain, be allowed to make fatal progress before we resort to any treatment worthy of the name? To be of any value it must be prompt, discreet, and resolute. There is no class of cases which so much requires that the authority of the physician should be supported by the friends of the patient, who should yield him implicit confidence and submit absolutely to his judgment. A firm front being at once presented to the disease, saves a great deal of irritation, and improves the chances of cure.

One of the principal divisions or chapters of the work before us is devoted to the pathology of insanity. The authors object to the distinction between organic diseases and those which are functional, on the ground that the further we advance in our knowledge of pathology, the more we discover changes which were previously not suspected to exist:

For example, fatty degeneration of the heart and epithelial desquamation of the uriniferous ducts are structural diseases to us; a short time ago their phenomena were regarded as functional. Facts of this kind would of themselves be sufficient to create distrust in the theory of functional disease; but many accomplished physicians still maintain that abnormal vital phenomena may be, and are likely to be, occasioned by dynamic aberrations alone, and that such phenomena are correctly designated as functional disease. We cannot concur in this opinion. . . . . What is called force of every description is connected with, if not dependent on, changes in the atoms of matter. Force is the hypothetic agent which underlies the phenomena of material change; and to affirm that dynamic modifications of vital functions may exist without alterations of material organisation, is to ignore the fundamental principles of philosophic physiology. All disease, therefore, in our opinion, is organic. Not only is this so with diseases which come under the common observation of the physician, without leaving traces of organic change—asthma, for instance, and angina and epilepsy—but mental and nervous diseases also of every kind and form. Not a thrill of sensation can occur—not a flashing thought or a passing feeling can take place—without changes in the living organism; much less can diseased sensation, thought, or feeling occur without such changes—changes which we are not able to detect, and which we may never be able to demonstrate, but which we are nevertheless certain of. For, whether we adopt the theory that the states and things which we call heat, electricity, vitality, &c., are distinct entities of what is called imponderable matter, or the far more probable theory, that they are only phenomena belonging to ordinary ponderable matter; an atom or a cell charged with electricity or heat, or in a state of chemical activity, is essentially in a different condition to a cell or an atom in chemical or electrical equilibrium with surrounding substances. On the lowest view of organic action, therefore, alterations of what are called dynamic force cannot exist without corresponding changes in material condition. If it is possible to suppose that the cells of a lining structure in a state of disease can only differ from the cells of the same structure in a state of health by an alteration in their electric states, this will in itself constitute a material difference, capable of being readjusted by appropriate remedies. But there is no ground whatever for supposing that vital force and electrical force are the same, or that anomalous action of living bodies ever depends upon the mere distribution or activity of such force. The only force capable of explaining any of the phenomena of life is the chemical one, and this only in a state of constant activity and interminable change. In a state of health such change takes place within a range whose limits permit beneficial, and restrict injurious, action; in a state of disease the range of chemical change is widened or contracted, so that mischief results from excess of action, or the well-being of the organism is lost by deficient action. In either case the chemical composition of the cells cannot fail to be altered from the standard of health, and alteration of chemical composition is the real groundwork of organic disease, since it invariably interrupts the healthy portion of the part affected. Those abnormal states which depend upon an altered condition of the blood are not less strictly organic than all other diseases; for not only can no change take place in the composition of the blood without in some degree affecting all the parts which are nourished thereby, but this fluid is, strictly speaking, itself a living cellular organism, and every change which takes place therein is organic. It may seem superfluous

that, after having expressed our opinion that the noblest functions of the healthy nervous system are invariably accompanied by organic changes therein, we should argue that such changes must exist when the functions are performed abnormally. It is true that the greater proposition, that function is always accompanied by organic change, includes the lesser proposition, that diseased function is so accompanied; but the latter has a difficulty to contend with from which the former proposition is free—it has to oppose and subvert a long-established erroneous theory."

Our space will not permit of our going into any discussion upon the various points raised in connexion with the pathology of insanity, but this part of the subject is very ably treated, and will abundantly repay attentive study. The author observes upon the remarkable fact, that serious diseases of vital organs will make fatal progress without manifesting the ordinary signs observed in sane persons suffering from similar maladies, and this fact suggests the necessity of a careful examination of all the important organs in insane persons of impaired health, even where there are no symptoms indicating particular disease.

In a discussion on the treatment of insanity, which forms the concluding chapter, the value of the various remedies which have at different times found special favour is considered. Tartrate of antimony and opium maintain their reputation as the most important, and the precautions and conditions to be observed in their employment are carefully pointed out. Of not less importance than the medical is the moral treatment of insanity, and on this subject Dr. Bucknill makes some very sound remarks. He says:

"In works which treat upon the moral treatment of insanity, it has been customary to prescribe rules for the guidance of the demeanour and behaviour towards the insane. From the excess of firmness which is demanded by some, one would think it needful that an alienist physician should have 'an eye like Mars, to threaten and command;' as if his chief functions were not unlike those of the worthy Mr. Van Amburgh. The universal gentleness and indulgence which others advocate would lead to an opposite extreme scarcely less adverse to the true interests of the patients. The truth, as usual, lies between; and the physician who aims at success in the moral treatment of the insane, must be ready 'to be all things to all men, if by any means he might save some.' He must, nevertheless, have a good backbone to his character, a strong will of his own, and with all his inflictions be able to adhere, with singleness of purpose and tenacious veracity, to the opinions he has, on sound and sufficient reasons, formed of his patient, and the treatment needful to be pursued towards him. With self-reliance for a foundation to his character, it requires widely different manifestation to repress excitement, to stimulate inertia, to check the vicious, to comfort the depressed, to direct the erring, to support the weak, to supplant every variety of erroneous opinion, to resist every kind of perverted feeling, and to check every form of pernicious conduct."

The work concludes with an appendix of cases, and is illustrated by a series of portraits of the different types of insanity, of great interest. We commend it to our readers in the full assurance that they will derive pleasure and profit from its attentive perusal, and that it will prove to them a sound and safe guide through some of the greatest difficulties which beset our path when our advice is sought, and we are called upon to deal with the most severe affliction which can befal the family of man.

REVIEW VI.

Clinical Lectures on the Principles and Practice of Medicine. By JOHN HUGHES BENNETT, M.D., F.R.S.E., Professor of the Institutes of Medicine, and Senior Professor of Clinical Medicine, in the University of Edinburgh, &c. &c. Second Edition, with 468 Woodcuts.—*Edinburgh*, 1858. pp. 951.

EVERY member of our profession must admit that in these modern days a new spirit has come over the face of medicine; that a great change, a positive revolution, has been passing into the practice of the art. If we look into the sanctuaries of our knowledge—into the classical authorities of the day—we find therein a strange want of accord between the theory of disease which is expounded and the practice which is enforced. The practice is for the most part novel—the practice of to-day; the theory is still impregnated with the dust of dogmatic and traditional authority, and still leavens the practice, and hampers and clogs its progress to a better state.

When we seek for the cause which has brought about these changes, we find it in the advancements made in Pathology, Diagnosis, and Clinical Medicine. The modern study of medicine has forced us to conclusions which are diametrically opposed, in many important particulars, to the doctrines of our early days; it has enabled us to strip experience of that false covering which has so long obscured the truth; to separate a true from a false experience, and a true theory from a false theory. Who can honestly search into the history of medicine and not confess that this vaunted experience has been as pregnant a source of error, as ever theory has been, in misguiding the healing hand of the physician? The reason why this is so is obvious enough. The facts about which experience is exercised, are most difficult of right appreciation. Disease is not a simple, but a very long and complicated process, and of this process we can seize only a few, and it may be the last, links in the series of antecedents and sequents which form it; and thus it is that we are apt to mark down as the effects of our remedies, results which have no relation to them. The fact observed may be true; but the consequence deduced from it is erroneous. The error consists in ascribing it to some especial antecedent to which in reality it has no reference. And thus has arisen that overwhelming mistake which has for so many ages obscured, and which still so fatally obscures, the practice of medicine—viz., that we arrogate to ourselves, and ascribe to the power of our art much of what belongs alone to the curative beneficent force of Nature.

What, then, is to rescue medicine from these errors, and give to it a firm and permanent basis? We answer unhesitatingly, a proper appreciation of the value of the facts which modern science has placed at our disposal. By these facts alone can we arrive at a true experience. We must have the courage to cast aside the false goddess which men have so vainly worshipped, and which they still so fondly cling to. We must abide by that true knowledge which is the off-

spring of an honest scepticism and a rational faith, enlightened by science. It is cruel and humiliating to the pride of Medicine to acknowledge that from the days of Hippocrates to our own it has put its faith in, and has practised, the most grievous errors; but the cruelty and humiliation must be submitted to if truth demand the sacrifice. Tradition and the great authorities of other days are to be highly respected, but they must not be permitted to outweigh the facts which the discoveries of this day have brought to light. With all due deference to the men of old, we cannot close our eyes to their shortcomings, where our clearer vision shows us that they were wrong.

What constitutes a sane theory and a sane experience in medicine? The answer to this question is of every importance. A true theory is a conclusion which may be logically deduced from a consideration of scientific facts, which is founded on scientific facts, and is in none of its parts contradicted by them. A true experience in medicine is that which is the resultant of the observation of numerous fitting inquirers, who, after due investigation, arrive each at a like conclusion—the conclusion not being contradicted by the observation of other equally capable observers.

By some such tests as these, it seems to us that we can alone arrive at a right appreciation of the meaning of theory and practice in medicine. The practice of our art is empirical; it is not guided by the rules of pure mathematical science. We can predicate nothing *à priori* of the effects of a remedy, and can only judge thereof by the observation of what follows its administration. But, as we have seen, a right judgment in this matter is most difficult; and what has been the bane of past generations, and what is the bane of our own generation, is the drawing of firm convictions and hasty conclusions from the results of a too narrow observation. The wide and uncontradicted assertion of a fact alone gives it the stamp of genuine value. Let us consider this by reference to a few examples..

Is it not certain that there is somewhere a fallacy in the conclusion that mercury is the proper remedy for secondary syphilis, when we find that half the profession administer mercury for it, and half the profession regard salivation as a positive poison in this disease? Must not there be something wrong either in the observation which teaches us that without salivation there can be no hopeful cure for pericarditis, or in the observation which *demonstrates* that salivation so used is a very hurtful appliance? Can their observation be received as true who tell us that quinine infallibly cuts short continued fever, so long as the observation of others contradicts the assertion? We must assuredly act contrary to all rules of reason if we accept as a true cure any method of treatment which does not bear the test of experience, as above enjoined. Wherever serious discordance of opinion reigns concerning the influence of a remedy over disease, we may be sure that its real effect has yet to be proved. We admit, as an established fact, the influence of quinine over ague, of hydriodate of potass over periostitis, of the salts of potass over acute rheumatism, because there is no dissonance of opinion respecting the influence of the remedies over these

diseases; and consequently facts thus obtained are the result of a true experience.

It may be, perhaps, a heavy trial for us to confess how many there are of our daily practical applications which will fail to bear this test of their real virtue; but until we draw a clear line of distinction between what is positively true and what is merely hypothetical in practice, we can never attain to a rational treatment of disease. Happy for us that we have begun at last to know our own ignorance, to learn the limits of our knowledge, and the extent of our powers. Science has given us at last something like a firm rock to rest our steps upon. Our knowledge may be very defective, but we now know that, as far as it goes, it is *sure*. We see how far we may reasonably trust it as a guide in the treatment of disease. The line between that which is demonstrated, and that which is hypothetical, is clearly and well defined. We may, indeed, have made but small advances towards a *positive* treatment of disease, through the light of this pathology; but in one sense the progress of our art is admirable. It is something, at all events, to have knocked away the props of ignorance and prejudice which have sustained the errors of old medicine. We have begun to ascertain the limits of our powers as curers of diseases—to learn what our art can and what it cannot do. We cease to arrogate to ourselves those kind offices which Nature, not our art, performs. These things our forefathers knew not, and could not have known, for the means of knowledge which has led us to these better things were closed to them. They thought that through their remedies they had a power over, and could control and destroy, diseases after a fashion which we know is impossible. We have learnt to guide the patient gently—*tute et jucunde*—to his cure through the *via mala* of disease. If we have gained nothing more than the being rid of the vicious theories which have hitherto directed, and of course viciously directed, the hand of the practitioner to his work, our gain is immense. Error was an essential and necessary associate of the practice of former days. It is not so now: our errors are voluntary errors, for which we are responsible. We can mark where our positive knowledge ends, and can estimate at their proper value the theories and the practices which we follow out in curing diseases. It is just this very knowledge of its fallibility—this philosophic estimate of its actual powers over disease—which distinguishes rational medicine from barren empiricism.

These are facts the truth of which has long been tacitly admitted by some of the philosophic minds of our profession, and which have now for the first time in Great Britain found a fitting exponent in Professor Bennett. Facts, and arguments in a consequent form, and data, were wanting to establish this new faith on an indisputable basis, to inaugurate this new practice. These facts and arguments are here supplied by Dr. Bennett in his 'Principles and Practice of Medicine.'

The appearance of this volume marks the period of a new epoch in the history of medicine. It has come at a most opportune moment, meeting the demand which, as we have said, has gradually been work-

ing its way into the minds of all of us. Old habits and associations still make us cling to the customary practice of our former life, even when our faith in its efficacy has been shaken. It is just such a monitor as this which comes happily in, and enables us (still halting in opinion), under the shelter of authority, to quit the old and enter on the new life of practice. We can conscientiously affirm that we know of no work in our language in which the practitioner and the student will meet with so scientific a guide to a knowledge of the principles of medicine as in this. Dr. Bennett, by publishing it, has bestowed an especial boon on the rising generation of medical aspirants. Every page of it bears the impress of original observation. The author has set down nothing which he has not himself tested or observed. The facts collected thus into one volume may be considered as the results of his accumulated experience; and few men living in our profession have worked harder and done more, theoretically and practically, to advance the position of medical knowledge. As we look through the volume, we meet with abundant proofs of the laborious exertions which Professor Bennett has made in the cause of scientific medicine. The text is profusely illustrated by well-executed woodcuts, of which there are four hundred and sixty-eight—that is, about one illustration to every second page. Without them, histological descriptions would be valueless. Then, again, the practical parts of the work, the diagnosis and the treatment, are exemplified by the details of two hundred and nineteen cases, which have been carefully selected from the records of his hospital experience. To these he appeals as tests of the truths which he would enforce. We will briefly follow the author through the different sections of his work, in order to give our readers an idea of the form and the matter of it.

Following out the general principles inculcated in his introduction, he first teaches the student how to obtain a knowledge of the nature of the disease he is investigating, by laying down for him the rules requisite for arriving at an accurate diagnosis. To this purpose the first section of the book is dedicated. The importance of the subject is manifest enough. Without a distinct diagnosis there can be no satisfactory application of the means of cure. Our author has entered on it *con amore*, detailing carefully a systematic method of conducting the examination of the patient, and showing the right application of the microscope and chemistry to the same end. What the microscope can show us, is of course illustrated by woodcuts. Under the head of Percussion, Professor Bennett strongly recommends the use of Dr. Winterich's hammer:

"The advantages it possesses are—1st, That the tone produced in its clearness, penetrativeness, and quality, far surpasses that which the most practised percussor is able to occasion by other means; 2ndly, It is especially useful in clinical instruction, as the most distant student is enabled to distinguish the varieties of tone with the greatest ease; 3rdly, It at once enables those to percuss who, from peculiar formation of the fingers, want of opportunity, time, practice, &c., are deficient in the necessary dexterity." (p. 36.)

Having long used the instrument ourselves, we can strongly endorse

Dr. Bennett's recommendation of it; we are surprised, indeed, that it has not yet found its way into more general use; it is of especial service in the examination of out-patients of hospitals and dispensaries, in whom the dress cannot be readily removed. Only this we would say to those who first essay its use: be merciful in your blows. We need hardly add, that under this head of examination of the patient, every diagnostic fact worthy of being known has been detailed by Professor Bennett.

But conjoined with diagnosis must necessarily be associated a knowledge of pathology, and therefore next follows the second section, which treats of this subject. It contains in a condensed form a complete account of our knowledge of the different morbid conditions of the body. This subject has within the last fifteen years undergone a complete revolution; organic chemistry and the microscope have changed all the ideas which pathologists previously held concerning these matters.

As an example of the author's method of applying principles in the practical treatment of diseases, we may extract the following passage from his work:

"All general diseases are accompanied by certain changes in the blood, but these changes are to be removed, not by operating on the blood directly, but by obviating or removing those circumstances which have deranged the stage of nutrition primarily affected. For instance, a very intense form of disease may be produced in infants, through improper lactation. The remedy is obvious; we procure a healthy nurse. Ischuria is followed by coma, from the accumulation of urea in the blood; we give diuretics to increase the flow of urine, and the symptoms subside. In the one case, we furnish the elementary principles necessary for nutrition; in the other, we remove the residue of the process. In both cases the blood is diseased, but its restoration to health is produced by acting on a knowledge of the causes which led to its derangement.

"In the same manner we might illustrate the indications for correct practice in the other classes of causes tending to derange the blood, which we have enumerated. Thus, although there be a proper quantity or quality of food, there may be circumstances which impede its assimilation; for instance, a too great acidity or irritability of the stomach, the use of alcoholic drinks, inflammation or cancer of the organ. It is the discovery and removal of these that constitute the chief indications of the scientific practitioner. Again, the capillary vessels become over-distended with blood, and the exudation of liquor sanguinis to an unusual amount takes place, constituting inflammation. How is this to be treated? In the early stage topical bleeding, if directly applied to the part, may diminish the congestion, and the application of cold will diminish the amount of exudation. But the exudation having once coagulated outside the vessels, acts as a foreign body, and the treatment must then be directed to furthering the transformations which take place in it, and facilitating the absorption and excretion of effete matter. This is accomplished by the local application of heat and moisture, the internal use of neutral salts to dissolve the increase of fibrin in the blood, and the employment of diuretics and purgatives to assist its excretion by urine or stool.

"The general principle we are anxious to establish from this general sketch of the nutritive function is, that diseases of nutrition and of the blood are only to be combated by an endeavour to restore the deranged processes to their healthy state, in the order in which they are impaired; that for this purpose, a

knowledge of the process of nutrition is a preliminary step to the proper treatment of their affections; that the theory of acting directly upon the blood is incorrect, and that an expectant system is as bad as a purely empirical one." (p. 111.)

Diseases of the nervous system have been an especial subject of the author's attention. His inaugural dissertation written on taking his degree of Doctor of Medicine, his articles in the Library of Medicine, his researches 'On Inflammation of the Nervous Centres,' and numerous other contributions which he has at different times made to this branch of pathology and practice, are proofs of his intimate acquaintance with the facts relating to it. We are not surprised, therefore, to find that he has treated this section at some length. He has given us a full, though condensed, account of the anatomy, physiology, and pathology of the nervous system. At page 112, he explains the most recent views held respecting the *diastaltic* (reflex) system of Dr. Marshall Hall, illustrating them by drawings taken from the beautiful dissections of Owsjannikow. Section IV., which tells of the diseases of the nervous system, is the most extended and richly illustrated with cases in the book. In addition, under the same head we have a curious and very interesting chapter on the subject of the influence of predominant ideas on the body, to which we shall afterwards allude more particularly.

After a general discussion concerning diseases of nutrition and innervation, the author brings us to a long description of the exudations —simple, cancerous, and tubercular. These exudations require the most careful study. A right understanding of them is necessary as a foundation for the treatment of these abnormal products, when met with in the different viscera of the body. Whether all the views of the author be corroborated by further inquiries or not, this at least is certain, that his method of treating these matters greatly simplifies their study. He maintains, that the same preliminary changes take place in the bloodvessels which give rise to the exudation in all these forms of diseases. The difference between them consists in the particular vital transformations which the exuded matter undergoes. As these pathological views are summed up by the author, we will quote his words:

"We observe in a simple or inflammatory exudation, that it may occur at all epochs in life; that it may attack all tissues, but most commonly attacks those which are very vascular; that it may be poured out in large or small quantities, and that it may occur with greater or less rapidity; hence the terms acute and chronic. We further observe, that the acute exudations are generally attended with symptoms of a peculiar character (inflammatory), and have a great tendency to cell or temporary formations, which rapidly break down, are absorbed and excreted by the emunctories; that the chronic exudations, on the other hand, have a tendency to fibrous or permanent formations, producing adhesions, strictures, indurations, &c.

"We observe in a cancerous exudation that it occurs for the most part in persons of adult or advanced life; that it may also occur in every tissue, but is by far most common in glandular or fatty organs, such as the liver or female mamma, and is very apt to attack the lymphatic glands *secondarily;* that its progress, although sometimes slow when very fibrous, becomes rapid when corpuscles abound in it; that there is a great tendency to the formation of the

most perfect forms of cell-life, which have the power of self-development, and thereby of spreading to neighbouring tissues; and lastly, that when, by pressure, ulceration is produced on free surfaces, it bursts through these in exuberant excrescences.

"We observe in a tubercular exudation, that it occurs for the most part in young subjects, between the periods of dentition and of adult age; that it may also occur in all tissues, but is by far most common *primarily* in the lymphatic glands, and afterwards in fibrous or albuminous textures, as the lungs and serous surfaces; that its progress is in general exceedingly slow; that there is no disposition to the formation of perfect cell-formation, but rather to abortive corpuscles, which form slowly, and slowly break down; that there is little tendency to absorption, but great liability to disintegration and ulceration; and finally, that the local changes are almost always preceded by derangement of the *primæ viæ*, and a group of symptoms known under the name of dyspepsia.

"Taking, then, the products of simple exudation—say pus—as a standard, we cannot fail to remark, that whilst the cell-development of tubercle is below, that of cancer is above, this standard. Of the three kinds of exudation, tubercle is the lowest, and cancer the highest in the scale.

"Of the ultimate cause producing this difference in the formative power of the exudation we are ignorant, but every kind of reasoning must lead to the conclusion, that these changes and effects depend, not upon the vascular system, which is the mere apparatus for the production of exudation; not upon the nervous system, which conducts impressions to or from this apparatus; and not on the texture which is the seat of the exudation, as that varies, whilst the cancerous or tubercular formation is the same, but in the inherent composition or constitution of the exudation itself." (p. 145.)

The exudation, however, instead of undergoing vital transformations, may die, and its death may occur in two ways: first, in a rapid manner, constituting mortification; and secondly, slowly, and thus giving rise to ulceration. These processes are fully described by the author, and treated by him with great novelty, and in a manner well calculated not only to simplify the subject, but also to enforce conviction of the truth of his opinions upon the minds of his readers.

One of the most important parts of the work is, in our opinion, the writer's account of morbid growths; it occupies fifty-two pages of the volume. Few persons have dedicated more time to, and laboured harder in, the microscopical investigation of tumours than Professor Bennett, and this circumstance gives a peculiar value to his description of them. When the specific cell-theory of Lebert was coming into general notice, and when the microscope was regarded by the younger members of the profession as a means for solving all their doubts and difficulties regarding the peculiar characters of any given tumour, Professor Bennett, in his work 'On Cancerous and Cancroid Growths,' published in 1849, distinctly pointed out the fallacy of such views. He there likened the microscope to the stethoscope, asserting that it was not of itself a sure and infallible guide to the truth, but that it was to be used only as another aid or addition to other kinds of methods of investigation. It is no little praise to him to say, that subsequent researches have proved the correctness of the statements he then published. The doctrines of Lebert have been proved to be too exclusive, and therefore in many particulars erroneous. The

proven correctness of our author's views in these matters, entitles him to demand at our hands a careful consideration of whatever further novel views he puts forth in this part of his work.

Discarding all classification based on the supposed innocent or malignant nature of morbid growths, he says—

"The best classification, therefore, is one founded on our knowledge of the compound textures of the growths themselves, assisted as far as varieties are concerned by their similitude to well-known objects or accidental circumstances, which have long been received in pathology as standards of comparison. Thus, the following arrangement appears to me capable of embracing all the known primary classes of morbid growths—

1. Fibrous growths	Fibroma or Inoma.*
2. Fatty growths	Lipoma.
3. Vascular growths	Angionoma.
4. Cystic growths	Cystoma.
5. Glandular growths . . .	Adenoma.
6. Epithelial growths . . .	Epithelioma.
8. Osseous growths	Osteoma.
9. Cancerous growths . . .	Carcinoma.

"All these primary divisions are susceptible of being subdivided, according to the presence of particular substances, or to fancied resemblances which have received names. Thus, the varieties of the above kinds of growths have long been determined by their substance presenting greater or less similitude to well-known objects, such as water, lard, flesh, brain, &c. &c." (p. 156.)

Of all these different kinds of growths, with their varieties, the Professor gives a minute description, which is richly illustrated by admirable woodcuts. The advantage of such illustrations is manifest. A remark made in the Preface is exactly true—viz., that all such descriptions, especially of objects made visible by means of the microscope, are unintelligible to the uninitiated without these helps.

The pathology of morbid growths contains a doctrine which, we apprehend, will not be quietly received by all pathologists. Dr. Bennett shows, and we think satisfactorily, that cartilage, cornea, and epithelium may, under certain circumstances, undergo increased growth of cells, and that they may increase and multiply endogenously; that is to say, by the growth of cell within cell. He gives representations from the works of Redfern and Kölliker (Figs. 258–9), which demonstrate the correctness of this fact in the case of cartilage. Now, if we recollect that this is the peculiar characteristic which serves to distinguish many kinds of so-called malignant growths, especially, for example, encephaloma and epithelioma, as well as that growth to which Mr. Paget has given the name of "myeloid," we cannot but admit the existence of an evident analogy between these so-called malignant growths and those above-mentioned. Yet, and as a proof of the confusion which thus arises in the use of medical terms, we find that the lesion, when it occurs in cartilage and in the cornea, is denominated inflammation, and is considered innocent; whilst, on the other hand, that which occurs in encephaloma and epithelioma (though of exactly a similar nature) is called cancer, and regarded as malignant. The

* Inoma, from ἶς, ἰνος, a fibre.

author, however, objects to this as an unnatural division of like pathological products. He considers that both lesions are the same, and are attributable to the peculiar power occasionally possessed by cells of developing themselves from surrounding nutritive substances, and after such fashion as to constitute the most dangerous and destructive of local growths.

"True theory," he says, "points out that all these lesions are equally destructive, in consequence of increased endogenous cell-growth, and practical experience has long determined the question of their being alike difficult to control." (p. 210.)

As regards the important point of the propagation of morbid growths, Professor Bennett cites cases for the purpose of proving, that all the different kinds of growths have returned after operation, and have appeared in various parts of the body. In short, he believes that all of them, under certain circumstances, may have a constitutional character; and he quotes M. Velpeau, who has, in his opinion, satisfactorily demonstrated the converse of this proposition,—viz., that growths of the most malignant character have been successfully extirpated and finally eradicated from the economy. But he adds—

"While thus it is contended that there is no growth which may not be malignant, and none which may not be innocent, it is not denied that some have a greater tendency to spread and affect the system than others. In reference to treatment, therefore, it becomes of the greatest importance to determine the laws which apparently govern the propagation and multiplication of morbid growths, or the circumstances which render—say carcinoma and epithelioma—more susceptible of being communicated to neighbouring and internal organs, than purely fibrous or osseous ones."

He then adduces several interesting facts to show that tumours are often propagated by invisible nuclei, and by the *fluid* of tumours, which have become infiltrated among the textures surrounding them. This is, in fact, too much overlooked by surgeons in their operations, and they consequently frequently cut out the tumour, but merely *through* the disease.

"Hence the neighbouring textures should be carefully scrutinized, and all those portions of them infiltrated with cancerous germs carefully removed. For this purpose the microscope ought to be a necessary instrument in the operating theatre, and every suspected tissue in the neighbourhood" (of the tumour) "examined by experienced histologists, before the lips of the wound are closed. This proceeding, which I recommended in 1849, has not, so far as I am aware, yet been practised by surgeons, but its propriety has been supported by Van der Kolk, and will yet, I believe, become the rule, when a knowledge of the pathology of morbid growths is better understood." (p. 217.)

We recommend these views to the careful consideration of operating surgeons; if the recommendation were adopted in practice, it is very reasonable to think that we should hear of fewer cases of cancers returning locally after operation.

Having given a full account of morbid growths of textures, the author next proceeds to describe the morbid degenerations of texture. These he groups under four heads, viz.,—1st, the albuminous; 2ndly, the fatty; 3rdly, the pigmentary; and, 4thly, the mineral

degenerations. Under the head of *Albuminous Degenerations*, are comprised—dropsical effusions; depositions of layers of albuminous matters; dense fibrous formations, such as occur in cicatrices, cirrhosis, certain hypertrophies, &c.; the diaphanous bodies, which are the result of precipitation; certain masses composed of molecules found in the brain and the kidney; waxy and colloid transformations, &c. The essential conditions which appear necessary for the production of this kind of degeneration, are—1st, extreme slowness of effusion from the bloodvessels; and, 2ndly, mechanical obstruction of the veins, giving rise to dropsy. Why muscles, cartilage, and the exudations should pass into fibroid degeneration, under pretty much the same circumstances as those under which they become fatty, remains to be explained. *Fatty Degeneration* may seize upon every texture, whether normal or abnormal, in the body. It occurs either in a molecular or cell form. The notion that certain softenings of the brain are caused by fatty degenerations of the capillaries, is opposed by Professor Bennett. He points out, that although such capillaries doubtless become fatty, they cannot be supposed to assume a secretive function and give off the large quantity of molecular fat which constitutes several forms of cerebral softening. He adheres to the opinion advanced by him in 1842—viz., that these fatty molecules are the results of the transformation of the exudation, and are inflammatory in their origin. This explanation, which at least coincides with facts long admitted in pathology, certainly appears not improbable. The muscles, and albuminous and fibrinous compounds, may be chemically transformed into fat. The process may be made to take place artificially out of the body, as when albumen is digested in water; and the same thing may occur in the living body under certain conditions. This explains why it is that all exudations, extravasations of blood, and tissues low in vitality or in aged persons, may become fatty.

Pigmentary Degeneration.—Under this head the author describes all that is known concerning alterations and formations of colour in the economy, whether red, yellow, brown, green, or black. He also ingeniously points out the relation of such pigments to the oily constituents of the frame, to the properties of the atmosphere, and to the nature of the soil or food.

Mineral Degenerations are to be distinguished from concretions, which Professor Bennett treats of separately; they may occur in every tissue of the body, in exudations and in morbid growths, changing their physical and destroying their vital properties. Concretions are non-organized and non-vascular productions, formed by the mechanical aggregation of various kinds of matters, and generally deposited in the ducts or cavities of the hollow viscera. The chapters on degenerations and concretions are well worthy a most attentive perusal. Much new matter will be found under the head of concretions, especially in the instances of the albuminous, the fatty, the mineral, urinary, prostatic, vegetable, fibrous, amyloid, and amylaceous concretions. All these items are richly illustrated.

The third section of the work refers to the change which has

taken place in modern therapeutics. Two most important subjects are here treated of, which involve indeed a crowd of secondary ones. The first is, the diminished employment of bloodletting and other antiphlogistic remedies in the treatment of acute inflammation; and the other, the influence of predominant ideas on the healthy and disordered functions of the body.

On the first of these subjects we shall not linger. The importance of it is seen in the extensive discussions which have of late been excited in the profession in reference to it. Most of us are now tolerably well acquainted with the facts which have been adduced to show, that bleeding is not the cure for inflammation. Every one admits that a change in the treatment of inflammation has found its way into general practice, but there is great difference of opinion as to the cause of this change. Dr. Bennett's facts and reasonings all go to show that bleeding neither is nor ever was the proper remedy for inflammation, and that the revolution in therapeutics has come about solely through the advances to a truer and better knowledge which have been made in medicine by those new lights, modern pathology and modern diagnosis. But Dr. Bennett herein meets with strenuous and highly-respected opponents. These gentlemen assert that our forefathers knew well enough what they were about, and that if they bled, they did it under a full knowledge of the correctness of the practice they pursued, and that we have no pretence for asserting that the practice could have been incorrect. It is true, these gentlemen add, we do not bleed in modern days, and the reason is this: disease is not now what it then was. It has changed its type. It was sthenic in those other times—it is asthenic now. Inflammation required bloodletting then, but it requires it not now. The ancients were right who practised venesection, and we are right who abstain from it. *Tout le monde a raison.*

Now, it is manifest that the dispute is not a vain one. If Dr. Bennett's argument be true, the days of venesection in the treatment of inflammatory diseases are numbered and past; but if the change-of-type theory be correct, then we must be ever on the look-out for another *shift* in the human constitution, which shall bring it back again to that sthenic state it bore in ancient times, and which will again reclaim for it in its inflammations the powerful use of bloodletting. In this section will be found fully stated the arguings both of Professor Bennett and his opponents.

Turn we from them to his next point, a very important one, and about which medicine has been hitherto too little inclined to busy herself—viz., the influence of the mind on the body. The fact has of course long been recognised in medicine, but here we find the subject, as it were, formularized by Professor Bennett. Thus he tells us, one in every twenty persons, by simply gazing intently at any object for ten minutes, will be thrown into a condition in which he or she may not only be made to act upon any train of ideas suggested to them, but in which motion and sensation may be influenced in a variety of ways. It behoves legitimate medicine to seize upon this fact, if such it be, and

rescue it from the hands of mesmeric quacks and charlatans of all descriptions, who have long availed themselves of it, and acted upon it. If it be true, and that it is true no one can reasonably doubt, it manifestly is one of very great importance in medicine. Many of the phenomena which can be occasioned by the process above mentioned, have long been familiar to physicians, as occurring in different forms of diseases, but that they may be produced artificially in healthy individuals, and that by the suggestive ideas of one person, the ideas of so large a proportion of the people may be regulated or directed so as to render them for the time as irresponsible as monomaniacs, are indeed startling propositions. Yet we agree with the author that they must be accepted as true, and that they cannot fail to exercise some day or other an important influence on the theory and practice of medicine. He says, at p. 298—

"By suggesting thoughts to patients in various ways — sometimes by speaking so that they may hear what is said, at others by directing their thoughts to certain subjects, and occasionally rendering these more vivid by repetition or by definite physical impressions, we can fix certain ideas strongly on their minds. These ideas act as stimulants or sedatives, according to their purport, and the current of thought directed to or withdrawn from particular organs or functions. Remarkable cases have been met with where a judicious application of this doctrine has removed insomnolence or various kinds of pain, spasms, and other evidences of excitement; where hysterical paralysis of the limbs or special organs of sense have been relieved or cured; and where the torpid functions of lactation, perspiration, defæcation, menstruation, &c., have been rendered more active.* That such results may be induced, must be admitted by all who reflect, 1st, on the undoubted fact that certain persons are and can be made slaves of dominant ideas; and 2nd, on the equally undoubted fact that such mental ideas are known by universal experience to exercise a stimulating or depressing effect on all the bodily functions. Hence why many drugs and systems of treatment which are inert or uncertain in their action, are supposed to act through the blood or on the tissues directly, although in truth they only operate by exciting expectant ideas, and through them indirectly on the parts disordered."

We cannot too strongly recommend this section to the attention of our readers. If it be true, as it certainly is, that the great classes of diseases of nutrition are now treated in a totally different manner from what they were only twenty years ago, and if diseases of the nervous system, in addition to the change required in their management so far as nutrition is involved, may be directly governed by purely mental processes, it must indeed be admitted that our art has undergone during these latter days a wonderful revolution. These changes we are only now beginning fully to appreciate; and here justice obliges us to declare that it is principally to the author of this work that the profession is indebted for the hastening impulse it has received in this direction. Not indeed that many of the facts upon which those changes are founded were unknown; they were for the most part admitted. Professor Bennett's great merit consists in his explanation of the facts, and in having demonstrated that such changes in practice are owing to the progressive strides of modern diagnosis

* See Braid on Hypnotic Therapeutics: Edinburgh Monthly Journal, 1853.

and pathology. These are the scientific foundations of medicine, and ought therefore to be more and more studied by those who cultivate it in sincerity and truth. Hence we look with some degree of apprehension on the proposed regulations of the College of Surgeons of England, which will have a tendency to diminish the scientific in order to extend the merely practical study of the profession. Certainly all possible development should be given to the latter, but for the student the former should be ever the great foundation of his education, the *sine quâ non* which will prove his sole armour against the promptings of scepticism and the attacks of villanous quackisms.

In Section 4 the author commences the practical part of the work, properly so called, with Diseases of the Nervous System. This is followed by sections on Diseases of the Digestive, Circulating, Respiratory, Genito-urinary, and Tegumentary Systems. Section 10 relates to Diseases of the Blood. The method followed by him is to give details of cases, which are carefully recorded under the heads of History, Symptoms on Admission, and Progress of the Case. In almost all the fatal cases, the post-mortem account is given, and also a microscopic examination of the diseased tissues. Then follows a commentary on each case, referring to the remarkable points in its diagnosis, pathology, and treatment. The section concludes with general remarks of a more systematic nature on the whole class of diseases. Many of the cases thus given are in themselves valuable and rare; a few are evidently simple illustrations of the more common affections met with in hospitals, but not on that account less valuable, as indicating the actual condition of medicine. The plan here followed admits of the communication of instruction in its most valuable form, and it has this additional advantage, that it enables the author to demonstrate more fully to the reader the actual results of his practice in the cases referred to. It is one, however, which is not favourable for the critic, and in the present case entirely precludes him from giving an analysis of so many detached particulars. All we can do is to indicate here and there a few of those facts which have more especially attracted our attention.

At p. 316, in speaking of acute hydrocephalus, Professor Bennett starts the doctrine, that the distinctions of the disease into inflammatory and non-inflammatory, have no real existence. At p. 324 there is an interesting remark:

> "When lymph is poured into the subarachnoid tissue at the base, it so compresses the vessels leading to the choroid plexuses and lining membrane of the ventricles, as to induce effusion; in other words, effusion follows and does not precede exudation."

Under the head of Cerebritis, we have a series of instructive cases, which conclude with an account of the morbid anatomy and microscopic examination of the disease, which, with the introductory pages to this section on Cerebral Softening, prove that the author has made long and careful researches into this subject.

The pathology of disease arising from obstruction of arteries, is succinctly put in the following passage:

"That foreign solid bodies floating in the blood would obstruct the smaller vessels and occasion exudations, was first shown by the experiments of Majendie, Cruveilhier, Gaspard, and others, who injected starch, quicksilver, and various substances into the blood, with the effect of producing fatal inflammations. Cases by Virchow, Kirkes, Tufnell, and others, have further shown the probability that the coagula and so-called vegetations formed in the interior of the heart, may in like manner be occasionally loosened, carried by the blood to a distant part, such as the brain, lungs, or extremities, and become impacted in a distant artery. If so, they may, by causing obstruction, induce hæmorrhage, exudation, and perhaps the kind of lesion from diminished nutrition of which I have just spoken. Many cases are now on record, and Cases 16 and 17 are good examples of them, where, in conjunction with valvular vegetations in the heart, clots have been found leading to important organs, causing in the same individual cerebritis, pneumonia, nephritis, splenitis, &c. But although it is certain that solid plugs may block up arteries and occasion serious results, it is by no means clear that all the instances of disease which have been cited as proof of this in the living body, really depend on arterial obstruction, or that because cerebral hæmorrhage and softening, or pneumonia, are, as is well known, common sequelæ of diseased heart, that therefore plugs of coagulated fibrin, derived from the last-named origin, should originate these secondary lesions. I need scarcely point out that increased or diminished impulse of the heart, arising from hypertrophy or fatty degeneration of its fasciculi, are equally powerful causes of cerebral disease, and that this very commonly in its turn occasions pneumonia by its paralysing influence on the vagi nerves. Besides, although clots are often found in the arteries associated with cardiac vegetations, I am not aware of any facts which demonstrate that the clot originating in one place is the identical clot which has been found in another. Much stress has been laid upon the form of the clot, the broken-off appearance of its ends, its structure, &c. It has been supposed that whilst spontaneous coagulations, or such as are induced by arteritis, are gradually converted into fibrous tissue, those coming from one place and impacted in a vessel at a distance, break down and undergo a disintegrating process. Attempts have even been made to show that the broken ends of impacted coagula correspond with and fit the surfaces of other coagula in the heart or on the cardiac valves, from which they have been supposed to originate. All I say with regard to such arguments is, that numerous investigations into the structure of coagula, under a variety of circumstances, have convinced me that we possess no certain means of distinguishing one clot from another, and that all such statements should be received with great caution." (p. 355.)

Professor Bennett, however, is forced to admit, from the striking pathological phenomena frequently observed during life, the high probability of the occurrence of obstruction by coagula.

The functional disorders of the nervous system are not treated of individually, but a classification is given of them, with definitions that have evidently been carefully framed by one well conversant with the whole subject. Under the head of pathology of these disorders are described, 1st, the congestive; 2nd, the diastaltic or reflex; and 3rdly, the toxic functional derangements. The treatment of congestive functional disorders of the nervous system the author thinks should be of a tonic and nutrient kind. Experience, he says, has satisfied him that bloodletting and antiphlogistics, if ever, are only very seldom beneficial in this case, and that nervous disorder is for the most part a symptom of exhaustion. In reference to chloroform as a remedy for local pain, he says:

"Why remove the consciousness of an individual by deep intoxication, with all its attendant risks, when the same effect can be produced by immersing the parts in a mixture of powdered ice and salt, without any risk at all?" (p. 408.)

The great principle to be followed in the treatment of diastaltic disorders of the nervous system, is to remove the local irritation which excites them, and in toxic disorders to support the system until the poison is exhausted.

Case 43 is an interesting one of poisoning by hemlock, which was originally published by the author in 1845. He shows that the drug produces exactly similar effects to those which are so graphically described by Plato in his account of the death of Socrates—that is, paralysis commencing at the feet and gradually creeping upwards until it affects respiration and the movements of the heart. We are told that Socrates was directed by the executioner to walk about, after taking the poison, until his limbs grew weary. He did so, and then lay down. After a time, when his feet and legs were squeezed, they were found to be insensible. The paralysis proceeded upwards to the abdomen, and now Socrates made a sign to Crito that his intellect was still unaffected. A short time after he was convulsed, his eyes became fixed, and he died. Such were also the symptoms in Professor Bennett's case. The hemlock had been eaten by mistake for parsley, and death followed in three hours and a quarter after its ingestion.

Passing over diseases of the digestive system, we come to Section 6, which treats of the disorders of the circulating system. Herein may be found one very important fact, as we think, at least—viz., evidence not only of the inutility, but of the danger of bleeding in pericarditis. Several interesting cases illustrative of this position are given, among which we may refer to Case 56, that of a woman with the rheumatic form of the disease, terminating in adhesion, and who subsequently died of fatty heart. Case 87, one of acute pericarditis supervening on phthisis, and in a state of constitutional debility, sufficiently shows the asthenic nature of inflammation.

Under the head of aneurism is recorded an exceedingly interesting case, in which the superior mesenteric artery and thoracic aorta were affected. The man was in the Edinburgh Infirmary during a period of two years and a half, became paraplegic from disease of the spinal column and cord, and ultimately died from the effects of a poisonous dose of aconite. We refer to this case because the mode of recording, and the commenting on it is done just in the way in which clinical instruction should be given.

In Section 8 we find the author warmly recommending Dr. Horace Green's topical applications to the larynx. Under the head of Bronchitis, he tells us that he has succeeded in introducing a catheter through the larynx, and injecting the bronchi themselves. He evidently thinks well of the practice, but he truly observes, that further experience is required to enable us to judge of its good effects.

The chapter on Pleurisy contains a number of interesting cases. Case 120, for instance, is one of effusion into the pleura, the progress of which was well marked by the physical signs. There were, however, so few coexisting functional signs, that the subject of it declared he was in perfect health, and could be with difficulty retained in the hospital. Case 122 is one of empyema, with fistulous openings between the lung, pleural cavity, and external surface. Case 123 is remarkable as an example of extreme pneumothorax, in which, as in Case 120, there were no functional symptoms.

"When (says the Professor) I demonstrated to the clinical class at the bedside the tympanitic sound over the right side on percussion, the absence of healthy respiration, the metallic notes with the respiratory murmurs, and the brazen amphoric vocal resonance, he himself denied that there was anything wrong with his chest, and smiled at the trouble we were giving ourselves in examining it."

Such cases are highly interesting, as indicative of the value of physical signs. But the most remarkable case in the book, perhaps, is the 124th. It was one of tubercular empyema, in which paracentesis was performed. The man eventually died from enormous distension of the stomach with air, and emphysema of its coats—a lesion now for the first time described A figure of the transposed viscera is given (p. 625). The mode of death may here be regarded as unique.

The story of phthisis is told at length. Professor Bennett's labours in this field are well known. He has, indeed, published a special work on tubercular disease of the lungs. To him we are indebted for the introduction of cod-liver oil into practice in this country; and we may here remark, that we have been lately astonished to find that physicians in high places exist who consider this remedy as one of no service at all. There is only one way in which we can explain this curious sentiment, and it is by supposing that they who make it have no experience of the effects which result from its use. Many years of trial of it have thoroughly satisfied us of its immense service in this disease. Professor Bennett has paid much attention to the subject of the curability of phthisis. Case 138 is of a lad who entered the hospital in the last stage of this disease, and who, after forty-three months' treatment, thoroughly recovered. The management of it reflects great credit on Dr. Bennett, and is useful as demonstrating the *nil desperandum* in medicine even in this fatal malady. The whole of this chapter well deserves careful perusal, and especially the section on the special treatment of the disease (p. 683). This section concludes with cases of carbonaceous lungs; the disease is common in the neighbourhood of Edinburgh, and all that is known on the subject is here detailed by the professor.

Section 8 treats of Diseases of the Genito-Urinary Organs. With regard to ovariotomy, the author defends the practice in certain cases. The pathology and diagnosis of the disease are carefully given.

Under the head of Nephritis and Pyelitis, there is detailed an interesting case (154) of desquamative nephritis, which proved rapidly fatal

through serous effusions and diminished flow of urine; the man caught the disease when in the hospital, and in consequence of his standing naked in the ward in order to anoint himself with an ointment for itch.

Section 9, on Diseases of the Tegumentary System, is very succinctly given. Skin diseases are usually a bugbear to students. Our author evinces a thorough knowledge of this part of practical medicine, and he greatly simplifies the whole subject. The definitions of the various orders are skilfully framed, and a knowledge of these rightly applied supplies the diagnosis. We admit, with Professor Bennett, that an artificial classification is indispensable in the present state of our knowledge, and that it is the only one which can bring the student into acquaintance with the multifarious forms of these diseases. Under the head of Treatment, may be mentioned the plan of local dressing by weak alkaline solutions, which in cases of vesicular and pustular affections have proved eminently useful. A good account of the dermatozoa and dermatophytes is here given; and also an excellent history of favus, illustrated by capital woodcuts.

Section 10 treats of Diseases of the Blood. Leucocythæmia, as we might expect, is fully treated of. We have in a former number (vol. x. p. 189) exposed the author's views on this subject. We are informed that since the publication of his monograph in 1851, eighteen additional cases of this disease have come under his notice. In one of these the diagnosis was formed from the condition of the blood, there being a difference of opinion as to whether the disease was splenic or ovarian.

The disease hitherto called pyæmia, Professor Bennett calls ichoræmia, after Virchow. He shows by facts and experiments that the mere mixture of pus with blood does not occasion the disorder, but that it depends upon a morbid poison. He says:

"The so-called pus-corpuscles which by some have been seen in the blood, are identical with the colourless cells of that fluid, and, if in excess, constitute white cell-blood. . . . . What has been called pyæmia is not dependent on pus-cells mingling with the blood, but on a matter derived from some kinds of pus which poisons the blood and occasions the secondary phenomena." (p. 852.)

Under the head of Glucohæmia, our author gives an analysis of the modern views on diabetes, and especially those of Bernard, Frerichs, and others. He also points to the importance of the observations made by Virchow, Busk, Carter, and others, as to the existence and even wide diffusion of starch-corpuscles throughout the animal economy.

We find that in Professor Bennett's hands, as in many others, the quinine treatment of continued fever, proposed by Dr. Dundas, of Liverpool, has proved a failure. His cases were carefully noted, and appear decisive of the impotence of the remedy.

In his chapter on Syphilis he shows himself to be an uncompromising enemy of mercury as a remedy for the disease. Alas! when shall there arise amongst doctors consonating opinions? When rather shall there cease to be such utter dissonance in opinion on matters of fact? *Here*, mercury is the physician's sheet-anchor in syphilis; *there*, he avoids

its use in the same disease as a hateful poison! Such lessons should teach us at all events to bear kindly with each other's sentiments. A chapter on the ethics of medicine brings this excellent work to its close.

We cannot shut the volume without congratulating the profession and the author on the publication of it. Its appearance seems to us to mark a new era in medicine—a starting-point from whence we may hope to march onwards, guided by the light of true science. Thus prosecuting our researches, we must arrive at last at something like unity of intention in the practice of our art, and thus experience will be brought into proper unison with theory.

If we look back into the history of medicine, we find that at every period there have existed grave dissensions among the practitioners of our art, respecting the methods of curing diseases. A continual series of perturbations, both in the theories and the practices of successive generations, are to be found recorded on its pages. Faith and Scepticism are here discovered at their work, the one asserting its uncompromising and unreasoning trust in the principles inculcated by authority, and the other indiscriminately rejecting the venerable and dusty records of the past, their rare truths and many errors, and asserting its sweeping and equally irrational disbelief in every fact which is not plain to its comprehension. Here we see the practitioner evoking his principles of practice from the consideration of his theory of medicine; and there again we find him rejecting all the theory, and deducing his practice from the results of his experience: in both cases relying with unswerving confidence on his chosen authority, never deeming it necessary to reckon up the value either of the theory or of the experiment in which he had put his trust.

Legitimate medicine has thus been incessantly occupied with intestine struggles. From the earliest days of the art down to our own, the patrons of Theory and the patrons of Experience have been continually on antagonistic terms, fighting their ineffectual battles. One after another, the elaborate systems worked out by them—at how much cost of wasted intellect?—have crumbled away under the weight of their own unworthiness. One after another, the idols of the Man and the Moment have been erected, worshipped, and have passed away, or remain but as memorials of his ill-directed genius. As the Poet Laureate sings—

> "Our little systems have their day,
> They have their day and pass away."

But still, amidst all these ruins of intelligence which surround and overshadow it, the great art of medicine has abided. Amidst all these aberrations of intellect, all these false deductions, and false practices, and false systems, the edifice still stands firm and erect—nay, it has gained strength and purpose from them; it has passed through its dark ages and its many trials with honour and with triumph; bright gleams of light there are which mark its progress onwards, even through the darkest of its passages. Signals of continual advance are indelibly inscribed on the pages of its history, monuments of its truth and real

nobility, and of the honest intention of its cultivators. This struggling life of medicine we might liken to the bark of Columbus, casting forth into unknown regions, beaten about by difficult seas, tossed hither and thither, the sport of every passing gale, at times threatened with destruction in the whirlpool, sometimes likely to perish through the weakness or folly of those who directed it, or steering wildly, without landmark to guide its course. But still that bark held on its way; still, amidst its temporary retrocessions and its deviations from the right path, it steadily progressed towards the land of promise and of hope. And why? Because there was a master genius, a true man on board, presiding over its movements. And thus has it been with medicine. It has within it a soul of real truth and goodness, which through all these many ages of trouble and of trial has guided and supported it onwards, bringing it still out of its errors and its difficulties.

We need not despair of our art. It still flourishes, despite of its own errors and of the villanous trickeries which under its name ever have been and still are palmed off upon the credulous masses of mankind. Honest and true men are busy repairing and beautifying the edifice. A greater light has been thrown over the study of medicine during the last half century, than has beamed upon it through the thousand previous years of its existence; and we may now look forward with sanguine hope to its true and scientific development. No higher praise can be bestowed upon a physician than this,—that he has assisted in this development, in the true advancement of his profession, that he has laboured successfully in the cause of humanity and of science, and such praise we unhesitatingly accord to Professor Bennett.

REVIEW VII.

Observations on Naval Hygiene and Scurvy, more particularly as the latter appeared during a Polar Voyage. By ALEXANDER ARMSTRONG, M.D., R.N.—*London*, 1858. 8vo, pp. 117.

FROM the period of the discovery, or more correctly of the adoption of the mariner's compass, and the consequent extension of maritime enterprise from mere coasting voyages to distant seas, and eventually to the circumnavigation of the globe, scurvy—a disease known to have prevailed from the most remote antiquity on land—continued, until within a comparatively recent time, to be the especial scourge of the fleets and mercantile marine of Europe.

Until the year 1796, scurvy had baffled and discomfited most important expeditions; it had sent back, covered with humiliation, commanders who otherwise would have returned with the laurels of victory; it had kept our ships lying powerless at anchor while our coasts were being menaced and insulted by the enemy; and even so late as during the last ten years of the past century, it had on more than one occa-

sion not only crippled the efficiency, but had endangered the very existence, of our Channel fleet.*

The history of scurvy, indeed, not only occupies a melancholy page in our naval records, but it further affords a remarkable illustration of the difficulties and delays that obstruct and retard the progress and adoption of practical truth. It is undeniable that during upwards of two centuries the naval annals of which are rife of the ravages and disasters of this scourge, we were in possession of a known means, not only of its cure, but also of its prevention, for a period beyond that usually necessary for the longest voyages.

Rousseus,† who wrote on Scurvy in 1564, remarks that seamen in long voyages cured themselves of this disorder by the use of oranges. Albertus,‡ in 1593, recommended for the cure of scurvy "the juices of acid, austere fruits, such as oranges." The voyage to India of Lancaster,§ in 1600, demonstrated that scurvy could be prevented by lemon-juice as clearly as did the celebrated voyage of the *Suffolk*‖ nearly two hundred years afterwards. Woodall,¶ who was Surgeon of St. Bartholomew's Hospital, and Surgeon-General to the East India Company, declared, in 1636, that the virtues of lemon-juice were superior to those of all other remedies in scurvy. From the early part of the sixteenth century, numerous works on Scurvy by authors of note have appeared in various parts of Europe, accurately describing the causes, symptoms, treatment, and prevention of scurvy. Among those who wrote on this disease during the last century, the names of Lind, Blane, and Trotter stand pre-eminent; and it is mainly to those distinguished men that we owe the introduction of lime-juice into our

* It is not generally known that Admiral Blake, one of Britain's best and bravest naval commanders, was compelled to leave his fleet and go on shore, sick with "fever and scurvy," at a most momentous period of his command in the Channel, and leave Penn, Lawson, and Monk to achieve that great and final victory over the Dutch, in which his renowned antagonist and rival, Van Tromp, was slain, and from which may be dated the real supremacy of our naval power.

† Baldwini Roussei de Magnis Hippocratis lienibus, Pliniique Stomacace ac Sceletyrbe, seu vulgo dicto Scorbuto, Commentarius. 1564.

‡ Scorbuti historia proposita in publicum. A Solome Alberto.

§ In 1600, Commodore Lancaster sailed from England on the 2nd of April with a squadron of four ships—viz., the *Dragon*, having 202 men; the *Hector*, 108 men; the *Susan*, 82 men; and the *Ascension*, 32 men; for the purpose of establishing the East India Company. On the 1st of August they arrived at Saldanha Bay, Cape of Good Hope, with the crew of the commodore in good health, while those of the other ships were so disabled by sickness and scurvy, that the commodore was obliged to assist them in lowering their sails and hoisting out their boats. The ships of the small squadron were in every respect similarly victualled, with this important exception, that the commodore's crew had administered to them, during the voyage, *three table-spoonfuls of lime-juice daily*.

‖ The *Suffolk* sailed from England on the 2nd of April, 1794; she had a supply of lemon-juice sufficient to serve out two-thirds of a liquid ounce daily to each man on board. She arrived in Madras Roads on the 11th of September without losing a man, with only fifteen men on the sick list, and none of them affected with scurvy. She was twenty-three weeks and one day on the passage without having any communication with the land.

¶ "The use of lemon-juice is a precious medicine, and well tried, being sound and good. Let it have the chief place, for it well deserves it. It is to be taken each morning, two or three spoonfuls, and fast after it two hours; and if you add one table-spoonful aqua vitæ thereto, to a cold stomach, it is the better. Some chirurgeons also give this juice daily to their men, as a *preservative*, which course is good, if they have store." He adds: "I dare not write how good a sauce it is at meat, lest the chief in the ship should waste it in the great cabin to save vinegar."—Woodall's Surgeon's Mate, p. 165.

navy, and the adoption of those sanitary regulations which have bestowed upon our seamen a degree of health enjoyed by no other class of the community.

In 1753, Dr. Lind (whom Sir Gilbert Blane has worthily designated the father of nautical medicine), then physician of the great naval hospital at Haslar, published his celebrated 'Treatise on Scurvy.' This work, which everywhere bears the stamp of great learning, industry, and research, as well as of vast personal observation of the disease, may be regarded as containing the essence of all that has been known of scurvy since the most remote time. The proofs of the prophylactic and curative power of fresh vegetables and lime-juice in scurvy, drawn from a great variety of sources, are here set forth with irresistible clearness and force.

"It would indeed," says Lind, "be happy for mankind if in all the various calamities and distresses to which they are subject, the means of relief were so well ascertained as they are in this painful disease."*

Sir Gilbert Blane, physician to the fleet in the West Indies from 1780 to 1783, submitted, in 1781, a memorial to the Lords of the Admiralty, containing suggestions "for preventing sickness and mortality among his Majesty's seamen in the West Indies," in which he states "that scurvy is one of the principal diseases with which our seamen are affected, and that this may be infallibly prevented or cured by vegetables and fruits, particularly oranges and lemons, or limes." Sir Gilbert had a high opinion of wine as an antiscorbutic, and he recommended its substitution for rum as an article of victualling in the fleet. We may here observe that during the blockade of the River Plate, in 1845, by the French and British squadrons, the latter suffered much from scurvy, while the former enjoyed an almost complete immunity from the scourge, owing mainly, as has been shown by Dr. Bryson, to their having a daily ration of red wine, while the English sailors had their usual allowance of rum.† In Captain Collinson's ship, the *Enterprise*, wine was issued to the crew instead of spirits, and with great advantage.

For the noble part that he took in urging those measures which led to the extinction of scurvy from the navy, as well as for effecting other important sanitary and administrative reforms in naval hospitals and fleets, this country owes much to the able, energetic, and ill-rewarded Dr. Trotter, physician to the fleet under Lord Howe, and author of 'Medicina Nautica,' and other well-known works. In his official capacity with the fleet at Spithead, in 1794–5, he had the constant pain and mortification to see squadrons returning to that anchorage, unable to keep at sea from the prevalence of scurvy among the crews of the ships. He carries our sympathies along with him

* Treatise on Scurvy, p. 544.

† On the respective Value of Lime-juice, Citric Acid, and Nitrate of Potash, in the Cure of Scurvy. By Dr. Bryson: Medical Times, vol. xxi. No. 559, p. 435. "It was observed that when the custom of drinking wine more freely was introduced into Holland, this distemper became less frequent. (Bruneri, Tract. de Scorbuto.) And among the first cures recommended to the world was wine with wormwood infused in it. (Part iii. chap. 1—Olaus Magnus.)"—Lind on Scurvy, p. 177.

when, with the enthusiasm of a patriot and philanthropist, and the earnest reasoning of a philosopher, he laments the terrible effect upon the vast number of seamen then under his care of the severe winter, which, by raising the price of vegetables and beef, led the Victualling Board to limit the issue to the fleet of those articles so essential to health, to one day in the week, with the result of inducing an outbreak of scurvy so severe as to endanger the safety of the Channel fleet. In the same spirit of humanity, we find him scouring the gardens round Portsmouth and Gosport for vegetables, and raising by subscription a few guineas among the officers wherewith to purchase oranges and lemons for the sick. When he tells us that the officers resigned their stock of poultry for the use of the sick and wounded of the fleet after the action of the 1st of June, 1794, the noble-hearted man says, "My heart warms with indescribable emotion while I relate a fact that deserves to be recorded with the pen of an angel."*

An application which Dr. Trotter made about this time to the Admiralty, urging the necessity of supplying the fleet with an increased allowance of fresh provisions, pickles, lime-juice, &c., was less successful. His letter was referred by their lordships to the Commissioners of Victualling, by whom, after due consideration, the whole supply, with the exception of some molasses, was *disapproved.*

Those who seek to excuse or to palliate the evils which *red-tapeism* inflicted upon our army during the late war with Russia, can at least plead that we are not essentially a military nation, and that we had long been unused to war. But it could not be urged at the latter end of the last century that we were not essentially a maritime power, or that we had been otherwise than well accustomed to naval warfare. Impartial truth compels us to record that the same baneful influence which in our day was so prejudicial to our forces in the Crimea, was at the period of which we speak a main cause of much discontent, and of much destruction of human life.

In 1795 a Medical Board for the navy was instituted by Earl Spencer, then at the head of the Admiralty, of which Dr. Blair and Sir Gilbert Blane were appointed physicians; and it was through their representation, grounded upon the overwhelming amount of proof in Dr. Lind's work, and the powerful advocacy of Dr. Trotter in favour of the efficacy of lime-juice in scurvy, confirmed by the results of the voyage of the *Suffolk* in the previous year, that this article was included in the regular victualling of the British Navy. From that time, although scurvy has from time to time appeared in gaols and among troops on shore—as in India, at the Cape, and in the Crimea—is to be dated the extinction, practically, of this scourge in our fleets. To use the words of Dr. Armstrong:

"From this period a new era dawned on our navy. The health of our seamen became wonderfully improved, and the efficiency of our fleets was greatly increased, when it was found that ships could keep the sea for any length of time, although deprived of that kind of diet, the want of which had been hitherto attended with such disastrous results. This happy state of things,

* Medicina Nautica, p. 78.

which has contributed so much to the efficiency of our navy, and to the greatness and prosperity of our country, became established by the introduction of lemon-juice and its judicious employment as a means for the prevention and cure of a disease that had previously been the dread of sailors, and the scourge of the sea.

"From this period, therefore, scurvy gradually disappeared from the navy of Great Britain, and is now seldom or never seen in the ordinary course of service; indeed, it should never appear, if proper precautionary means were adopted, and if due discretion and judgment were exercised in using an agent of such undoubted power and efficacy, as long experience has proved lemon-juice to be, in the prevention and cure of scorbutic disease.

"Since the disease has, therefore, I may say, almost disappeared from the navy, it is only to the history of the Polar voyages that we must look for the record of its occurrence. If, by preventing or curing a most destructive malady, a simple but powerful antiscorbutic has enabled us to circumnavigate the world with safety, it has likewise, by the same beneficial agency, enabled us to establish the greatest maritime discovery of the age, the long sought-for 'North-west Passage,' and thus to enlarge the limits of our empire, by the discovery of vast territories in the hitherto unknown regions of the Pole." (pp. 5, 6.)

As concurring in an eminent degree with the supply of lime-juice to raise the health of the navy to its present high standard, due weight must be given to the new scale of provisions introduced about the beginning of the present century—improved in quality as well as in quantity; the great attention given to a proper internal economy on board ships; the substitution of iron tanks for water-casks; the establishment of sick bay messes; and other hygienic measures, that have from time to time been adopted. The means taken to promote the mental culture of the seamen have also contributed in no inconsiderable degree to effect the revolution that has taken place in the sanitary state of the navy. "Hilarity and cheerfulness," Dr. Armstrong well describes, are "the great moral antagonists to disease."

The voyage of Captain Cook in the years 1772–5 is commonly, and most justly, put in favourable contrast with that of Lord Anson, which preceded it by little more than thirty years. Cook's ship, the *Resolution*, with a crew of one hundred and eighteen persons, lost one man only during the whole three years of the voyage, and this death was caused by pulmonary consumption; while Anson lost in two years four-fifths of the crew of his ship, the *Centurion*. But it is not correct to attribute, as is sometimes done, immunity from disease enjoyed by the crew of the *Resolution* to the use of lime-juice. Cook was supplied with portable soup, sweet wort, sour crout, vinegar, and the *robs* (inspissated juices) of lemons and oranges. Those who read carefully the eloquent address of Sir John Pringle, who, as President of the Royal Society, presented the Copley medal to the great circumnavigator for his paper, containing "An Account of the Method he had taken to Preserve the Health of the Crew of his Majesty's Ship, *Resolution*, during his late Voyage Round the World," will find that Captain Cook took comparatively small account of these *robs** as a remedy

* "These preparations being only sent out upon trial, the surgeon of the *Resolution* was told at a conjecture how much he might give for a dose, but without strictly limiting it.

against scurvy, and that his main reliance in warding off this disease was in malt, which he, as well as the surgeon, Mr. Patten, pronounced "the best antiscorbutic sea medicine yet discovered."*

Dr. Armstrong, who everywhere else is remarkably accurate in his statements and quotations, with a very natural bias in favour of a remedy which rendered him such good service in warding off scurvy from the *Investigator* for a period in the Polar Sea unprecedentedly long, has fallen into the common error of ascribing the happy result of the *Resolution* voyage in some degree to "the judicious use of lime-juice." The voyage of Captain Cook would for ever have commanded the admiration of mankind had it effected nothing more than proving what may be accomplished by a firm but humane system of discipline, with an earnest attention to cleanliness, dryness, and clothing; and by adopting every means to obtain for a ship's crew those products of the earth which are necessary for the maintenance of health, and which are more or less abundant in all climates. The devotion of this remarkable man to his crew can be best understood by his own words:

"We came to few places where either the art of man or the bounty of nature had not provided some sort of refreshment or other, either in the animal or vegetable way. It was my first care to procure whatever of any kind could be met with by every means in my power; and to oblige our people to make use of them, both by my example and authority."†

The writings of Lind (1757)‡ had clearly indicated what attention to the simple laws of nature could effect upon the health of a ship's crew; and the voyage of Cook set the question at rest. It is therefore anything but creditable to the discipline and economy of the Royal Navy, that notwithstanding the valuable lesson taught by that voyage, a ship of war, up to 1796, should have continued to be an object of aversion among seamen, from the prevalence of scurvy, dysentery, putrid ulcer, and fever; the liability to which was mainly due to a low bodily tone, the consequence of a scant and unwholesome supply of food, and inattention to the commonest sanitary observances. From the moment that the salutary changes of which we have spoken were carried into effect, the health of our navy began to improve; and in process of time it reached, as we have before said, to a degree that has no parallel among any other class of men.§

The experiment was made with the quantity specified, but with so little advantage that, judging it advisable to lose no more time, he set about the cure with the wort only, whilst he reserved these *Robs* for other purposes. As they (the *Robs*) had been reduced to a small proportion of their bulk by evaporation upon fire, it is probable they were much weakened by that process."—Sir John Pringle's Discourse, appended to Cook's Voyage, vol. ii. p. 384.

* "I am well convinced from what I have seen the wort perform, that if aided by portable soup, sour crout, sago, sugar, and currants, scurvy, that maritime pestilence, will seldom or never make its appearance among a ship's crew on the longest voyages, proper care with regard to cleanliness and provisions being observed."—Extract from Mr. Patten's Journal, Pringle's Discourse, appended to Cook's Voyage, vol. ii. p. 380.

† Cook's Voyages, vol. ii. p. 192.

‡ Dr. James Lind wrote an essay, On the most effectual Means of Preserving the Health of Seamen in the Royal Navy, in which he treats of foul air, malignant fever, the unwholesomeness of new-built ships, diet, portable soup, new-baken bread, the use of the juice of lemons, ventilators, exercise, cleanliness, &c. What more can any sanitarian write upon at the present day?—Mr. Rawlinson's paper, read before the Health Department of the National Association for the Promotion of Social Science: Transactions, p. 491.

§ In proof of what is here assumed, we would submit a reference to the able Statistical

Scurvy, according to Dr. Armstrong, has always occurred to some extent in the ships of the various Polar expeditions, and almost invariably during the first winter in the ice. In the *Investigator* no disease of a scorbutic character appeared until after a sojourn of two winters in the ice, or upwards of two years and three months from the date of the ship's commission. As all the ships employed in Polar service have differed but little in tonnage, internal arrangements, victualling, and general equipment, and even in the *matériel* of the crews, it becomes, as Dr. Armstrong observes,

"A subject for inquiry, to what circumstance it was owing, that one ship, in no respect better equipped or victualled than the others, but in some respects placed under comparatively disadvantageous circumstances, enjoyed an immunity from the disease for a longer period than has been hitherto known, and was visited with less comparative mortality than has ever occurred on board any ship wintering in the Polar Sea." (pp. 7–8.)

To solve this important question forms a main object of Dr. Armstrong's volume; and for that purpose he commences by narrating the various circumstances connected with the voyage which may have favourably or prejudicially influenced the health of the crew of the *Investigator*.

H.M.S. *Investigator* sailed from England in January, 1850, with a complement of sixty-six officers and men, all of whom had been selected for the particular service on a voyage of discovery to the Polar Sea, which it was resolved to enter through Behring's Strait. With the exception of being one day at anchor in the Straits of Magellan, no communication was had with the land until the ship arrived at the Sandwich Islands, six months after leaving England. During this time the crew, besides being twice subjected to the extremes of equatorial heat and winter's cold, were much exposed to wet and bad weather; the ship, from a succession of heavy gales, having become leaky, the decks being crowded with provisions, and the ventilation being thereby rendered imperfect.

The ship entered the ice on the 2nd August, 1850, and about the middle of October she was frozen up in winter quarters, the crew having previously been employed in much harassing work, and exposed to cold and fatigue, while their minds had been kept in a state of morbid agitation, either elated by hope or depressed by despair, as the perilous position in which the vessel was placed excited either the one or the other emotion. The crew notwithstanding entered into their winter quarters in a state of good health and efficiency.

With regard to the victualling of the ship, everything that prudence and forethought could suggest was provided. In addition to the usual provisions, which were of first-rate quality, the ship was supplied with Gamble's preserved meats, Edwards's preserved potatoes, pearl-barley, peas, rice, currants, cranberries, pickled cabbage, onions, walnuts, &c.

Dr. Armstrong appends a table, exhibiting the scale of victualling from the period of the ship's commission until that of her abandonment 3½ years afterwards.

Reports on the Health of the Navy, by Dr. Bryson, and a comparison of the results therein with the health statistics in other services, as well as in civil life.

From this table it appears that full allowance was continued for twenty-one months, that after this period circumstances demanded a reduction from full to two-thirds allowance, the quantity of vegetables remaining but little changed, and the amount of lemon-juice being left unaltered; and that from September, 1852, a further reduction was made, when the issue of vegetables was limited to 2½ ounces daily, and the lemon-juice to half the quantity formerly issued.

While frozen up in these dreary regions, musk-oxen and reindeer were occasionally procured, and during each summer, wild ducks, geese, and ptarmigan afforded the crew a pound of fresh meat weekly. A limited supply of scurvy-grass and sorrel was also obtained in the summer of 1852.

Dr. Armstrong is of opinion, in which we coincide, that the full allowance of provisions is inadequate to maintain health in a Polar climate:

"Having now fully stated the nature and quality of our diet, I may here remark that I consider the full allowance inadequate to maintain health in a Polar climate, from the necessity there exists for a larger supply of carbonized food in such a region. If the supply of food is not equal to the demand, and is deficient in the proportion of carbon necessary for the wants of the system, that supply must be furnished from some other source, otherwise the vital process of respiration cannot be fully maintained. The tissues of the body, therefore, which are rich in carbon, must furnish that element, and hence the body gradually wastes, and debility and disease ultimately ensue. Men are less capable of resisting cold in the second year in a Polar climate than they are in the first, and so on for every subsequent year of their sojourn. Their state of body is either at or above par, on first entering the ice, from the recent effects of wholesome diet and fresh vegetable food, which enable them to generate sufficient heat to meet the exigencies of the climate; and what the allowance of food fails to supply is taken from other sources within the system. Now, in the second year these resources are obviously not so abundant, and a larger instead of a smaller quantity of food is necessary to generate heat, and to make up the deficiency. If this additional supply be not forthcoming, the results which I have before mentioned must necessarily take place." (pp. 16, 17.)

We have already stated that scurvy has always appeared to some extent among the crews of Arctic expeditions. In Sir James Ross's voyage in search of Sir John Franklin, the crew suffered severely from scorbutic disease, having lost seven men within eighteen months, owing, it is believed, to inferiority in the quality of the provisions, and especially of the lemon-juice, with which they were furnished.

Sir William Burnett, in a letter to the Secretary of the Marine Department to the Board of Trade, dated 7th January, 1852, after stating that the nitrate of potash has no remedial influence whatever in scurvy, that the citric acid (crystallized) had in many instances proved useful, and that the lemon was generally efficacious, both as a remedy and prophylactic, adds, that as the lemon-juice supplied to the navy had for many years been of inferior quality, as proved beyond doubt in some late voyages, it was determined, on the fitting out of the expedition of Collinson and Maclure, to prepare the lemon-juice in a manner that would ensure its excellence and maintain its efficacy unimpaired for any length of time.

The usual method of preserving lemon-juice and lime is to add to the expressed juice ten per cent. of alcohol in the shape of rum. On this occasion one half of the supply was slightly boiled, then strained, and on being cooled, bottled. A small quantity of olive-oil was poured upon its surface in the neck of the bottle, which was corked up and sealed over. The second half was prepared simply by adding ten per cent. of brandy with a stratum of oil on the surface, as in the other case. Both the juices were found, on the return of the expedition, to be in the most perfect state of preservation.

Dr. Armstrong seems to have exercised great care in the selection of men for the particular service, and the utmost vigilance in maintaining in health the good crew thus obtained. He considers it of importance that each man took his daily allowance of lime-juice in presence of an officer,* and to these circumstances, along with close attention to ventilation and warmth between decks, cleanliness, and regulated exercise every day when the crew were not employed on the ship or in the chase, he attributes in great measure the high standard of health enjoyed by the ship's company, and their immunity from scurvy for a period unprecedentedly long in the Polar Sea.

With regard to the cause of scurvy, Dr. Armstrong considers, with Lind, Blane, and other authors, that the continued use of salt provisions, defective ventilation, want of cleanliness, and exposure to cold and moisture, are strong predisponents to the disease. The most eminent writer on Scurvy of the present day, Dr. Budd, adopts the view of Bachstrom,† that a long abstinence from "green herbs, vegetables, and fruits, is alone the cause of the disorder;" and contends, that salt meat and impure air have no effect whatever in causing scurvy, and that any influence that cold and moisture may have in giving rise to this disease has been much overrated.‡

No one would now credit the assertion, come from what authority it might, that salt *per se* has any power to cause scurvy; but that it may do so by deteriorating the nutritive qualities of meat, and thus inducing vitiation of blood, and consequent debility, among those who are compelled to live upon a certain allowance of salted provisions, may be more readily admitted. Cold and moisture, if in sufficient intensity and if long continued, by lowering vital power, concur with other causes to favour the scorbutic diathesis; and the same may be said of impure air, and of the enervation produced by residence in a tropical climate.

The records of the voyages of Anson, of La Perouse, of Parry, of King, and others, bear ample evidence of the influence of cold and moisture as predisponents to scurvy;§ and among other proofs of the exhausting effects of the tropics in increasing liability to this disease among men imperfectly supplied with vegetable food, we may cite the

* The allowance in the navy is half an ounce daily, with the same quantity of sugar, after fourteen days on salt provisions. In Polar service these quantities are doubled.

† Observationes circa Scorbutum. Auctore Joanne Fred. Bachstrom.

‡ Article, Scurvy, by Dr. Budd: Library of Medicine, vol. v. pp. 64–8.

§ Some very apposite observations on this subject are to be found under the head of Scorbutus, in Climate of the United States, by Samuel Forrey, M.D., pp. 338–340.

testimony of Dr. Foltz, who, in his 'Report on Scorbutus, as it appeared on board the United States Squadron in the Gulf of Mexico, in 1846,' thus sums up the causes of the disease—

"Protracted cruising between the tropics, unwholesome and innutritious salt provisions, vitiated atmosphere on board resulting from imperfect ventilation, at times a reduction in the quantity of water, and in the 'Raritan' despondency and disappointment from being kept on board ship after the expiration of the period for which many of the crew had shipped."*

Nyctalopia, and other symptoms of scurvy, commonly manifest themselves after the third month among the crews of ships of war blockading rivers on the west coast of Africa; and we believe that in these cases the induction of the disease is promoted by the prostrating effects of that baneful climate.

We think, then, with Dr. Armstrong, that the continued use of salt provisions, cold and moisture, harassing duties, and we may add, the weakening influence of a tropical climate, all concur to the more rapid development of scurvy among men deprived of or inadequately provided with fruits or vegetables.

In treating of the disease, Dr. Armstrong considers scurvy under three heads—viz., 1, scorbutic debility or mild scurvy; 2, scurvy proper; and 3, acute scurvy. The symptoms and course of each division are clearly and intelligibly set forth; and the remarks on the pathology of the disorder indicate industry and research, but our space will not allow us to discuss them. The details of three cases of scurvy that terminated fatally, by diarrhœa, ascites, and pericarditis respectively, are valuable from the judicious treatment employed, and from the careful and concise manner in which they are narrated.

With regard to the hygienic suggestions scattered throughout the volume, they may be very advantageously read, especially by the young medical officers of the public services, and well deserve the earnest attention of the owners as well as of the officers of the mercantile marine.

Dr. Armstrong raises a most important question when he alleges that scurvy is more prevalent now than formerly among the seamen of our mercantile marine. The merchant service, as has been well said, forms "the source and resource of our naval power," and the condition of its seamen (amounting at this day to 200,000) is consequently a subject which must ever engage the attention of governments, and indeed of all who are interested in the well-being of that arm of the public service by which our glories have mainly been achieved, and our high position among nations maintained. The Merchant Seaman's Act (17 & 18 Vict., cap. 104) provides for increased sleeping room on board ship, ventilation, improved victualling, and, under certain circumstances, medicines and medical stores, including lemon-juice.† As it

* Dr. Foltz's Report, p. 19.

† "The following rules shall be observed with respect to medicines, medical stores, and antiscorbutics (that is to say),—"1. The Board of Trade shall from time to time issue and cause to be published a scale of medicines and medical stores suitable to accidents and diseases arising on sea voyages.—2. The owner of every ship navigating between the United Kingdom and any place out of the same, shall provide and cause to be constantly

seemed to us scarcely probable that these provisions of the act were so little attended to as to be wholly inoperative, we resolved, as far as possible, to remove all doubt in the matter, and applied in various quarters for information on the subject. By the courtesy of Captain John Hoskins Brown, R.N., Registrar-general of Seamen, we had access to log-books and other documents in his office; and to the same able servant of the Crown we are indebted for the following elaborate, valuable, and most significant statistical details, to which, from limited space, we are here wholly unable to do justice, but which speak unmistakeably for themselves.

Number and Tonnage of Vessels belonging to the United Kingdom and Channel Islands employed solely as Foreign-going Ships in the Years 1852–7, *with their Crews. Also the Number of Seamen whose Deaths have appeared in the Articles of Vessels arriving at the various Ports of the United Kingdom during the same Years, distinguishing those who died of Scurvy.*

Years.	Vessels.	Tons.	Crews (exclusive of masters).	Total number of deaths reported.	Deaths from scurvy.
1852	7580	2,449,364	110,769	2298	17
1853	8357	2,791,224	122,091	3263	10
1854	7418	2,759,120	114,639	2799	35
1855	7957	3,018,951	123,733	3292	50
1856	8551	3,190,011	127,805	3606	32
1857	8100	3,168,105	124,580	3474	28

Deaths from Scurvy, classified according to Nature of Voyages.

Years.	Mediterranean.	Africa.	Mauritius.	Red Sea.	East Indies and Australia.	S. America and Pacific.	West Indies.	North America.	Davis' Strait.	Total.
1852	2	...	...	...	13	2	...	...	...	17
1853	...	1	1	...	6	1	...	1	...	10
1854	...	10	1	1	14	9	...	...	...	35
1855	...	8	...	...	32	9	1	...	...	50
1856	1	13	...	1	15	1	1	...	...	32
1857	...	1	1	1	16	4	...	...	5	28

kept on board such ship a supply of such medicines and medical stores in accordance with the said scale.—3. The master or owner of every foreign-going ship (except those bound to European ports or to ports in the Mediterranean Sea, and also except such ships or classes of ships bound to ports on the eastern coast of America north of the thirty-fifth degree of north latitude, and to any islands or places in the Atlantic Ocean north of the same limit as the Board of Trade may from time to time exempt from this enactment), shall also provide and cause to be kept on board such ship a sufficient quantity of lime or lemon-juice, or of such articles as the Board of Trade sanctions as substitutes for lime or lemon-juice, and also of sugar and vinegar.—4. The master of every such ship as last aforesaid, shall serve out the lime or lemon-juice or other such articles as aforesaid, and sugar and vinegar to the crew, whenever they have consumed salt provisions for ten days, and so long afterwards as such consumption continues, the lime or lemon-juice and sugar

We learn also that the annual admission of scurvy cases into the *Dreadnought* hospital-ship still averages about thirty, and that nearly the same number are found with scorbutic taint, although entered for other diseases.

Now these facts, and it is undeniable that they are facts, are certainly discreditable to a service to which we are in other respects so much indebted, and which contributes so largely to the greatness of this country.

We assert most unhesitatingly that scurvy ought rarely, if ever, to occur in any ship whatever. If means have been found to annihilate practically the existence of this disease in the royal navy, the same means ought to be made available for its extinction in the mercantile marine. We have good reason to believe that provisions and lime-juice of an inferior quality are not unfrequently supplied by ship-owners, and that hygienic measures but too often find small favour with the masters and other officers of merchant ships.* We therefore earnestly hope with Dr. Armstrong that some stringent measures may be taken by the Board of Trade which shall procure a *bonâ fide* official

daily at the rate of half-an-ounce each per day, and the vinegar weekly at the rate of half-a-pint per week to each member of the crew.

"And if in any such ship as aforesaid such medicines, medical stores, lime or lemon-juice, or other articles, sugar and vinegar, as are hereinbefore required, are not provided and kept on board as hereinbefore required, the master or owner shall incur a penalty not exceeding twenty pounds; and if the master of any such ship as aforesaid neglects to serve out the lime or lemon-juice or other articles, sugar and vinegar, in the case and manner hereinbefore directed, he shall for each such offence incur a penalty not exceeding five pounds; and if any master is convicted in either of the last-mentioned penalties, and it appears that the offence is owing to the act or default of the owner, such master may recover the amount of such penalty and the costs incurred by him from the owner."—17th & 18th Vict., cap. 104, § 224.

* The following extract from the log-book of a ship called the *Colgrain*, is one of the numerous instances of scurvy attacking a ship's company which are found in the logs at the Registrar-general of Seamen's office:

"*Ship* Colgrain [*Temperance Ship*], *on a Voyage to San Francisco, sailed from London April 30th*, 1856.

"Aug. 23rd, 1856, midnight. Lat. 57°50′ S. Long. 75°20′ W.—George Wall, A.B., off duty on pretence of illness, says he has a sore finger and his arm affected; we think, however, he is skulking on account of bad weather.—Sept. 2nd. George Wall having returned to duty for a few days, is again off duty with scurvy.—Sent to San Francisco Hospital Nov. 23rd. Died Dec. 6th.

"Sept. 13th. Lat. 48°10′ S. Long. 80°56′ W.—James Reilly, seaman, laid up with rheumatism, which has lapsed into scurvy.—Nov. 1. Died last night.

"Sept. 17th. Lat. 44°13′ S. Long. 79°30′ W.—George Proctor off duty, with swelled ankle, something like dropsy, and which ultimately proved to be scurvy. (For the last three weeks serving out sago, tapioca, and preserved soup, and two ounces more sugar a week to the crew.)

"Oct. 24th.—David Greig, A.B., for the last few weeks has been unable to go off the deck through swelled legs from scurvy; has given in, the swelling having increased.

"Thomas Proctor also off duty the following day.

"Nov. 16th. Off San Francisco.—John Davidson off duty from scurvy. And following day, Aug. Gustaffsen from same cause.

"Nov. 22nd. At San Francisco.—Sent to English hospital, David Greig, John Davidson, Thomas Proctor, Aug. Gustaffsen, and George Anderson, with scurvy."

It is sometimes alleged that in temperance ships the absence of spirits conduces to scurvy; we believe, however, that spirits possess no antiscorbutic power whatever; but we should like to see beer issued occasionally to the crews of such ships. At the Victualling Committee which sat about six years ago at the Admiralty, and made various improve-

inspection of all provisions, medicines, and medical stores for our merchant seamen, and thus ensure their being properly furnished both as regards quality and quantity.

We repeat emphatically that if the regulations of the Board of Trade are faithfully and honourably carried out, scurvy ought to be a disease as little known in the ships of the merchant service as it is in those of the royal navy.

We sat down to this book with the impression that enough had already been written on scurvy—that, in fact, the subject was exhausted; but we are now bound to acknowledge that in our opinion Dr. Armstrong has shown good reason for the publication of his volume, and that in doing so he has furnished us with a valuable addition to the literature of scurvy and naval hygiene.

We learn in the preface that the observations on scurvy formed part of the journal of Dr. Armstrong's practice while serving in the *Investigator*, which obtained for him the honour of the Blane Gold Medal, an award of the adjudicators which we feel assured would have met the fullest approbation of the venerable founder himself, had he lived to the present day.

ments in the victualling of the fleet, a proposition was made, but not adopted, to substitute beer for spirits in the royal navy.

All cases of ships arriving in England, the crews of which have suffered from an unusual amount of sickness or mortality during the voyage, are officially investigated by the Board of Trade. The following is one of such cases, for which we have also to thank Captain Brown:

"James Douglas Mowat was master of *Samuel Boddington*, of London. Sailed from London Feb. 21st, 1857, for Kurrachee. The crew consisted of eighteen Asiatics and twelve Europeans. Few of the Asiatics had a slight touch of the scurvy going out. The lime-juice and sugar were regularly served out. The Asiatics were discharged, as per agreement, at Kurrachee, when the cargo was out. Five of the Asiatics were reshipped for the homeward voyage, including the tindall, six new hands, also two cooks, and one steward, in all fourteen Asiatics. The European crew same as before. At St. Helena, on Dec. 9th, I shipped two more Europeans to strengthen the crew, as winter was coming on. The crew was free from scurvy when I left St. Helena, where I obtained two gallons of lime-juice. Provisions served out to the Asiatics, one pound of rice, one pound of beef or fish, dall, ghee, as customary. Tea and sugar once a day; biscuit at the same time as the tea and sugar; no short allowance during voyage. Curry stuff and salt as much as asked for. On Jan. 8th one of the Asiatics (new hand) was sick and off duty with the scurvy; gave him castor-oil and tincture of rhubarb; sago and wine for nourishment; also lime-juice, nitre, vinegar, &c. Man died Jan. 19th. Jan. 9th another Lascar was taken ill and died on the 24th. Another was taken ill Dec. 26th, 1857, and died Jan. 10th, 1858. Another Dec. 28th, and died Jan. 10th, all from the effects of scurvy. None of the Europeans had the slightest symptoms, nor any others, excepting the four men who died, neither of whom were taken out in the ship, but were new hands shipped at Kurrachee.

"Wages and effects accounted for to the shipping-master as per regulations. The entries of all these transactions are properly entered in the official log-book, and duly signed by the master, mate, and stewards.

"Forecastle was cleaned out every morning when the poop was washed. In cold weather washed out once or twice per week. Provisions were served out to the Lascars twice daily, and they cooked it themselves.

"By Col. Hughes: Lime-juice was served out to the sick men in a bottle whenever they asked for it. There is about a gallon now on board the ship.

(Signed) "J. D. MOWAT."

It appeared in the course of the investigation that these four Lascars had all been in prison for some time, and had been released only a week before they embarked in the *Samuel Boddington*. Lime-juice, it is true, was served out, but only when the crew asked for it. Had they been seen to take it, the result might have been otherwise.

Besides the other undoubted merits of the journal, Sir Gilbert would have found in it another and most satisfactory proof that he had not exaggerated the prophylactic and curative power of lemon-juice in scurvy, when he stated in the 'Select Dissertations' that it was "peculiar and exclusive, when compared to all other remedies"—that it was "*sui generis—nil simile nec secundum.*"*

Review VIII.

1. *On the Archetype and Homologies of the Vertebrate Skeleton.* By Richard Owen, F.R.S.—*London*, 1848. pp. 172.
2. *Principes d'Ostéologie Comparée, ou Recherches sur l'Archétype et les Homologies du Squelette Vertébré.* Par Richard Owen.—*Paris.*

Principles of Comparative Osteology; or, Researches on the Archetype and the Homologies of the Vertebrate Skeleton. By Richard Owen.

3. *On the Nature of Limbs.* A Discourse delivered on Friday, February 9th, at an Evening Meeting of the Royal Institution of Great Britain. By Richard Owen, F.R.S.—*London*, 1849. pp. 119.

Judging whether another proves his position is a widely different thing from proving your own. To establish a general law requires an extensive knowledge of the phenomena to be generalized; but to decide whether an alleged general law is established by the evidence assigned, merely requires an adequate reasoning faculty. Especially is such a decision easy where the premises do *not* warrant the conclusion. It may be dangerous for one who has but little previous acquaintance with the facts, to say that a generalization is demonstrated; seeing that the argument may be one-sided: there may be many facts unknown to him which disprove it. But it is not dangerous to give a negative verdict when the alleged demonstration is manifestly insufficient. If the data put before him do not bear out the inference, it is competent for every logical reader to say so.

From this stand-point, then, we venture to criticise some of Professor Owen's osteological theories. For his knowledge of comparative osteology we have the highest respect. We believe that no living man has so wide and detailed an acquaintance with the bony structure of the vertebrata. Indeed, there probably has never been any one whose information on the subject was so nearly exhaustive. Moreover, we confess that nearly all we know of this department of biology has been learnt from his lectures and writings. We pretend to no independent investigations, but merely to such knowledge of the phenomena as he has furnished us with. Our position, then, is such that, had Professor Owen simply enunciated his generalizations, we should have accepted them on his authority. But he has brought forward evidence to prove

* Select Dissertations on Medical Science, by Sir Gilbert Blane, Bart., vol. i. p. 27.

them. By so doing he has tacitly appealed to the judgments of his readers and hearers—has practically said, "Here are the facts; do they not warrant these conclusions?" And all we propose to do, is to consider whether the conclusions *are* warranted by the facts brought forward.

Let us first limit the scope of our criticisms. On that division of comparative osteology which deals with what Professor Owen distinguishes as "special homologies," we do not propose to enter. That the wing of a bird is framed upon bones essentially parallel to those of a mammal's fore-limb; that the cannon-bone of a horse's leg answers to the middle metacarpal of the human hand; that various bones in the skull of a fish are homologous with bones in the skull of a man—these and countless similar facts, we take to be well established. It may be, indeed, that the doctrine of special homologies is at present carried too far. It may be that, just as the sweeping generalization at one time favoured, the embryonic phases of the higher animals represent the adult forms of lower ones, has been found untrue in a literal sense, and is acceptable only in a very qualified sense; so the sweeping generalization that the skeletons of all vertebrate animals consist of homologous parts, will have to undergo some modification. But that this generalization is substantially true, all comparative anatomists agree.

The doctrine which we are here to consider is quite a separate one —that of "general homologies." The truth or falsity of this may be decided on quite apart from that of the other. Whether certain bones in one vertebrate animal's skeleton correspond with certain bones in another's, or in every other's, is one question; and whether the skeleton of every vertebrate animal is divisible into a series of segments, each of which is modelled after the same type, is another question. While the first is answered in the affirmative, the last may be answered in the negative; and we propose to give reasons why it should be answered in the negative.

In so far as his theory of the skeleton is concerned, Professor Owen is an avowed disciple of Plato. At the conclusion of his 'Archetype and Homologies of the Vertebrate Skeleton,' he quotes approvingly the Platonic hypothesis of ἰδέαι, "a sort of models, or moulds in which matter is cast, and which regularly produce the same number and diversity of species." The vertebrate form in general (see diagram of the *Archetypus*), or else the form of each kind of vertebrate animal (see p. 172, where this seems implied), Professor Owen conceives to exist as an "idea"—an "archetypal exemplar on which it has pleased the Creator to frame certain of his living creatures." Whether Professor Owen holds that the typical vertebra also exists as an "idea," is not so certain. From the title given to his figure of the "ideal typical vertebra," it would seem that he does; and at p. 40 of his 'Nature of Limbs,' and indeed throughout his general argument, this supposition is implied. But on the last two pages of the 'Archetype and Homologies' it is distinctly alleged that "the repetition of similar segments in a vertebral column, and of similar elements in a vertebral segment,

is analogous to the repetition of similar crystals as the result of polarizing force in the growth of an inorganic body;" it is pointed out that, "as we descend the scale of animal life, the forms of the repeated parts of the skeleton approach more and more to geometrical figures;" and it is inferred that "the Platonic ἰδέα or specific organizing principle or force, would seem to be in antagonism with the general polarizing force, and to subdue and mould it in subserviency to the exigencies of the resulting specific form." If Professor Owen's doctrine is to be understood as expressed in these closing paragraphs of his 'Archetype and Homologies'—if he considers that "the ἰδέα" "which produces the diversity of form belonging to living bodies of the same materials," is met by the "counter-operation" of "the polarizing force pervading all space," which produces "the similarity of forms, the repetition of parts, the signs of unity of organization," and which is "*subdued*" as we ascend "in the scale of being;" then he implies the somewhat questionable belief that the properties which the Creator has given to matter have hindered the realization of His designs. If, on the other hand, Professor Owen holds, as every reader would suppose from the general tenor of his reasonings, that not only does there exist an archetypal or ideal vertebrate skeleton, but that there also exists an archetypal or ideal vertebra; then he carries the Platonic hypothesis much further than Plato does. Plato's argument, that before any species of object was created, it must have existed as an idea of the Creative Intelligence, and that hence all objects of such species must be copies of this original idea, is tenable enough from the anthropomorphic point of view. But while those who, with Plato, think fit to base their theory of creation upon the analogy of a carpenter designing and making a table, must yield assent to Plato's inference, they are by no means committed to Professor Owen's expansion of it. To say that before creating a vertebrate animal, God must have had the conception of one, does not involve saying that God gratuitously bound Himself to make a vertebrate animal out of segments all moulded after one pattern. As there is no conceivable advantage in this alleged adhesion to a fundamental pattern—as for the fulfilment of the intended ends it is not only needless, but often, as Professor Owen argues, less appropriate than some other construction would be (see 'Nature of Limbs,' pp. 39, 40), to suppose the creative processes thus regulated, is not a little startling. Even those whose conceptions are so anthropomorphic as to think they honour the Creator by calling Him "the Great Artificer," will scarcely ascribe to Him a proceeding which, in a human artificer, they would consider a not very worthy exercise of ingenuity.

But whichever of these alternatives Professor Owen contends for—whether the typical vertebra is that more or less crystalline figure which osseous matter ever tends to assume in spite of "the ἰδέα or organizing principle," or whether the typical vertebra is itself an "ἰδέα or organizing principle"—there is alike implied the belief that the typical vertebra has an abstract existence apart from actual vertebræ. It is a form which, in every endoskeleton, strives to embody itself in

matter—a form which is potentially present in each vertebra; which is manifested in each vertebra with more or less clearness; but which, in consequence of antagonizing forces, is nowhere completely realized. Apart from the philosophy of this hypothesis, let us here examine the evidence which is thought to justify it.

And first as to the essential constituents of the "ideal typical vertebra." Exclusive of "*diverging appendages*" which it "may also support," "it consists in its typical completeness of the following elements and parts":—A *centrum* round which the rest are arranged in a somewhat radiate manner; above it two *neurapophyses* converging as they ascend, and forming with the centrum a trianguloid space containing the neural axis; a *neural spine* surmounting the two neurapophyses, and with them completing the neural arch; below the centrum two *hæmapophyses* and a *hæmal spine*, forming a hæmal arch similar to the neural arch above, and enclosing the hæmal axis; two *pleurapophyses* radiating horizontally from the sides of the centrum; and two *parapophyses* diverging from the centrum below the pleurapophyses. "These," says Professor Owen, "being usually developed from distinct and independent centres, I have termed 'autogenous elements.'" The remaining elements, which he classes as "exogenous," because they "shoot out as continuations from some of the preceding elements," are the *diapophyses* diverging from the upper part of the centrum as the parapophyses do below, and the *zygapophyses* which grow out of the distal ends of the neurapophyses and hæmapophyses.

If, now, these are the constituents of the vertebrate segment "in its typical completeness;" and if the vertebrate skeleton consist of a succession of such segments; we ought to have in them representatives of all the elements of the vertebrate skeleton—at any rate, all its essential elements. Are we then to conclude that the "diverging appendages" which Professor Owen regards as rudimental limbs, and from certain of which he considers actual limbs to be developed, are typically less important than some of the above-specified exogenous parts—say the zygapophyses?

That the meaning of this question may be understood, it will be needful briefly to state Professor Owen's theory of 'The Nature of Limbs;' and such criticisms as we have to make on it must be included in the parenthesis. In the first place he aims to show that the scapular and pelvic arches, giving insertion to the fore and hind limbs respectively, are displaced and modified hæmal arches, originally belonging in the one case to the occipital vertebra, and in the other case to some trunk-vertebra not specified. To give a colour to this assumption of displacement, carried in some cases to the extent of *twenty-seven* vertebræ, Professor Owen cites certain acknowledged displacements which occur in the human skeleton to the extent of half a vertebra—a somewhat slender justification. But for proof that such a displacement *has* taken place in the scapular arch, he chiefly relies on the fact that in fishes the pectoral fins, which are the homologues of the fore-limbs, are directly articulated to certain bones at

the back of the head, which he alleges are parts of the occipital vertebra. This appeal to the class of fishes is avowedly made on the principle that these lowest of the Vertebrata approach closest to archetypal regularity, and may therefore be expected to show the original relations of the bones more nearly. Simply noting the facts that Professor Owen does not give us any transitional forms between the alleged normal position of the scapular arch in fishes, and its extraordinary displacement in the higher Vertebrata; and that he makes no reference to the embryonic phases of the higher Vertebrata, which might be expected to exhibit the progressive displacement; we go on to remark, that in the case of the pelvic arch, he abandons his principle of appealing to the lowest vertebrate forms for proof of the typical structure. In fishes, the rudimentary pelvis, widely removed from the spinal column, shows no signs of having belonged to any vertebra; and here Professor Owen instances the perennibranchiate *Batrachia* as exhibiting the typical structure: remarking that "mammals, birds, and reptiles show the rule of connexion, and fishes the exception." Thus in the case of the scapular arch, the evidence afforded by fishes is held of great weight, *because* of their archetypal regularity; while in the case of the pelvic arch, their evidence is rejected as exceptional. But now, having, as he considers, shown that these bony frames to which the limbs are articulated are modified hæmal arches, Professor Owen points out that the hæmal arches habitually bear certain "diverging appendages;" and he aims to show that the "diverging appendages" of the scapular and pelvic arches respectively, are developed into the fore and hind limbs. There are several indirect ways in which we may test the probability of this conclusion. If these diverging appendages are "rudimental limbs"—"future possible or potential arms, legs, wings, or feet," we may fairly expect them always to bear to the hæmal arches a relation such as the limbs do. But they by no means do this. "As the vertebræ approach the tail, these appendages are often transferred gradually from the pleurapophysis to the parapophysis, or even to the centrum and neural arch."* Again, it might naturally be assumed that in the lowest vertebrate forms, where the limbs are but little developed, they would most clearly display their alliance with the appendages or "rudimental limbs" by the similarity of their attachments. Instead of showing this, however, Professor Owen's drawings show that whereas the appendages are habitually attached to the pleurapophyses, the limbs in their earliest and lowest phase, alike in fishes and in the lepidosiren, are articulated to the hæmapophyses. Most anomalous of all, however, is the process of development. When we speak of one thing as being developed out of another, we imply that the parts next to the germ are the earliest to make their appearance, and the most constant. In the evolution of a tree out of a seed, there come first the stem and the radicle; afterwards the branches and divergent roots; and still later the branchlets and rootlets; the remotest parts being the

* Arch. and Hom., p. 93.

latest and most inconstant. If, then, a limb is developed out of a "diverging appendage" of the hæmal arch, the earliest and most constant bones should be the humerus and femur; next in order of time and constancy should come the coupled bones based upon these; while the terminal groups of bones should be the last to make their appearance, and the most liable to be absent. Yet, as Professor Owen himself shows, the actual mode of development is the very reverse of this. At p. 16 of the 'Archetype and Homologies,' he says:—

"The earlier stages in the development of all locomotive extremities are permanently retained or represented in the paired fins of fishes. First the essential part of the member, the hand or foot, appears: then the fore-arm or leg; both much shortened, flattened, and expanded, as in all fins and all embryonic rudiments of limbs: finally come the humeral and femoral segments; but this stage I have not found attained in any fish."

That is to say, alike in ascending through the vertebrata generally, and in tracing up the successive phases of a mammalian embryo, the last-developed and least-constant division of the limb is that basic one by which it articulates with the hæmal arch. It seems to us that, so far from proving his hypothesis, Professor Owen's own facts tend to show that limbs do not belong to the vertebræ at all; that they make their first appearance peripherally; that their development is centripetal; and that they become fixed to such parts of the vertebrate axis as the requirements of the case determine.

But now, ending here this digressive exposition and criticism, and granting the position that limbs "are developments of costal appendages," let us return to the question above put—Why are not these appendages included as elements of the "ideal typical vertebra?" It cannot be because of their comparative inconstancy; for judging from the illustrative figures, they seem to be as constant as the hæmal spine, which is one of the so-called autogenous elements, and in the diagram of the 'Archetypus,' the appendage is represented as attached to every vertebrate segment of the head and trunk, which the hæmal spine is not. It cannot be from their comparative unimportance; seeing that as potential limbs they are essential parts of nearly all the Vertebrata—much more obviously so than the diapophyses are. If, as Professor Owen argues, "the divine mind which planned the archetype also foreknew all its modifications;" and if, among these modifications, the development of limbs out of diverging appendages was one intended to characterize all the higher Vertebrata; then surely these diverging appendages must have been parts of the "ideal typical vertebra." Or, if the "ideal typical vertebra" is to be understood as a crystalline form in antagonism with the organizing principle; then why should not the appendage be included among its various offshoots? We do not ask this question because of its intrinsic importance. We ask it for the purpose of ascertaining Professor Owen's method of determining what are true vertebral constituents. He presents us with a diagram of the typical vertebra, in which are included certain bones, and from which are excluded certain others. If relative constancy is the criterion, then there arises the question—What degree of constancy

entitles a bone to be included? If relative importance is the criterion, there comes not only the question—What degree of importance suffices? but the further question—How is importance to be measured? If neither of these is the criterion, then what is it? And if there is no criterion, does it not follow that the selection is arbitrary?

This question serves here to introduce a much wider one:—Has the "ideal typical vertebra" any essential constituents at all? It might naturally be supposed that though some bones are so rarely developed as not to seem worth including, and though some that are included are very apt to be absent, yet that certain others are invariable: forming as it were the basis of the ideal type. Let us see whether the facts bear out this supposition. In his "summary of modifications of corporal vertebræ" (p. 96), Professor Owen says:—"The *hæmal spine* is much less constant as to its existence, and is subject to a much greater range of variety, when present, than its vertical homotype above, which completes the neural arch." Again he says:—"The *hæmapophyses*, as osseous elements of a vertebra, are less constant than the pleurapophyses." And again:—"The *pleurapophyses* are less constant elements than the neurapophyses." And again:—"Amongst air-breathing vertebrates the *pleurapophyses* of the trunk segments are present only in those species in which the septum of the heart's ventricle is complete and imperforate, and here they are exogenous and confined to the cervical and anterior thoracic vertebræ." And once more, both the *neurapophyses* and the *neural spine* "are absent under both histological conditions, at the end of the tail in most air-breathing vertebrates, where the segments are reduced to their central elements." That is to say, of all the peripheral elements of the "ideal typical vertebra," there is not one which is always present. It will be expected, however, that at any rate the centrum is constant: the bone which "forms the axis of the vertebral column, and commonly the central bond of union of the peripheral elements of the vertebra," (p. 97), is of course an invariable element. No: not even this is essential.

"The centrums do not pass beyond the primitive stage of the notochord (undivided column) in the existing lepidosiren, and they retained the like rudimental state in every fish whose remains have been found in strata earlier than the permian æra in Geology, though the number of vertebræ is frequently indicated in Devonian and Silurian ichthyolites by the fossilized neur- and hæmapophyses and their spines." (p. 96.)

Indeed, Professor Owen himself remarks that "the neurapophyses are more constant as osseous or cartilaginous elements of the vertebræ than the centrums" (p. 97). Thus, then, it appears that the several elements included in the "ideal typical vertebra" have various degrees of constancy, and that no one of them is essential. There is no one part of a vertebra which invariably answers to its exemplar in the pattern-group. How does this fact consist with the hypothesis? If the Creator saw fit to make the vertebrate skeleton out of a series of segments, all formed on essentially the same model—if, for the maintenance of the type, one of these bony segments is in many cases formed

out of a coalesced group of pieces, where, as Professor Owen argues, a single piece would have served as well or better; then we ought to find this typical repetition of parts uniformly manifested. Without any change of shape, it would obviously have been quite possible for every actual vertebra to have contained all the parts of the ideal one—rudimentally where they were not wanted. Even one of the terminal bones of a mammal's tail might have been formed out of the nine autogenous pieces, united by suture but admitting of identification. As, however, there is no such uniform typical repetition of parts, it seems to us that to account for the typical repetition which *does* occur by supposing the Creator to have fixed on a pattern vertebra, is to ascribe to Him the inconsistency of forming a plan and then abandoning it. If, on the other hand, Professor Owen means that the "ideal typical vertebra" is a crystalline form in antagonism with "the idea or organizing principle;" then we might fairly expect to find it most clearly displaying its crystalline character and its full complement of parts in those places where the organizing principle may be presumed to have "subdued" it to the smallest extent. Yet in the Vertebrata generally, and even in Professor Owen's *archetypus*, the vertebræ of the tail, which must be considered as, if anything, less under the influence of the organizing principle than those of the trunk, do not manifest the ideal form more completely. On the contrary, as we approach the end of the tail, the successive segments not only lose their remaining typical elements, but become as uncrystalline-looking as can be conceived.

Supposing, however, that the assumption of suppressed or undeveloped elements be granted—supposing it to be consistent with the hypothesis of an "ideal typical vertebra," that the constituent parts may severally be absent in greater or less number, sometimes leaving only a single bone to represent them all; may it not be that such parts as *are* present show their respective typical natures by some constant character: say their mode of ossification?

To this question some parts of the 'Archetype and Homologies' seem to reply, "Yes;" while others as clearly answer, "No." Criticising the opinions of Geoffroy St. Hilaire and Cuvier, who agreed in thinking that ossification from a separate centre was the test of a separate bone, and that thus there were as many elementary bones in the skeleton as there were centres of ossification, Professor Owen points out that, according to this test, the human femur, which is ossified from four centres, must be regarded as four bones; while the femur of birds and reptiles, which is ossified from a single centre, must be regarded as a single bone. On the other hand, he attaches weight to the fact that the skull of the human fœtus presents "the same ossific centres" as do those of the embryo kangaroo and the young bird.* And at p. 104 of the 'Homologies,' after giving a number of instances, he says:

"These and the like correspondences between the points of ossification of the human fœtal skeleton, and the separate bones of the adult skeletons of inferior animals, are pregnant with interest, and rank among the most striking illustrations of unity of plan in the vertebrate organization."

* Nature of Limbs, p. 40.

It is true that on the following page he seeks to explain this seeming contradiction by distinguishing

"between those centres of ossification that have homological relations, and those that have teleological ones; i. e., between the separate points of ossification of a human bone which typify vertebral elements, often permanently distinct bones in the lower animals; and the separate points which, without such signification, facilitate the progress of osteogeny, and have for their obvious final cause the well-being of the growing animal."

But if there are thus centres of ossification which have a homological meanings, and others that have not, there arises the question—How are they always to be distinguished? Evidently independent ossification ceases to be a homological test, if there are independent ossifications that have nothing to do with the homologies. Add to which, that there are cases where neither a homological nor a teleological meaning can be given. Among various modes of ossification of the centrum, Professor Owen points out that "the body of the human atlas is sometimes ossified from two, rarely from three, distinct centres placed side by side" (p. 89); while at p. 87 he says:—"In osseous fishes I find that the centrum is usually ossified from six points." It is clear that this mode of ossification has here no homological signification; and it would be difficult to give any teleological reason why the small centrum of a fish should have more centres of ossification than the large centrum of a mammal. The truth is, that as a criterion of the identity or individuality of a bone, mode of ossification is quite untrustworthy. Though, in his "ideal typical vertebra," Professor Owen delineates and classifies as separate "autogenous" elements, those parts which are "usually developed from distinct and independent centres;" and though by doing so he erects this characteristic into some sort of criterion; yet his own facts show it to be no criterion. The parapophyses are classed among the autogenous elements; yet they are autogenous in fishes alone, and in these only in the trunk vertebræ, while in all air-breathing vertebrates they are, when present at all, exogenous. The neurapophyses, again, "lose their primitive individuality by various kinds and degrees of confluence:" in the tails of the higher Vertebrata they, in common with the neural spine, become exogenous. Nay, even the centrum may lose its autogenous character. Describing how, in some batrachians, "the ossification of the centrum is completed by an extension of bone from the bases of the neurapophyses, which effects also the coalescence of these with the centrum," Professor Owen adds:—"In *Pelobates fuscus* and *Pelobates cultripes*, Müller found the entire centrum ossified from this source, without any independent points of ossification." (p. 88.) That is to say, the centrum is in these cases an exogenous process of the neurapophyses. We see, then, that these so-called typical elements of vertebræ have no constant developmental character by which they can be identified. Not only are they undistinguishable by any specific test from other bones not included as vertebral elements; not only do they fail to show their typical character by their constant presence; but when present, they exhibit no per-

sistent marks of individuality. The central element may be ossified from six, four, three, or two points; or it may have no separate point of ossification at all: and similarly with various of the peripheral elements. The whole group of bones forming the "ideal typical vertebra" may severally have their one or more ossific centres; or they may, as in a mammal's tail, lose their individualities in a single bone ossified from one or two points.

Another fact which seems very difficult to reconcile with the hypothesis of an "ideal typical vertebra," is the not infrequent presence of some of the typical elements in duplicate. Not only, as we have seen, may they severally be absent; but they may severally be present in greater number than they should be. When we see, in the ideal diagram, one centrum, two neurapophyses, two pleurapophyses, two hæmapophyses, one neural spine, and one hæmal spine, we naturally expect to find them always bearing to each other these numerical relations. Though we may not be greatly surprised by the absence of some of them, we are hardly prepared to find others multiplied. Yet such cases are common. Thus the neural spine "is double in the anterior vertebræ of some fishes" (p. 98). (And we may parenthetically remark that, joining this duality existing in the lower Vertebrata with the facts that in the higher Vertebrata the neural spine "may be developed from two lateral halves," and that where there is arrest of development, as in *spina bifida*, these lateral halves continue separate, Professor Owen might, had it suited him, have argued that the neural spine consists of two vertebral elements which usually coalesce: the evidence would have been much the same as that which leads him to class the parapophyses as separate elements from the centrum.) Again, in the abdominal region of extinct saurians, and in crocodiles, "the freely-suspended hæmapophyses are compounded of two or more overlapping bony pieces" (p. 100). Yet again, at p. 99, we read—"I have observed some of the expanded pleurapophyses in the great *Testudo elephantopus* ossified from two centres, and the resulting divisions continuing distinct, but united by suture." Once more "the neurapophyses, which do not advance beyond the cartilaginous stage in the sturgeon, consist in that fish of two distinct pieces of cartilage; and the anterior pleurapophyses also consist of two or more cartilages, set end on end" (p. 91). And elsewhere referring to this structure, he says:—

"Vegetative repetition of perivertebral parts not only manifests itself in the composite neurapophyses and pleurapophyses, but in a small accessory (interneural) cartilage, at the fore and back part of the base of the neurapophysis; and by a similar (interhæmal) one at the fore and back part of most of the parapophyses." (p. 87.)

Not only is it, however, that the neural and hæmal spines, the neurapophyses, the pleurapophyses, the hæmapophyses, may severally consist of two or more pieces; but the like is true even of the centrums.

"In *Heptanchus* (*Squalus cinereus*) the vertebral centres are feebly and vegetatively marked out by numerous slender rings of hard cartilage in the

notochordal capsule, the number of vertebræ being more definitively indicated by the neurapophyses and parapophyses. . . . In the piked dog-fish (*Acanthias*) and the spotted dog-fish (*Scyllium*) the vertebral centres coincide in number with the neural arches." (p. 87.)

Is it not strange that the pattern vertebra should be so little adhered to, that each of its single typical pieces may be transformed into two or three?

But there are still more startling departures from the alleged type. The numerical relations of the elements vary not only in this way, but in the opposite one: a given part may be present not only in greater number than it should be, but also in less. Thus in the tails of homocercal fishes, the centrums "are rendered by centripetal shortening and bony confluence fewer in number than the persistent, neural, and hæmal arches of that part"—that is, there is only a fraction of a centrum to each vertebra. Nay, even this is not the most heteroclite structure. Paradoxical as it may seem, there are cases in which the same vertebral element is, considered under different aspects, at once in excess and defect. Thus, speaking of the hæmal spine, Professor Owen says:—

"The horizontal extension of this vertebral element is sometimes accompanied by a median division, or in other words, it is ossified from two lateral centres; this is seen in the development of parts of the human sternum; the same vegetative character is constant in the broader thoracic hæmal spines of birds; though, sometimes, as e.g., in the struthionidæ, *ossification extends from the same lateral centre lengthwise—i.e., forwards and backwards, calcifying the connate cartilaginous homologues of halves of four or five hæmal spines, before these finally coalesce with their fellows at the median line.*" (p. 101.)

So that the sternum of the ostrich, which according to the hypothesis should, in its cartilaginous stage, have consisted of *four or five transverse* pieces, answering to the vertebral segments, and should have been ossified from four or five centres, one to each cartilaginous piece, shows not a trace of this structure; but instead, consists of *two longitudinal* pieces of cartilage, each ossified from one centre, and finally coalescing on the median line. These four or five hæmal spines have at the same time doubled their individualities transversely, and entirely lost them longitudinally!

There still remains to be considered the test of relative position. It might be contended that, spite of all the foregoing anomalies, if the typical parts of the vertebræ always stood towards each other in the same relations—always preserved the same connexions, something like a case would be made out. Doubtless, relative position is an important point; and it is one on which Professor Owen manifestly places great dependence. In his discussion of "moot cases of special homology," it is the general test to which he appeals. The typical natures of the "alisphenoid," the mastoid, the orbito-sphenoid, the prefrontal, the malar, the squamosal, &c., he determines almost wholly by reference to the adjacent nerve-perforations and the articulations with neighbouring bones (see pp. 19 to 72): the general form of the argument being—This bone is to be classed as such or such,

because it is connected thus and thus with these others, which are so and so. Moreover, by putting forth an "ideal typical vertebra," consisting of a number of elements standing towards each other in certain definite arrangement, this persistency of relative position is manifestly alleged. The essential attribute of this group of bones, considered as a typical group, is the constancy in the connexions of its parts: change the connexions, and the type is changed. But the constancy of relative position thus tacitly asserted, and appealed to as a conclusive test in "moot cases of special homology," is clearly negatived by Professor Owen's own facts. For instance, in the "ideal typical vertebra," the hæmal arch is represented as formed by the two hæmapophyses and the hæmal spine; but at p. 91 we are told that

> "The contracted hæmal arch in the caudal region of the body may be formed by different elements of the typical vertebra: e.g., by the parapophyses (fishes generally); by the pleurapophyses (lepidosiren); by both parapophyses and pleurapophyses (*Sudis*, *Lepidosteus*), and by hæmapophyses, shortened and directly articulated with the centrums (reptiles and mammals)."

Add to which that, in the thorax of reptiles, birds, and mammals, "the hæmapophyses are removed from the centrum, and are articulated to the distal ends of the pleurapophyses; the bony hoop being completed by the intercalation of the hæmal spine" (p. 82). So that there are *five* different ways in which the hæmal arch may be formed—four modes of attachment of the parts different from that shown in the typical diagram! Nor is this all. The pleurapophyses "may be quite detached from their proper segment, and suspended to the hæmal arch of another vertebra;" as we have already seen, the entire hæmal arch may be detached and removed to a distance, sometimes reaching the length of twenty-seven vertebræ; and, even more remarkable, the ventral fins of some fishes, which theoretically belong to the pelvic arch, are so much advanced forward as to be articulated to the scapular arch—"the ischium elongating to join the coracoid." With these admissions it seems to us that relative position and connexions cannot be appealed to as tests of homology, nor as evidence of any original type of vertebra.

In no class of facts, then, do we find a good foundation for the hypothesis of an "ideal typical vertebra." There is no one conceivable attribute of this archetypal form which is habitually realized by actual vertebræ. The alleged group of true vertebral elements is not distinguished in any specified way from bones not included in it. Its members have various degrees of inconstancy; are rarely all present together; and no one of them is essential. They are severally developed in no uniform way; each of them may arise either out of a separate piece of cartilage or out of a piece continuous with that of some other element; and each may be ossified from many independent points, from one, or from none. Not only may their respective individualities be lost by absence or by confluence with others; but they may be doubled, or tripled, or halved, or may be multiplied in one direction and lost in another. The entire group of typical elements may coalesce into one simple bone representing the whole vertebra: and even, as in the

terminal piece of a bird's tail, half-a-dozen vertebræ, with all their many elements, may become entirely lost in a single mass. Lastly, the respective elements, when present, have no fixity of relative position: sundry of them are found articulated to various others than those with which they are typically connected; they are frequently displaced and attached to neighbouring vertebræ; and they are even removed to quite remote parts of the skeleton. It seems to us that if this want of congruity with the facts does not disprove the hypothesis, no such hypothesis admits of disproof.

Unsatisfactory as is the evidence in the case of the trunk and tail vertebræ, to which we have hitherto confined ourselves, it is far worse in the case of the alleged cranial vertebræ. The mere fact that those who have contended for the vertebrate structure of the skull, have differed so astonishingly in their special interpretations of it, is enough to warrant great doubt as to the general truth of their theory. From Professor Owen's history of the doctrine of general homology, we gather that Duméril wrote upon "la tête considérée comme *une* vertèbre;" that Kielmeyer, "instead of calling the skull a vertebra, said such vertebra might be called a skull;" that Oken recognised in the skull *three* vertebræ and a rudiment; that Professor Owen himself makes out *four* vertebræ; that Goethe's idea, adopted and developed by Carus, was, that the skull is composed of *six* vertebræ; and that Geoffroy St.-Hilaire divided it into *seven*. Does not the fact that different comparative anatomists have arranged the same group of bones into one, three, four, six, and seven vertebral segments, go far to show that the mode of determination is arbitrary, and the conclusions arrived at unworthy of confidence? May we not properly entertain great doubts as to any one scheme being more valid than the others? And if out of these conflicting schemes we are asked to accept one, ought we not to accept it only on the production of some thoroughly conclusive proof—some rigorous test showing irrefragably that the others must be wrong and this alone right. Evidently where such contradictory opinions have been formed by so many competent judges, we ought, before deciding in favour of one of them, to demand a clearness of demonstration much exceeding that required in any ordinary case. Let us see whether Professor Owen supplies us with any such clearness of demonstration.

To bring the first or occipital segment of the skull into correspondence with the "ideal typical vertebra," Professor Owen argues, in the case of the fish, that the parapophyses are *displaced* and wedged between the neurapophyses and the neural spine—removed from the hæmal arch and built into the upper part of the neural arch. Further, he considers that the pleurapophyses are *teleologically compound*. And then, in all the higher vertebrata, he alleges that the hæmal arch is *separated* from its centrum, taken to a distance, and transformed into the scapular arch. Add to which, he says that in mammals the displaced parapophyses are mere processes of the neurapophyses (p. 133): these vertebral elements typically belonging to the lower part of the

centrum, and in nearly all cases confluent with it, are not only removed to the far end of elements placed above the centrum, but have become exogenous parts of them!

Conformity of the second or parietal segment of the cranium with the pattern vertebra, is produced thus:—The petrosals are *excluded* as being partially ossified sense-capsules, not forming parts of the true vertebral system, but belonging to the "splanchno-skeleton." A centrum is *artificially* obtained by sawing in two the bone which serves in common as centrum to this and the preceding segment; and as it is admitted that in fishes these two hypothetical centrums are not simply coalescent, but connate, it follows that this bisection is unwarranted, save for convenience. Next, a similar *arbitrary bisection* is made of certain elements of the hæmal arches. And then, "the principle of *vegetative repetition* is still more manifest in this arch than in the occipital one:" each pleurapophysis is double; each hæmapophysis is double; and the hæmal spine consists of six pieces!

The interpretation of the third and fourth segments being of the same general character, need not be detailed. The only point calling for remark being, that in addition to these various modes of getting over anomalies above instanced, we find certain bones referred to the *dermo-skeleton.*

Now it seems to us, that even supposing no antagonist interpretations had been given, an hypothesis reconcilable with the facts only by the aid of so many questionable devices, could not be considered satisfactory; and that when, as in this case, various comparative anatomists have contended for other interpretations, the character of this one is certainly not of a kind to warrant the rejection of the others in its favour, but rather of a kind to make us doubt the possibility of all such interpretations. The question which naturally arises is, whether by proceeding after this fashion, groups of bones might not be arranged into endless typical forms. If, when a given element was not in its place, we were at liberty to consider it as *suppressed*, or *connate* with some neighbouring element, or *removed* to some more or less distant position;—if, on finding a bone in excess, we might consider it now as part of the *dermo-skeleton*, now as part of the *splanchno-skeleton*, now as *transplanted* from its typical position, now as resulting from *vegetative repetition*, and now as a bone *teleologically compound* (for these last two are intrinsically different, though often used by Professor Owen as equivalents);—if, in other cases, a bone might be regarded as *spurious* (p. 91), or again as having *usurped* the place of another;—if, we say, these various liberties were allowed us, we should not despair of reconciling the facts with various diagrammatic types besides that adopted by Professor Owen.

When, years ago, we attended a course of Professor Owen's lectures on Comparative Osteology, beginning though we did in the attitude of discipleship, our scepticism grew as we listened, and reached its climax when we came to the skull: the reduction of which to the vertebrate structure, reminded us very much of the interpretation of prophecy. The recent delivery at the Royal Society of the Croonian

Lecture, in which Professor Huxley, confirming the statements of several German anatomists, has shown that the facts of embryology do not countenance Professor Owen's views respecting the formation of the cranium, has induced us to reconsider the vertebral theory as a whole. Closer examination of Professor Owen's doctrines, as set forth in his works, has certainly not removed the scepticism generated by his lectures: on the contrary, that scepticism has deepened into disbelief. And we venture to think that the evidence above cited shows this disbelief to be warranted.

There remains the question—What general views are we to take respecting the vertebrate structure? If the hypothesis of an "ideal typical vertebra" is not justified by the facts, how are we to understand that degree of similarity which most vertebræ display?

We believe the explanation is not far to seek. All that our space will here allow, is a brief indication of what seems to us the natural view of the matter.

Professor Owen, in common with other comparative anatomists, regards the divergences of individual vertebræ from the average form, as due to adaptive modifications. If here one vertebral element is largely developed, while elsewhere it is small—if now the form, now the position, now the degree of coalescence, of a given part varies; it is that the local requirements have involved this change. The entire teaching of comparative osteology implies that differences in the conditions of the respective vertebræ necessitate differences in their structures.

Now, it seems to us that the first step towards a right conception of the phenomena, is to recognise this general law in its converse application. If vertebræ are unlike in proportion to the unlikeness of their circumstances, then, by implication, they will be like in proportion to the likeness of their circumstances. While successive segments of the same skeleton, and of different skeletons, are each in some respects more or less differently acted on by incident forces, and are therefore required to be more or less different; they are each, in other respects, similarly acted on by incident forces, and are therefore required to be more or less similar. It is impossible to deny that if differences in the mechanical functions of the vertebræ involve differences in their forms, then community in their mechanical functions must involve community in their forms. And as we know that throughout the vertebrata generally, and in each vertebrate animal, the vertebræ, amid all their varying circumstances, *have* a certain community of function, it follows necessarily that they will have a certain general resemblance—there will recur that average shape which has suggested the notion of a pattern vertebra.

A glance at the facts at once shows their harmony with this conclusion. In an eel or a snake, where the bodily actions are such as to involve great homogeneity in the mechanical conditions of the vertebræ, the series of them is comparatively homogeneous. On the contrary, in a mammal or a bird, where there is considerable heterogeneity in their circumstances, their similarity is no longer so great.

And if, instead of comparing the vertebral columns of different animals, we compare the successive vertebræ of any one animal, we recognise the same law. In the segments of an individual spine, where is there the greatest divergence from the common mechanical conditions? and where may we therefore expect to find the widest departure from the average form? Clearly at the two extremities. And accordingly it is at the two extremities that the ordinary structure is lost.

Still clearer becomes the truth of this view, when we consider the genesis of the vertebral column as displayed throughout the ascending grades of the vertebrata. In its first embryonic stage, the spine is an undivided column of flexible substance. In the early fishes, while some of the peripheral elements of the vertebræ were marked out, the central axis was still a continuous unossified cord. And thus we have good reason for thinking, that in the primitive vertebrate animal, as in the existing *Amphioxus*, the notochord was persistent. The production of a higher, more powerful, more active creature of the same type, by whatever method it is conceived to have taken place, involved a change in the notochordal structure. Greater muscular endowments presupposed a firmer internal fulcrum—a less yielding central axis. On the other hand, for the central axis to have become firmer while remaining continuous, would have entailed a stiffness incompatible with the creature's movements. Hence, increasing density of the central axis necessarily went hand in hand with its segmentation: for strength, ossification was required; for flexibility, division into parts. The production of vertebræ resulting thus, there obviously would arise among them a general likeness, due to the similarity in their mechanical conditions, and more especially the muscular forces bearing on them. And then observe, lastly, that where, as in the head, the terminal position and the less space for development of muscles, entailed a smaller lateral oscillation, the segmentation would naturally be less decided, less regular, and would be lost as we approached the front of the head.

But, it may be replied, this hypothesis does not explain all the facts. It does not tell us why a bone whose function in a given animal requires it to be solid, is formed not of a single piece, but by the coalescence of several pieces which in other creatures are separate: it does not account for the frequent manifestations of unity of plan in defiance of teleological requirements. This is quite true. But it is not true, as Professor Owen argues respecting such cases, that "if the principle of special adaptation fails to explain them, and we reject the idea that these correspondences are manifestations of some archetypal exemplar, on which it has pleased the Creator to frame certain of his living creatures, there remains only the alternative that the organic atoms have concurred fortuitously to produce such harmony." This is not the only alternative: there is another, which Professor Owen has overlooked. It is a perfectly tenable supposition that all higher vertebrate forms have arisen by *the superposing of adaptations upon adaptations.* Either of the two antagonist cosmogonies consists with

this supposition. If, on the one hand, we conceive species to have resulted from acts of special creation, then it is quite a fair assumption that to produce a higher vertebrate animal, the Creator did not begin afresh, but took a lower vertebrate animal, and so far modified its pre-existing parts as to fit them for the new requirements; in which case the original structure would show itself through the superposed modifications. If, on the other hand, we conceive species to have resulted by gradual differentiation under the influence of changed conditions, then it would manifestly follow that the higher heterogeneous forms would bear traces of the lower and more homogeneous forms from which they were evolved.

Not only, then, do we find that the hypothesis of an "ideal typical vertebra" is irreconcilable with the facts; but we see that the facts are interpretable without gratuitous assumptions. The average community of form which vertebræ display, is explicable as necessarily resulting from natural causes. And those typical similarities which are traceable under teleological modifications, would obviously exist if, throughout creation in general, there has gone on that continuous superposing of modifications upon modifications which is displayed in every unfolding organism.

Review IX.

Transactions of the Pathological Society of London; including the Reports of the Proceedings of its various Sessions from 1846–7 *till* 1856–7. Eight Volumes, 8vo.—*London.* Printed for the Society.

The study of pathology, for its own sake, commends itself to every thoughtful physician; and in the belief that union is strength, a Society devoted to the cultivation of this interesting branch of medical science could not fail to secure the active co-operation of a very numerous body of medical men, especially in the metropolis of Great Britain. Institutions for the special cultivation of pathology have now been established in most of the metropolitan and in many provincial towns of Great Britain, America, France, Germany, and Italy; but to Dublin, in this country, in 1830, must be assigned the merit of having been the first city in which a Pathological Society was organized. Encouraged by the success which appeared to attend the proceedings of such institutions, and invited by the peculiar interest which invests the topics discussed at the meetings of such societies, several medical men of London met together in the month of February, 1846, and agreed upon the issue of a circular to such members of the profession as were known to be more particularly interested in pathological studies. Having received ample encouragement to proceed in this praiseworthy undertaking, a provisional committee elaborated a plan for the organization of the Pathological Society of London, as it is now constituted and named. They invited the support and co-operation of the profession at large, not only in

London, but throughout the kingdom, in prosecuting the science of pathology in every possible way, and by all means that could increase and advance our knowledge regarding the nature of diseases. At the first meeting of the Society, held on the 20th October, 1846, there were enrolled *one hundred and six* members. It now numbers no fewer than *three hundred* ordinary and *nine* honorary members. Its popularity as a society, therefore, may fairly be considered to be increasing; and when we look at the list of those who have been its presidents and office bearers, and at the list of the Officers and Council elected at the General Meeting in January, 1857, and finally to the Members of the Society as a whole, we cannot fail to perceive names the most distinguished in the ranks of our profession —of world-wide reputation—men, moreover, of the largest practice, the very busiest of doctors, who nevertheless find time to devote their attention to the highest pursuits of the science of medicine, and to work hand in hand with their younger brethren, often less favoured by the emoluments of an extensive practice.

The Pathological Society of London having been in active operation during the last ten years, it may not be considered premature if we institute some inquiry as to the results which have accrued to the science of medicine, or which are likely to accrue, from the operations of this Society, as exhibited in the volumes of their published Reports. In so doing, we may perhaps succeed in giving an indication of the progress of pathological science as set forth in the 'Transactions' before us. In them we ought to find expression given to the matured opinions which are held by the most advanced British school of pathology; and as the work of the Society mainly deals with the nature of disease as exhibited in the records of morbid anatomy, we expect to find the fullest details of all morbid appearances, embracing the chemistry and microscopy of morbid products, associated with lucid clinical histories of the cases which have furnished the morbid specimens exhibited to the Society, the results of the bedside investigation of disease. On a foundation such as this we might hope to see the science of pathology, in the widest acceptation of the term, elucidated and advanced by the active co-operation of the members of the Pathological Society of London.

Whatever opinion may be arrived at regarding the work done by this Society, and the general results so obtained, there can be no doubt, when we examine the records of the past ten years, that the zeal and assiduity of the members of the Society have not diminished, but rather increased. Year after year the volumes of the 'Transactions' have deservedly acquired an increasing reputation. In demonstrating the practical usefulness of this Society, there is one fact in its history which strikes us as highly significant—namely, that at the first meeting of the Society for the winter of 1855–56, on Tuesday, the 16th October, the permission of the Society was sought for by the printers of its 'Transactions' to reprint and republish the early volumes of its Reports, then out of print. A permission was of course most willingly granted, and we quote the circumstance to show

the value in which the recorded works of the Society have been held.*

Those only who have prepared and arranged pathological records can appreciate the labour implied in the preparation and publication of these volumes. The chief burden of this labour has been borne by Dr. Quain, and the Pathological Society cannot be too grateful to him for his exertions to hand down to posterity an accurate account of the work that has been done. The care and labour bestowed at an early period in selecting and arranging the material of the 'Transactions' had an immediate, and has also had a progressively beneficial effect upon the exertions of the Society. The very appearance of the records of the material brought before the Society stimulated the members to select their cases, and to give the descriptions and histories of them with more care than at first was bestowed upon them. The evidence of this will be obvious to any one who takes the trouble to compare the first volume of the 'Transactions' with the eighth, or even with the second. The chief improvement, in the first instance, was a decline in the number of specimens exhibited; while a fuller history was given of the cases during life, and the general nature of the communications assumed a much more important character.

Nevertheless there is still abundant evidence in the volumes before us that great inequality has prevailed in the description of specimens brought forward at the meetings. While some descriptions and histories are given with the greatest care, other specimens are described in the most careless manner possible. All are brought before the Society with the intention, doubtless, of throwing some light on the nature of disease; but of what value to science is a record such as we now quote, where five lines serve to convey the information vouchsafed? "This specimen was removed from ———, aged ——, that the disease was noticed —— or —— years before the operation, and seemingly commenced in ——. The patient made a favourable recovery, and has remained well since." Let us not, however, reflect upon the surgeons only, but quote another instance which ought to illustrate one of the most important topics in pathology—namely, typhus and typhoid fever. We give the following example, heading and all, just as it appears in the Reports:

"*Typhoid Disease of the Intestines.*

"The patient, a man, aged twenty-two, had fallen into the river, and was admitted into hospital in a state of collapse, suffering from occasional severe rigors. He lingered nine days, and died comatose. The lungs were both congested, with lobular inflammation; the spleen was enlarged, soft, and black; and the liver soft.

"Prior to the accident, the patient had slept in the same room with two boys affected with fever."

Such examples are beyond the pale of comment. Not even a word is said in the latter about the intestines. It is greatly to be regretted

* Medical Times and Gazette.

that many similar specimens may be found in the 'Transactions' of this Society, and especially in the earlier volumes; and as we would much rather notice what is good, and say nothing about the bad, we shall once for all maintain that statements such as these, which we prefer not to particularize by references, never ought to have occupied the time of the Society, and far less found a place in their records.

As a whole, the eight volumes before us constitute a rich mine of pathological wealth. But every one knows that the contents of a mine imply a most multifarious mass of material, and, moreover, that the richness of a mine is very much enhanced or diminished in value by the mode in which its material is worked out, brought to the surface of the earth, and there arranged and prepared for sale. We desire to impress upon the Society that much has yet to be done before the volumes of its 'Transactions' can realize their full value to science. The wealth and richness of the material they contain is not yet worked out. A mass of pathological facts has been thrown out without regard to order, and no one yet knows the valuable material that is contained in these volumes. As they exist, however, and if the same plan is to be carried out in the publication of all future volumes recording similar facts, it must be obvious to every thinking man that facts so recorded and published must rapidly pass into the state of useless lumber. Facts, considered merely as facts, every one knows to be most worthless; and while we have many proverbs which tell us that "facts are facts all the world over," that "facts are stubborn things," and so on, yet in order that facts may become of value, they must be connected in some way with those of a similar kind, and with things already known. The relation of facts to the existing state of science must be established, and for this purpose they must be collected and arranged so that their relation with things already known may be readily seen and appreciated. The individual facts must be brought to bear upon each other—they must be compared and analysed, so that the expression of some general deduction may be brought about.

The material contained in the volumes before us may be said to consist of cases containing some account of diseases and dissections, collected together and published without any natural pathological connexion. The arrangement of the material is of the most primitive order, being that into which the different organs of the body may be arranged anatomically into systems. Baillie's 'Morbid Anatomy,' contains a much more comprehensive classification of its contents, compared with medical knowledge at the time at which it was written. The object of the Society whose 'Transactions' are now before us, as stated in its bye-laws, is the cultivation and promotion of pathology by the exhibition and description of specimens, drawings, microscopic preparations, casts and models of morbid parts; and the 'Transactions,' as published, are merely records of facts in morbid anatomy. No one now can question the value of morbid anatomy as a chief corner-stone in the foundation of a rational pathology. Morbid anatomy, as illustrating the nature of disease, has gradually grown out of those facts

which exist in the accumulated experience and observation of ages. As a science, however, many of its details are of modern origin, and not a few of them are developed for the first time in the Transactions of Pathological Societies. Histology, for example, as illustrating pathology, is of modern origin; it is but yet in process of development, and its advantages, as illustrating the nature of diseased states and morbid processes, are largely illustrated in the reports before us. The value of *morbid anatomy* and *histology* as the basis of rational pathology was early recognised by physicians; and the diligence with which pathologists of the seventeenth, eighteenth, and nineteenth centuries have laboured to obtain an insight into the nature of diseases, as expressed through the vestiges left by the existence of morbid action in a part, has undoubtedly resulted in many an elaborate but often confused collection of such facts. In its relation to the progress of medicine and to pathology, the science of morbid anatomy ought to be a history or a living record, whose pages must be ever open to receive the observations which are constantly being made by those engaged in pathological pursuits, and a record from which one may ascertain at any time the conditions under which morbid changes or new formations in the body have taken place. The pages of this record undoubtedly show that in many points the science which elucidates the nature of diseases is in a transition state; and the position of the science of medicine itself, so far as it deals with pathology, must result from a re-arrangement of the innumerable details which the records of morbid anatomy and histology can disclose and unfold. Many very vigorous efforts have been made, in former and especially of late years, to arrange and classify such knowledge. Bonnetus, Morgagni, Sandifort, Baillie, Laennec, Cruveilhier, Craigie, Alison, Andral, Rokitansky, Hasse, Wedl, Jones, Sieveking, and Virchow, have done their utmost to extricate systems of pathological science from the immense mass of matter existing in varied forms and places. During the past thirty years especially, the science of pathology has been prosecuted with greatly increasing ardour. The simple observation and collection of pathological facts has engrossed the attention of numerous observers in Great Britain, France, Germany, Sweden, Russia, and America. Great and good results have doubtless accrued from this; but we think these results are not yet so great nor so good as they might have been, nor as we hope they may yet be. More accurate distinctions of diseases, before confounded, have undoubtedly been established. Information more full and accurate has been obtained regarding diseases that have hitherto remained very imperfectly known; and in almost all directions the boundaries of the science of medicine have been extended by the study of morbid anatomy. The horizon of pathology has thus become so enlarged as to be encompassed by no ordinary range of observation and of intellectual vision.

Following in the wake of physiology, chemistry, natural history, and the natural sciences generally, our knowledge regarding the nature of disease and of diseased processes may be said to have acquired a most firm and substantial foundation. When our investigations into the nature of

diseases are prosecuted in the spirit of such sciences, and with all the advantages of the mental training which belongs to the student who has diligently followed out such courses of study, the broad and comprehensive light of knowledge thus cast upon pathology guides our conclusions and guards our belief. When observation and experiment are followed as the sole methods of investigation, our opinions regarding the nature of disease inevitably become *one-sided;* and it is only by the inductive and deductive reasonings of such a comprehensive philosophy as that to which we refer that the influence of such *one-sided* views can be successfully counteracted. Nevertheless, the extensive horizon of pathology is still enveloped in a most unsatisfactory mist—nay, in many places and on many subjects we are encompassed by "a darkness that might be felt." The advantage to science which has resulted from the extensive accumulation of facts has been accompanied with great evils—evils which seem to be increasing to such a degree, that the more we look into the matter the more overwhelming do they appear to be.

To increase such evils, we do not hesitate to say that the 'Transactions' of the Pathological Society of London and of similar societies, AS PUBLISHED, are doing their utmost. The published records of facts in morbid anatomy are scattered in every direction. They have been sown broadcast over the world, so that it is no exaggeration to say that the usual period allotted to human life is quite inadequate to consult the volumes which contain such facts. The periodical literature of the day contains a very large amount, scattered and unclassified necessarily from the very mode of publication. Truly, then, we may say with Chomel, "that no man possesses all the pathological knowledge contained in the records of his art;" and one of our most industrious and learned pathologists, who has done much in his day, no less justly observes that, "so great is the accumulation of materials, yet so dispersed and multiplied, that the most intrepid diligence is disconcerted, and the most indefatigable perseverance is exhausted," by attempts to extricate and reduce to order the chaotic heap of material.

Now, then, let us consider to what does that system tend, the object of which is to exhibit specimens at Pathological Societies, and to record the facts then and there stated? Who is to classify and connect even the most important of these facts? How is their importance to be determined, if an arbitrary selection is made? Who shall reduce to order from their chaotic state, the numerous, the isolated, the unconnected facts recorded by the numerous individual members of Pathological Societies? How shall we reconcile the discordant statements? How shall anomalies be explained, and how shall the essential be distinguished from what is accidental? He will assuredly deserve well of science who will exhibit in a connected and systematic form the deductions and inferences which may flow from a careful analysis and comparison of even those facts which have been recorded and published by Pathological Societies.

In systematically publishing session after session, and year after year, collections of facts such as those recorded in the 'Transactions of the

Pathological Society of London,' one would expect to find that some gigantic system of pathology would in time be raised upon a broad and substantial foundation; and being thus gradually developed, something deserving the name of a science, illustrating the nature of diseases, would at length rear its head from the mass of brains, hearts, lungs, stomachs, intestines, bladders, kidneys, supra-renal capsules, uteri, bones and joints, which from time to time have been heaped upon the tables of the Pathological Society. Such a result, however, does not appear likely to follow. On the contrary, facts upon facts are being heaped up in the most incongruous manner, and published merely in that order in which the part affected can be arranged anatomically. For this reason many a fact appears to be merely the description of a curiosity in morbid anatomy—the record of a severe or of an extreme case. A thorough anatomical and physiological account of the origin and progress of a disease or morbid state, is not attempted to be developed by any arrangement or classification of similar cases in progressive series, or even in a series such as might result from bringing together like things to like. The progress of a morbid action through its several stages is thus left without illustration, when in numerous instances it might have been so illustrated, and the ultimate issue of the process demonstrated as a necessary result in abiding organic mischief, in recovery, in death or destruction of the part or the individual. No one can dispute the comparative merits of the two methods of recording the results of morbid anatomy; and no one has expressed better what we are attempting to enforce, than a highly distinguished physician of our own time. Dr. Latham thus writes—

"So far as morbid anatomy contemplates the last or latest results of disease, that are fixed, irremediable, and unalterable, its value is very small. But so far as morbid anatomy contemplates *disease in progress*, and scrutinizes and explains its organic processes, its value is very great."

If we analyse the subject matter which is shown in the 'Transactions' to have engaged the attention of the Pathological Society of London, it may be observed that the nature of the researches in which they have been engaged may be referred to the following heads:—

First,—The most numerous specimens brought before the attention of the Society have reference merely to the *discovery of alterations in organs.*

Second,—A few have reference to the *means, modes,* or *methods* of research by which alterations or deviations of parts from the normal state are observed to have taken place, and to the nature of the morbid changes discovered. These are so few that we may single them out here, and mention them shortly as worthy of attention:—On the Weight of the Heart in Health and Disease, vi.,* 119; On the best Method of Dissecting the Ear for Pathological Investigation, iv., 257; Observations on the Structure of the Synovial Membrane in Articular Cartilage, ii., 101; On the Pathological Anatomy of the Glans Penis and Urethra, vi., 235; On the Ophthalmoscope, its Practical Uses in the Diagnosis of Diseases of the Eye, viii., 323 and 324; On the Mode of Ascer-

* The Roman numerals indicate the volume of the Society's Transactions; the Arabic numbers indicate the page.

taining the Specific Gravity of Brain Tissue, v., 31 ; On the Specific Gravity of a doubtful Tumour removed from the Sheath of the Flexor Tendons, viii., 379.

Third,—We have numerous cases also illustrative of alterations upon the functions of organs and the life of the body.

Fourth,—Cases there are again which dwell upon the relation of the morbid changes to the symptoms of disease, as exhibited by the history of the case during life.

Inquiries similar to these have been the subject of research and observation, and the main object of the study of morbid anatomists from the earliest periods. Such inquiries are the foundation of all our special pathology, and with reference to the diagnosis and prognosis of disease, of the very first importance.

Fifth,—Not a few specimens of disease have been brought under the attention of the Society which illustrate in a striking manner "*the affinities of diseases and of diseased processes.*" We would especially include all the cases referred to under the following headings:—1. Lesions resulting from the action of chemical poisons. 2. Lesions resulting from the action of disease or morbid poisons. 3. Lesions resulting from the action of enthetic or implanted morbid poisons. 4. Parasites, and lesions resulting from the presence of parasites. 5. Lesions resulting from the action of constitutional causes, such as prevail in rheumatism, gout, tuberculosis, carcinoma, and the like.

The minute tissues, and in some instances the chemical composition, of organs in health and disease, are here the chief material which has engaged the attention of the pathologist. Organs differing from each other are found to have exhibited great similarity of lesion, sometimes associated with the fact that they have certain tissues in common entering into their composition. The kind of observation which characterizes the researches into the effects of constitutional diseases illustrates topics the highest and perhaps the most abstruse in pathology. Investigations of this nature were first suggested by Bonn of Amsterdam, instituted afterwards in this country by Cullen, John Hunter, and Carmichael Smyth, were subsequently reduced by Bichat to a system which has immortalized his name, and have ultimately led to the greatest advances in histology which characterize the modern anatomy both of healthy and of diseased structures. All we know regarding the local existence of lesions, their mode of origin, their progress of development, the future course and extension of morbid changes, has been traced and learned by this kind of observation, and thus some knowledge regarding the affinities of disease and of diseased processes has been acquired through the aid of morbid anatomy.

Sixth,—Observation sometimes takes another direction as recorded in the 'Transactions' before us—namely, that by which *textural changes*, recognised by certain essential characters, are found to have been common to a great variety of parts—e.g., exudations, the products of inflammation generally; alterations by degeneration of tissue, as shown by changes in its consistence and specific weight of parts; hypertrophy, as shown by increase in absolute size and weight;

atrophy of parts; peculiar deposits taking the place of the normal substance, and so on. Such mode of research connects very intimately local conditions with the constitutional origin of diseases, and has been largely associated with the observations on those sets of specimens to which we have just referred as illustrating lesions of constitutional origin and development. This mode of research was originally instituted by Abernethy, Bayle, Laennec, Andral, Cruveilhier, and Carswell.

Seventh,—A large amount of time and space has been judiciously devoted to investigations regarding the origin of malformations, the transposition and degeneration of tissue viewed in relation to their analogy with those laws which regulate the processes of organization throughout the animal kingdom; and traced from the earliest periods of embryonic existence through the various phases of development through which they reach their more or less perfect states. The Society is largely indebted to Dr. Peacock for information of this kind.

Eighth,—We observe and desire to call attention specially to the fact that a very great number of specimens, and reports on them, can only be classified under the heading of "*undetermined*" or "*peculiar*" *forms of disease.* These are chiefly to be noticed under the topics of *carcinoma, tumours, diseases of the kidney, uterus, and degenerations of tissue generally.* This is no doubt due to the varied opinions entertained by different pathologists as to the nature of many of the morbid changes found associated with such diseases; but we are also glad to observe, that three or four men associating themselves in the examination of particular points on which the difference of opinion exists, and finding that the variance is connected with observation merely, they are able in many instances to clear up the mystery, by observing the subject from different points of view. Thus we have most valuable joint reports on morbid specimens, which form a most interesting and instructive feature in the later volumes of these 'Transactions.' We would recommend to the Society a still more extended organization of the Members of the Society into sub-committees, to carry out specific investigations, and to publish their results as joint reports.

Time and space will not permit us to do anything like justice to the volumes before us; and when we find such a body of eminent medical men uniting themselves in this way, to advance by accurate and combined observation the science of disease, such research and observation ought invariably to challenge observation and inquiry. Combined with the valuable records of a contemporary Society—namely, the Royal Medico-Chirurgical, the published Transactions of these two Societies ought to be the most valuable reference books of our profession. Such a mass of valuable matter do they contain, that we conscientiously think no one ought to write upon a medical subject without at the same time consulting the archives of these societies.

The following table will give a numerical idea of the work done in the Pathological Society during the different sessions since it first held its meetings in 1846:—

Classification adopted in the 'Transactions,' describing the Specimens which have been exhibited at the Meetings of the Pathological Society of London.

		Vol. i.—1846-8. 1st & 2nd Sessions.	Vol. ii.—1848-50. 3rd & 4th Sessions.	Vol. iii.—1850-2. 5th & 6th Sessions.	Vol. iv.—1852-3. 7th Session.	Vol. v.—1853-4. 8th Session.	Vol. vi.—1854-5. 9th Session.	Vol. vii.—1855-6. 10th Session.	Vol. viii.—1856-7. 11th Session.	Totals.
I.	Diseases, &c., of the Nervous System . . .	23	20	25	15	12	22	18	19	154
II.	„ „ Organs of Respiration.	12	10	11	5	4	13	11	19	85
III.	„ „ Organs of Circulation .	73	26*	46	25	22	20	30	34	276
IV.	„ „ Organs of Digestion .	68	37	46	27	30	35	25	40	308
V.	„ „ Genito-Urinary Organs	32	27	37	29	24	27	22	23	221
VI.	„ „ Osseous System . . .	36	60†	32	7	10	12	20	14	191
VII.	„ „ Organs of Special Sense	2	6	14	5	3	6	4	8	48
VIII.	„ „ Supra-renal Capsules .	...	...	...	...	...	...	...	9	9
IX.	Tumours, &c.	...	...	...	...	...	...	...	22	22
X.	Miscellaneous Specimens, including Skin, Entozoa, Malformation, &c. . . .	33	37‡	32§	8	9	24	25	11	179
XI.	Specimens from the Lower Animals. . .	...	...	...	...	9	...	...	2	11
	TOTALS	279	223	243	121	123	159	155	201	1504

Although we have made a classified analysis of every one of the 1504 records in these Transactions, the limits of a review will permit us to touch only upon a few topics. We have been especially attracted by the very valuable results which have been brought about by the use of the microscope in elucidating the nature of many morbid changes; and we would here especially notice the beautiful drawings which illustrate especially the last two volumes. In this respect the volumes have progressively improved.

We shall now shortly notice some of these results. In doing so we shall observe no special order; and space will not permit us, nor do we think it is desirable, to fill in any deficiency of information from other sources. We desire simply to exhibit to our readers a few of the facts recorded in the volumes before us.

We would notice especially the minute alterations or degenerations of tissue which are described throughout the eight volumes, commencing with—

Pulpy Thickening of the Synovial Membrane—Ulceration or "Shedding" of Cartilage.

In a case not very advanced in the first-mentioned of these morbid states, the synovial membrane was found connected with a soft greyish structure which formed prominent fringes, overlapping and encroaching considerably on the surface of the articular cartilage. The marginal zone of the cartilage, for a varying extent, was converted into a kind of fibrous tissue, and blended with the altered synovial membrane. More internally the cartilage was grooved on the surface, and overlapped by the fringe of the newly developed

* Of which two are undescribed.
† Of which three are undescribed.
‡ Of which four are undescribed.
§ Of which ten are undescribed.

structure. The fibrous tissue into which the cartilage was transformed was of an imperfect kind, *not* divided into distinct fibres, and *not* containing any of the natural cells of the cartilage, but strewed over with numerous oil drops and yellowish molecules. Some interesting changes were also seen taking place in the cartilage itself. At a certain distance from the margin where the change was proceeding, the cells of the cartilage were quite natural, and in the ordinary proportion to the basis substance. Nearer the margin they were enlarged, and contained young cells in their interior, while close to the margin itself they were prodigiously developed, crowded with young cells, and had encroached considerably on the intervening basis substance. One or two large groups projected from the margin into the fibrous tissue, and there were *débris* of several others strewed about the neighbourhood. The cells, formed by endogenous growth within the original cell, contained generally each a small oil drop, but were principally filled with a transparent matter; but the quantity of oil contained in them was so small, that fatty degeneration does not seem to be the essence of the morbid change. The intervening basis substance had a finely granular aspect, and was not perceptibly altered from its healthy state, save that it was greatly encroached upon and diminished by the absorbing action of the unnaturally developed cells. One of the enlarged cellular groups near the margin had a long diameter of $\frac{1}{112}$th of an inch, and a short diameter of $\frac{1}{107}$th of an inch.

The peculiar synovial structure, under whose influence these changes seem to have been wrought, was found to possess a low, but definite and significant, type of organization. It consisted principally of well-formed nuclei of the ordinary appearance, and granular matter, with which were mingled a few puriform and circular cells. These elements were contained in an exterior enveloping membrane, very thin, of whitish aspect, and nearly of homogeneous texture. There was scarcely any appearance of stroma fibres throughout the contained mass, except that there existed a good many large vesicles or loculi, from $\frac{1}{56}$th to $\frac{1}{37}$th of an inch in diameter, which had nearly homogeneous envelopes, and were filled with materials similar to that which surrounded them. Bloodvessels, with very delicate walls, ramified through the mass.

This description does not support the view usually entertained, that "pulpy thickening" is merely an exudation of lymph, which becomes organized into a kind of false membrane. It is not probable that any false membrane could at once assume such a form and arrangement. Dr. Handfield Jones is therefore led to conclude that "pulpy thickening" of the synovial membrane is a "special growth," and not a product of a chronic inflammatory process. From the observation of Mr. Key and Professor Goodsir regarding ulceration of articular cartilage, there is some reason to believe that "pulpy thickening" is *a variety in the form of ulceration of cartilage;* and that the thickening is due to the higher development of the vascular fringes of the same tissue, by means of which Mr. Key describes the erosion and removal of the cartilage to be effected; while in the cartilage itself the same

changes are observed to be going on as were first described by Mr. John Goodsir. The structural condition of the synovial membrane thus altered is that which may be considered the best adapted for absorbing into its substance the *débris* of the cartilaginous tissue.*

The removal of cartilage from the articular extremities of bone is still a pathological problem; and we find in another part of the Society's Transactions,† that several different morbid processes are supposed to produce the result. The late Mr. Key considered it to be effected either through a change in the organization of the cartilage, or "through the agency of a structure probably evolved for that special purpose." Mr. John Goodsir considers it to be effected through the instrumentality of nucleated cells, which take up the *débris* of the removed cartilage during their own growth; these cells being the most important components of a gelatinous or false membrane which forms between the cartilage and the bone. MM. Mandl, Henle, and Mr. Birkett, are of opinion that it is effected through a suspension of the nutrient function of the cartilage, in consequence of a diseased state of the capillary vessels of the bone. Mr. Redfern, of Aberdeen, is of opinion that during the process the cells of the cartilage became enlarged, rounded, and filled with corpuscles in lieu of healthy cells. They then burst and discharge their contents, while the hyaline substance splits into bands and fibres, and thus becomes removed through abnormal nutrition, and not through either attrition or diseased secretions, as Rokitansky supposes.

Lastly, Mr. Gay looks upon the removal of cartilage as effected by two different processes, to one of which he applies the term "shedding" of cartilage. He does not consider that the cartilage undergoes any morbid change in consequence of disordered action originating in its own tissues.

It is very evident from these varied opinions that the process is not yet clearly understood, and that probably the variations are due in some measure to the descriptions being made from very various specimens at different stages in the process; and where sometimes the synovial degeneration, sometimes the cartilage substance, sometimes the bone, sometimes a new substance between the bone and its cartilage, or altered medullary membrane of the spongy osseous tissue near the cartilage, has appeared to be the chief agent in the process. The "fatty degeneration" of the cartilage itself, so well described by Mr. Adams,‡ is a not unfrequent prelude to the ulceration and removal of the cartilage substance. In those examples of this morbid state in which the disease is the least advanced, the solid contents or nuclei of the cartilage cells appear to have become converted into very minute spherules of oil, by which the cells are more or less completely filled. This may be regarded as the first indication of the disease.

In those parts of the cartilage in which the disease is in a more advanced stage, the spherules of oil before noticed, by their coalescence have formed globules of various sizes, by which many of the cells

* Dr. Handfield Jones, vol. ii. p. 106, November, 1848. † Vol. vi. p. 299.
‡ Vol. ii. p. 117, in South's Translation of Chelius' Surgery, 1846.

appear distended and enlarged, and no trace of cell-membrane being distinguishable, these cells present the appearance of irregularly-shapen cavities, excavated in the intercellular matrix, and filled with particles of oil. The intercellular matrix next undergoes the same morbid change as the contents of the cells and the cell walls. This change is indicated by the appearance of minute globules of oil, arranged either in irregular lines, and thus channelling the cartilage, or of globules congregated together in large cavities of various forms. When the disease is thus far advanced, and nearly all the cartilage is converted into oil, then those globules of this substance which were formed at the expense of the intercellular matrix cannot be distinguished from those produced by the degeneration of the cartilage cells and their contents. Where this condition of the articular cartilage is accompanied with disease of the synovial membrane, the part of the cartilage nearest to the membrane is much more affected than that next the bone, so that a vertical section of cartilage affected with this disease exhibits the fatty degeneration in all its stages. In some specimens even the bone itself connected with the affected cartilage, is in a state of fatty degeneration; but this state is much less obvious in the bone than in the cartilage. How far one or all of these processes under which we find the cartilages of a joint yielding to the ulcerative process, may be the concurrent result of a common cause thus exercising a morbid influence on the nutrition generally of the tissues composing a joint, is a question which remains for still more extended investigation.

That the destruction and ulceration of cartilage may commence by an inflammatory process occurring in the bone itself, we have an interesting example in the records of comparative pathology occasionally furnished to the Society by the veterinary surgeons. We cannot appreciate too highly the information which medical science may acquire from this source of pathological study. Unlike the treatment which must apply to man, when a horse's leg does not improve, it may not be amputated, but the beast is summarily slain, and thus an interesting view of a morbid joint is obtained almost at any time one may desire to examine it. The subject of the instructive illustration to which we would here refer was an omnibus horse, aged nine years. He was under medical treatment for lameness due to inflammation of the os carenæ, from October, 1853, to 6th February, 1854, when he was slain twelve days after the occurrence of sudden and severe lameness, the treatment adopted having failed to afford relief. The post-mortem examination, described by Mr. Simonds,

"Showed destruction of the cartilage on the outer half of the superior articular surface of the bone. A circular-shaped opening existed in this part, which not only penetrated the cartilage, but extended to the depth of nearly half an inch into the bone. The cartilage surrounding the opening had a thickened edge, was of a softer consistence than natural, and of a white colour. This thickened edge overhung to a slight extent the cavity beneath. In every other part of the joint the articular cartilage was healthy. A section of the bone showed that the disease had originally begun in the bone, and was doubtless of an inflammatory nature at its beginning. This inflammation led

gradually to the destruction of the cartilage, commencing on its attached or under surface. The cartilage being thus undermined, and thinned at the same time, it is probable that the increase of the symptoms, which took place twelve days before the horse was slain, depended on the giving way of the cartilage, which had up till that time bridged over the cavity in the bone." (v., p. 349.)

Alteration or Degeneration of the Minute Tissue of the Heart.

The pathology of sudden death, or of comparatively sudden death, associated with such a lesion, is illustrated by 29 cases, scattered throughout the eight volumes of these Transactions. In all of these cases the microscope has demonstrated various forms of alteration or degeneration, more or less advanced, in the minute fibrillæ of the heart's substance.

The recognition of fatty degeneration in the minute tissue of voluntary muscle led to the recognition of analogous changes in the tissue of the heart by Corvisart and Laennec. The subject has subsequently been greatly elucidated by Rokitansky, Paget, Ormerod, Quain, and Handfield Jones. We cannot here discuss the points on which great variety of opinion prevails amongst these observers regarding the nature and sequence of phenomena in the morbid process we are about to notice. On this head we refer the reader to the admirable papers of Dr. Handfield Jones on Fatty Degeneration, contained in the April and July numbers of this Journal in 1853. It is sufficient for our purpose, and fair to the Society, if we point out and arrange the facts contained in their records regarding a subject which still requires extended observation.

Having made a tabular analysis of these 29 cases, under the following headings, (1) age; (2) previous health and condition of the patient; (3) cardiac symptoms; (4) pulse; (5) cardiac sounds; (6) other symptoms; (7) mode of death; (8) post-mortem appearances; we shall give a condensed result of the inquiry in the order now written.

(1) As to age and social condition of the patients. Of the 29 cases, 16 are described as females, 4 of whom were of the higher rank of life, 1 was a prostitute, and the social positions of the others are undescribed. Of the males, of whom there are 13 cases described, 3 were of the higher ranks of life, the others are described merely as men, workmen, farmers or countrymen.

The average age of females whose death was connected with the degenerations of cardiac tissue about to be noticed, appears to have been forty-six years. The average age of males whose death was connected with these degenerations appears to have been fifty-two years. The youngest patient was a male infant six months old. The oldest male and the oldest female appear each to have been seventy-six years. The youngest female was ten years of age; and the youngest male, excluding the infant, was a boy eleven years of age.

In the female a tendency may be observed to the degeneration at a much earlier age than in the male. But between the years of fifty and eighty, in both sexes, the greatest number of cases have occurred.

(2) Of the previous general health and condition of the cases.—

Eight of them are described as delicate, short, thin, of spare make, weak, nervous, or reduced by previous illness, such as miscarriage, menorrhagia, hæmorrhoids. One of these cases was an infant six months old, of premature birth.

Eight other cases are described as strong, stout, fat, muscular, or hearty. Of these some are noted as sedentary, unaccustomed to active employment, but temperate in habit. Some had suffered from slight attacks, such as of bronchitis, "liver complaints," "spasmodic pains of the stomach." Although stout and well grown, others are described as being at the same time anæmic and pale.

A third class of cases had evidently suffered from previous attacks of acute disease, but appeared to have recovered from them to some extent. Of these, two had been in India; and of these two, one had since suffered from occasional headache with several apoplectic seizures after fifty-five years of age. The other had been thirty years in India, where he had been exposed to malaria, with frequent attacks of intermittent fever. One boy (aged ten) had fever at the age of four years, afterwards chorea, and subsequently scarlatina and rheumatism five months before death. A female of dissolute habits had suffered from rheumatism; and a farmer from extravasation of blood in the arachnoid cavity three years before death. Two patients are described as having had much mental anxiety and domestic distress, inducing great irritability and nervousness. Five cases are described as having to all appearance enjoyed excellent health.

(3) *Cardiac symptoms* occurred sometimes suddenly after exposure to cold, with pain in the left side, shortness of breath, and palpitation; and in cases associated with angina pectoris the pain was sometimes excessive, shooting down the left side and arm, especially after any excitement. In other cases, again, the cardiac symptoms merely consisted of a dull pain in the region of the heart or ensiform cartilage before death, associated perhaps with dyspnœa. In some of the cases, on the other hand, the angina pectoris continued for many (five) years before death. In some more obscure cases, again, the cardiac affection betrayed itself by cough, dyspnœa, and general debility, a sense of oppression at the chest, and desire to draw a deep breath; and in some cases especially the breathing was embarrassed and aggravated by exertion. In 17 out of the 29 cases death had been so sudden and unexpected, that cardiac symptoms had not been observed, although in one of these cases cardiac disease was suspected by the medical attendant to be present.

(4) *The condition of the pulse*, after the cessation of any acute attack of disease, has been recorded in 12 out of the 29 cases. It is noted as "irregular and unequal;" as "feeble, rapid, and irregular;" as "feeble and intermitting, or occasionally so every eight or ten beats;" as "large, jerking, and compressible—one hundred and six in a minute, afterwards one hundred, but small, jerking, and regular to the last;" as "small and feeble, but no irregularity or intermission." In one case the pulse was observed for two years and a half previous to death to be irregular and uncertain in its action, a condition

which disappeared during an attack of bronchitis, but re-appeared as the patient recovered from the bronchitic attack, varying from seventy to eighty in a minute. In other cases where its condition had been long noted, it is stated to have been weak, irregular, and intermitting, numbering at first eighty, becoming weaker, oftener intermitting, and more slow (sixty to seventy) shortly before death, or small and feeble. The slowest pulse is recorded as fifty-five. In one case of death under the influence of chloroform the pulse at first was ninety-four, regular, and of average firmness; subsequently, under its influence, it was accelerated, and suddenly began to get smaller, weaker, and imperceptible.

(5) *The sounds of the heart* have in 9 cases been observed to be modified. They are described in 1 case as "a loud bellows-murmur all over the chest," and in this instance there was complete destruction of the mitral valve by rupture. In another case there was extensive dulness over the heart, and a loud bellows-murmur with the first sound. In this case, also, the edges of the mitral valve were fringed with bead-like vegetations. In a third case a diastolic bruit existed in the region of the aortic valves, the heart's action ultimately becoming tumultuous and the sounds obscured, attended with a rolling action four days before death. In this case a band of lymph was found extending across the aortic orifice, with irregular vegetation over the sigmoid valves, while at the opening of the coronary artery a false aneurism opened into the muscular substance of the left ventricle. In a fourth the systole was attended with a loud and prolonged bruit, loudest over the mitral valve, followed by a distinct natural second sound, to which succeeded a remarkably prolonged interval of rest, and the rhythm was frequently irregular. Associated with these sounds was some thickening of the aortic valves, and especially a peculiar degeneration of fibrinous matter towards the base of the heart. In a fifth case the area of the precordial dulness was less than natural, and there was a systolic endocardial grating murmur, with feeble impulse. In this case the mitral valve was also thickened.

In four only of the cases do we find the cardiac sounds so modified, while the valves are so unaltered, as to leave no doubt that the heart's sounds so modified were probably due to the morbid condition of its muscular tissue. They were observed in the following sixth, seventh, eighth, and ninth cases, in the first of which the impulse and sounds of the heart were noted to have been feeble six weeks before death. In the seventh case the impulse is noted to have been feeble and of limited extent, the second sound being very indistinct, and there was no murmur. In the eighth case the heart's action was feeble and irregular, the sounds being weak, but no murmur. In a ninth the sounds were muffled, but no bruit existed.

(6) *The other symptoms* associated with these remarkable degenerations are of very various kinds. In some cases severe vomitings and faintings are recorded, or a peculiar sinking and sense of faintness, with profuse perspirations; paleness and lividity, with urgent dyspnœa for some weeks before the fatal result; sudden aggravation of

the cardiac symptoms, or of cough or general debility some days before death. In one case pneumonia, pericarditis, and pleurisy, co-existed. In another case there appeared to be evidence of great deterioration of the blood, with an eruption of purpuric spots on the skin; the gums swollen, soft, and spongy; the breath faint and disagreeable; the tongue pale, and almost clean. Thirteen days before death this patient became unsteady in gait, with impaired vision and vacant countenance, dilated and sluggish pupils, headache, and pains round the orbits, attended with spectral illusions, partial blindness, and hæmorrhage from the nose and bowels. Other than anæmia, there appeared to be no organic lesion of the brain; the vessels are described as healthy. The patient was a boy eleven years of age.*

The general symptoms in a fourth case consisted of slight dyspnœa with symptoms of acute rheumatism; and, four days before death, the dyspnœa became urgent. In a fifth case, sudden and severe attacks of shortness of breathing were observed on exertion for four years previous to death. These attacks were relieved by ether and ammonia, but were followed by faintness and exhaustion, with coldness and lividity of the face and extremities.† In a sixth case, biliousness was a troublesome attendant, with a sense of heaviness and oppression referred to the sternum. Two months before death there was general *malaise*, numbness of the fingers of the left hand, with tingling or uneasy sensations over the surface generally.‡

A seventh patient suffered from attacks of headache at intervals of a few months, associated with flashes of light before the eyes, and darkness of half the objects seen. Such attacks lasted about a week, attended with loss of speech, impairment of memory, and feebleness.

In an eighth case there was headache for a fortnight before death.

In a ninth case occasional attacks of diarrhœa are recorded, progressive loss of flesh, and an acute rheumatic attack.

In three cases attacks of angina pectoris are recorded. In one of these, six weeks before death, the attacks became so violent that the patient lived in constant dread of the pain, which was of a most excruciating kind, accompanied with a feeling of suffocation. Three days before death he cut his throat in one of these paroxysms, but the wound was superficial, and healed. A convulsive fit two hours before death continued ten minutes.

(7) *The mode of death* in these cases may be thus described:—In eleven of them death was absolutely *sudden* or *instantaneous*.

Of the other cases, *two* of the deaths were *comparatively sudden, in bed*, after having previously been up at the night-chair.

The subjects of three other cases were *found lying dead*—two in bed, and one on the top of a cab in the street.

In three cases a death struggle had existed of from three to five minutes' duration.

In one case four paroxysms of rigor, nausea, and "spasms of stomach," with small and contracted pulse, occurred at intervals within twenty-four hours, and at last suddenly carried off the patient.

* Dr. Bristowe, vol. v. p. 93. † Vol. iii. p. 82, Dr. Quain.
‡ Vol. iii. p. 264, Dr. Quain.

One patient died in a paroxysm of angina pectoris.

One died under the influence of chloroform.

The death of four may be regarded as gradual, by apnœa, associated with complex forms of disease, such as fever, rheumatism, endocarditis, pneumonia, pericarditis, pleurisy, two or more of these diseases co-existing in the same case.

One death was due to effusion into the corpus striatum; another to coma after a scalp wound, which did not appear to be due to the cephalic injury. Lastly, one died, gradually sinking, having suffered from hemiplegia, pericarditis, and general feebleness.

Of the cases of absolutely sudden or instantaneous death, nine died from rupture or laceration of some part of the texture of the heart. They died literally, not figuratively, of broken hearts—their cords were torn asunder, or the fibres of the substance of the heart were lacerated. In one there was sudden rupture of the mitral valve. In five the substance of the left ventricle was ruptured—two at the upper and posterior part (where, according to Rokitansky, the lesion is said to be rare), three across or along the anterior wall, and more or less close to the septum. In one there was rupture of the septum itself, penetrating the right ventricle. In another there was rupture of the walls of the ventricles on the posterior surface where they meet the septum. Lastly, there was in one case rupture of the anterior wall of the right ventricle, parallel to the septum.

(8) *Regarding the post-mortem appearances and the nature of the morbid change observed in the minute tissue of the heart*, considerable variety of description may be observed in the 'Transactions.' We would classify the results as follows:—

1. Cases in which there appears to be at first an interstitial deposit or exudation thickening the substance of the heart, and in which both the deposit and the sarcal particles within the sarcolemma undergo degeneration. Of this nature are cases described by Dr. Risdon Bennett (iii., 273), Dr. James Bird (iii., 276), and Dr. Ogle (iii., 281). In this class of cases there is evidence generally of previous or existing pericarditis, and sometimes rheumatism; the heart, moreover, is hypertrophic, increased in weight, and the fibrillæ present undoubted evidence of extreme brittleness. (Barlow, iv. 71; Bristowe, v. 84; Peacock and Bristowe, v. 102; Dr. Cholmeley, vi. 147; Bristowe, vi. 148.)

Dr. Bennett thus describes the condition of the heart in the case recorded by him:

"A section showed the walls of the ventricle to be increased in thickness, and encroached upon, and in parts replaced by, some adventitious product. Towards the base the muscular tissue gradually disappeared, at the expense of its outer part, so that at the distance of three quarters of an inch from the aortic valves, and from that point upwards, it was entirely wanting, and was replaced by a firm, dense, slightly translucent fibrous tissue, which extended some little way upwards on to the aorta, and downwards on the exterior of the muscle, gradually losing itself in the substance of the pericardium."

Several masses of deposit were also found in the muscular substance, of an irregular form, opaque, yellowish, and somewhat firm. "This

deposit was found to consist of cells, of fatty particles, and of degenerated muscular fibres. These cells were generally spherical, in size from $\frac{1}{1000}$th to $\frac{1}{2000}$th of an inch in diameter, having very thin walls, easily broken down, and containing a few granules." A few larger cells showed nuclei in their interior. The cells were generally placed in lines, so as to give the appearance of fibres. This deposit was of a doubtful nature as to its being malignant, tuberculous, concrete, pus, or fibrinous deposit, such as is sometimes seen in the spleen. This latter view is the one adopted by Dr. Bennett, Mr. Simon, and Dr. Quain. The deposit in the heart resembled a deposit figured by the late Sir Robert Carswell in the fourth fasciculus of his 'Morbid Anatomy,' plate 3.

In some cases the deposit appears to be so circumscribed by a boundary line, where blood-globules are abundantly present, that had the process of softening continued with surrounding vascular activity, "a circumscribed abscess must have been the result; and had absorption of the deposit taken place, a fibrous degeneration would have been found." (Dr. Quain, iii. p. 281.)

The simultaneous existence and progress of both forms of morbid process now being noticed is exemplified in a case described by Dr. Ogle (iii. 282).

In a case described by Dr. Bristowe, the heart weighed twenty ounces. The lesion, he writes, "appears to consist in the deposition of lymph among the muscular fibres, which lymph becoming converted into fibroid tissue, compresses, and ultimately replaces the muscular substance." (vi. 150.)

2. Cases in which there is a very moderate amount of degeneration of tissue, or alteration in the bulk of the heart; but in which the functional disturbance is serious in the extreme, and the case rapidly fatal in its issue (ii. 186, Dr. Williams).

3. Cases in which the whole heart does not seem involved, but in which, amongst apparently healthy tissue, a portion of substance had undergone degeneration—the evidence of degeneration consisting of "disappearance of the cross-markings," and "the fibres being filled with oleo-albumen, or fatty granules; and the coronary artery ossified or obstructed, going to the seat of degeneration." (Dr. Quain, ii. 188; iii. 262, 270, 273.)

4. Cases in which the death could not be ascribed alone to fatty degeneration of the heart, but where other lesions existed—such, for instance, as fatty degeneration of the cerebral arteries, resulting in death by apoplexy; or in cases in which apoplectic seizures occur (iii. 85)—a lesion we owe so much to Mr. Paget for elucidating; or in which, according to Dr. Quain, obstruction to the flow of blood from the head leads to congestion of the brain and hæmorrhage. The source of the obstruction, as he thinks, is due to the want of power in the right ventricle (on account of the degeneration) to maintain the circulation through the lungs—the blood therefore accumulating in the brain, causes death by apoplexy, for the most part meningeal (ii. 190).

5. There are cases in which degeneration of the fibre is not the only condition, but cases in which the fibre is encroached upon and rendered

powerless by fatty tissue intruding on and covering the fibres. Atrophy, wasting, or disappearance of the proper muscular tissue, seems to be one of the immediate results of this encroachment, terminating in a thinning of the muscular parietes of the organ (i. 192). Dr. Quain distinguishes this form as "fatty growth upon the substance of the heart." Along with this special atrophic result, some of the fibres that remain undergo the fatty degeneration also within their own sarcolemma. This homogeneous sheath seems in most cases to remain intact. The nuclei within disappear, or break up into streaks of oil dots. Thus true myoline ultimately comes to be replaced by minute opaque molecules (the nature of which has not been in all cases determined), or actually by small drops of oil.

6. There are cases of this degeneration which result in the "cardial apoplexy" described by Cruveilhier, where hæmorrhagic spots or extravasations, of various sizes, occur in the substance of the muscular tissue. The tissue is found in the state of fatty degeneration, and the coronary artery, leading to the degenerate tissue and hæmorrhagic spot, is ossified and narrowed (Dr. Quain, i. 192; ii. 190).

7. There are cases in which the pathology of the lesions whose evidence is traceable after death, is of a very complex kind—namely, cases in which the degeneration of the tissue of the heart has been associated with softening of the brain. The microscopic examination of the heart in such a case showed, that while portions taken from the septum of the ventricles appeared most healthy, the primitive fasciculi from three other parts

"Presented a chain of fat-like globules running down its middle for a short distance; and a multitude of oil globules floated in the field of the microscope, having previously, as it seemed, occupied the interstices of the primitive fasciculi. In portions taken from the right ventricle, which had more greasiness of tissue, a few fasciculi had a large accumulation of the fat-like globules in them—middle or narrow chains of them between the fibrillæ; while others had lost the transverse striæ, either altogether, or in a part of their length, and seemed to be formed of multitudes of minute shining granules, disposed for the most part in longitudinal lines, but of not quite equal size, and of larger diameter than the divisions of the natural fibrillæ. Lastly, portions taken from the sides of the rent in the left ventricle showed scarcely a primitive fasciculus which was not thus altered, and for the most part in a more advanced degree, the longitudinal arrangement of the shining particles being no longer apparent; while in many instances they were so large as to present more obviously the characters of fat globules. In all the specimens large fat globules floated free round the muscular fasciculi" (iii. 267).

"While all the parts in the centre of the brain appeared healthy, the arteries at the base appeared extensively diseased, and in an extreme degree, being opaque and rigid, and in parts much contracted in diameter. These changes are by far most advanced in the basilar artery, and the lateral branches of the circle of Willis. The parts of the brain supplied by these arteries alone presented appearances of disease. The crura-cerebri at the lower surface, to the depth of a line, were softened to a pulpy consistence. The left half of the pons Varolii was obviously flattened and contracted, and showed a considerable loss of medullary substance, replaced by a quantity of brownish cellular substance, in a very irregularly-shaped cavity, which extended downwards towards

the olivary body, and the anterior pyramid of the left half of the medulla oblongata (iii. 266, Dr. Baly).

The evidence from the history of this case is not sufficient to determine how far the lesions in the heart and brain commenced and advanced simultaneously or not. What appeared to be the primary attack was expressed by paralysis, referrible to the lesion in the pons Varolii; while increasing impairment of muscular power was referrible to the softening of the crura cerebri. The changes in the heart and brain were perfectly analogous and similar, the consequence of disease of the nutritive arteries.

Alteration of Tissue in the Liver.

Several interesting observations have been made by Dr. Handfield Jones on the condition which is called the "nutmeg liver."

The most common form in which this lesion is seen, is that which depends principally on partial congestion of the parenchyma of the lobules—"*the hepatic venous congestion of Mr. Kiernan in the second degree.*" There are also cases, however, in which the biliary congestion, as well as the venous congestion, is extremely marked; and when no other alteration can be perceived by the naked eye, yet in which the secreting structure is most gravely and extensively deteriorated. The form of the organ in this condition is scarcely at all altered: its capsule is smooth, free from false membrane or other traces of chronic inflammation. Its consistence, however, is greatly increased; it is much less fragile, and tears much less easily than natural. On the surface of a section, deep red patches of sanguine congestion are observed coalescing irregularly with each other, and leaving in their interspaces pale greyish or slightly yellow spots. Under the microscope, it is seen that the cells in the central parts of the lobules, which are the seat of congestion, are gorged with biliary matters, and appear as round or oval masses of dark yellow or reddish substance. They are much less numerous than in the healthy state, and no longer lie closely in contact. This condition of the secreting structure is coextensive with the congestion of blood. Beyond this, towards the exterior of the lobules, a very different condition is found to exist. Here the cells are no longer distinct from biliary engorgement. On the contrary, they are pale, stunted, starved, and often scarcely discernible, or they appear as mere *débris* in the midst of an amorpho-granular basis substance. In some cases the fissures are greatly enlarged; instead of having a diameter of $\frac{1}{333}$ of an inch or less, they measure as much as $\frac{1}{54}$th, or even $\frac{1}{44}$th of an inch, the increase in width taking place at the expense of the lobules, which thus become materially diminished. The investing membrane of the lobules is very greatly increased, and becomes much more condensed and more distinctly fibrous, appearing in some instances to be continuous with the amorpho-granular substance which infiltrates the pale, non-congested portion of the lobules. The cases where the fissures become thus greatly enlarged, Dr. Jones considers as examples of further stages of the degenerative process. The foramina existing at the interlobular spaces of the lines are no longer

apparent, the pressure of the newly-formed fibrous tissue having obliterated the corresponding branches of the portal vein.

The essential changes thus described appear to consist in the effusion of an unhealthy plasma, not only in the canals and fissures, but in the external part of the lobules, which becomes solid, and constitutes an amorpho-granular basis-substance, compressing the capillaries, and stunting the secreting cells. At the same time, the thickened fibrous tissue of the canals and fissures still further obstructs the arrival of blood in the lobules, which thus are at last only supplied by the reflux of blood into the ultimate twigs of the hepatic vein, and the capillaries which immediately surround them. Thus the marked congestion in the interior of the lobules is accounted for, and the peculiar biliary engorgement of the cells in this situation.

In two of the cases observed by Dr. H. Jones, there was contraction of the mitral orifice of the heart, by thickening of the valve and chordæ tendinæ, confirming the view that the lesion in the liver is a fibrous degeneration of the glandular structure, from an unhealthy state of the plasma supplied to it from the blood, and not an inflammatory action producing effusion of lymph. The degenerating process now described does not terminate in the contracted "hobnail" condition which is the result of cirrhosis. In such livers there is no considerable lesion of the secreting glandular cells. The change described cannot be seen or appreciated without the aid of microscopic examination (ii. 68).

The following remarkable alteration is described as having occurred in the liver and right kidney of a girl, eighteen years of age, labouring under amenorrhœa, cough, and palpitation, followed by heart disease, jaundice, diarrhœa, vomiting, erysipelas, and death. Amongst other lesions, the liver was of a deep yellow colour. The inter-lobular veins were highly congested; the lobules themselves not at all. The secreting structure was shown by the microscope to be greatly affected. On the margins of the lobules the cells still existed, and their nuclei were distinctly visible, though they were gorged with bile of a deep yellow tint. In by far the greater extent of the lobules, however, no cells could be discerned. A densely aggregated group of oil-globules formed a zone concentric to this marginal one, where the bile-laden cells still retained their forms. Within this there was nothing but coarse granular and amorphous matter, with a few groups of oil-globules. This degeneration occupied nearly one-half the extent of some lobules.

The kidney on the right side presented a large patch of a yellow colour, extending throughout the cortical substance, accurately bounded and separated from the surrounding healthy part by a broad line of a deep red colour. Sections of the altered portions showed that the secreting tubuli were principally affected, in some specimens being completely destroyed, numerous coarsely granular cells, the remains of their epithelium being alone visible. In the majority of sections the tubes still remained, but so bloated and opaque as to be evidently unfit for the discharge of their functions, resembling in many respects the diseased tubes seen in kidneys after scarlatina. Several of these tubes were seen to be invested with a basement membrane, in the

interior of which were dense masses of large coarse epithelial cells, which completely filled the cavity and obstructed the passage. The matrix was unaffected. No trace of fibrinous or other deposit could be found in the portions examined by Dr. Jones and Mr. Simon, though Dr. Bence Jones had found a considerable quantity of free oily matter in a specimen from the same kidney. It was a question of interest regarding these specimens noticed by Dr. Handfield Jones, as to the probability of such changes being due to a similar cause in both organs—namely, *a very acute form of degeneration, rapid in process, and unaccompanied by any traces or signs of inflammation* (i. 276).

A similar condition is thus described by Mr. Simon, who seems to take a different view of the origin of such deposits. The lesion is recognised in the dead body as an abrupt change of colour and consistence in circumscribed patches of the gland. The parts affected are quite bloodless, and vary in their shades of yellow, from the lightest tint to a deep orange colour. They are surrounded by a ring of bright vascularity, and in their general appearance resemble the so-called "fibrinous deposits" of the spleen. Their increase of consistence is very remarkable; and the appearance is due to the extreme gorging of the tubules with materials of secretion—a state similar, he remarks, to what he describes as subacute inflammation of the kidney—only it is of much greater intensity, and confines itself to patches of the gland. In such parts the tubes are crammed with secreted material, and are so compact and turgid that the passage of blood between them (the tubes) is interrupted, and the affected portion of the kidney becomes anæmiated. Hence the zone of arterial congestion at the limit of the obstructed portion (i. 282).

In illustration of these cases of minute alteration or degeneration of tissue, whether in the substances composing joints, in the tissue of such solid organs as the heart, the liver, the kidney, and the spleen, and even in the interesting cases of degeneration of the mucous tubes and glandular part of the stomach, so well described by Dr. Handfield Jones, we think that some valuable information might be got by applying the same method of research to the morbid parts as was done by Dr. Sankey in regard to the brain—namely, to observe, not only the absolute weight, but also the specific weight (see on this subject vol. v. p. 31, of the 'Transactions,' and the last number [2] of Dr. Lionel Beale's 'Archives of Medicine.')

Of some Peculiar Forms of Malignant Disease.

These were exhibited by Mr. Simon, and one is worthy of note as a case in which the secondary deposit presented all the general characters of tubercle in the liver: the original disease had been in the rectum. The secondary deposit in the liver seemed compressed and coherent, but not connected by fibre or blood-vessels. The tubercular character increased towards the centre of the mass, becoming merged at the circumference in the common features of cancer. It appeared that the malignant multiplication of germs in the liver must have been of immense rapidity, so that

the first formed elements had been thrust, by successive new formations, away from bloodvessels, into the centre of a mass nourished only at its circumference. Thus these central parts (all the elements of which were imperfect cells) had undergone atrophy and compression, "existing in the state of an excretion, and exhibiting characters analogous to those of the shrivelled cells in the centre of a follicular tumour."

This latter sentence has been dictated by the peculiar view entertained by Mr. Simon regarding the nature of cancer—namely, that a cancer is "substantially a new excretory organ;" that it is a growth which arises for the purpose of eliminating from the system an unhealthy matter which is generated within it. In the absence of any proof of this theory, we are much more inclined to adopt the view advocated by not a few, that in this case the cancer elements had become tuberculized; that the tuberculization was a local process by which the secondary cancer elements in the liver became metamorphosed into tubercle by endogenous development, atrophy, shrivelling, and desiccation of the textural elements composing the cancerous exudation (i. p. 127). The association or co-existence of tubercle with cancer is a very interesting topic of inquiry. It is further illustrated and discussed at some length in vols. iii. 254, iv. 35, 41, and in a note at page 102 of the same volume.

An interesting example of the probable propagation of cancer by continuity of surface, is also furnished by Mr. Simon. In a case of renal cancer, the prostate gland had been the seat of encephaloid disease, and the question suggested itself, how far this superficial cancer of the prostate was not due to the descent of cancer germs from the kidney, and their development in the mucous membrane of the prostate. The probability of this is enhanced by the fact, that in another case straggling patches of fungous growth were traced down the ureters. These did not include the submucous tissue, but were mere efflorescences of cancer on the epithelial surface. In both cases the renal disease was common encephaloid cancer (i. 128).

Medullary sarcoma of the kidney in a child from the seventh till eighteen months old, when it died: all the functions and secretions being apparently undisturbed, the diagnosis remained obscure, and disease of the kidney was not suspected. The two kidneys weighed conjointly five pounds, and had lost all resemblance to renal tissue or form of kidney. Microscopically the proper secreting tissue was present, but in a morbid state. Its cortical portion had undergone the most complete degeneration, and in the cases scarcely any of the uriniferous tubes had a natural appearance. Several were increased in size, and lined with numerous cells, in some degree retaining the normal arrangement. A still greater number of the tubes were atrophied, and contained irregularly-formed cells and granules, and were surrounded by morbid structures, consisting of a fibrous struma, ill defined, with nucleated cells, two or three times the size of blood discs, oral, spindle-shaped, caudate and doubly caudate. The disease appeared to commence by the infiltration of morbid matter, *external* to the proper secreting apparatus (Mr. Obré detailed the case, Dr. Quain examined the kidneys, 1st March, 1847).

On some Peculiar Forms of Disease.

A peculiar form of Bright's disease exhibited by Dr. C. J. B. Williams, and believed to be uncommon, in which the kidneys were large (eleven ounces each), cortical substance of a uniform pale buff colour, devoid of granular irregularities, and exhibiting under the microscope an accumulation of large granular cells, with an unusual predominance of oil globules. The predominance of fatty matter was considered sufficient to account for the non-contraction of the organs. The case was one during life attended by a peculiar ochreous diarrhœa, and where the urine became almost solid by heat. Dr. George Johnson examined the kidneys, and found that the opaque dark patches upon them consisted of convoluted urinary tubes, filled, distended, and in many places ruptured by an accumulation of globules in their interior. One hundred grains dried in a vapour, left a residue weighing 31½ grains, which digested in ether, left on evaporation $3\frac{2}{10}$ grains of fatty matter. Some of the tubes appeared quite healthy, others were shrunken and atrophied (i. 96, 10th Nov., 1846).

A boy, nineteen years of age, apparently in good health, four or five days before death had symptoms of entire suppression of urine, hæmorrhages from the nose and bowels, and coma, but no dropsy. The kidneys weighed eight ounces, were of a claret hue, and largely granulated. The tubes presented detached epithelium, but no oily or albuminous matter (i. 97, Mr. Busk, 2nd Nov., 1846).

Another form of granular kidney is described, in which those organs were large and white, the capsule stripping off with undue ease, and leaving a smooth surface, in some parts stellated with closely-aggregated white spots. These spots consisted of *interstitial* deposits, composed of fat. There were other large contracted spots of a deep purple colour, so that the recent kidney appeared like a piece of variegated marble. The pelvis of the organ was dilated, and the mouth of the renal artery was patulous. The renal vein was filled with a fibrinous coagulum, extending to its ultimate ramifications; regarding this we are told that "there was evidence of the inflammatory origin of this coagulum." The patient had been under the care of Dr. Addison in Guy's Hospital, and had on admission swollen legs and abdomen, puffy face, and dropsical conjunctiva, accompanied with much bronchial obstruction; the heart was apparently healthy; the urine of specific gravity 1·012, densely coagulable, about two pints and a half being passed in the twenty-four hours. About a month after admission a severe attack of rigors came on, with much febrile excitement and great dyspnœa. The amount of urine diminished to a pint in the twenty-four hours, was densely coagulable, and contained lithates in abundance. A second attack of rigors followed nine days afterwards, with aggravation of all the symptoms, and death followed by asphyxia. The lungs were emphysematous, with pleuritic adhesions on both sides; the bronchia and their divisions were also dilated; slight atheromatous deposits were observed in the aorta, but in the venous system generally no disease was seen (Dr. Barlow, 5th April, 1847, i. 98.)

Urinary Deposits.

A specimen of cystine exhibited by Dr. Garrod was passed by a policeman, aged thirty-three, with dyspepsia; tongue loaded with a white fur; thirst, lips dry and cracked, acid taste in the mouth, appetite good, except when nausea prevailed, which attended the passage of small calculi, which he passed from time to time; the bowels were regular. There was often a papular eruption about the face and neck; his perspiration was sometimes very sour; no disease in the heart, lungs, liver, or spleen. His father had occasionally passed red gravel.

Characters of the deposit.—Whitish appearance, refracting light; does not disappear by heat or acetic acid, but alkalies and the mineral acids both dissolve it. The crystals are in the form of six-sided laminæ, with a tendency to aggregate, not in the form of rosettes, but arranging themselves at right angles to each other.

The urine, apart from the deposit, has a pale yellow colour, an acid reaction, which soon disappears; the fluid becoming alkaline, with the formation of crystals of triple phosphate of ammonia and magnesia, which appear on the surface and edges of the fluid as a film; specific gravity 1·014 at 60° F. The quantity passed in twenty-four hours was fifty-four fluid ounces, and the quantity of uric acid in the same time about nine grains. The calculi passed were of pure cystine, crystallized on the surface like raw sugar.

1000 *parts of the Urine gave on analysis:*—

Water	969·009
Solid matter	10·993
Urea	10·125
Uric acid	0·375
Extracted matters, ammoniacal salts, &c.	13·059
Fixed salts	7·432
Soluble in water	5·995
Insoluble in water	1·437

It contained at the same time much more unoxidized sulphur than ordinary urine, most probably in the form of dissolved cystine.

A rare Form of Uterine Disease, associated with Mollities Ossium.

It occurred in a female who died from an acute attack of pneumonia, and from whose history, detailed by Dr. J. Hall Davis, no information is obtained as to the origin of the constitutional disease (mollities ossium).

The disease in the uterus consisted of *ulceration of the whole or chief part of its lining membrane*, under which the parietes of the organ became softened in structure, much as they do in pregnancy, and generally irregularly thinned in substance. The cavity was considerably dilated, and contained coagula, unhealthy fœtid pus, and portions of shreddy fibrin adhering with greater or less tenacity to the internal surface. The os uteri remained pervious, or was plugged by rough mucus. Cases of this form of disease were rare, had been mistaken for pregnancy, and they all had a tendency to a fatal termination. In connexion with the mollities ossium, it was interesting to note that the patient had lost eleven inches in height in the twelve

years preceding her death. She had been married sixteen years, at the age of twenty, and had three living children during the first four years and a half; she then had a dead child after a severe labour, and shortly before this pregnancy the commencement of the uterine disease is dated.

Specimens of the bones were examined by Dr. Sharpey, the late Mr. Dalrymple, and Mr. John Quekett, who all remarked that the Haversian canals and the bone cells were much larger than natural; that in parts near the edges of the bone forming the wall of the Haversian canals the canaliculi were but slightly visible, or had entirely disappeared; that the bone along these edges was extremely transparent; that numerous nucleated cells, blood discs, and fatty matter, occupied the cancellated structure and some of the Haversian canals.

Examined chemically by Dr. Garrod, 100 parts of the bone dried at 212° F., yielded:—

Phosphate of lime	16·40
Carbonate of lime, with phosphate of magnesia	4·88
Fatty matter	20·35
Gelatine, yielding matter	58·37

Healthy bone, according to Berzelius, yielding of—

Earthy matters	66·70
Animal matters	33·30*

Many very interesting topics we can now merely name, regretting that time and space prevent us doing more;—

The cases of myeloid tumours related in vol. viii., pp. 380, 346, 387 (Hutchinson, Bryant, and Cooper Forster), and the remarks on the nature of these tumours, vii. pp. 336 and 368, are well worthy of notice; also:—Dr. Wilks' cases of syphilitic (?) disease of the liver in the same volume, p. 240. Dr. Bristowe's cases of dysenteric ulceration in connexion with pneumonia, vol. viii., p. 66. In vol. vi., Dr. Peacock gives an excellent account of the cysticercus fasciolaris, as it exists in the mouse's liver, with drawings, p. 372; and in connexion with the subject of entozoa, there is an excellent account of the trichina spiralis, by Drs. Rainey and Bristowe, in the fourth volume of the 'Transactions.' In the same volume, an interesting case of epithelial cancer of the calvarium is related by Dr. Habershon, and reported on by Dr. Bristowe and Mr. Simon.

Dr. Salter's account of lesions of the teeth, and Dr. Andrew Clark's illustration of tubercle and tuberculous sputa, are specially deserving of commendation.

We have sought in the preceding remarks to place before our readers a few of the many valuable facts which, but for the Pathological Society of London, might not have come to light at all, or might have remained semi-buried in the private notes of a practitioner or the case-books of a hospital. We think that the Society has conferred a great benefit upon medical science, and we trust that it will continue to flourish and to advance. We might have dwelt at length on many more points of interest, which the minute examination of the

* Vol. i. p. 104 and 102.

first ten annual reports has suggested, but we must draw to a close, and in doing so we would say one word to the Society. If it is as flourishing as it seems to be, we earnestly impress upon it, that for its own reputation, as well as for the sake of science, some little time, labour, and money ought to be expended in carefully examining its archives, with the view of collecting from the reports before us such facts, data, and interesting cases as will undoubtedly furnish valuable information regarding the statistics of disease. By such means ultimately a system of pathology would be reared which would be an honour to the Society and a boon to science.

REVIEW X.

1. *Notes on the Surgery of the War in the Crimea; with Remarks on the Treatment of Gunshot Wounds.* By GEORGE H. B. MACLEOD, M.D., F.R.C.S., formerly Surgeon to the Civil Hospital at Smyrna, and to the General Hospital in Camp before Sebastopol, Lecturer on Military Surgery in Anderson's University, Glasgow, &c.—*London*, 1858. pp. 439.
2. *On Gunshot Wounds of the Thorax, and the Treatment pursued for them in the Crimea contrasted with that which was followed in former Campaigns.* By GEORGE LAWSON, F.R.C.S., late Assistant-Surgeon to the Rifle Brigade, Surgeon to the Great Northern Hospital and Western General Dispensary.—*London.* pp. 20.

WE propose to review the works before us solely as they bear upon the surgery of the late war in the Crimea, and therefore omit alluding to some interesting remarks by Mr. Macleod upon the history, physical characters, climate, and geology of that country, as well as upon military hygiene, nursing, and transport. We must state, however, in justice to the army surgeons, upon whom it was at one time attempted to cast much unfair obloquy, that throughout Mr. Macleod bears impartial testimony to their energy and devotion, as well as to the perverseness with which their efforts were so lamentably encountered by the executive authorities. Early in June, 1854, as gallant and splendid an army as England ever equipped landed at Varna. Its numbers, amounting at that time to 15,000 men, were soon augmented by the addition of upwards of 10,000. The country in which it lay was of most beautiful description to the eye, but of most dangerous character to the health. The long shallow lakes, the exuberant low-lying woods, thick tangled wild flowers and verdant grass, presented, to the eye of all but the initiated, one of the most charming stations for an inactive army. But every element of its sanitary character was bad. Large surfaces of shallow water, surrounded by level spongy lands, indented by little hollows, dried and cracked by the recession and evaporation of the winter floods, low brushwood rank in vegetation, bounding uplands; these, with a high temperature and a deficiency of potable water, supplied nearly all the possible combinations of physical destructiveness. In July cholera broke out, when a better field for its ravages could hardly be imagined.

"It is not, however," says Mr. Macleod, "for the purpose of repeating the tale of the heavy losses of our army in Bulgaria that I make these remarks. It is in order to indicate what a weakening and deleterious effect the residence in that country exerted on the survivors, and how much its effects must have told on the issue of disease and accident afterwards. There is no one fact which more completely illustrates this pernicious influence, than what all surgeons who served in Bulgaria will remember, that numbers of men, without being absolutely diseased, or yet so ill as to be fit for hospital, or perhaps even for medical treatment of any kind, yet fell off in appearance, lost appetite, flesh, and colour, became listless and weak, and that almost every one who had seen the campaign out was conscious of a considerable difference in his state of health after he landed. The standard of health, in short, was lowered, the vital forces were diminished, previous to embarkation for the Crimea in September."

The majority of the recruits who joined the army early in 1855, and who supplied many of the wounded of that year, were far from being well chosen. Many of them were raw boys, ill-conditioned, below the standard age, undeveloped in body, unconfirmed in constitution, often selected on account of their precocious growth; they were very quickly "used up." If severely wounded, they almost invariably died. Now all these points must be taken into consideration in estimating the value of medical returns, and the comparative success of different forms of treatment.

But there is another element which demands attention when estimating the surgical records of the war—namely, the use of the new rifle with its conical ball. The rifle used by the Russians was little inferior in range or force to our Minié, while its conical deep-cupped ball was much heavier. The long conical Russian ball, with a deep cup in its base and three shallow rings, ranged 1200 yards, weighed one ounce and six drachms; while the English Enfield ball weighed only one ounce and two scruples. The greater precision in aim, the immensely increased range, the peculiar shape, great force, and unwonted motion imparted by the new rifles to their conical balls have introduced into the prognosis of gunshot wounds an element of the utmost importance. Immense comminution of bone has been their most prominent effect. The amount of laceration of the soft parts seems to depend on the distance at which the missile is fired. The widespread destruction of bone often rendered consolidation impossible, so that amputation had more frequently to be had recourse to; the distance from the trunk at which that operation has to be performed being diminished by the same causes, the resulting mortality has been greatly increased.

But beside the more destructive nature of the small arms employed, cannon and mortars were used on both sides of a calibre and range never before tried in any war. When Paré thought the cannon of his day so enormous and destructive, what can we say of the huge sea service mortars and immense cannon used to defend and attack Sevastopol, compared with which those of the last century are as toys? The fragments of our modern shells must be as weighty as the whole projectile known to our forefathers; and the grape so freely used in the East were half as large as the round shot fired from the field guns in the Peninsula.

Mr. Macleod has failed to satisfy himself in all cases as clearly as the description of authors would lead to suppose as to the characteristics which distinguished the wound of entrance from that of exit. He denies the clearly-marked inversion of the lips of the former and eversion of those of the latter.

"If the speed of the ball be great, and no bone have been struck, then there is little difference in either the size or discoloration of the wounds; but if the flight of the projectile be so far spent as to be retarded by contact with the body, then the wound of exit will considerably exceed in size that of entrance. This is especially true of conical balls. If, on the contrary, the ball be fired close at hand, so that its speed is not sensibly diminished by its passage through a limb, then the difference in size will be very small, and may even be in favour of the wound of entrance, as I had twice an opportunity of observing." (p. 105.)

There seems to be no doubt that balls should always be extracted *when possible*, even when imbedded in bone. Malgaigne, who has related several cases in which balls have remained without causing harm, concludes thus,

"It is necessary to mention these fortunate cases as evidence of the resources of nature, but they hardly serve to weaken the force of the prognosis, when a ball cannot be extracted, or the essential indications of this sort of lesion—the extraction of the foreign body. This indication is, then, that of first importance."

But it must not be inferred from this advice that it is desirable to "poke about in the dark," as would in most instances be the case where a ball had penetrated deeply into the muscles of the thigh; and we scarcely think that Mr. Macleod fully expresses this caution. When the injured bone is superficial, all fragments, bits of cloth, &c., should be taken away; the ball, if felt, should be extracted. But there are cases upon record in which the present condition of the wounded in whose bodies balls have long been lodged, justifies our statement, that a prolonged exploration on the part of the surgeon would have been as unnecessary as hazardous. Colonel B——, Foot Guards, had the tibia broken by a ball about the middle of the leg. In this case a large portion of the bone was removed in its entire substance, together with the ball, upon the spot. Fortunately, the fibula was intact, and the patient recovered. He is in good health, and when heard of last was in command of the 23rd Regiment in Oude. But another officer of the same regiment has at the present moment a ball imbedded in the upper third of the thigh, where it has remained three years, after extensively shattering the femur. The surgeon left it in the wound without search. A ball has frequently to be cut out from under the skin in the opposite side of the body or limb. As is the case in severe injuries occurring in civil practice, the nerves most commonly escape injury; but when such an accident does happen, the parts below the seat of injury may waste. "I have seen the hand several times waste," says Mr. Macleod, "when some of its nerves had been injured by a ball." (p. 114.) We have seen the same occur to the whole upper extremity from a wound in the shoulder involving the axillary plexus.

Primary hæmorrhage after gun-shot wounds, when it does occur, proceeds more commonly, says Mr. Macleod, from the veins; but at the same time, "making due allowance for the elasticity, strong coat, mobility, and form of the arteries, it is difficult to understand how they escape injury." The late Mr. Guthrie insisted upon the gush of blood which commonly follows such a wound; yet no one can fail to be struck with the accuracy of the old remark, that primary hæmorrhage on the field of battle is rare, although ball-wounds have apparently passed through the course of the great vessels.

The *eccentric course* pursued by balls, on which much has been written, seems to be chiefly limited to the old round ball, and not to that of conical form, which seldom fails to take the shortest cut through a cavity or limb.

We are happy to find that Mr. Macleod expresses himself in favour of the employment of anæsthetics in military surgery. But he discusses the subject briefly and gently, out of consideration, we suppose, to the feelings of that distinguished medical officer whose name will be handed down to posterity in association with the subject. The French do not seem to have used chloroform so extensively as the English, and Baudens tells us that they had no fatal accident to deplore from its use, although during the Eastern campaign it was employed thirty thousand times or more. In one division of our army there seems to have been a single fatal case. A man, aged thirty-two, of the 62nd Regiment, was about to have a finger removed. The chloroform was administered on a handkerchief as he sat in a chair. Death was sudden, and artificial respiration, which was the means of resuscitation employed, failed to restore him. No pathological condition sufficient to account for death was found post mortem. It is remarkable that most of the fatal cases have occurred in the performance of minor operations.

Mr. Macleod devotes a considerable portion of Chapter VI. to the important subject of hæmorrhage from gun-shot wound, and although we cannot find space for more than the leading points, we recommend a careful perusal of these pages. The speed of the ball at the moment when it comes in contact with an artery has a good deal to do with the injury it inflicts. If it be in full flight, it may so cut open the vessel as to allow of instantaneous hæmorrhage; whereas, if its speed be much diminished, the contusion it occasions opposes immediate, but favours secondary bleeding. (p. 138.) Hæmorrhage should be distinguished into three periods—"primary," occurring within twenty-four hours; "intermediary," between that and the tenth day; and "secondary," that which takes place at a later date. The intermediate hæmorrhage is due to sloughing resulting directly from the injury; the secondary proceeds from morbid action, such as ulceration attacking the part.

No man at all severely wounded by gun shot can be considered safe from hæmorrhage till his wound is closed; but yet, after twenty-five days, the danger may be said to be in a great measure overcome.

The French, although generally applying the ligature at the seat of injury in primary hæmorrhage, perform Anel's operation when the bleeding appears late. The teaching of Dupuytren and Roux has done much to prevent "the English practice being so fully followed as it is with us." (p. 143.) We prefer, however, "the English practice," and quote from page 145 the following case, the treatment of which may be impugned.

A Russian boy, who had sustained a compound fracture of the leg at Inkermann from gun shot, was received into the French hospital at Pera a few days afterwards. On the fifteenth day from the date of injury, profuse hæmorrhage took place from both openings. Pressure failed to arrest it. We can have no hesitation in saying that under such circumstances the clear indication was immediate amputation. But if that measure were declined, the only mode of arresting hæmorrhage, which threatened a fatal result, would have been to secure the bleeding vessels at the seat of injury—a proceeding undoubtedly hazardous and difficult. However, a different course was pursued. The popliteal artery was tied on the inner side of the limit, between the vastus and the hamstring muscles. The foot remained very cold for four days, then violent reaction set in, and on the eighth day from the ligature of the main vessel hæmorrhage recurred, both from the original wound and the incision of ligature. Pressure was again tried in vain. The superficial femoral was next ligatured on the tenth day from the deligation of the popliteal. Four days after the bleeding returned from the wound, and pressure then seemed to check it. The ligature separated from the femoral on the twelfth day after its being applied, and the third day after—i.e., the twenty-fifth day from the first occurrence of the hæmorrhage—bleeding having again set in from the wound, the limb was amputated high in the thigh, and the unfortunate patient ultimately recovered. (p. 145.)

The cases in which attempts were made in the East to arrest hæmorrhage from stumps, by applying a ligature to the main vessel above the extremity of the stump, were, according to Mr. Macleod, singularly unfortunate. Well-applied pressure along the course of the principal vessel, adapted to diminish the circulation through it, has sufficed, in some few most threatening cases, finally to arrest bleedings. But in these cases the implication of the main artery was clearly made out. Guthrie recommends the re-amputation of the limb in the event of hæmorrhage from a thigh stump, which cannot be commanded by the application of a ligature to the bleeding point. Mr. Macleod would be disposed to tie the iliac. We rather think the practice of Mr. Guthrie the safer.

Tetanus following wounds was uncommon. "I know," says Mr. Macleod, "of six cases only which occurred in camp, and seven which took place at Scutari. In no case did it occur after the twenty-second day." (p. 153.) Hospital gangrene, too, was rare, even in the abominably filthy state of the barrack hospital at Scutari; and the exemption must have been due to the determination on the part of the surgeons to "make the best of what they had," and not from causes

affecting temperature or climate, for the French suffered from this disease most dreadfully.

We now proceed to the chapters of this interesting work which are devoted to injuries of different regions of the body.

Injuries of the Head.—From April 1, 1855, to the end of the war, the return, says Mr. Macleod, shows a total of 630 cases of gun-shot wounds of the head, attended by contusion merely, and 8 deaths among these cases. Of gun-shot fracture without *known* depression, 61 cases and 23 deaths. Of cases of fracture and depression followed by sensorial disturbance, 74 cases and 53 deaths. Of wounds penetrating the cranium, 67 cases and 67 deaths. Of 19 cases in which the skull was perforated, all died. The trephine was employed twenty-eight times, and of these cases 24 ended fatally. (p. 175.)

We first direct attention to the fact that of sixty-seven penetrating wounds of the cranium there was not a single instance of recovery. So heavy a mass of lead as a conical ball tearing through the cranium and its contents leaves but a faint chance of recovery. We need hardly observe that Minié rifle bullets do not *bound off* from a skull, as was innocently supposed not long ago by a civil practitioner of surgical eminence.

The interesting point in the present subject, however, is undoubtedly the propriety of the use of the trephine; and Mr. Macleod's remarks are opportune, inasmuch as there exists among some of the more adventurous and active-minded younger surgeons of the present day a strong inclination to reintroduce the frequent employment of that peculiar instrument. We have heard of a surgeon who gave chloroform to his patient to produce insensibility before perforating the skull after injury.

Of 28 cases which were trephined, 24 terminated fatally; and of the four recoveries, it might be interesting to hear the opinion of the surgeons under whose care they were put. We have reason to believe that the following statement from Mr. Macleod would be endorsed by all those who had experience in modern warfare:

"As to the use of the trephine, the cases and time for its application, less difference of opinion, I believe, exists among the experienced army surgeons than among civilians; and I think the decided tendency among them is to endorse the modern 'treatment by expectancy,' and to avoid operation except in rare cases. In this I believe they judge wisely, for when we examine the question carefully, we find that there is not one single indication for having recourse to operation which cannot, by the addition of pertinent cases, be shown to be often fallacious; while if we turn to authorities for advice, we find that not a great name can be ranged on one side which cannot be balanced by as illustrious on the other." (p. 186.)

Mr. Macleod might have added, that in nearly all the London hospitals, the use of the trephine is generally abandoned, although it still struggles faintly for a doubtful supremacy in one or two institutions.

Wounds of the Chest.—In striking contrast with the preceding returns stand the results of wounds of the thorax. Out of 533 wounds of all

descriptions in this part of the body, occurring from April 1, 1855, to the end of the war, there was a mortality of only 2·24 per cent., while in the case of wounds of the head the mortality was 5·77. But beside this difference, it is remarked that most of the fatal results in chest wounds were due to other causes. The dangers of a penetrating wound of the lung are, primarily, hæmorrhage and collapse; secondarily, inflammation and its products. Hence there are laid down two distinct stages for treatment—1, that during which there is internal hæmorrhage and collapse; 2, that which follows and is accompanied by reaction and inflammation. We do not find much importance attached (p. 237) to the rule that the patient should be laid upon the wounded side. Mr. Macleod speaks with diffidence and uncertainty upon the point of practice so strongly laid down by Guthrie and Hennen respecting venesection: "With our modern notions on bleeding, it is often difficult to reconcile the necessity, which experience shows us there is, for energetic depletion when reaction sets in. The majority of our patients were certainly not subjects in which this remedy could be pushed." (p. 237.) And further on: "That many most excellent recoveries were made without having recourse to the lancet is undoubtedly true; but not a few, *I fear*, died from want of it." Now, it is obvious that Mr. Macleod's own mind is wavering between the results of his own experience and respect for authority, and we will therefore bring before the reader the results of the experience of Mr. Lawson, who served during the same war with the Rifle Brigade:

"The patient may perish immediately if the injury to the part be severe: large bloodvessels may be torn through, and he may die of hæmorrhage. One shock, from the severity of the injury, may carry him off in a few hours. Should he, however, survive the first effects, pleuro-pneumonia and its consequences may kill him, or even at a later period he may sink under the exhausting effects of profuse suppuration. (p. 7.) The old military surgeons advised in these cases free venesection—in the first instance, for the arrest of hæmorrhage, and afterwards for the prevention or cure of pneumonia."

It was evident, said Mr. Guthrie, that to succeed, no limit should be placed to the abstraction of blood in the first instance but the decided incapability of bearing further loss. Further directions are given to prevent syncope, by giving stimulants, &c., that the flow may not be arrested by natural causes.

"Any man," continues Mr. Lawson, "who has read Mr. Guthrie's late and early works, will observe that a change was gradually coming over the spirit of his dream; and the experience of the past war certainly does not sustain the imperative orders of the old Peninsular surgeons. When the hæmorrhage takes place externally sufficient blood may be lost to produce faintness and the desired arrest of bleeding. When the hæmorrhage proceeds from a wound in the lung, venesection is the patient's only chance; but having succeeded in placing him out of danger of death from bleeding, the lancet case should be closed."

It is possible that further experience will yet check the employment of the lancet. Of what use could venesections have been in the case of Private Hannihan (Macleod, p. 241), in whom the removal of the ball was followed by a tremendous gush of blood from the incision

made? After death the left side of the thorax was found more than half full of mostly fluid blood. How can such a measure be beneficial when large branches of the pulmonary vessels are torn across? We apprehend that nature indicates such treatment when reaction is coming on, with excitement of the circulation, and danger of inflammatory effusion into the substance of the lung; but we doubt whether the abstraction of blood from the basilic vein will arrest the circulation in any part of the pulmonary tissue so long as respiration continues. Neither are we prepared to assent to the doctrine, that when blood is effused in great quantity into the cavity of the pleura, it would be good practice to "open the cavity freely by enlarging the wound, so as to allow the blood to escape freely, and thus favour the contraction of the lung and the closure of the vessel." (Macleod, p. 243.) It is important to remember that, as Mr. Guthrie often remarked, when no adhesions are formed, a ball or other foreign bodies which are retained in the chest are generally found lying on the diaphragm in the angle formed by it and the costal walls.

Wounds of the Abdomen.—Flesh contusions of the abdomen were fatal in the proportion of 17 out of 101 cases. But penetrating wounds with lesion of the viscera were fatal in 36 cases out of 38, while 60 deaths occurred out of 65 when the abdomen was perforated. It is obvious that the treatment need be very simple. No attempt should be made to follow the ball; the wound should be lightly covered, and the patient placed in such a position as will relax the abdominal walls. It is in wounds of the abdomen that the treatment by "*débridement*" retains its last footing. The fear of strangulation by the strong fasciæ or between the muscles is assigned as the claim it has to adoption in these wounds. But experience, while it has overthrown this cause of anxiety, has shown that a positive evil is occasioned by the practice in so far that the abdominal walls are weakened by it, and hernia is more apt to occur. This step, then, is abandoned here, as in all other regions, unless an absolute necessity arise for its adoption. In the case of narrow wounds through the deep muscles of the back, through which fæces ooze, but cannot get a free escape, in similar wounds penetrating the bladder, or in cases in which a large amount of blood has been effused into the abdomen, it may be necessary to enlarge the wound. (Macleod, p. 264.) We have not space to narrate many interesting cases illustrating the position which the author takes relative to the proper line of treatment, but must refer the reader to the work. The analysis of the last chapters—namely, Chapter X., On Compound Fractures of the Extremities, Gun-shot Wounds of the Hand and Foot; Chapter XI., Gunshot Wounds of Joints, Excision of Joints; and Chapter XII., On Amputations—will extend this review beyond the usual limits.

"Of all the severe injuries recorded in battle," says Mr. Macleod, "none are of more frequent occurrence or of more serious consequence than compound fractures. . . . In the Crimea these injuries were peculiarly embarrassing and extraordinarily fatal. . . . It can hardly be doubted that the great striving after conservatism, which influenced all the surgeons of our army, was one main

cause of that mortality which attended these injuries. (p. 278.) Two circumstances seem to have had chiefly to do with the irreparable character and mortality of compound fractures of the thigh in the Crimea—firstly, the state of the men when wounded; and secondly, the effect on bone of the new kind of ball with which most of these injuries were inflicted. The vast majority of the men had ulcerated intestines, having suffered from dysentery; they had been living up to their income of health, and so utter failure was the sure result of increased expenditure. Many looked well at first; the injury might not appear very severe; the bone was broken, but it might not be comminuted, and so the golden opportunity was allowed to pass, and we entered on a road which led to death, whether through the portal of amputation or any other. The struggle soon began: suppuration set in; the disease which lurked 'in blood and bone' showed itself; diarrhœa appeared, and would not cease; the patient's stomach refused the only food which could be procured; he got weak, emaciated, and irritable. Things got from bad to worse, and the great regret remained that an operation was not done at first, as the inevitable result demonstrated the uselessness of any other proceeding." (p. 282.)

The inevitable shifting of the position of the army involved a corresponding amount of disturbance of the sick, as the plan Esmarch says they adopted in Holstein—namely, that of leaving the patients to fall into the hands of the enemy in order to obviate the necessity of transport—was not considered applicable in the case of the allied forces before Sebastopol. The heavy conical ball, such as that used by the Russians, produces effects very much more severe than those caused by the old round ball. It never rests in a bone, channels or perforates its substance, without SPLITTING it like a wedge. Nor does it ever come to mark a bone with any touch more gentle than what occasions its utter destruction. The greater part of the femur may be so split, although the fissure, as Stromeyer has shown, seldom transgresses the line of the epiphysis. A small opening may lead to the seat of fracture, but the whole shaft may be rent from end to end. The splintering which ensues far exceeds that which occurred in the warfare of past times; and when to these considerations is added the chance of other foreign bodies, such as pieces of accoutrement or cloth remaining between the broken fragments, reasons will be seen for the practice of enlarging the *exit* wound, especially if it be the more dependent (p. 289). M. Hutin, of the Hôtel des Invalides, remarks, "I have given several observations taken from several hundreds, in order to show that the portions of non-extracted bone end sooner or later by setting up eliminative action, which is always painful, always dangerous, and at times fatal."

Mr. Macleod has found a record of only *three cases* in which recovery followed a compound fracture in the upper third of the thigh without amputation. In two of them the injury was occasioned by round balls, and the comminution was slight. In the third case, it was not possible to ascertain the species of ball that caused the injury. One patient, an officer in the 17th Regiment, was in the highest health at the time when he was wounded. The ball was removed a little below the trochanter, by Dr. Ward, of his regiment; and yet I am informed, adds Mr. Macleod, that although the limb was in a very good condition when the patient left for England, the trouble which it has

since given him, and the deformed condition in which it remains, make it by no means an agreeable appendage. The second man was found lying in the dockyard-stores of Sebastopol; the splintering was not great, and the injury (inflicted by a round shot) was comparatively slight. The third man had his limb consolidated, in so far that a mass of callus was thrown out, but he died of purulent poisoning, and never left the Crimea. The French had hardly any recoveries. Their surgeons in the East fully acknowledged the hopelessness of these cases, but the fatality of amputation was little behind that of preservation.

M. Ribes, as is well known, failed to find a single case of recovery after compound fracture or amputation in the middle of the femur, among 4000 cases which he examined in the Invalides at the period of his first visit, but during subsequent years he saw seven cases there of "cured" compound fractures, five of whom died after many years of great suffering arising from the injury, and the two others he lost sight of.

Long years of suffering, constant abscesses, exfoliations, atrophy, sensitiveness to the slightest atmospheric changes, shortening, and deformity, the development of phthisis, are among the results of a "cure" of a compound fracture by gun-shot in the middle of the thigh.

The conclusions at which Mr. Macleod arrives are, we think, the following. Under circumstances of war similar to those which occurred in the East, amputation should not be attempted in cases of compound comminuted fractures of the thigh when situated in the upper third (p. 302). Because, although it is unfortunately too true that death will most commonly follow in whatever way we may decide, yet when we do not operate, the patient may live in comparative comfort for several weeks; in the other case he has to undergo a very fearful operation, and almost certainly dies within a few days. But that immediate amputation should be had recourse to in the case of a like accident occurring in the middle or lower third, the results being far more satisfactory.

We think that the rule here laid down cannot be received without some limitation, and that a good surgeon would have to take into consideration not only the seat but the amount of injury, upon further evidence than the tabular returns in the work before us. We apprehend that medical officers could furnish Mr. Macleod with cases which would induce him to modify his unfavourable opinion, and with that view we will quote an instance of severe gun-shot injury to the lower extremity, which occurred in the practice of Mr. Bostock, the surgeon-major of the Scots Fusileer Guards.

Colonel H——, Scots Fusileer Guards, had the upper third of the thigh shattered by a ball; it could neither be felt nor got out, and the only choice was to leave it where it was with all the fragments of femur, or to amputate close to the trochanter. This gentleman is now in good health; the limb is two inches shorter than the opposite; the ball remains, but gives no pain or trouble. What would have been the fate of this officer had either amputation or resection been performed?

We pass to gun-shot wounds of joints, pronounced by John Bell in his treatise on Gun-shot Wounds to be "mortal." The gravity of the accident depends on the size and construction of the articulation, and the extent of the injury. In the case of the hip or knee, the result is usually fatal when the injury has been inflicted by a conical ball. The wound of a ginglymoid articulation, therefore, can be hardly, as "a general rule, more severe than that of a ball-and-socket joint," as Mr. Macleod observes, chiefly from its more complex structure. Alcock lost three out of four cases of gun-shot wound of the hip, and "in the fourth case, where recovery took place, the joint itself, there is some reason to suspect, was but remotely affected."

Even when a small matchlock ball inflicts a wound on the hip-joint, the results are most serious. Alexander M'Phail, aged thirty-three, was wounded by such a projectile, at the battle of Dubba, in Scinde, March 24th, 1843, which entered a little above the great trochanter of the right limb anteriorly, and was lost. His leg became powerless. On May 6th he was attacked with trismus, and died on the 9th. The ball was found imbedded in the head of the femur, which, with half the brim of the acetabulum, was shattered, and the capsular ligament formed the sac of an abscess, which contained a considerable quantity of pus and spiculæ of bone. The orifice of the wound had closed some time previous to death.

"The knee," says Mr. Macleod, "presents an injury, when penetrated by gun-shot, of the gravest description. Taking much interest in these cases, I visited every one I could hear of in camp, and can aver that I have never met with one instance of recovery in which the joint was distinctly opened, and the bones forming it much injured by a ball, unless the limb was removed; yet the returns show several recoveries after such wounds, some of which, at any rate, I cannot but think are founded on error." (p. 311.)

The details of the following case were supplied by Deputy-Inspector Taylor, from Chatham:

"Private George Haynes, aged thirty-one, 47th Regiment, was wounded at the Alma by a grape-shot, which entered on the outer side of the ligamentum patellæ, and passed upwards through the knee-joint, shattering the patella in its course, and making its exit at the anterior aspect of the thigh, about its middle, partially fracturing it. The greater portion of the patella was removed in the course of treatment, as well as various fragments of the femur; but firm union of the latter, as well as anchylosis of the joint, fortunately took place. At the time of his discharge he could sustain his weight upon the limb, and could walk about without crutches."

It is often very difficult to know whether the joint has been opened or not, particularly if the ball is a small one, and it is also to be remembered that swelling may be merely the result of a bruise. Military surgeons of all times have recognised the necessity of removing the limb early in these cases, when the articulating ends of the bones have been fractured by a ball, and the experience of the late war fully bears out the practice. Equally decided are the opinions of Guthrie, Larrey, and Esmarch. It is a rule of deplorable necessity, already given by the best authorities. We think, therefore, that Mr. Macleod would be disappointed in his favourable expectations, were

he to carry out his idea "of laying the articulation open at an early period in these cases, so as to permit the extraction of all foreign bodies, and the free escape of the pus, which must afterwards be formed." We likewise disagree with him, when he says that if the attempt is to be made to save the limb, the most rigid antiphlogistic treatment must be followed; local bleeding by leeches, and the application of cold, &c. His own words (p. 317) show the nature of the dangers to be apprehended,—"hectic, with its common accompaniment diarrhœa, purulent absorption, with secondary implication of internal organs, and tetanus." If such an attempt were made, the patient would require all possible support from the beginning, and we should trust to opium and not to leeching, for the alleviation of pain and the control of inflammation.

The shoulder-joint has recovered well in several cases where a ball has passed through part of it, and even in cases in which a good deal of the head of the bone has been destroyed; but Mr. Macleod suspects, and with good reason, that the after consequences are not always so encouraging as the rapid healing would lead one to expect.

The returns show the following results as having been obtained from the resection of joints from the 1st April, 1855, till the end of the war:

Head of the femur	5 primary, of which 1 recovered; 1 secondary, fatal.
Knee-joint	1 secondary, fatal.
Os calcis, and part of the astragalus. . . .	1, recovered.
Os calcis alone.	1, recovered.
Head of the humerus	8 primary cases, 1 death; 5 secondary cases, no death.
Head of the humerus, and part of the scapula	1 secondary, followed by death.
Elbow-joint.	13 primary, 3 deaths; 4 secondary, died from causes not connected with the operation.
Partial of elbow-joint	3, no death.

Now from this list we should exclude operations on the os calcis and astragalus, which hardly come under the category of resection of joints; and if we look at the remainder of the list we shall see expressed that which civil practice has contributed to confirm—namely, that while excision of the articular extremities of the humerus, or of the bones comprising the elbow-joint, may be undertaken, if necessary, with a fair prospect of success, a similar operation in the hip or knee offers most discouraging results. Of the former there were performed thirty-four operations, with only nine deaths; while of the latter there were seven operations, and six deaths.

The reasons which induce Mr. Macleod to support the operation of excision of the head of the femur (resection of the hip), are thus expressed:

"The hip was resected six times—five being primary, and one a secondary operation. One of the primary alone succeeded. Such success, although

small, is yet encouraging as compared with the results obtained from amputation at the hip, for which the operations of excision were substituted, for, as will be hereafter seen, out of at least ten amputations at this joint in our army, and some thirteen among the French, none survived; and, in one case at least, the fatal result very rapidly followed the operation." (p. 338.)

These six operations were performed successively by Mr. Macleod, Mr. Blenkins of the Guards, Staff-Surgeon Crerar, Mr. O'Leary of the 68th Light Infantry, Dr. Hyde and Dr. Combe of the Royal Artillery. The case of Mr. O'Leary being the successful one, we will briefly narrate the particulars. The patient, aged twenty-five, was wounded on the 19th of August by a fragment of shell (not a conical ball), which struck him over the great trochanter, and fractured it. It was thought that the fracture ran into the joint—a supposition which was disproved at the operation, as the head of the bone was uninjured. Five inches were removed in all. After operation, the limb was slung to the beam of the hut. The patient recovered in three months. The limb is now shorter than the opposite, and the movements are limited. The man cannot walk without the aid of crutches.

Mr. Macleod thus sums up:

"As to the comparative advantages of amputation and excision at the hip in cases of compound fractures of the head and neck of the femur by gun-shot, some hint may be got from our experience in the Crimea. Out of 23 cases of amputation which took place in our army or in that of the French, not one recovered; and nearly all died miserably very shortly after operation. All those, on the other hand, on whom excision was practised, lived in comparative comfort, all without pain, for a considerable time. One survived more than a month; one died from causes unconnected with the operation (?); and one case recovered entirely. The chance of saving life is thus manifestly on the side of excision." (p. 346.)

To have rendered these tables complete we should have the report of a number of cases in which no operative proceeding of any kind was adopted, that we might learn the results of leaving the wounded to nature. That recovery may take place after very severe injuries is more than probable; and Esmarch refers to a case operated on by Dr. Ross, and related in No. 41 of the 'Deutsche Klinik,' 1850, which ended fatally, where the operation was performed two years after injury, and in which the patient must have survived a corresponding time.

We have not yet seen a case in civil practice in which the head of the femur has been excised *with success*, even for disease. By "success" we mean, that the patient has subsequently had a useful limb, or even one fit for support. But such cases can scarcely be considered under the same head with gun-shot injuries; and we think that merit is due to Mr. Macleod for putting forward so clearly the apparent hopelessness of trying to save the patient by the removal of so large a part of the body as the whole lower extremity. We accept, however, Stromeyer's assertion with some qualification, "that although the splitting of the bone barely extends into the capsule, yet excision should be at once performed, as suppuration is sure to be set up in the articulation, and

death by exhaustion follow." Whatever may be the probable result of leaving things alone, let us remember that Seutin, Oppenheim, and Schwartz have all excised the hip for gun-shot injuries, but not with success; and that perhaps it may be worth the while of the army surgeons to reconsider this view of treating these most serious injuries in the event of the occurrence of another war.

The knee-joint was resected once only during the war, but the case terminated fatally. The name of the operator was Mr. Lakin. We wish that we could share Mr. Macleod's enthusiasm for this operation:

"Admiring as I do the brave attempts which have been made in civil practice to save limbs by excising the knee, I regret that it should not also be extended to military practice. Fergusson speaks of more than 100 cases having been now operated on in civil practice, and Butcher has shown that the mortality is greatly less than what succeeds amputation of the thigh." (p. 354.)

But in all these reports we must separate young subjects from adults—the former generally doing well under all circumstances after operation—and we shall then find that out of a list of 31 cases operated on in civil practice from July, 1850, to December, 1854, 23 were under twenty-one years of age; of the remaining 8, between twenty-one and forty-two years, 3 died from the effects of the operation. Further experience has thrown a still deeper shade over the bright anticipations of the most ardent supporters of the operation.

The question of primary or secondary amputation seems fairly to be decided in favour of the former:

"The experience in the Crimea in favour of early operation was unequivocal in both armies, and needs no illustration from me." (p. 359.)

Chloroform has done much to render the success of primary amputation, as contrasted with secondary, yet more marked. In page 368 there is a table showing the increase of the mortality as we approach the trunk, taking the primary amputations alone as giving the most unbroken series. Thus, in the tarsus it amounted to 14·2 per cent.; in the leg, 22·2; at the knee-joint, 50·0. The thigh, lower third, 50·0; middle third, 55·3; upper third, 86·8. At the hip-joint, 100·0.

"If a calculation is made of the mortality succeeding amputation of the thigh from gun-shot wounds alone . . . then the average mortality per cent. of primary operations would appear to be 56·5, and of secondary 79·9; while if the operations performed in civil hospitals for injury are alone calculated, then the average mortality of primary operations would appear as 69·6 per cent., and secondary 75·4—a result somewhat different from what is usually obtained." (p. 377.)

Amputation through the knee-joint was performed in our army six times primarily, three of which were fatal; and once secondarily, with a fatal result. This very old operation, says Mr. Macleod, has lately been creating some interest in the profession, and was often performed by the French surgeons in the Crimea. It requires that the injury should be confined to the bones of the leg, and that the soft parts should not be so much lacerated as to prevent the formation of a flap.

Of the 5 cases operated on in the British General Hospital, one

died of phagedænic sloughing on the forty-third day; another died of enteritis on the sixty-seventh day, the stump being healed to a point; a third sunk from exhaustion on the ninth day after operation; a fourth never fairly recovered from the shock; while the fifth and last case recovered under the care of Dr. George Scott, who operated on him.

"But," adds Mr. Macleod, "if cases were selected for the operation in which the femur remained intact, and, the leg-bones being destroyed, a sufficiency of flap could be got from the calf or the front of the leg, and if the amputation was performed early, I firmly believe, with Malgaigne, that it is 'Encore une de ces opérations trop legèrement condamnées, et qui lorsqu'on a le choix mérite toute préférence sur l'amputation de la cuisse dans la continuité.'" (p. 381.)

Of amputation of the leg there was a ratio of mortality per cent. of 31·4 for the primary, and 66·6 for the secondary.

As to the operation just above the ankle, which has of late years caused so much discussion on the Continent, we had, so far as I know, no experience in our army (p. 383); but the French had a good number, which, as regarded the condition of the stumps, were not by any means promising; the mortality, however, is undoubtedly low, being only one-sixth or one-seventh per cent.; while the mortality of amputation at the place of election is 55 per cent.

The ultimate success of the operation is in great measure connected with the application of a properly-constructed artificial limb, by which the cicatrix will not be irritated. Of amputations at the ankle-joint, it was remarked that Symes' operation was as useful and as successful in its results as usual. Pirogoff's modification was several times tried at Scutari; but Mr. Macleod saw none of these cases, and is ignorant of their results.

Pus poisoning was unquestionably the chief source of mortality in the East after amputation, especially after secondary operations; and Mr. Macleod seems to consider that it is a question on which it is difficult to decide, whether or not, when that accident is common, it would not be a justifiable practice to ligature the chief vein at the time of operation. He expresses his firm belief in Stromeyer's views on absorption by the veins of the bone. The secondary deposits were generally found in the lungs. We fear that tying the vein would prove no safeguard against purulent infection.

Before concluding, we must express our conviction that both Mr. Macleod's work and the Essay by Mr. Lawson, reflect great credit on the authors. They show an independence of thought which requires only experience to lead to important results on many yet doubtful points on surgery. The work of Mr. Macleod is written clearly, modestly, and concisely; and perhaps the highest compliment that we can pay his Notes is, that they are interesting, instructive, and readable to the end. We trust that he may obtain all the credit to which his industry and perseverance entitle him.

Review XI.

An Essay on Wasting Palsy (Cruveilhier's Atrophy). By William Roberts, B.A., M.D. Lond., Scholar of the University of London; Physician to the Manchester Royal Infirmary, Fever Wards, and Lunatic Hospitals, &c. With Four Lithographic Plates.—*London*, 1858. pp. 210.

We have at various times placed before our readers accounts of the investigations of different observers into the nature of the disease characterized by atrophy of one or more sets of muscles—a disease in which the primary changes seem to occur in the muscles, and one which in some respects resembles paralysis dependent on a lesion of the nervous centres. Without entering into a discussion as to priority of observation, it certainly does appear that Cruveilhier was the first correctly to interpret the phenomena; he was disposed to regard the affection as dependent upon a disease of the spinal cord, until a post-mortem assured him that wide-spread muscular atrophy and paralysis could exist without any lesion of the nervous centres.* The first cases that he observed fell under his notice in 1832 and 1848; subsequently he met with two cases in which he found the anterior roots in a state of atrophy closely corresponding to the extent of the muscular degeneration occurring in each case. This, however, as Dr. Roberts also justly remarks in the work before us, by no means proves that the primary fault was in the nerves. On careful examination of all the evidence now at our command, there can be no doubt that there is a primary affection of the muscles, to which our author's term, or the designation of Cruveilhier or Aran, which is endorsed by Valentiner, Virchow, and others, may be applied.

The work of Dr. Roberts supplies us with a detailed history of this peculiar affection; the first chapter is devoted to this part of the subject; in the second chapter the general physiognomy of the disease is given, and we here meet with several cases which have fallen under the observation of the author, and which prove that his work is something more than a compilation. In the same chapter we find a careful analysis of no less than 105 cases which Dr. Roberts has collected from the records of the profession; this is extremely valuable to any one wishing to familiarize himself with the whole literature of the subject. The frequency of the complaint, its anatomical distribution and symptomatology, are the subject matter of the third chapter.

The disease presents two main forms; in the one the body generally is affected, yet without in reality implicating all the muscles, for there are a few which seem to be altogether exempt; the muscles of mastication and those of the eyeball, including the levator palpebræ, have not

* The following are the references to the disorder in this Journal: Vol. vii., April, 1851, p. 538, Aran on Progressive Muscular Atrophy; Vol. xvi., October, 1855, p. 532, Valentiner, a Contribution to the Doctrine of so-called Paralysie Musculaire Progressive; Vol. xvii., April, 1856, p. 527, Virchow on Progressive Muscular Atrophy; Vol. xviii., July, 1856, p. 252, on Paralysis from Muscular Atrophy, by M. Cruveilhier. Dr. Meryon's paper on Granular and Fatty Degeneration of the Voluntary Muscles appeared in the Medico-Chirurgical Transactions, vol. xxxv., and is fully referred to in the Medico-Chirurgical Review, vol. ii. p. 336.

yet been known to be involved; still the disease invades the locomotive system so universally in many cases, as to justify the distinction of a general from a partial form, only a few muscles or sets of muscles being affected in the latter. Scarcely two instances are exactly alike in the combination of muscles implicated, and the relative degree in which they have suffered: hence an almost endless variety of feature. Yet in the partial form the disease shows a manifest predilection for the upper extremities; in 60 cases of this variety we find that, according to Dr. Roberts (p. 108), the upper extremities were exclusively affected forty-eight times. The chief symptom of the disease is loss of motor power in one or more muscles, corresponding to the degree of atrophy affecting the muscle.

"The muscle goes on decaying slowly for months and years; its force declines at the same deliberate pace, so that it is only in extreme cases that any part is reduced to absolute immobility. The correspondency claimed between the bulk of the muscle and its contractile power is a distinctive feature of the disease, and serves to separate it in a marked manner from hemiplegia, paraplegia, facial paralysis, and indeed from any palsy depending on an injury to the cerebro-spinal centre or a nervous trunk."

It is, however, undeniable

"that in certain instances this exact correspondence between the wasting of the muscles and the loss of power is departed from. The palsy sometimes overshoots the atrophy, and sudden variations of amendment or aggravation have been observed not at all consonant with our ideas of a defect dependent solely on an equably acting cause, such as muscular degeneration."

Next to the wasting and coincident loss of power in a group of muscles, the most prominent symptom is described by our author, and universally observed, as consisting in muscular vibrations, twitchings or tremors affecting individual muscular fasciculi, which it appears may be set down as a diagnostic sign of the invasion of granular and fatty degeneration of the muscles. Great assistance in determining the diagnosis is afforded by Duchenne's method of local galvanization of the affected muscles. By applying the induced current to the individual muscles, and ascertaining the degree of electric contractility that they possess, we are able, as a general rule, to determine whether the paralysis is dependent upon lesion of the nervous centres or of the muscles. According to Duchenne, the readiness with which a muscle responds to the electric stimulus is in direct ratio with the extent to which fatty degeneration has proceeded, and he considers "that a palsy accompanied with an elevation, or even perfect preservation of the electric muscular contractility, or one in which it is abolished, or depressed out of all proportion to the grade of the atrophy, may at once be confidently pronounced *not* to be an example of wasting palsy." In a well-marked case of muscular atrophy now under our care, the loss of contractility is in the exact ratio of the wasting process, and is well shown by the galvanic test.

Tactile sensibility is scarcely ever affected; in 3 only of the 105 cases collected by Dr. Roberts was there a little numbness of the finger ends; pain is a much more frequent accompaniment of the disease; it may have the character of neuralgia or resemble the pain of

rheumatism. Excepting that the individuals affected with progressive muscular atrophy are subject to chilliness, the general health does not commonly suffer, and unless the disease invades the trunk so as to involve the muscles of respiration, there is no direct danger to life. It is essentially a chronic affection, but its duration varies considerably. Its termination may be in recovery; it may be arrested or it may prove fatal.

"The greatest duration of a case ending in recovery was two years and a half, and the shortest eight months. Fatal cases have not been known to terminate under a twelvemonth, while one lingered for twenty-three years, another for eight, and four more beyond four years. It is important to note that cases which issue in recovery run a much briefer career than those which end in death." (p. 128.)

The fifth chapter of Dr. Roberts's book contains some interesting remarks on the etiology of the disease. The general form appears to recur at all periods of life, but both general and partial muscular atrophy are met with chiefly during adult life and before the decline. Males are very much more subject to the disease than females—in the proportion of five to one, a difference depending probably upon the greater exertion required of men than of women. Hereditary influences prevail to a large extent in the production of muscular atrophy; but it does not appear, as one might expect, that in the individual liability, muscular weakness plays a principal part.

"Several times it attacked men of remarkable muscular power, glowing health, and unusual physical endowments. In nine instances a certain weakness had existed from early youth, but seven of these owned a hereditary cause, and the eighth was not a simple case, being complicated with mollities ossium. Indeed it might be said without exaggeration, that the victims of wasting palsy are the strong, the healthy, and the intelligent."

Besides hereditary predisposition, cold and wet, and hard work, appear to be the only special causes discoverable. Syphilis is also mentioned, but our author can only find one case in which this cause seems to have operated, to which we could contribute another not yet published.

The morbid anatomy of the disease is carefully detailed in the sixth chapter. The main features consist either in a simple removal of the sarcous elements of the muscles affected, or in a substitution for them of granular and adipose matter. A corresponding change in the external and microscopic appearances will take place. When there is much fatty deposit there will necessarily be little wasting, but in both cases the change of function will have been the same during life. In the fifteen cases in which post-mortems were made the brain was found free from disease; in nine out of thirteen where the spinal cord was examined, it was healthy; four presenting deviations from the normal condition. Of these, three showed softening, and the fourth amylaceous degeneration, but the parts affected differed in each case, so that no conclusion as to the intimate nature of the disease could be drawn from these conditions.

The last chapter of the work is devoted to the treatment of the affection. Where so much is new and obscure, and where the opportunities for testing the efficacy of remedies are comparatively so rare, it

is not surprising that the results of Dr. Roberts' treatment should not as yet have been very satisfactory. Yet there are other diseases with which we are more familiar, and the therapeutics of which are scarcely more advanced than those of progressive muscular atrophy. The rational view of the disease would naturally induce us to look for the remedies among those which improve nutrition and act as local stimulants to the muscular and nervous tissues. Dr. Roberts advocates the employment of mercurials and iodides as alteratives, and where we have a constitutional disorder like syphilis, or when the poison of lead is in the background, it is probable that these remedies may aid in restoring the affected muscles. Thermal and sulphur-baths have been found beneficial, a circumstance we specially alluded to in speaking of Dr. Wetzla's little book on the curative properties of Aix-la-Chapelle.* It appears, however, that of all the various remedies, none is so decidedly efficacious as the localized application of galvanism, or to use Duchenne's term, Faradization. Being essentially a local disease, it is but natural that we should seek for a local remedy, which by insuring the full action of the feeble muscle, and thus maintaining its proper function, should also maintain the integrity of its structure. This indeed is found to be the case if the induced current is applied immediately to the muscle affected. Duchenne's and Becquerel's works give us the best and most satisfactory information upon these points, and encourage us to place much faith in the application of galvanism to the muscles in which fatty degeneration has commenced its ravages.

In taking leave of Dr. Roberts's book, we can only express the gratification we have experienced in its perusal. It contains a good summary of what is known on the subject of progressive muscular atrophy, and is evidently written by a man who thinks and observes for himself. We cannot quite consent to the propriety of the term which Dr. Roberts substitutes for that selected by Cruveilhier; it does not appear to us more distinctive or definite. It is well not to multiply names, but where a change is required, it should be made so as to insure a more ready comprehension of the meaning to be conveyed. For our parts, we think that "Muscular Palsy" would,† by indicating at once the main symptom and the manifest seat of the disease, meet the requirements of the case better than Wasting Palsy; but we are content to abide by the term chosen by Cruveilhier, or perhaps, best of all, to name it after him, "Cruveilhier's disease." One suggestion, and we have done. We think that Dr. Roberts might improve his tables: they are not always quite intelligible, nor have we been able by analysis in all cases to satisfy ourselves that the items and the totals correspond. He has evidently bestowed considerable labour upon them, but we think that by simplifying them, the end he has in view, of laying certain results more palpably before his readers, would be more completely and readily gained.

* British and Foreign Medico-Chirurgical Review, Oct., 1856, p. 457.

† This was written before we had received Dr. Friedberg's book on Myopathia, which will be reviewed in our next.—ED.

PART SECOND.

Bibliographical Record.

ART. I.—*Ophthalmic Hospital Reports, and Journal of the Royal London Ophthalmic Hospital.* Edited by J. F. STREATFIELD. Nos. 1–3.—*London*, 1857–58.

A PERIODICAL of moderate price, recording the march of ophthalmic science in this country, has been a desideratum. The 'Annales d'Oculistique' and the 'Archiv für Ophthalmologie,' afford valuable means for inter-communication on the Continent. With us, though the medical serials are open to communications on ophthalmology as on other branches of medicine, ophthalmologists have not possessed a special vehicle for their literary efforts, and their productions have not received that prominence which the importance of the subject demands.

We are happy to say that in the journal before us there is promise that due regard will be had to the quality of the papers published, that controversy is to be discouraged, and regard is to be had to the standard of the articles accepted; indeed, if we may judge from the contents of the three first numbers, a prosperous career may be predicted for this our youngest literary brother.

The history of the 'Ophthalmic Journal' is briefly this. The medical staff of the Royal Ophthalmic Hospital, conscious of the extent of the field presented to them at that institution, decided, on the 25th of August last, to issue a periodical record of ophthalmic observation and experience, considering that at Moorfields alone much valuable information is gained and lost that might be advantageously preserved. With proper liberality they have opened its pages to other contributors, and the papers that have yet appeared are of a high order.

The contents of each number are arranged under three heads—Facts and Observations, Original Communications, and Quarterly Reports of the Operations at Moorfields. Of this division we entirely approve, and also of the practical character of the various communications.

The first number contains a brief but interesting account by the editor, Mr. Streatfield, of a plan that he has adopted with success, for reinstating the pupil when obstructed by false membrane; a valuable paper by Mr. Bowman, On the Treatment of Lachrymal Obstruction; and a third by Mr. Poland, On Protrusion of the Eyeball.

The second number is principally devoted to the retina, and to the treatment of glaucoma, first suggested by Von Gräefe, which has excited an active controversy on the Continent and in Great Britain. Besides a paper by Mr. Critchett on the subject, there is a valuable article by Mr. Hulke, On the Morbid Changes which occur in the Choroid and Retina, with the Ophthalmoscopic Appearances to which they give rise.

Colloid disease of the choroid, a recent addition to our knowledge, mainly due to the labours of Donders and Heinrich Müller, is accurately described. It must not be confounded with colloid cancer, to which it has no affinity; it is characterized by the inner surface of the choroid being studded over with small transparent, highly refractory bodies, which project more or less from the surface, and have a globular, club-like, or button-like outline. Donders places their origin in the hexagonal pigment cells; Müller, on the other hand, considers them adventitious thickening of the elastic lamina supporting the hexagonal epithelium; Mr. Hulke states that the appearances favour the supposition that they have their origin in the chorio-capillaris beneath the elastic lamina. The colloid globules are very prone to calcify, forming strong glandiform masses or beads of a dull glassy aspect. This peculiar disease is most commonly met with after middle age, and its earlier stages are not attended by any appearances which can be appreciated without the microscope; but later, the choroid presents under the ophthalmoscope a blotched, patchy appearance, due to the irregular distribution of its epithelium.

Dr. Bader has written an interesting paper on an obscure disease of the retina, which he proposes to term "Softening of the Retina," and we regret that our space will not admit of our doing more than bespeak attention to this communication, which has been drawn up with great care, and is illustrated by several figures in chromo-lithography. The introduction of superior illustrations is a feature in the journal, and increases its utility.

The third number is more varied in its contents, and the circle of its contributors is extended. There is an account by Dr. Von Gräfe of his operation for glaucoma, which he considers to have been misunderstood and mis-stated in a recent paper; though the editors have wisely determined, as a rule, to reject controversial communications, this is a proper exception, enabling us to comprehend precisely the views and the objects of the able Berlin professor. There is a paper by Mr. White Cooper, illustrated by several sketches, giving an account of a very singular microphthalmic family, in which three children of perfectly healthy parents present in a marked degree arrested development of the eyes, of the teeth, and of the frame generally. Mr. France contributes a curious case of duality of pupils in both eyes; and singular examples of coloboma iridis and eccentric position of the pupils are recorded by Mr. Dixon and Mr. White Cooper. Dr. Bader has furnished the particulars of the ophthalmoscopic and histologic appearances in certain cases of near and weak sight; and Mr. Streatfield describes a new mode of treatment in entropium and

trichiasis, by excising a strip of the tarsal fibro-cartilage parallel with the margin of the eyelid.

Thus we have presented to us a liberal and varied bill of fare. If the work be conducted on the same principles and with the same care that is apparent in the first three numbers, the editor looking to the quality of the communications without favour or affection, and rejecting such as have a marked tendency to self-laudation, the Journal will be received with respect, and cannot fail to meet with the support of the profession.

ART. II.—*Transactions of the Medical Society of the State of New York for the Year* 1858.—*Albany*, 1858. pp. 655.

CONSIDERABLE activity appears to prevail among the medical men of the United States; we often have occasion to introduce the results of their labours to our readers, commonly in terms of high praise, though at times not without some expressions of disapproval. The volume before us is the produce of a single State, which has other important literary representatives. The papers, partly only of local and personal interest, partly presenting features deserving of the attention of the profession at large, show the practical tendencies of our American brethren, and prove that they do not belie their relationship to the mother country.

The President of the Society, Dr. Millard, opens the 'Transactions' with an able hygienic address on air, exercise, and light, delivered before the members of the Society and the members of the Legislature. Three biographical sketches of medical men well known in the State of New York—Drs. Ely, Spencer, and Reynolds, follow. Dr. Van Buren supplies a well-written history of anæsthetics, while the employment of amylene is discussed by Dr. Orton. The two next papers treat of cerebro-spinal meningitis, which appears to have been epidemic of late in some parts of the United States, as it has also in Europe.* These papers are both very loosely drawn up, and the evidence of the spinal meninges having been uniformly affected is not satisfactory; the account of the cases related is extremely scanty, and neither of the observers appears to have instituted any post-mortem examinations, although unfortunately the disease was very fatal. Dr. Thomas, the author of the first paper, states that the disease "prevailed with a fearful ratio of mortality in central and western New York" in the spring of 1857. He describes it thus:

"In some cases its approach is accompanied by some disturbance of the stomach and bowels—as slight nausea and vomiting, and moderate diarrhœa. This state is accompanied with slight chills and pain in the head and back, sometimes very severe. After a short time, perhaps a few hours, these slight disturbances of the system will be followed by delirium, an anxious countenance, great restlessness, cool or cold skin, and a frequent irregular pulse. As the disease progresses, their muscles become affected with spasmodic contractions, and especially those of the back and neck, which often assume a permanent

* The reader will find an account of the epidemic of spinal congestion observed at Niort, in France, by Dr. Gaunè, in the Medico-Chirurgical Review for April last, p. 539.

rigidity, confining the body or neck to one position for days, and sometimes even weeks. In the strongly-marked congestive form the irregular pulse, the cold surface, and coma more or less profound, usher in the disease. In the course of a few hours, reaction becomes established, the pulse becomes frequent and strong, and not unfrequently retains its irregular action; the skin hot and dry; urgent thirst, severe pain in the head and back, local and general spasms, torpid bowels, irritable stomach, ejecting green morbid secretions, delirium, head drawn to one side, or fixed firmly backward, petechiæ, and an irritable and excessively tender surface. The morbid condition of the tongue varies with the progress of the disease; in the early stages being slightly covered with a white or yellow brown fur, and later in the disease, dry and red, or a dark brown coat is found to cover it."

The disease appears to be of a malarious origin, prevailing in parts where all the causes of malaria exist; and hence it occurs during the damp weather of February, March, and April, and again in the months of November and December. In spite of this theory, which appears to be borne out by facts, the influence of the old phlogistic doctrine, and the corresponding belief in the antiphlogistic powers of mercury, induce both Dr. Thomas and Dr. Kendall to trust mainly to large doses of the salts of this metal. The results of their treatment, however, were by no means satisfactory, and we think that if they adhered consistently to the malarious theory, and treated any future cases with quinine and brandy, they would be able to render a more favourable report. The 'Transactions' contain several other medical, obstetric, and surgical papers, to some of which we may be able to refer in the Quarterly Reports.

We cannot, however, conclude this notice of them without adverting especially to the very elaborate paper of Dr. Brinsmade, the Vice-President of the Society, in which he registers the results of an analysis of all the cases of disease which have occurred in his practice during twenty-one years. Having preserved records of all the cases which he has attended since the commencement of his professional career, he publishes the analysis, in order to serve as an inducement to his medical brethren to follow his example, so as to obtain a fair insight into the endemic and epidemic constitution of the State of New York. Valuable as are mortuary registers, such a registration of diseases would prove of equal, if not of greater value, in determining various sanitary questions. We wish Dr. Brinsmade every success in his undertaking, and would be glad to find that medical men generally, in his own and in other countries, kept as complete registers as he has done, so that it were possible to secure an uniform system of registration of disease.

ART. III.—*Health and Disease, their Laws; with Plain Practical Prescriptions for the People.* By BENJAMIN RIDGE, M.D., F.R.C.S., &c. &c.; author of 'A System of Glossology,' &c. &c. —*London*, 1858. pp. 624.

EVERYTHING in nature presents itself to the author of 'Health and Disease' in a triple form; gender, for instance, is masculine, feminine,

and neuter; in arithmetic we have the rule of three; the chemist has acid, alkaline, and neutral agents; and the threefold arrangement is maintained in the constituents of the human body, which is divided into hard, soft, and fluid substances, each of which again admit of a threefold subdivision. Diseases follow the same law, for their diagnosis is divisible into the exciting, the proximate, and the latent cause,

"All of which may be proved and verified by means of, (*a*) the tongue, (*b*) the pulse, (*c*) the history and general symptoms of the case. The characters of disease, in like manner, have three divisions—(1) acid, (2) alkaline, (3) neutral; their subdivisions, again, being (*a*) organic, which are incurable; (*b*) functional, which are curable; (*c*) nervous, which are remediable."

Again,

"The laws of diet and hygiene may be classed under three distinct divisions —(1) the ingesta, that produce in the system, through their ultimate reduction by digestion, a great amount of acidity, as, for instance, all boiled and salted meats, as well as direct acid diets; (2) those which, in their last actions of digestion, produce less acidity than the former, or are in their action on the system when first taken of a decidedly alkaline character, as roasted and broiled meats, &c.; (3) the neutral elements of diet, which seem to have no influence in either way."

Dr. Ridge, in spite of his affection for number three, is not consistent in his attachment, for the number of chapters which he devotes to the consideration of the various topics connected with health and disease, is neither three nor a multiple of three, but twenty-five; in them, as may be supposed from the samples taken from his introduction which we have given above, he favours us with a large amount of hypothesis, for which we must refer our readers to the book itself.

Those who have patience to peruse the 624 pages of the work, will find some practical wisdom underlying the writer's hypotheses; but we fear, that from addressing himself at the same time to the medical and general public, he will experience the fate of those who essay to sit upon two stools at once. If he could be prevailed upon to give in a diminished form some of his chapters—as those on nurses and nursing, and on ventilation, with the essential practical points contained in some of the other chapters, shorn of verbiage and hypothesis, he would insure for himself a wider circle of readers than the bulky volume now before us is likely to obtain. His precepts might then become what they now profess to be, plain and practical, and the people might learn to appreciate them.

ART. IV.—*Archives of Medicine.* Edited by LIONEL S. BEALE, M.B., F.R.S., Physician to King's College Hospital, &c. No. II.—*London*, 1858. pp. 53 to 160.

THE second number of this interesting periodical is now before us, and we are able to recommend it to our readers as strongly as we did its predecessor. The information it contains is very varied, and although the learned editor favours us with several communications himself, several of his own immediate colleagues and other observers have come

to his support. The number opens with the conclusion of Dr. Todd's remarks on the treatment of acute inflammations, in which the author brings forward his arguments for the employment of alcoholic stimulants in these affections; Dr. Scott Alison follows with a description of his new instrument, the stethogoniometer, destined for measuring the inclination of the thoracic walls. Dr. Farre supplies an article on the exfoliation of the epithelial coat of the vagina, in which he maintains that the current view regarding the form of that passage is erroneous; Dr. Johnson next gives cases showing that solidification of the lung does not necessarily increase vocal vibration; cases of calcareous deposit in the brain by Dr. Ogle, articles by Dr. Guy, by Mr. Henry Lee, by Dr. Marcet, Mr. Hulke, Dr. Handfield Jones, and Dr. Von Bose, besides several valuable papers by the editor, contribute to make this number very attractive. Most of the papers are well and intelligibly illustrated.

ART. V.—*Treatise on the Principles and Practice of Veterinary Medicine and Surgery.* By WILLIAM HAYCOCK, Veterinary Surgeon and M.R.C.V.S. Part I. pp. 160.—*London*, 1858.

2. *Edinburgh Veterinary Review and Annals of Comparative Pathology.* Published quarterly. No. I., July, 1858.—*Edinburgh.*

WERE it not that Mr. Haycock coquets with homœopathy, and occasionally speaks of mercurius instead of mercury, of calendula, rhus toxicodendron, and other remedies specially favoured by homœopathists, evidently with a view to captivating the wealthy supporters of the sect who may also be the proprietors of large studs and stables, his pathology and his mode of treatment would not have led us to suspect the tendency. Dr. Anthony Todd Thompson, Dr. Druitt, Dr. Copland, are among the authors quoted in support of the author's doctrines; while Dr. Dudgeon is the only homœopathist whose opinions are brought forward, and only for the purpose of allowing Mr. Haycock to maintain as a sort of placebo to his conscience, that "cases do occur in which it is necessary to resort to other than homœopathic means for their cure." His homœopathic colleagues will scarcely admit his orthodoxy as a member of their faith. A dose of two ounces and a half of tincture of opium (p. 86), two-drachm doses of ammonio-citrate of iron (p. 89), six-drachm doses of aloes (p. 143), four to six drachm doses of aromatic ammonia (*ibid.*), are as legitimate doses for horses as one can desire, and throughout the book we find none but the most superficial indications of a practical leaning towards homœopathy. The book is in all other respects a good one, and but for the ethical principles involved, we should entirely agree with the author's views, which are lucidly expressed, and in accordance with the teachings of science.

We can more unreservedly speak in praise of the second work at the head of this notice; the initial number of what, we sincerely trust, will prove a valuable series of quarterlies, devoted to the science of veterinary medicine in all its important bearings. We believe that

great as England has long been in the practical development of its agricultural resources, and the promotion of horse and cattle breeding, and sheep-farming, and in the cultivation of all the domestic animals that surround us, our countrymen are behind other countries in the appreciation of the intimate relation existing between human and veterinary hygiene and medicine. We owe a great debt of gratitude to the brute creation for the numberless aids they have afforded us in the advancement of human anatomy, physiology, and medicine; we shall continue to derive manifold benefits for ourselves in advancing the knowledge of the relative influences that affect both great classes of the creation, while we may hope, in many ways, to improve the condition of the animals committed to our charge, by a careful study of the sanitary and morbid conditions that affect them. Dr. Lindsay, in an admirable article contained in the first number of the 'Veterinary Review,' well developes and fortifies the position that "the studies of the diseases of man and the lower animals borrow and reflect light mutually on each other;" and he goes on to show that the transmission of disease from man to animals, and *vice versâ*, are governed by laws which it is manifestly and palpably our common interest to study. It was but in our last number* that we drew attention to the labours of Mr. Gant, who has shown the error our agriculturists run into, when they regard the artificial fattening of animals as the only object to be pursued by cattle clubs; the various inquiries connected with the hygienic influence of Smithfield and the slaughter-houses of London, with the influence of scrofulous cows upon the health of our infant population, with the deterioration of our streams upon the health of their finny inhabitants and the food of the population, need only be adverted to, to crowd upon our minds reasons why, in an hygienic point of view, veterinary medicine and comparative pathology deserve to rank high in the estimation of men of science and of the population at large.

The first number of the 'Veterinary Review' is, as we have said, full of promise; the right spirit appears to animate the writers, and they have a large and important field to cultivate. Besides Dr. Lindsay, whose article we have already quoted, Mr. Gamgee, Sen., Mr. John Gamgee, Mr. Dudfield, Mr. Dickinson, and Mr. Horsburgh, contribute original papers; while reviews, editorial articles, extracts from British and Foreign journals, and other matters bearing upon animal hygiene and medicine, occupy the remainder of the number.

ART. VI.—*Silver Sutures in Surgery:* The Anniversary Discourse before the New York Academy of Medicine. By J. MARION SIMS, M.D., &c.—*New York*, 1858. pp. 69.

THE substance of Dr. Sims' views is contained in the two following extracts:—

"The next eight years will not find an educated physician anywhere who will dare to use silk sutures, for the silver thread will now become as essen-

* July, 1858, p. 173.

tial to the dressing-case as the needle itself; and if I may be allowed to venture a prediction, I will say that fifty years hence the statistics of our hospitals will show a vast improvement in their bills of mortality after great operations, and this improvement will be due mainly to the use of silver as a suture." (p. 45.)

"With silver there is no inflammation, no suppuration, no cutting out of sutures, no gaping or retraction of flaps, and therefore no necessity for disturbing the dressing till all is firmly united and permanently well."

The author tells us that these are no vain imaginings; for although enthusiastic (which we can readily believe), he is not wildly so; we must, nevertheless, descend from the exalted ground from which he would have us view this subject, to the consideration of the particular ways in which a silver wire differs from any other. In the first place, then, it must, we think, be allowed, that a metal ligature is not so irritating as silk, and that it will remain in the living tissues without being the cause of irritation for a very considerable period. Secondly, a metal ligature secures the perfect and undisturbed apposition of the parts, which it directly holds in contact; and thirdly, it maintains that contact without exerting that strain and tension which an elastic thread must necessarily in some measure produce. That these may be important objects in the practice of surgery we would be the first to allow, but that they would prevent a wound from ever taking on an unhealthy action or from suppurating, we cannot stretch our imagination sufficiently to believe.

Dr. Sims informs us that, in 1845, he "conceived the idea of curing vesico-vaginal fistula." (p. 9.) . . . . "After nearly four years of fruitless labour, silver wire was fortunately substituted for silk as a suture, and, lo! a new era dawns upon surgery." !!

We must be excused for reminding Dr. Sims and our readers, that in the 'British and Foreign Medical and Chirurgical Review,' for April, 1846, we drew attention to the employment of platinum wire as a ligature thread; and as early as the year 1832, Dr. Mettauer, of Virginia, employed metallic sutures with perfect success. In the thirteenth volume of the 'American Journal of Medical Sciences,' for 1833, Dr. Mettauer relates very modestly the particulars of a case of recto-vaginal fissure which he had thus treated.

We cannot, therefore, allow that Dr. Sims first conceived the idea of curing vesico-vaginal fistulæ, or of employing metallic ligatures in their treatment. Nor do we regard the tone of self-assertion which pervades the Discourse compatible with the dignity of science, or suitable to the occasion when it was delivered.

ART. VII.—*Urethro-Vaginal and Vesico-Vaginal Fistules, &c.* By N. BOZEMAN, M.D., of Montgomery, Ala.—*Montgomery*, 1857.

Dr. BOZEMAN's ingenious and important contrivance of the shield-suture as a means for the cure of vesico-vaginal fistulæ, has been described on a former occasion in this journal. In the present publication the author gives a valuable account of the nature and varieties

of this distressing accident, of the particular modifications of his method required for the treatment of the different forms, and a history of cases. He classifies the cases under five heads:—

1st.—All fistulæ causing a communication between the urethra and vagina.

2nd.—Fistulæ established at the expense of the trigonus vesicalis.

3rd.—Those situated in the *bas-fond* of the bladder.

4th.—Those formed at the expense of a part or the whole of the vesical trigoni and the root of the urethra; of the trigoni and *bas-fond* of the bladder; or, all three of these regions together.

5th.—Includes all fistulæ complicating the cervix uteri, either with or without injury of this organ. Of the author's 37 fistulæ, 11, or about one-third, were of this last description.

Of the first class, Dr. Bozeman considers an injury, not strictly a fistula, consisting in a rent of the urethra extending from the meatus backwards, as the most unfavourable form of all the urethral injuries. The shortening which the urethra undergoes is attended by an irritable condition of the sides of the rent, and an escape of a small portion of the urine into the vulva during micturition, the fluid running down upon the thighs, and adding very much to the annoyance of the sufferer. Dr. Bozeman says the following procedure, simple and easy to perform, is suitable to this case. Pare the edges of the cleft; introduce sutures in the ordinary way. The difficulty of getting rid of the action of the urine passing along the newly-formed channel is a chief obstacle to success. Dr. Bozeman meets it by a modification of his button or shield. The catheter, which is indispensable, has no support at the meatus, and hence its motion irritates and prevents union by first intention. To obviate this, the button is moulded to the curve of the urethra, and a notch is made at the lower end. When this is secured *in situ* by compressing shot upon the several sutures, the end of the notch projects forwards and in front of the meatus, forming a stationary point upon which the catheter may rest, without in the least interfering with the denuded edges of the cleft. The catheter should be introduced before securing the button, and ought not to be removed until the cure is complete. When the suture-apparatus is removed, the catheter should be cleansed, replaced, and worn for three or four days longer. The pressure may now be taken off from the tender cicatrix by means of a loop attached to a bolt carried round the body of the patient. If this precaution be not observed, and the catheter be allowed to hang down, the rent is almost certain to be reproduced. The English male elastic catheter No. 5, is the best.

With regard to the treatment of fistulæ involving the cervix uteri, Dr. Bozeman points out the difficulty of execution of Jobert's operation of cystoplasty, the danger of peritonitis attending it, and the risk of failure. He proposes another procedure, which he describes as simple, easy, and almost free from danger. In 1855, he met with his first case of vesico-vaginal fistula, complicated with a rent in the cervix uteri; he then first demonstrated the practicability and safety of

paring the cervix and lodging sutures in its substance. This dealing with the cervix is one essential principle of his operation; the other, is the drawing down of the uterus to the level of the fistula. The form of the button is of course adapted to the peculiarities of the individual case. The author does not find any inconvenience from the drawing down of the uterus, either from dragging upon the sutures, or from the displacement. The uterus rises again after the operation. We must refer our readers to the pamphlet itself for the minute details of the operations required in this class of cases. We feel bound to state that the work is a very valuable practical contribution to the surgery of the genito-urinary passages. By ingenuity and perseverance it is now possible, by availing ourselves of the improvements introduced by Dr. Bozeman and others, to cure many cases of a most distressing affliction which a few years ago were regarded as hopeless.

ART. VIII.—*Essay on the Pathology of the Blood and its Containing Vessels.* By THOMAS WISE, M.D., F.R.S.E., F.R.C.P.E., late Hon. East India Company's Medical Service.—*Edinburgh*, 1858. pp. 388.

THIS essay consists of two parts. The object of the first part when it was first published in Calcutta, in 1831, appears to have been to describe "the changes in the blood" which occur in "vascular diseases." While the author was still absent in India, the first part of this essay was reprinted in London about ten years ago. To that he has now added a reprint of the second part, on the diseases of vessels, with a few of the drawings in explanation of his views. We think the author has done himself injustice by connecting these two parts together in one volume. When we consider the very valuable additions which have been made to the pathology of the blood by Virchow, Kirkes, Hasse, Paget, Vogel, Becquerel and Rodier, and others, the first part of this book bears evidence of most antiquated pathology. The second part treats of the diseases of the arteries, veins, and capillary vessels, in the space of 104 pages. The whole work bears evidence of much learning, and numerous references are given to valuable facts; but we do not perceive that any new matter is added to science by this publication.

ART. IX.—*The Ophthalmoscope, its Mode of Application Explained, and its Value shown in the Exploration of Internal Diseases affecting the Eye.* By JABEZ HOGG, Assistant-Surgeon to the Royal Westminster Ophthalmic Hospital, Vice-President of the Medical Society of London, Member of the Council of the Microscopical Society of London, &c.—*London*, 1858. pp. 107.

THE ophthalmoscope has become one of the recognised appliances of ophthalmic surgery, and its value is more apparent as its use is better understood; indeed, it is only by practice that any observer can

become familiar with the normal appearances of the interior of the eye, and can recognise the significance of the changes which indicate a departure from its natural state. A magnificent work is now in progress of publication at Vienna,* illustrating the morbid conditions of the eye as seen by means of the ophthalmoscope, but the author having the advantage of the "State Press," is enabled to bring out his work at a cost infinitely more moderate than could possibly be done in England, where the expense of good illustrations is fearfully great. Yet without *good* illustrations, no book professing to exhibit the morbid conditions of the eye can be taken as a safe guide.

In this respect the work before us, excellent in many points, falls short of what might be desired. The first plate (which is printed in colours) is an approximation to the object in view, but several of the woodcuts entirely fail to convey an idea of the truth. It may be that the copy before us is an exception, but the cuts at pages 89 and 96 are decided failures, partly from the imperfection of the printing.

After a sketch of the history of the ophthalmoscope, Mr. Hogg describes the manner in which it should be used, and adduces various authorities in its favour. The remainder of the work is devoted to the investigation of normal and morbid changes seen by the aid of the instrument. The subject is elucidated by a great variety of facts and cases from several sources. The following throws light on that obscure disorder of vision, nyctalopia—day-blindness.

T. J., aged forty-six, a sailor in the Queen's service, was sent home from Constantinople. In the month of March, while in the Crimea, after having lived entirely on salt provisions, without vegetables, and at the same time exposed to the cold winds and hail during three weeks' work in snow, he was attacked with a bleeding at the nose, which lasted five hours and ceased when he was made warm and comfortable. Four days after this, his eyes were much inflamed, and ran with water. This attack lasted about three weeks, during which time he found he could only see about nightfall. He was unable to find his way about, and was sent to hospital, where he was cupped, blistered, and bled from the arm until he was much weakened, his sight not being in the least improved. He was then sent to the hospital at Constantinople, and a tonic plan of treatment prescribed. After a short lapse of time, he was able to work a little by night, but not at all in the daytime. He tried several kinds of spectacles without benefit. When examined with the ophthalmoscope, the lenses appeared perfectly clear, the retinoid vessels large, numerous, and distinctly seen; over the papilla optica a bluish patch and a general greyish appearance of the optic nerve; the fundus had a curiously mottled appearance, rather dark in colour, like pigmental masses seen through a red gauze; one spot larger and darker was fixed near the inner side of the optic nerve. There were many floating specks of a dark grey colour. The patient was ordered to take mist. ferri co. thrice daily, and continued to do so with advantage until he left England for his home in Hanover.

* Beiträge zur Pathologie des Auges, von Dr. Eduard Jaeger. See No. xxxiii. of this Review.

The perusal of this case impresses us with the conviction that the nature of the disorder was thoroughly mistaken in the commencement, and that the depleting system adopted was the worst that could have been devised. The connexion of hemeralopia with scurvy has been satisfactorily established, and though the opposite condition of sight existed here, that in reality was of small importance, whilst there can be no doubt that the poor fellow's blood was impoverished and his system exhausted by the salt diet, the hardships, and the profuse epistaxis. Had he at first been well fed and liberally supplied with bark and lemon-juice—a valuable combination—his chances of recovery would have been a hundred-fold greater. The nature of the case seems to have been recognised in the hospital at Constantinople, and also by Mr. Hogg, whose treatment was most judicious.

For other cases we must refer to the little volume itself, which is a useful manual, and may be consulted with advantage by those interested in the study of the eye.

ART. X.—*Die Complicirten Luxationen.* Von Dr. ALBERT SCHINZINGER, Privatdocent an der Universität Freiberg.—*Lahr*, 1858. 8vo, pp. 53.

Compound Luxations. By Dr. ALBERT SCHINZINGER.

THE author adduces four cases of compound luxations which he had an opportunity of observing; to wit, one of the radio-ulnar joint, which did well after removing a portion of the ulna; one of the knee forward, from which the patient recovered after secondary amputation; one of the tibia inwards and of the foot outwards, where, after removing a portion of the bone, recovery ensued; one of both tibia and fibula forward and outward, a somewhat rare accident, where the articular end of the fibula was removed and a cure effected. These cases are given in full detail, and the treatment throughout seems to have been judicious. The author next discusses the occurrence and frequency of compound luxations in the different joints, deducing his facts from the writings of Cooper, Malgaigne, and others. He then enters very fully into the treatment of accidents of this description, under the heads of re-position, resection, and amputation, pointing out the leading indications which ought to guide the practitioner.

ART. XI.—*Summary of New Publications.*

AMONG the numerous works received during the past quarter, which is always the least productive in the *annus medicus*, we would first signalize as deserving special attention Mr. Gray's 'Descriptive and Surgical Anatomy,' a work that offers many attractions, and bears evidence of much labour having been bestowed upon it. A physiological monograph 'On the Thymus in Health and Disease,' by Dr. Friedleben of Frankfort, also gives promise of instruction as the uncut pages yield to the paper-knife. Physiology is further represented by a second number of Dr. Schiff's 'Physiological Manual.'

Hygiene and sanitary science bring us an important and elaborate document by Dr. Greenhow, illustrating the preventive influences that we may exercise over death and disease; Dr. Robertson favours us with a pamphlet on sanitary science, with which may be coupled a *brochure* by the Rev. H. Leach, 'On the Dwellings of the Poor.' Dr. Gavin Milroy's paper 'On the Sickness and Mortality of the French Army during the Campaign in Turkey and the Crimea;' and Mr. Jeffrey's work 'On the Clothing, Housing, and Employment of the British Army in India,' may be mentioned under the same head. Dr. Silvester issues a reprint of a valuable paper 'On the True Physiological Method of Restoring Persons who have been Submerged;' and from Oxford we have received Dr. Acland's 'Report on Fever occurring in the Parish of Great Horwood, in Buckinghamshire.'

An important and laborious treatise 'On the Pathology of the Urine,' by Dr. Thudichum, illustrated in the style of Funcke's well-known Atlas, has come to hand, to which, with most of the works that we can now only dispose of thus summarily, we hope soon to advert more fully. A much enlarged edition of Dr. Radcliffe's well-known work 'On Epilepsy and other Convulsive Affections' has appeared, in which the author enlarges upon, and brings forward fresh arguments in favour of, those views regarding the nature of muscular contraction which are peculiarly his own, and which we analysed when reviewing the first edition. The second part of Dr. West's excellent 'Lectures on the Diseases of Women,' has made its appearance. Dr. Beale continues the periodical publication of his work entitled 'The Use of the Microscope in Clinical Medicine,' of which the third number is before us.

A highly interesting book, equally interesting to the surgeon and to the physician, comes to us from Dr. Friedberg, of Berlin, in which the author treats of muscular palsy and degeneration in a more extended sense than that in which it has hitherto been regarded. Though too late for review in conjunction with Dr. Roberts's work, and though open to various objections, we are able to recommend it already as deserving to be attentively studied. The fifth fasciculus of Mr. Maclise's illustrated work 'On Dislocations and Fractures' has appeared.

Mr. Hood, in a well written pamphlet, records his experience in the treatment of sprained ankles by supporting bandages, applied firmly to the part after subsidence of the first swelling and redness. As a valuable contribution to the history of medicine, we would specially mention Dr. Wilson's elaborate and instructive paper 'On the Existence of the Castor Fibre in Scotland.' Besides the new Journals spoken of in the Bibliographical Notices, we have to introduce to our readers the first number of the 'New York Dental Journal;' the Irish Roman-Catholic Journal, 'Atlantis,' contains in its second number some very erudite articles. In conclusion, we would advert in strong terms of commendation to a 'Manual of Photographic Manipulation,' by Mr. Lake Price.

PART THIRD.

Original Communications.

Art. I.

The Bloodletting Question in Olden Times. By Thos. K. Chambers, M.D., Physician to St. Mary's Hospital.

Now that the question of the propriety of bloodletting in the treatment of disease seems again likely to invite public attention, it may be amusing to our readers, and perhaps not unprofitable, to reflect on the way in which the same subject affected men's minds seventeen centuries ago. It is the duty of an historian to avoid partisanship, and not always wise for him to prophesy; still, it must be remarked that the danger to which we are now drifting seems to be not dissimilar from the prejudices which Galen then found prevalent in Rome. The risk to a patient now is, not as twenty years ago, that he will be bled unnecessarily, but that bleeding will be abstained from when really requisite. Human nature does not alter, though some fallacies leading perhaps to strangely different results enter into our minds through the same portals as they did into our forefathers; and the records of the past, rightly interpreted, cannot but teach wisdom for the future. Let us then fancy ourselves in the metropolis of the world, prosperous and glorious under the rule of the Antonines, in the latter half of the second century, and let us hear Claudius Galen lecture the public in his pleasant chatty style 'Against certain Erasistrateans:*

"When I first came to Rome† I found some physicians who were so averse to venesection, that sometimes, when a man was scarce able to breathe from congestion, they would not employ this treatment. There was a woman, just under twenty-one, who, after suppression of the catamenia, had a flushed face, with loose cough and dyspnœa, whom they treated by bandaging the limbs and depriving her entirely of food; but they would neither open a vein nor let me do so. And on account of their being acquaintances of the woman's household and senior practitioners, more faith was had in their opinion than in mine. I made no more attempts to persuade them to bleed, but I asked if there was any objection to set up a derivation of blood towards the uterus by means of drugs calculated for that object. And when they consented, I immediately got the midwife usually employed by the patient, and desired her to use them. But she said she had already applied remedies of this sort at the proper time—namely, when the catamenia might normally be expected; and she named the drugs—all of tried efficacy—which she had administered to the woman, so that

* Galeni Opera, Edit. Kühn, vol. xi. p. 187.
† viz., in the thirty-fourth year of his age, A.D. 165.

no one could suppose that it was from the inefficiency of these medicines that relief had failed to be given. When I heard this, and moreover that the menses had been already suppressed four months, I had another consultation with the medical men to try and persuade them to bleed. When they refused I wondered why, if they were anxious to evacuate the superfluous blood through the uterus by opening the mouths of the numerous veins there, yet they should think the evacuation injurious when it was made by opening any other vein. They stated that 'superfluous blood could be evacuated by fasting alone,' without having recourse to treatment such as I proposed. So I held my tongue and took my leave, in despair about the woman, on account of the cough and dyspnœa. I expected that she would either spit blood from the chest, or from the lungs by the bursting of a bloodvessel, or would have laryngitis, or pleurisy, or pneumonia; and my hope was, as a choice of evils, that she would have pleurisy, for I was afraid, in case of laryngitis and pneumonia, that the risk would be imminent, and that in case of hæmoptysis, that the occurrence of it would be fatal. And such turned out to be the result. For as she was coughing very violently, blood was thrown up. And now some non-professional persons complained of the doctors who opposed the bleeding, and hopes were expressed that now at least, though not before, they would be shamed into permitting the treatment. When they would not give way, but desired the bandages round the limbs to be tightened, and persisted in the attempt at derivation towards the uterus, and in continuing the starvation, I took my leave, persuaded that I could effect nothing on account of the gentlemen's age and celebrity. And very shortly afterwards the patient was seized with an incurable difficulty of breathing, and died.

"Under the hands of the same physicians who opposed bleeding there also died several patients with laryngitis. And there was another patient, too, who through the whole winter had been living high, and taking no exercise, and in the spring was as red in the eyes and face as a man kept for a long time with his head on the ground and his legs in the air, and he died suffocated after five days' illness.

"Next there was a fourth patient—a woman,—who was ill at the same time that the catamenia were suddenly stopped, whom these enemies to bleeding brought to death's door. They kept her for three days absolutely without food, because she had a continued fever; on the fourth day they gave her the smallest possible quantity of slops; on the fifth they ordered fasting again, and then she got violently delirious, jumped up, and ran screaming about out of doors, and the attendants had great difficulty in restraining her violence. She, however, was saved by nature, through a copious effusion of blood from the nostrils.

"This was a circumstance which should excite our admiration, and at the same time teach us what a powerful influence bloodletting has in such affections, for immediately after the hæmorrhage from the nostrils the woman was freed from all her symptoms.

"Now previously to this I had shunned having any communication with the medical men, guessing what they would say against the use of venesection. But since it was so very clear to all that the woman's life was saved by the evacuation of the blood, I recalled to their memory the fatal cases, expressing an opinion that perhaps those, too, would have been saved if they had been bled. And I gave sundry reasons for it. But these gentlemen involved the matter in a maze of words, twisting the argument round and round, and up and down, came to no conclusion. However, they at last ended by taking refuge in Erasistratus, stating that it was 'shown by him in his First Book on Loss of Blood that it was better to apply ligatures to the limbs than to bleed.'"

Here is the matter brought to a point. The "blood-funkers"

(*αἱμοφόβοι*)* with an absence of originality truly Roman, had no reason for refusing to bleed beyond the opinion against it of Erasistratus, who had been dead four hundred years. Strongly in accordance with human feelings must his teaching have been to have prevailed so long in opposition to that of Hippocrates and his successors! What could his grounds have been? In the first place, he was an anatomist,† and a morbid anatomist; and having observed in the bodies of those who died of febrile affections the arteries congested with black venous blood, whereas in animals or men killed in health they were empty, he thought that the cause of fever was the blood getting from the veins into the arteries. So, argued he, the best way of treating the disease is to keep the blood in the veins; and he tried to effect this object by the expedient of putting ligatures round the limbs. Secondly, he looked to local congestion as the originating cause of all diseases,‡ as well of those commonly called idiopathic as others, and consequently that the treatment of both must be the same. Seeing, then, before his eyes (we may suppose) septic or low fever get well the quicker without bleeding, his theory led him to conclude the same of all fevers, even when accompanied or caused by obvious local inflammation. Thirdly, he was a pupil of Chrysippus, who was a devoted Pythagorean, with religious scruples, probably of Egyptian origin, against shedding of blood, which scruples his shrewd courtly pupil§ replaced by physiological arguments. Fourthly, seeing that the blood was formed out of the food, he conceived that withholding nutriment was quite as sure, at the same time that it was a safer, way of diminishing the amount of circulating fluid than venesection. He therefore adopted the practice, common enough in France still, though not practised in Britain, of "*diête absolue.*"

* This was a nickname bestowed on them by a rich patient of Galen's who was converted to venesection by the rapid cure of his butler's ophthalmia. There is a good deal of humour in our author's account of the man meeting him with opened eyes as he got out of his carriage, and of the master's instantaneous invention of the Greek compound for the practitioners who had been trying their hands unsuccessfully on the case for months.—See his Therapeutical Essay on Phlebotomy, chap. xvii.

† See the quotation by Galen's opponents of their master's words in the first chapter of the lecture, "Καίτοι τὸ πλῆθος τοῦ αἵματος αἰτία τῶν νόσων." And again, Erasistratus is stated to have ascribed all febrile action to the blood becoming wedged (*σφηνωθῆναι*) in the arteries. Meth. Med., vol. vii. p. 2.—(Edit. Kühn, vol. x. p. 461.)

‡ Erasistratus was one of the founders of the great anatomical school of Alexandria in the third century before Christ. He first traced the origin of the veins to the heart, named the tricuspid valves, and assigned to them their true office, described the lacteals, distinguished the nerves of motion and sensation, and was bold enough to confess he could find no use for the spleen. He was a skilful operator, and invented a sound called for a long time after his name.

§ One instance of the *savoir faire* of Erasistratus is amusing. He was physician to Seleucus Nicator, king of Syria, and was consulted by him about the heir-apparent, Antiochos. He finds that the young man's complaint is love, and that the beloved object is one whom it will be easily understood would create no slight commotion at Court when known. So he informs the Sovereign mysteriously of the nature of the difficulty, saying that the poor youth will probably die unless he possesses the object of his affections, and that she is no other than Mrs. Erasistratus. His majesty, thinking, like the public in general, that medical men are their absolute property, begs the doctor to oblige him but this once, and save his heir's life by the gift of the lady. Then does our shrewd prophet interpret his parable—"*thou* art the man; it is *your* queen Stratonice that is the only available prescription." Strange to say, the king was unpractical enough to be persuaded by such an *ad hominem* argument.—Biographie Universelle, Article "Erasistrate."

Considering what we owe to Erasistratus as a physiologist, one feels grieved at being obliged to extract his arguments secondhand from the works of such a vigorous opponent as Galen. The above, however, seems to be a fair statement of the mode of deduction by which he arrived at a conclusion influencing practice so many hundred years after his death as Galen's time, and at a place so distant as Rome. But stay—before we wonder at their power, are we quite sure that closely analogous influences do not weigh with us now?

In the first place, are we not often too much governed by theoretical explanations of what is seen in the dead body? Are we not too apt to forget the difference between that flabby decomposing substance and the brimming whirl of life, such as we see it faintly pictured in a frog's web under the microscope?

Secondly. Do we not still see confounded into one common mass, and treated with one treatment, cases of low and cases of phlegmonous inflammation? We see, e.g., statistical tables of pneumonia, typhous and croupous together, where *all*, old, young, and middle-aged, have been bled, and antagonistic tables where *none* have been bled; and, as one might expect, the results are often numerically the same. Will not some one make a table of cases where those were bled who *ought* to have been, and where those were not bled who *ought not* to have been? It may be safely predicted, that a much larger percentage of recoveries will be exhibited. See how the same fallacy leads to opposite results; the Erasistrateans thought that low fever was, like inflammatory fever, only the manifestation of a local lesion; the first is better *without venesection*, so therefore must be the latter: the Broussaists, the other day, re-advanced the same doctrine, but as the latter is better *for venesection*, blood was taken from both.

Thirdly. Do we not often succumb to popular prejudices against what our reason tells us is right? John Hunter himself used unfairly the typical expression of the Mosaic writings, about the blood being the life, and a great deal of the horror with which venesection is popularly regarded seems due to the Hebrew synonym of "bloodshedding" for murder. Two days before the writing of this paragraph, a leading London physician, in stating to the writer his objection to bleeding, expressed himself as "*guiltless* of the blood of his patients."

Fourthly. Do not many of us commit the same error as Erasistratus, by thinking that the diminution of *quantity* is the only aim in taking blood? It is true that by starvation less will be made, and consequently that the total amount will be reduced. But then the reduction of quantity is only a temporary and partial result desired. The more important intention should be the improvement of *quality*, by removing some of the effete constituents destroyed by the disease, and so making room for as much fresh new blood as the system can furnish. Viewed in this light, bleeding can never have its place supplied by starvation; indeed, a sufficiency of nutriment is essential to its best success.

Let two things be remarked in the above quotation as to Galen's mode of educating his mind. First, that he allowed a few well

observed and striking instances to influence him more than any general impression derived from the diluted experience of extended practice. He may be right or wrong, but this is certainly his way of arguing, both with himself and his readers. Secondly, he lays great stress on Nature's mode of cure, and knows he cannot err in imitating it. Without hesitation he must be pronounced right in this.

The specimen given of our author's easy writing is quite sufficient to explain why our limits do not allow us to exhibit the way in which, through eight more chapters, he descends in a cataract of words upon the elderly practitioners of the metropolis. This is not by any means the only example of his unbridled tongue; he himself tells us of his concluding an argument with Σὺ εἶς μῶρος,* which, however true, is unconciliating to an adversary; and it is not surprising that he soon made his adopted home too hot to hold him, and went back to Pergamos after four years' sojourn, only to return when sure of Imperial patronage and protection against the natural consequences of such phrases.

A few observations, however, of his in the latter chapters of the above-quoted lecture must not be passed over. He says that loss of blood certainly does not increase the tendency to inflammation; because gladiators who bled much after incised wounds in the circus usually had their injuries healed by the first intention. On the contrary, it would appear that this unintentional venesection was a favourable occurrence. He makes also a pertinent remark on the danger of excessive abstinence —namely, that the stomach is apt to be so much weakened thereby, that food when afterwards administered is not digested. Again, he gives us a useful hint about the way in which the authorities of great names are sometimes quoted as having employed a particular line of practice, without any reference being made to the success, or the contrary, which attended it. Thus two instances accurately detailed by Erasistratus of the treatment of inflammation without bleeding were thrown in Galen's teeth by his opponents; but when they are looked into, behold, both are fatal! Dr. Markham has pointed out an instance of the same identical fallacy leading to an opposite result in our own day. Andral, led by the authority of his masters, advocates a practice of venesection justly called excessive. But when, with rare ingenuousness, he details the cases in which it was practised, a mortality much above the average is made evident.

Another tract written by Galen, seemingly about the same time, is a more direct commentary on the arguments employed not so much by the disciples as the master; it is entitled, 'On Bleeding, against Erasistratus,' and he may fairly be considered to have given a sufficient answer to all the arguments brought forward, at least at that period, against the practice.

Having thus cleared the ground of those gainsayers who represented venesection as in itself objectionable under all circumstances, he lays before the public his well-known 'Therapeutical Essay on Phlebotomy.'†

* See his treatise On the Presence of Blood in the Arteries. Edit. Kühn, vol. iv. p. 728.
† Edit. Kühn, vol. xi. p. 250.

He publishes this as an appendix to his lectures 'On the Preservation of Health,' and 'On Rational Therapeutics' (Θεραπευτικῆς μεθόδου λόγοι ιδ), not voluntarily, as he says at the end of the fifth chapter, but in consequence of external pressure; so that it may be looked upon in the light of an "Apology." It is a rich mine of suggestions, from whence authors have dug most freely, sometimes gold, and sometimes dross. So that it is rare to find any writing on the subject where, acknowledged or unacknowledged, at first, second, or third hand, some scraps of the loquacious Greek do not appear.

A specimen may be here given of his mode of referring his argument to rational physiology, extracted from a chapter in this essay, explaining how the heart and other organs are affected by changes in the fluids. By translating his imperfect chemical nomenclature into the more accurate language of the present day, it is singular how closely he is made to approach the speculations of our cotemporaries; and one sighs in reading the first sentence to think he should have hovered so near the brink of discovering the circulation, which the world was obliged to do without for fifteen centuries more. The way in which anatomy, chemistry, and mechanics are, as handmaids to physiology, brought to bear on practice, is quite in the style of the best school of modern rational medicine, and is really a model to be followed.

"The blood not only supplies nutriment to the organism, but continuously keeps up the animal heat, just as a hot-air stove is kept alight with firewood, so that the whole house is warmed by it.* As, then, this fire is damped, sometimes by too many logs being thrown on to it at once, sometimes not by their being in too great quantity, but by their being wet, sometimes by none at all or too few being put on; so the heat derived from the circulation may be reduced below its natural standard either by the supply of blood exceeding the demand, or by the demand exceeding the supply, or by the incombustible quality of the blood. Or, on the other hand, it may be increased either by the heating quality of the blood, or by the natural demand being absent. Now, whatever change takes place in the combustion at the centres of circulation is quickly communicated to the rest of the body.

"But occasionally, as I have often shown in other memoirs, some one organ alone is morbidly altered in temperature; and that may be referred to two sources—namely, sometimes to the heating or cooling nature of the fluids, sometimes to a morbid crasis only. Just in the same way the structure of the heart may be subjected to morbid changes in a demonstrable manner from two sources—namely, either from the combustibility or the contrary of the fluids, or from some deficiency in them.

"The quickness or slowness of this combustion has been demonstrated to depend on the amount of food and drink ingested, and on the amount of repose or activity of body and mind. But as in the abdominal viscera there often occurs perfect digestion, the ingesta degenerating into mucus or bile, or undergoing

* The Pompeian House at the Crystal Palace has misled many into a contempt for a Roman's notion of making himself comfortable in cold weather. But a different tale is told by the elaborate warming apparatus in the basement of Constantine's Palace at Treves (all brick architects should visit Treves); and the above illustration of Galen's shows that the application of the ἑστία (focus) to warm all the rooms in the house by tubes was common among all classes. There was only one stove in each tenement: for "focus" is used by Horace as a synonym for a country house; and by an economical arrangement, not only did that warm the apartments, but, as appears from Cato, was used also for cooking (Cato de R. R., 10 and 11), and for baking bread (id. 75).

some other morbid chemical change, or remaining entirely unaltered, and fermenting into flatus—just so when the blood-making functions are deficient, there will occur peculiar states of the fluids in arteries and veins analogous to the above-named derangements of digestion in the abdomen.

"Now since the heat and moisture of a substance seem to augment the rapidity of its chemical decompositions, especially if it be in a warm place, it will be a necessary consequence of this principle, that the nutritive matters distributed to the system through the medium of the abdominal viscera should undergo somewhere or other some species of decomposition, whenever they have not that tendency arrested by being converted normally into healthy blood. But since the decompositions which take place in matters capable of supporting combustion heighten the temperature, therefore the blood in which the above-described decomposition is going on will exhibit an abnormal degree of heat. . . . . When that high temperature is once generated, the whole body is easily heated by it, just as the whole house is by a great fire in the stove. This condition of system the Greeks call πυρετὸς (fever)."

The rules given by Galen for the guidance of the practitioner are too diffuse for "our limits." Perhaps under the pressure of editorial scissors he might have condensed his thoughts into theses more suited to citation. However, the guiding principles seem to be:

1. That you are not to treat the disease, but the man; that you are to judge of the propriety, the amount, and necessity for repetition of bloodletting, by the individual symptoms exhibited in each case, and not by the nomenclature.

2. That you are to observe, also, the natural constitution of the patient—e.g., the extremes of life, youth and old age, cause bloodletting to be badly borne. Certain races, such as the soft-fleshed Celtic nations,* do not stand it.

3. That you also take note of epidemic influences—e.g., not to bleed much in the dog-days (in Italy); and in moist, warm weather, when of course septic poisons are most rife.

4. That you are not to mistake physiological changes for morbid; such, for example, as the fulness of pulse which accompanies the first stage of digestion, for permanent fulness.

5. That you are to take blood from vessels which communicate directly (καθ ἵξιν) with the chief seat of inflammation.

6. That often, in spite of apparent or real general debility, it is desirable to take blood; since the benefit to the locally affected part, and the consequent benefit to the system, compensates for the depletion.

This rationalizing of Hippocrates' empirical practice of bleeding established it on a firm basis. So that, with the exception of an occasional "unhappy revival by some chemists" (Van Swieten), the theories of Erasistratus have not again raised their heads; and the future history of the controversy has an interest more curious than practical. We will not attempt to follow it here, but will confine ourselves to the sketch given of one period, which, even if not the most critical, certainly has found the most spirited historian.

* Galen means the true Celt with "milk-white throat," "golden hair" (Virgil), or "flaxen poll" (Claudian); his "love of drinking and brawling, loud voice, fierce eyes, and haughty insolence; and his equally pugnacious blue-eyed wife, with her swollen neck and gnashing teeth, using her fists like catapults." (Ammianus Marcellinus.)

Art. II.

On Physical Morphology, or the Law of Organic Form. By James Hinton, M.R.C.S.

In studying the development of the mammalian ovum my attention was struck by the form in which the laminæ dorsales make their first appearance. The layer of cells which constitutes the germinal membrane being completely formed, and separated at one point from the enclosing membranes, the laminæ dorsales rise up in this portion as two parallel ridges or folds. The thought suggested itself to me that interstitial increase of the germinal membrane, under the limiting influence of the external capsule of the ovum, must result in a folding of the membrane upon itself just in some such manner. If a flexible layer increase in length while its ends remain at the same distance from each other, it is wrinkled up; by laying a handkerchief on a table, placing the hands firmly upon it at a short distance apart, and gradually approximating them, such folds may be produced.

The idea thus suggested to my thoughts led me to further investigation, and many instances soon presented themselves in which the forms assumed by developing structures seemed at least to be distinctly traceable to the mechanical conditions that were present. The law which prevails so generally in the vegetable world, that buds are formed in *axils*, occurred to me in this light. For an axil is an interspace, a point of separation, at which the resistance to the outgrowth of the plastic material might naturally be supposed less than at other portions of the stem. Following this clue, I perceived that the conception of gemmation in axils appeared applicable, to a large extent, to the processes in which development consists. The eye and the ear bud out in the interspaces between the primary divisions of the encephalon; the vascular lamina is formed between the two layers of the germinal membrane; the allantois insinuates itself between the layers of the amnion, while the amnion itself and the ventral laminæ repeat the process observed in the formation of the laminæ dorsales.

Everywhere I met with facts of the like apparent significance: the coiling up of the intestines would be a simple result of the greater length of the bowel than of the cavity in which they are contained, and answers to a series of such foldings as I first referred to; the convolutions of the cerebrum would necessarily arise from the expansion of its surface within the cranium.*

Instances of this kind, multiplying indefinitely in whatever direction I looked, and becoming more convincing the more carefully they were examined, there was gradually forced upon me the perception that all organic form was determined by simple mechanical conditions. Which conclusion, startling as it appeared on its first enunciation, I had no sooner clearly grasped, than I perceived it to be self-evident. It presented itself to me thus:

* This has been observed by Mr. Solly in his work upon the Brain.

Organic form is the result of motion.

Motion takes the direction of least resistance.

Therefore organic form is the result of motion in the direction of least resistance.

This is the position which I now propose to illustrate and maintain.

Organic form is the result of motion. By this expression nothing more is meant than that, as we consider form to depend upon the position of the particles of which any body consists, so, in the case of organic bodies, these particles must have assumed their various positions by moving into them. I use it as a postulate in this abstract statement, because it is the simplest formula I can find to express our necessary conception of the facts.

That motion takes the direction of least resistance also is an axiom. It is involved in the meaning of the words; for by resistance is meant that which *preventing*, thereby necessarily *directs*, the motion.

It is necessary, however, to notice an ambiguity which may here present itself. Motion doubtless takes the direction of least resistance, but every motion must have an original direction, and a momentum which enables it to overcome a greater or less amount of resistance. Do these circumstances detract from or destroy the value of the axiom?

Certainly they do not practically. Mechanics, as an art, reposes on it, and with none the less certainty or success because these conditions have to be remembered.

Nor does the axiom appear to me to be even theoretically defective. It is true every motion must be in a certain direction, but this direction must have been assumed under the operation of the same laws as determine its subsequent course. We here, as in every case, strike upon a chain which has to the human intellect no beginning. Whatever we may suppose concerning the primary origination of motion, of every motion that we can perceive or conceive we must say that it is such as it is because motion takes the direction of least resistance. And the fact that impulse or momentum *overcomes resistance* only reminds us that we are apt to use the word resistance in too limited a sense. For what is it that resists motion but force? and what is force but that which, if unresisted, produces motion? It is therefore motion, or the cause of it, that is the true resistance to motion. Thus we of course include the momentum of the moving body among the resistances to be considered, and the axiom assumes the utmost logical completeness. An opposing resistance deflects or changes motion, or is overcome by it, according to whether it be greater or less than the resistance to such change or deflection presented by the momentum. For the momentum clearly becomes a *resistance* in relation to such change or deflexion. If it were not so, indeed, the axiom itself would be unmeaning.

These few remarks may sufficiently guard against a misconception of the general statement which I have introduced thus broadly. Fortunately there is the less need to dwell upon such speculative views, because the position to be established is a matter of fact and demonstration.

It is remarkable that, in the various hypotheses which have been framed to account for the forms of organic bodies, no attention has been paid to the fact that they are formed, as it were, under pressure, that the process of expansion in which growth consists takes place under conditions which limit it in definite ways. It must surely have been from overlooking this circumstance that a mode of speaking has established itself among us, as if there were in the organic tissue a power of forming itself into peculiar shapes, as if masses of cells, by some power of their own, could mould themselves into complicated structures. How strangely all such modes of speaking (howsoever they may be disguised, or whatever specious terms may be called in to conceal their nakedness) are at variance with all the principles which are held in regard by us who use them, whether as students of nature or as professing to recognise a higher agency than nature's, it is needless to point out. It suffices to show a way of escape from them. For it is certain that such assumptions would never have been tolerated either in our words or in our thoughts, if we had not been driven to them by our inability to refer the phenomena to demonstrable or intelligible causes, such as science, properly so called, concerns herself with.

I say, therefore, that a release from the imagined necessity of assuming such inherent virtues in organic bodies is afforded by two simple facts—1st, that the increase in bulk of developing structures takes place under resistance; and 2ndly, that we can, in very many cases, trace the forms assumed by organic bodies, or parts of them, to the operation of the ordinary mechanical laws taken in connexion with other conditions known to exist.

In some instances it has been found impossible to ignore this relation of morphological changes to mechanical conditions, in spite of opposing hypotheses. In the case of the ovum, for example, the cells of the germinal membrane are said to become apparently hexagonal by mutual pressure, arising from the increase of the mass. Doubtless this is natural and true; we could not force ourselves to attribute this change of figure to any other cause. But if this be so, does not the folding of the membrane into laminæ stand in an equally simple relation to pressure from increase of mass? And if some laminæ, why not all? The admission of mechanical conditions as normal agents in morphological change, in any one instance, involves the necessity of taking them into consideration in all, and of admitting no other agents except in cases in which these are demonstrably insufficient. How entirely this principle has been neglected it is curious to reflect, nor perhaps does the history of the human intellect furnish a more striking example of the power of a hypothesis to enslave thought and deaden observation. So intent have we been on pursuing the specific vital tendencies, or the final causes manifested in the uses of the parts, that it would appear as if we had entirely forgotten that living matter is matter after all. "The tail (of the cercaria), which was previously employed for locomotion, *is now useless, and falls off*"!*

* Agassiz and Gould's Comp. Phys., p. 343. Bacon's warning has not yet lost its bearing: "To say that the hairs of the eyelids are for a quickset and fence about the

Perhaps nothing has contributed so much to divert attention from real to hypothetical causes of morphological change, as the fact that structures entirely alike to the eye, and under analogous external conditions, undergo very different changes. This is especially the case in the development of the ovum, which being alike in almost all animals, so far as we can observe, is yet the parent of the boundless diversity of form that animated nature exhibits. No theory has seemed capable of accounting for this fact but that of a peculiar power inherent in each germ; yet when the phenomena are contemplated simply, and without such haste to refer them to their cause, the mystery becomes much less, and even disappears. For, what though the appearance to the eye, and even to the microscope, of all ova be the same, is it not certain that there is a difference of structure which escapes our observation? Nay, does not the ascription to them of different powers involve that very difference of structure or composition which it is supposed to supersede? And what can be simpler than that germs of different *structure* should, under like circumstances, undergo different changes? It is to be considered, also, that the external conditions of the ova of different animals are not precisely alike; they have only a general correspondence, while the nutritive materials and the molecular changes on which the process of growth depends also present differences in each case. Would it not be time enough to invent specific powers when these known conditions have been proved insufficient? Add to all this that each change of structure in the process of development modifies all the succeeding ones, and it becomes no longer hard to understand how, from even imperceptible incipient diversities, the widest contrasts of form may accrue. Every divergence is continually multiplied.

But how come the germs to differ? Clearly because formed under differing conditions. They are diverse, because their structure is the result of motion in the direction of least resistance. There is no *beginning* in a germ.

I shall proceed to mention some instances in which the production of organic forms by motion in the direction of least resistance has struck my own attention. But I do not design to make a formal induction of such cases, still less to present the evidence on which the proposition rests. That no man can do, for such evidence must embrace nothing less than every living form. The proof for each man must be his own observation, the testimony of his own senses. Let any person who wishes to put it to the test take any developing part of a plant or animal, and watch the process. Let him endeavour to trace the causes which determine the form that is assumed, and see whether it be not the fact that the expanding tissue adapts itself to the mechanical con-

sight; or that the firmness of the skins and hides of living creatures is to defend them from the extremities of heat or cold; or that the bones are for the columns or beams, whereupon the frame of the bodies of living creatures is built; or that the leaves of the trees are for protecting of the fruit; or that the clouds are for watering of the earth; or that the solidness of the earth is for the station and mansion of living creatures, and the like, is well inquired and collected in metaphysic; but in physic they are impertinent. Nay, *they are indeed but remoras and hindrances to stay and slug the ship from further sailing, and have brought this to pass, that the search of the physical causes hath been neglected and passed in silence.*"—Advancement of Learning, Book ii.

ditions that are present, just in such way as any other expanding substance of similar consistence would do. He may not in very many cases be able to succeed in this examination, the process may be beyond his grasp; our means of exploration must be greatly improved before it can be otherwise; but before even the most moderate attention these cases become daily fewer.

It may be objected here, that in manifest instances development takes place in the direction, not of the least, but of the greatest resistance, as in the growth of the root beneath the soil. Such cases constitute a class of facts most necessary to recognise; but a little reflection suffices to show that they do not, as indeed they cannot, affect the principle.

The growth, or expansion, must exist before any question can arise of the direction it shall take; the molecular actions which result in organic increase must be presupposed. Now, these molecular actions come into operation under laws which are unquestionably fixed and determinate, and which it may not be impossible to ascertain, but of which no theory is attempted here. In the germinating seed the vital action commences first, and exists most powerfully in the radicle; the root, therefore, has the first tendency to grow. From this point the application of the morphological law commences.

It is the more necessary to bear in mind this consideration because it is of constant application. In almost all cases of growth or development the vital action manifests itself in some parts rather than in others; it exhibits foci, as it were, of greatest energy. It is only by duly marking these that the effect of the mechanical conditions in determining form can be appreciated.

In truth, however, the formation of the root furnishes a beautiful illustration of the law of least resistance, for it grows by insinuating itself, cell by cell, through the interstices of the soil; it is by such minute additions that it increases, winding and twisting whithersoever the obstacles it meets in its path determine, and growing there most, where the nutritive materials are added to it most abundantly. As we look on the roots of a mighty tree, it appears to us as if they had forced themselves with giant violence into the solid earth. But it is not so; they were led on gently, cell added to cell, softly as the dews descended, and the loosened earth made way. Once formed, indeed, they expand with an enormous power, but the spongy condition of the growing radicles utterly forbids the supposition that they are forced into the earth. Is it not probable, indeed, that the enlargement of the roots already formed may crack the surrounding soil, and help to make the interstices into which the new rootlets grow? Nor is there any good reason for assuming that the roots encounter from the soil a greater resistance to their growth than the portions of the stem meet with from other causes. We must not forget the hard external covering of the parts exposed to air and light.

Like this are the cases in which fungi grow up beneath great pressure, which they overcome. The opposition to the law of least resistance is here also only apparent. The plant is altered in form in

proportion to the pressure on it, if it be great; and manifestly the pressure is overcome precisely when the resistance to growth in any other direction, arising from causes in its own structure, becomes greater than such pressure. It is impossible even to think otherwise or to express the contrary without uttering a contradiction. We are naturally prone to under-estimate the force exerted by molecular actions as compared with those mechanical agencies which more directly present themselves as force to our sensations.

Throughout almost the whole of organic nature the spiral form is more or less distinctly marked. Now, motion under resistance takes a spiral direction, as may be seen by the motion of a body rising or falling through water. A bubble rising rapidly in water describes a spiral closely resembling a corkscrew, and a body of moderate specific gravity dropped into water may be seen to fall in a curved direction, the spiral tendency of which may be distinctly observed. Theoretically, the explanation of this fact is very simple: the motion of the falling body being resisted, is deflected or turned at right angles,* and a motion constantly turned at right angles, and yet continuing, is a spiral. In this prevailing spiral form of organic bodies therefore, it appears to me, that there is presented a strong *prima facie* case for the view I have maintained. Parts which grow freely, as the horns of animals, and the roots of plants when caused to grow in water, often present the spiral form in great perfection; if a thread be uniformly wrapped round the buds of a tree in early spring, so as to prevent their expansion, they will frequently grow into a spiral. The spiral form of the branches of many trees is very apparent, and the universally spiral arrangement of the leaves around the stem of plants needs only to be referred to. If now we examine more deeply, the spiral form may be traced with scarcely an exception in every organ of the body. The heart commences as a spiral turn, and in its perfect form a manifest spiral may be traced through the left ventricle, right ventricle, right auricle, left auricle, and appendix. And what is the spiral turn in which the heart commences but a necessary result of the lengthening, under a limit, of the cellular mass of which it then consists? it is just such a folding as constitutes the laminæ in the germinal membranes, as one of which laminæ or wrinkles, indeed, it may at its first appearance be regarded. The intestines fold themselves, by similar increase under resistance, in like manner into spirals; the head is formed by a turning of the anterior extremity on itself. The entire embryo, indeed, takes half a spiral turn, being twisted so as to lie with its side upon the yelk, the heart being on that side, so that of the mammal we may say that it represents a left-handed spiral.†

It would be tedious to go through the cases which illustrate this point, for indeed it is one process that is observed in all. Is there any organ which does not commence thus, a ridge, a lamina rising up,

* In theory it might not be deflected if both the body and the medium were perfectly homogeneous, but in fact the deflection is certain to occur.

† See Glasgow Medical Journal, July, 1853. Dr. A. Thomson on a case of Transposition of the Viscera. The opposite direction of the spiral in the animal and vegetable worlds is very interesting.

a turning at right angles, an increase under limitation, of which that first lamina dorsalis may be taken as the type? In the adult frame, is there any part that is not spiral more or less decidedly? The spinal column describes a spiral from the pelvis to the skull; the ribs have every one of them a spiral twist; the skeleton of the arm and leg are spiral, though but slightly; the hand and foot are each an expanded spiral. The very meatus of the ear winds spirally, and the tympanic bone of the fœtus describes a spiral turn. The resemblance in form of the ramifying organs to those of the vegetable kingdom is too obvious to be insisted on. In the lower forms of the animal world the spiral form is even more plainly evident than in the vertebrata.

Every one must have noticed the peculiar curling up of the young leaves of the common fern. The appearance is as if the leaf were rolled up, but in truth this form is merely a phenomenon of growth. The curvature results from the increase of the leaf, it is only another form of the wrinkling up, or turning at right angles by extension under limit.*

The rolling up or imbrication of the petals in many flower-buds is a similar thing; at an early period the small petals may be seen lying side by side, afterwards growing within the capsule, they become folded round one another. It appears to be thus also that many cavities are formed in organic structures; the ovaries of plants, for example, into which, then, we may easily understand that the ovules should gemmate, even as the viscera into the cavities of the thorax and abdomen. Thus, too, the lateral ventricles of the brain appear to be formed, the hemispheres in their expansion rise up and leave a central hollow like the fern leaf; nor should we omit to notice that this cavity has a distinctly spiral form. The bulgings in the large intestine between the bands of muscular fibres, are a simple instance of the same thing. As hollow protrusions from the brain the organs of sense arise.

It is hardly necessary to point out how simply the production of septa, which forms so important an element in development, comes within the scope of this principle. They are but laminæ rising up; ridges from extension turned at right angles. From the septa of the heart we may pass to the valvulæ conniventes of the intestine; they are palpably one fact, the difference is of degree. The corpus callosum, and perhaps other commissures of the brain, are the same thing, and in the corpus striatum and optic thalamus may we not recognise bulgings formed on the same principle upon the crura cerebri?

I cannot resist the temptation that arises in this connexion, to suggest a theory respecting the striæ in voluntary muscles. According to Mr. Savory,† those markings commence at the boundaries of the fibrils, and proceed gradually to the centre. Now, precisely this appearance would result if the internal layer of the wall of the fibril greatly increased in length; it would inevitably be raised in folds which would pass inwards from each side, and might meet in the

* This was kindly pointed out to me by my friend, Dr. Gull.

† Philosophical Transactions, 1856.

centre, as do the septa of the heart or new vessels in forming parts. This, however, is but theory ; it has only in its favour the simplicity, and the great frequency in other parts of the body, of the process which it supposes. We see it constantly in the subdivision of cells by the bending inwards of their walls.

Here are a few instances in which I think I have seen the effect of mechanical conditions in determining form. The strawberry-leaf consists of three leaflets, of which the central one is symmetrical, the lateral ones unsymmetrical. If it be examined before it unfolds, the cause of this difference may be traced ; the lateral leaflets, each folded on itself, are placed in contact side by side, the effect of which is, that the inner portion of each is truncated as it were ; being covered in by the outer, it has not room to grow to an equal size. The central leaflet is free, and expands equally on each side.

If a flower-bud be opened at a sufficiently early period, the stamens will be found as if moulded into the cavity between the pistil and the corolla, which cavity the anthers exactly fill ; the stalks lengthen at an after period. I have noticed also in a few instances, that in those flowers in which the petals are imbricated, or twisted together, the pistil is tapering as growing up between the petals ; in some flowers which have the petals so arranged in the bud as to form a dome (as the hawthorne, e.g.), the pistil is flattened at the apex, and in the bud occupies a space precisely limited by the stamens below, and the enclosing petals above and at the sides. I have not, however, satisfied myself that this holds good in all cases.

I have endeavoured to trace the formation of the pea within the pod. It seems to take place thus : the seed grows into a cell containing fluid, springing up, from the point of attachment of such cell, by a narrow pedicle which expands as it increases, and divides into the two cotyledons. In the interspace between these is formed the plumule, which is thus but the first "gemmation in an axil." Should we not conceive the plumule to be formed, when the resistance to the increase of the lobes of the seed is greater than that to a growth from the axil between them ? When made to germinate in water the radicle lengthens and bursts the containing capsule, assuming a spiral twist, then the plumule gradually increases also, but in less degree, rising up into an acute curve before its extremity is freed from its position between the cotyledons. Simple as it is, nothing can better illustrate the nature of those folds, elevations, or turnings at right angles, in which almost all the organs of the body have their origin, than this curving of the plumule as it grows, its free end being fixed by the pressure of the cotyledons ; it is increase under limit.*

Mr. Tyndal, in discussing the movements of glaciers, relates some experiments, which go to prove that pressure causes a splitting or lamination in ice and other plastic substances, at right angles to the direction in which it is applied.†

* So powerful an influence has mechanical pressure on growth that, as stated by Mr. Lindley, those endogenous trees of which the external layer cannot expand, are stopped in their growth by the resistance which is thus opposed to the formation of the new bundles of fibres.

† Westminster Review, 1857.

It is difficult to avoid connecting this fact with some of the phenomena of organic development in which lamination, or the splitting of a mass into parallel layers, occurs so frequently, and plays so large a part. Consider the division into two layers of the germinal membrane which takes place only in the vertebrata, animals destined to a comparatively high development; the lamination of the plumule of the seed, the leaf-bud of the plant. In reflecting on the cause of these things, do not our thoughts involuntarily recur to the pressure that continually arises, must arise, in the growing tissues? What shall we say of the primary cleavage of the yelk in directions at right angles to each other?

In endeavouring to trace morphological change to its causes, we cannot overlook the very striking connexion between growth and decomposition in developing organisms. Continually in the ovum the central portion of the germ mass liquefies, while the circumference developes, one portion seeming to serve as food to another. It is thus that the vascular system is chiefly formed, the walls assuming consistency as the central parts are dissipated. Surely it is not a mere fancy that finds in this decomposition one source of the *force*, which produces the growth of the adjacent parts. To remember this relation of decomposition and growth would render simple many things in the living body that are otherwise mysterious. Let it be conceded that where there is decomposition there is a source of force which may be manifested in the production of *vital* changes, and a flood of light is poured upon development. For decomposition is a process ever apt to occur, and it is a known result of pressure. "The cells in the embryo sac," says Dr. Carpenter, "deliquesce again as the embryonic mass increases in bulk and presses upon it."*

If such decomposition, besides producing growth, tend also to increase the bulk of the organized mass (and what can be more certain, when we consider the gaseous nature of some of the organic elements), certain processes in development are seen to be perfectly intelligible. Take, e.g., Dr. Carpenter's account of the fertilization of the plant. I conceive a process of decomposition is set up in the pollen, when it falls upon the stigma.

"The pollen grains fall upon the stigma and begin to absorb the viscid mucus which bedews its surface. In consequence of this absorption, the inner membrane or proper cell wall becomes distended, and either breaks through the thinner points of the external envelope, or pushes this before it so as to form one or more long slender projections, which are known as the pollen tubes. These insinuate themselves among the loosely aggregated cells of the style, and grow downwards until they reach its base, a distance in some cases of several inches. Arrived at the ovarium, they direct themselves towards the micropyle of the ovules, and entering these they make their way towards the embryo sac, usually through a channel formed by the diffluence of a sort of cord formed of peculiar cells that previously passed from the apex of the embryo sac to thatof the mammillary protuberance of the nucleus."†

Just such, again, is the germination of the seed: the decomposition

* General and Comp Phys., Third Edition, p. 898.
† Op. cit., p. 896.

of the albumen produces at once vital action and expansion, and growth takes place first in the radicle, then in the plumule, these being the directions of least resistance.

May not the curious fungus which forms in certain caterpillars be classed with these? First occupying the body of the animal, it finds its way out as it increases always at the junction of the head with the body, the direction of least resistance.

My conception of the nature of these changes is illustrated by the fact that separated portions of cactus will grow and increase in size while they gain no increase of weight. The starting point here I take to be a decomposition analogous to that which takes place in a germinating seed.

Does not the power of repair resolve itself into an instance of growth in the direction of least resistance? Is not a wound an axil? and the granulations which form in it, or the new member which grows in the place of a lost one, do not they correspond to the buds which form in axils in the growth of plants or the development of the embryo? The solution of continuity removes the resistance of the external investiture. Is there, therefore, any basis for the supposition of a special power by which a living body can recover itself from accident or injury? The law of its formation involves its repair. So if some leaves be incised, buds spring up from the cut surface; the hydra gemmates from a wound. These are artificial axils. I do not mean to imply that no other circumstances are to be regarded in relation to the reparative process. Irritation, of whatever kind, produces special modifications of the vital action, but I suggest that the general fact of the repair of wounds is an illustration of growth taking the direction of least resistance. The new material is deposited where the resistance to expansion is removed; is it not deposited there rather than in other portions of the body, because the resistance at that point is least? We know that repair is effected at the expense of the general nutrition, and we know, too, the effect of scabbing, or pressure otherwise applied, in limiting the process of granulation. Perhaps I may state the case thus: if growth take the direction of least resistance (other circumstances being the same), then it is certain that wounds must be repaired.

Are not the mammary glands gemmations in the axils of the upper extremities, and the external sexual organs in those of the lower, the former altering their position in the general growth of the body? And is there not a close parallel between the mammæ and the external sexual organs; the scrotum or labia answering to the glandular portions; and the penis or clitoris to the nipples, these latter, moreover, being erectile? Do not the sexual parts represent the two mammæ coalesced?

In truth an entirely new conception of homology arises out of the recognition of a LAW OF FORM; a parallelism of various organs, according to the dynamical conditions exhibited in their morphology, which embraces all parts of all bodies, and extends itself through the whole of organic nature. On this point I will confine myself to one

suggestion. Observe the form of the intestinal canal (I speak of the mammalia), straight for a short distance from its orifice, then convoluted, then terminating in an expanded portion, the stomach. Compare this now with the form of the tubuli in the kidney—a straight portion, a convoluted portion, an expansion, the Malpighian corpuscle. Again, take the nervous tubules in the brain; there is a straight portion, the white substance; is not the grey matter a convoluted *cortical* portion (like that of the kidney), and are not the cells intermingled with the convoluted fibres expansions, not without a formal resemblance to the Malpighian bodies? I do not wish to erect any strained analogy; perhaps this is a fancy altogether, but to my mind there is indicated by this similarity of form a similarity of mechanical conditions not without its interest.

Development, then, is due to increase under limit; it is determined by resistance. Is it not self evident? Conceive an ovum germinating with all other circumstances unaltered, but with no external limitations, no membranes, no uterus, nothing to check expansion in any form. Could anything else result but a shapeless multitude of cells? But if it be so, let us fairly face the position that we take. The mechanical limitations must act mechanically, and *form* be the result of mechanical conditions. Consider how every organism intended to develope is subject to external resistance, the tough capsule of seeds, the shell of eggs, the womb of the vivipara. Are not the marsupials and monotrems which escape so early from the uterus less developed than animals whose gestation is more prolonged? Think of the firm sheath of every muscle, the capsule of every viscus, the bony case of the nervous system; remember how every free surface in the body is covered with cells, and with cells alone. If the membranes of the brain be divided, a cellular fungoid growth protrudes; is it not that the maintenance of the organization of the brain demands the resistance of its coverings?

Using the term "uterus," therefore, to denote a definite external resistance to extension, is it not an axiom that *everything is* DEVELOPED *in a uterus?*

It should be remarked here that the forms of parts or animals are often greatly altered, after their first development, by growth under conditions different from those in which the development takes place. Growth modifies the form which development primarily determines; the body moulded within the uterus, expands freely thereafter without external resistance. Hence the result is changed, but not the law. A single instance will make clear my meaning: in the early bud the anther constitutes the entire length of the stamen; as the flower expands the stalk has room to grow.

There is one other class of cases to which it is necessary to refer; those, namely, in which the form immediately results not from growth or expansion, but from wasting; an extreme instance of which is presented in the cellular formations constituting the pith of plants. Indications of such relative decay are of great frequency in the animal body, especially in the higher grades of development, but this slight

mention may suffice for them here. It is clear that the law of least resistance, which means no more than that mechanical conditions determine mechanical results, applies equally to them. The wrinkling up of the lining of the corpus luteum, partly we may be sure from the contraction of the capsule, is a marked example.

If it should be remarked that there exist in developing structures certain definite modes or operations of force, such as attractions or repulsions in particular directions, which serve to determine the form assumed, apart from any influence of the visible mechanical conditions, this is willingly admitted to be true. The morphological law suggested does not contravene, but rests upon, these phenomena. They may be regarded in two ways; either as constituting part of the molecular process in which nutrition consists, as instances of those local manifestations of growth before referred to, and which are presupposed as the foundation on which the law is based; or perhaps more properly they may be themselves considered as coming within its scope. In so far as these changes consist in the motion of particles, the law of least resistance may be asserted of them, or at least cannot be denied. Such molecular changes, indeed, form no portion of the evidence on which the proposition can be based, inasmuch as the nature of the process and all its conditions are as yet beyond our investigation. But that in so far as they consist in motion they conform to the nature of motion, we may be quite sure. The *structure* of the germ must be such as to determine the operation of whatever chemical or other forces come into play within it, to produce motion in these particular directions.

This, then, is my argument. The illustrations I have adduced may be insufficient, or unsatisfactory, or false, or misconceived, but no defect of this kind in the proof can affect the proposition; for it rests upon necessary laws of thought. Physical morphology is like an applied geometry; if I have failed in the application, others will certainly succeed.*

Resistance to motion is of necessity an opposing force; force and resistance are indeed interchangeable terms, two aspects of the same thing, as when the two hands are pressed together, each mutually resists the force applied by the other. Viewed in relation to this law of least resistance, therefore, the idea of organization is beautiful. It is the result of motion in the direction of least opposing force. Certainly: how should it be anything else? Is not organization a perfect mutual adaptation and exact conformity to each other of all the parts, even to the minutest details, an absolute rightness and order? And how should this be attained except through motion in the direction of least opposing force? What else is this law of motion but that exact rightness seen from the human point

* It may be urged that in magnetism and other forces, and in human actions, we have instances of motion to which this conception of least resistance is not applicable. It would be too great a licence to enter on a discussion of these matters under cover of an inquiry into organic form; but it appears to me that, *in so far as they come within the domain of the physical*, the conception of least resistance as determining the form and direction of the action is neither inapplicable nor infertile in results.

of view? Does it not mean that each minutest part determines the being of every other, a perfect mutual interaction and subordination, a rightness from the very first, and through every step, that must end in a completed rightness at the last? Let us try to think of this, freeing our minds from preconceptions. How can there be other than perfect order and adaptation in that organization in which each part has had its equal share in the moulding of the whole? There unity must be, and beauty, and most exquisite harmony, for there law has been perfectly fulfilled. What else is it than that each particle of the growing organism goes where it is most wanted (as *we* might say), where there is most room for it; it is like water finding its level, the very idea of exact adjustment. Manifestly it involves the existence of specific forms, such as we see, and forms maintained and repeated as we see. Constancy of forms means constancy of conditions.

Contrast with it any other supposition respecting vital forms. What of a specific tendency to a definite form in each animal? Is not that as much like "Nature abhors a vacuum" as we can well conceive? Is it anything but a deceptive formula, used to give an appearance of accounting for that which is in truth a mere matter of observation wholly unaccounted for? Not to mention that it supposes something to *act* before it *is*, is it not open to the practical refutation that the case is not as stated? Nature only abhorred a vacuum to a certain and a variable extent, so do living things assume a definite specific form only to a variable degree of accuracy. Monsters and deformities of all kinds disprove the hypothesis altogether. Besides, we know how, by artificial means, organic forms are altered. Tie up a bud upon a tree so as to prevent its growing, and in most cases another bud, that would not otherwise have appeared at all, will be developed in its place. Is not this proof that of all possible buds in any given case, that one develops to the growth of which there is the least resistance?

And what are we to say of that revived doctrine of "Ideas," the type of which is the typical vertebra, and respecting which, if we had not profited by the experience of other ages, as fierce a conflict would arise as was ever waged between realist and nominalist of old? Let it be granted that it is an admirable means of grouping and arranging our ideas; let it be granted even indispensable, while no law was known for vital forms, no order and necessity otherwise discoverable, and therefore no ground for scientific treatment. Surely it was never regarded as satisfactory or final. There is no need to argue against the conception; until such place is claimed for it, it may well be left to die in honour when its work is done. Nor is there any fear that aught of value in the homological doctrines, for which it has served as an expression, would perish with it. They have a deeper foundation than that which has been claimed for them.

All that I maintain is involved in these words of Mr. Huxley's: "The lateral canals (of hydatina) are much longer than the body, and are *therefore* disposed in coils here and there."* Only let the prin-

* Medical Times and Gazette, 1856, Lecture v. p. 81.

ciple here recognised be applied to every case to which it is found applicable.

Here I should cease. But it would be affectation to ignore that the view I have taken will be felt by some as contravening the design that they delight to recognise in nature; as another step towards excluding God from his creation. I do not feel it so. I may not enlarge upon this aspect of the question, but the entire subject has been so mixed up with theological ideas that I may be permitted briefly to indicate my own view. I hold all vital forms to be what we call necessary, but it is the necessity of rightness that I recognise, and no other. God's act in Nature appears to us under the form of *physical* or merely passive necessity, but that is our infirmity and defect of vision. It is necessary truly, every least fact and part of it, but necessary by a truer, deeper necessity than we perceive, the necessity that Love should do infinitely well and wisely. Welcome to me are all proofs of necessity, all indications of law, all demonstrations that things could not be otherwise than they are. Never does nature bring us nearer to God than when science excludes from it all arbitrariness, and teaches us to say, This must be as it is. For an intellectual we must learn to substitute a moral conception of creation; we need to rise above contrivance, it is holiness that claims our reverence in nature. Well said Bacon, "The three true stages of knowledge are as the three acclamations, *sancte, sancte, sancte;* holy in the description or dilatation of His works, holy in the connexion or concatenation of them, and holy in the union of them in a perpetual and uniform law." Was Newton ever held to be an irreligious philosopher? Yet of him "it is recorded that whilst contemplating the simplicity and harmony of the plan according to which the universe is governed, his thoughts glanced towards the organized creation, and he remarked, 'Idemque dici possit de UNIFORMITATE EA, quæ est in corporibus animalium.'"*

ART. III.

On the Influence of the Vagus upon Respiration. By W. GILCHRIST, M.D. Edin.

ALTHOUGH the physiology of the vagus nerve, in its most important and essential relations, is now pretty well made out, through the experimental studies of Reid, Bidder, Volkman, Weber, Traube, and others, still there remain certain points in its physiology upon which our knowledge is by no means so perfect as might be desired, and which demand inquiry of a more searching and intimate nature than those greater and more general questions already ascertained. It is the object of this short communication to point out the difference of opinion that exists amongst physiologists upon one of the most important functions of this nerve, and to give the results of my own

* Dr. Carpenter, op. cit., p. 559.

observations on the subject, made during the past summer in the Physiological Laboratory of Berlin.

In regard to the respiratory process, the vagus may be viewed under two aspects—as directly, and as indirectly, connected with the function. Under its indirect relations are to be considered the purposes of the inferior and superior laryngeal nerves, endowing the larynx with sensibility and its muscles with motion; but, excluding these, there exist certain correlations between the state of the periphery of the nerve and the action of the inspiratory muscular apparatus, which may be regarded as more directly influencing the respiratory process.

In 1847, Traube pointed out* that a slight irritation of the central ends of the vagi, still in connexion with the medulla oblongata, produced an increased number of respirations; but that a stronger irritation produced a tetanic contraction of the diaphragm, or in other words, a fixed and prolonged inspiratory position. The precise results which Traube drew from his experiments are the following:†

"1. Tetanization of the central trunks of the pneumogastric nerves, which have been cut through in the neck, produces a perfect suspension of respiration.

"2. This is, however, so to speak, nothing more than an involuntary prolonged deep inspiration, as appears from the immediate inspection of the diaphragm, after the cavity of the abdomen has been laid open. One sees then, during the whole continuation of an induced current, the phreno-costal bundles of the diaphragm seized with tetanic contraction, so that its arch appears much flattened.

"3. During the continuation of the induced current, the muscles of expiration remain perfectly inactive.

"4. As it has long been known, the frequency of respiration extraordinarily diminishes after division of the vagi. If the central ends (i.e., the part of the nerves still in connexion with the medulla) are tetanized with weak currents, one observes that the number of the respirations is increased almost to its original amount.

"5. If one with a pair of scissors quickly cuts through the pneumogastrics, he produces almost constantly an interruption of the respiration, or rather a deep inspiration, which may last for seven seconds."

From these facts Traube draws this conclusion,

"There exist in the pneumogastric nerves not only motor fibres, belonging to the œsophagus, larynx, &c., &c., and sensitive fibres, the irritation of which produces pain and expiratory movements, but also centripetal fibres, the irritation of which produces involuntary inspiration."

Since these original observations of Traube, experiments have been made by many observers, but all have not found the same result. Amongst those who, with him, hold that the effect of strong irritation of the nerves is a continued inspiration, or rather a continued inspiratory position of the diaphragm, are Lindner,‡ and Kölliker and Müller.§ Very different, however, is the observation of Eckhard, who, in his work on Nerve Physiology, states that when both nerves are at the same time exposed to an intense electric irritation, the central

* Traube. Zur Physiologie des Nervus Vagus. Medicinische Zeitung, 1847.

† Pflüger. Hemmung's Nervensystem, 1857, p. 10.

‡ Lindner. De nervorum vagorum in respirationem efficitate, Berol, 1854.

§ Kölliker and Müller. Würzburger Verhandlungen, 1855, p. 233.

organ of the breathing motions discontinues its activity, the latter cease, and certainly as it appears in the position of *expiration* or in one very similar to it. One observes that the diaphragm assumes a position corresponding to an expiration, as can be seen in rabbits when the skin is removed from the sharp edge of the liver.*

I may here remark that this mode of observation is by no means good, and that to observe accurately the state of the diaphragm, the liver ought to be removed from its position, so as to get a view of the diaphragm itself. One further reference is requisite to complete the history of this subject, and to show the bearings of my own observations. Aubert and Tschischwitz, from a number of experiments, have attempted to explain these different results in this way:—according to them, the nature of the results depends upon the intensity of the irritation employed, and they have found that with different strengths of electric currents, different positions of the diaphragm are produced. The general results of their experiments are the following:†

1. Very weak currents of the induction apparatus produce acceleration or diminution of the respiratory rhythm, or they are productive of no effect at all.

2. Stronger currents cause a stillstand of the diaphragm in the position of expiration or contraction.

3. Very strong currents produce a standstill of the diaphragm in the position of expiration or relaxation.

From these quotations it appears that, according to one, the effect of an irritation of the centrally-connected ends of the divided vagi is an inspiratory movement; according to another observer, the constant and natural result of such an irritation is an expiration; and in the opinion of a third, it is possible to find both inspiration and expiration, the production of the one or the other being dependent upon the degree of irritation to which the nerves are subjected. From these observations, what are we to conclude? Must we suppose, as Kölliker does,

"That the consequences of irritation of the central ends of the vagi upon the respiratory movements are very different, which is not difficult to imagine when one considers that the respiratory movements consist of a double action?"

I think such a view is certainly not at all philosophical, and is also inconsistent with what we already know of other laws regulating fundamental and at the same time complex functions. Indeed, it is very difficult to suppose that a given cause, applied in a uniform way to a typically uniform organic structure, can produce an essentially different effect. I would rather attribute these differences of result to our imperfect acquaintance with the conditions occurring in the methods of research.

The general mode in which my experiments were performed was the following. The rabbit being secured firmly to a small table, a flap of skin was taken from the centre of the neck, and the vagi dis-

* Eckhard. Grundzüge der Physiologie des Nervensystems, 1851, p. 130.

† Moleschott's Untersuchungen, vol. iii., 1857, p. 273.

sected out on both sides. In preparing the nerves, the utmost care was taken not to touch them with the forceps, and in order to isolate them, I found it most convenient to seize with the forceps the connective tissue lying around them, and holding them between me and the light, I could see that I had the nerve and nothing but the nerve; then with a sharp-pointed knife I cut the cellular tissue away from the sides of the nerve, upwards as far as the superior laryngeal nerve, and downwards to the base of the neck. By this mode of preparing the nerves, I could be sure, *first*, that no other tissue but that of the nerve was included in the part to be tetanized; and *secondly*, that the nerve itself, and the superior laryngeal also, were not compressed or injured in any part of their course. These precautions seem to me to be very essential as regards the result of the experiment. The nerves being isolated and again placed between the muscles, the abdomen was then laid open by a longitudinal and two transverse incisions, the ligamentum suspensorium of the liver was cut, and that organ being pulled downwards and to one side, the motions of the diaphragm could be most minutely observed.

As regards the mechanical portion of the experiment, I at first employed an induction apparatus of the ordinary size, but afterwards a larger and more powerful one, both of which had a prolonged board by which the second coil could be so placed as to produce currents varying from the slightest strength to an intensity far beyond what could be borne by a man.* The ends of the nerves were placed on the platinum plates of the apparatus invented and described by Du Bois-Reymond, and in order to interrupt the progress of the current, the *key* described by Pflüger, but invented also by Du Bois, was used. For irritation experiments on nerves, nothing can be more convenient than this key apparatus, for with the slightest movement of your forefinger, you can instantly produce irritation, and by as simple a movement can instantly suspend it.

It has long been known that after division of the vagi, the respiration at once becomes slower. In the cases in which I observed this effect, the respiration was changed as follows:

Rabbit I.—Before section, respiration 60; after section of one nerve, 52; after section of both, 32.

Rabbit II.—Before section, 62; after section of one nerve, 54; after section of both, 32.

Rabbit III.—Before section, 60; after section of one nerve, 92; after section of both, 28.

Rabbit IV.—Before section, 108; one nerve cut, 112; a few seconds later, 80; animal struggled—respiration thereafter, 108; second nerve divided, respiration 36.

Rabbit V.—Breathing irregular; before section, 80; right nerve cut, respiration 60; thereafter, breathing irregular, consisting of long inspirations varied with a series of short momentary inspirations, 104;

* This apparatus is called in Germany Schlitten-Apparat, and has been described by Du Bois-Reymond, in his work on Animal Electricity, vol. ii., part i., p. 393.

some seconds after, 76; after section of left nerve, 104; some seconds after, 76; after tracheotomy, 60.

From these cases it is seen that in two instances the breathing diminished considerably after section of one nerve, and still more on section of the other; but in the third and fourth cases after division of one nerve, the respiration instead of falling, as usually is the case, rather increased, but on section of the other nerve at once fell to less than a half of the original ratio. Again, in Case 5, even after the division of both nerves, the respiration (though of an extremely irregular character) was by no means reduced in ratio, though no doubt it would ultimately become so.

The cause of diminution of the respiratory movements after section of the vagus has not yet been fully explained—at least, its cause has not been demonstrated. Longet attributed it to narrowing of the glottis, consequent on section of the recurrent nerve; but Traube has shown that if the recurrent alone is divided, respiration is diminished, but not to the same extent as when the vagi are cut. From the same cause we find that when a tube is inserted into the trachea, the ratio of breathing is diminished; that when tracheotomy is performed, after the nerves are divided, an increase in the ratio is a frequent result, so that diminution of the passages has some share in the production of this diminished ratio; and as to the remaining cause, we may suppose from the effects produced by irritating the nerves, that they convey to the medulla oblongata an impression as to the state of their periphery, or, as it has been termed, a "sense of the necessity for breathing," produced probably by the stimulation of the venous blood and of the atmospheric air. Although this function is probably specially localized in the pneumogastrics, there can be no doubt that the nerves of general sensation can convey to the breathing centre an impression of the state of the blood, and can so far supply the place of the vagi when their course has been interrupted. Herein, too, may lie the explanation of the fact, that in every case in which the vagi are divided, the respiration is not immediately diminished after section of one or even of both nerves, for in such cases the animals being strong, and not at once prostrated by the shock, may, through the medium of the nerves of general sensation, become aware of the necessity for an increased effort in breathing, and by the exercise of volition may even succeed so far as to breathe at a quicker ratio than they did before the nerves were divided. I particularly remarked that the animals in which I noted this result were large and strong. As to the influence of a slight irritation of these nerves, confining that term to such an irritation as is productive of no marked inspiratory or expiratory movements of the diaphragm, physiologists are not quite agreed; thus Traube and Eckhard found the respiration increased, Kölliker and Müller that it was diminished, Aubert and Tschischwitz that it was sometimes increased, sometimes diminished, and in some cases not affected at all. In my own experiments I did not in every case make numerical observations on the effect of slight currents, but from what

I have observed, I am inclined to agree with Traube, that by weak electric currents the respiration is accelerated. In one case I had very distinct numerical proof of this, for when the second coil of the induction apparatus was placed at a certain distance from the first, the application of the current to the nerve produced an increase of eight respirations in the minute, whilst with a current very slightly weaker, no change was produced, and with one a very little stronger, a moderate inspiration occurred. It is a matter of no small difficulty to determine the effect of a weak current, because after division of the vagi there is more or less irregularity of breathing; and indeed, without employing any irritation at all, I have found such a difference in the respiration at one moment from what occurred in the next, that it becomes a very nice question to determine in how far an acceleration or diminution of the respiratory rhythm is due to this irregularity, or to the action of the weak irritation. In this mode is to be explained the difference of result found by different observers.

To determine the effect of a stronger current was more the object of my inquiry, and from thirteen experiments made with this view, I have come to the following conclusions:

1st. That expiration is *not* the normal and constant result of a strong irritation of the vagus, as stated by Eckhard, Budge, and others; but that if it occurs, it is depending upon some complication in the experiment.

2nd. That expiration is not produced by *very strong* currents, as stated by Aubert and Tschischwitz. With the powerful induction apparatus, which I was enabled to use through the kindness of Prof. Du Bois-Reymond, I employed currents varying from the most feeble to such a strength as has perhaps never been used in this experiment, and on no occasion have I found expiration as a result. I do not, however, mean to deny that expiration does occur sometimes in this experiment, but I have convinced myself that its cause is not the intensity of the current, since with the strongest, as with much weaker currents, I have always seen the same result. Moreover, besides this positive evidence against this view, I may add a fact of a negative kind—viz., that on one occasion Kölliker found expiration produced in a dog by a very weak current.

3rd. *That the constant and normal result of irritation of the vagi is inspiration.* In thirteen experiments, in which the nerves were prepared with the utmost care, this effect was most constantly observed by myself and others. In some animals inspiration was produced by a current of weaker intensity than what was required to produce the same in others; but I cannot say I have obtained any data to lead me to suppose that a connexion exists between the intensity of the current and that of the contraction, since with the strongest current the contraction has not appeared to be different from that produced by a weaker. With one nerve only the results were essentially the same. One fact to be noted regarding the contraction is that frequently—indeed, generally—it remains for a considerable number of seconds after the irritation has been suspended, and then one sees the diaphragm

relaxing quite gradually, and as it were by stages; and then follows very frequently a tumultuous kind of respiration, in which the expirations are particularly deep. In one instance I opened the trachea so as to get a view of the glottis. Its motions were distinctly seen, and on observing it whilst a current was applied to the nerves, it was observed to close after the irritation had occurred, and to remain so for several seconds. This observation I repeated (in the same experiment) several times. This closure of the glottis must no doubt have some influence on the duration of the inspiration, inasmuch as if the glottis remained spasmodically closed, the diaphragm must remain in a flattened and contracted-like condition, even although the state of tetanic contractions produced by the current had ceased.

Although I feel from these trials quite convinced that the general and normal result of the irritation of the vagi is an inspiratory action, I would have wished to be able to give facts proving the cause to which the expiration found by other observers was due. This, however, I have not been able to do, and although it would be possible to frame hypotheses for the explanation of such an occurrence, I forbear doing so, having no substantial facts on which to found an opinion.

ART. IV.

On the Relation of Common and Tactile Sensibility in Disease.—By E. H. SIEVEKING, M.D., Physician to St. Mary's Hospital, Fellow of the Royal College of Physicians, &c.

THE current teaching of physiologists has hitherto been, that the faculty by which we perceive painful sensations and the faculty which enables us to distinguish the different characters of surfaces that come into contact with our integuments, are identical; and that the channels of common sensation which convey pain, are the same as the channels of tactile sensibility which convey the impression of touch. Schiff and Brown-Séquard have, however, recently essayed to show the error of these views physiologically. The opinions of these distinguished inquirers do not entirely agree with regard to the channels by which the different impressions are conveyed to the sensorium, but both hold that common sensibility and tactile sensibility manifest themselves to the individual by the aid of different sets of fibres.* I have arrived at a similar conclusion by pathological observation, and

* Schiff's views are briefly these, that the white posterior column of the cord alone conduct tactile sensation, while common sensation passes by the grey matter, neither portion of the cord co-operating with, or acting for, the other in the performance of the function of sensation. (Moleschott's Untersuchungen, 1858, iv. 1.) On the other hand, Brown-Séquard, whose delicacy of operating it is impossible to see without feeling great confidence in the conclusions which he puts forward in his lectures, maintains, in reference to the present question, that "most of the elements which are employed as conductors of purely tactile impressions seem to pass by the same parts of the spinal cord as those which transmit the impressions which produce pain." Brown-Séquard holds that section of the posterior columns of the cord induces hyperæsthesia in all parts lying posterior to the divided part, a point that is specially denied by Schiff. For the arguments and experiments upon which Brown-Séquard bases his views, we refer the reader to his lectures which are appearing in the Lancet, and particularly to the fourth (Lancet, July 31, 1858).

in the present condition of our knowledge on the subject it may be useful to adduce any well-authenticated fact that assists in elucidating the subject. The question, then, at present before me is this: are the different impressions made upon the cutaneous surface differently received at the point of contact? are there distinct organs by which the impressions are conveyed to the organ of consciousness? or, are the media by which the impressions are conveyed identical, the difference in the impression depending upon the interpretation given by the central organ? In the latter case, the inference would be, that there are elective affinities between a portion of the brain for impressions attributable to common sensibility, which are not received by another portion of the same organ which has an affinity for the impressions of touch. The first view appears to me much more consonant with what we know of the working of nature and of the laws that govern organic mechanism.

The term, common sensibility, I employ to denote the perception of a more or less painful impression made upon the surface; tactile sensibility may be defined as the power of distinguishing with greater or less facility the inequalities or other characters of surfaces brought into contact with the skin. The simplest way of determining the delicacy of the tactile sensibility of a part, is by employing two points moveable on a graduated beam, and impressing them at the same time upon the part to be tested; the smallest distance at which the person experimented upon can distinguish the two points as distinct impressions, is a measure of the tactile sensibility of the part. Until the two points are felt in this way, the impressions seem to be produced either by a single point or by a broad suface. There is no relation between the tactile effect and the amount of pain produced, unless it be an inverse ratio, for if actual pain result from the force with which the points are made to impinge, it would rather overpower than promote tactile sensibility.

The capability of distinguishing weights has been attributed, and apparently with justice, to a distinct property of the muscles, which has been termed the muscular sense. In this way sensation has already been narrowed. It appears to me that pathological facts that have come under my notice justify our proceeding yet further, and separating from one another common sensibility and tactile sensibility.

We frequently meet with cases in which the patient expresses himself in terms leading the physician to suppose that the two kinds of sensibility are distinct; but as long as the patient's impressions are only subjective, and incapable of actual measurement or delineation which can be made demonstrable to others, they are likely to be set down among the capricious phenomena of nervous derangement, our ignorance of which we cloak with such terms as hysteria, hypochondriasis, spinal irritation, and the like. In the short notice which I gave of the æsthesiometer, and its mode of application,* I showed, by an

* See a description of Dr. Sieveking's æsthesiometer in the British and Foreign Medico-Chirurgical Review, January, 1858, p. 280. The æsthesiometer may be obtained from Messrs. Elliot, philosophical instrument makers, 30, West Strand.

instance culled from actual practice, how subjective numbness might coexist with perfect tactile sensibility. As I am disposed to offer a different interpretation for the phenomenon from what I did then, or rather to define the interpretation more precisely, and as it will serve for comparison with the cases I wish now to refer to, I quote it again :—

"E. M'M——, aged fifty-two, suffered for six months before the first consultation from numbness and formication of the left hand, with severe nocturnal pains along the tips of the fingers and at their metacarpal ends; the patient rarely had pain in the thumb and none in the palm of the hand. There was frequent vertigo. To determine the character of the numbness, the æsthesiometer was applied, and the patient was found to distinguish one-tenth of an inch equally well at the tips of the middle and third fingers of both hands; the instrument aided in the determination of the diagnosis by showing that the numbness was purely subjective, and not the result of a true paralytic affection."

I have recently had two cases under my care at St. Mary's Hospital, which, each in a different way, prove to my mind that there are in man two distinct functions, and therefore two distinct sets of nerves through which they act, viz.,—common sensibility and tactile sensibility; the former being manifested by pain, the latter by the power of distinguishing the character of the surfaces with which a part is in contact. It is manifest that in health the two must apparently be identical, because the painful impression and the impression produced by the character of the impinging body, will necessarily be referred to the same spot. In disease, however, this may be otherwise, and if we can satisfactorily show that one of these functions can exist without the other, the legitimate inference is scarcely to be avoided, that they are not identical, and that the media by which they manifest themselves, and through which the individual becomes conscious of them, are also distinct.

The cases to which I shall refer have many points of interest apart from the question at issue. The space at my disposal will not, however, permit of my doing more than stating the facts to which I desire to draw attention, hoping thereby to induce medical men to watch cases of paralysis that may occur to them, with a view of still further elucidating this not uninteresting question.

One of the patients, a man above forty, labouring under cerebral disease—probably incipient softening of the hemispheres—not accompanied by acute or violent symptoms, was, at the time of the inquiry, affected with sensory paralysis and partial motor paralysis, the latter more marked in the right than the left lower extremity. The loss of sensibility appeared about equal in all four extremities; but on applying the æsthesiometer, I found that the man was perfectly and readily able to distinguish a little more than one-tenth of an inch at the tips of his fingers, a distance but little greater than that which healthy persons, whose touch has been more cultivated, can distinguish.

The other patient is a young woman, aged twenty-one, who for about a year has been labouring under progressive paralysis, attacking first the motor powers of the lower extremities, and gradually extending upwards to the arms. Galvanism, applied in feeble currents to the different muscles, produces a ready reaction; the palsy cannot therefore depend upon a primary degeneration of the muscles, a view that is also negatived by other circumstances. Moreover, reflex action is readily excited in the legs. The question of diagnosis here is one of intense pathological interest, and would in itself deserve to be treated in a special paper. The point of the present communication is contained in the statement that in this girl common sensibility is actually exalted; but that, although she feels the prick of the æsthesiometer very acutely, she is unable, if the two points are widely separated, and even made to impinge upon different fingers or on a finger and a part of the hand, to recognise more than one impression at a time; she has once or twice said that she felt the application of two points as a scratch, but taken in conjunction with other tests to determine her power of touch, it is manifest that, if not absolutely destroyed, it is much impaired, for she is unable to determine whether a surface is rough or smooth; on the legs she cannot distinguish two points even at a foot apart.* The perception of temperature remains.

I do not see how we can explain these cases otherwise than by assuming that there are two kinds of cutaneous sensibility—one destined to warn us by pain, the other by the various impressions which we can sum up by no better term than tactile sensibility or touch,—for which there are two distinct kinds and sets of conductors to carry them to the cerebrum. In the one case, sensibility to pain is diminished or destroyed, and touch persists; in the other, the sense of touch has been annihilated, and the sense of pain remains.

* In both cases the necessary precautions were taken to prevent any collusion, which in the girl was the less possible because she is all but blind.

PART FOURTH.

Chronicle of Medical Science.

HALF-YEARLY REPORT ON MICROLOGY.

By John W. Ogle, M.D., F.R.C.P.
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Part I.—Physiological Micrology.

EPITHELIAL SYSTEM.

*Ciliated Epithelium.**—Bidder, speaking of ciliated epithelium, states that it can be distinguished on nasal mucous membrane when quickly dried as well as when in a fresh condition. Hoyer, in a dissertation concerning the structure of nasal mucous membrane, states that these cells in man, mammalia, and reptiles, are elliptical on transverse section, and increase in thickness from the entrance to the nose up towards the olfactory region, being longest and finest in the olfactory region. On the addition of chromic acid and on disintegration, they shrink, and their nuclei are displaced. The well-known yellow colour of the nasal mucous membrane which he finds whenever the membrane is covered by ciliated epithelium, Hoyer attributes to a finely granular contents of the epithelial cells, and not to any particular pigment. Like Reichert, he only recognises a single layer of epithelial cells, and looks upon the appearance of a more complicated lamination as a deception. Bidder and Kupffer observed cilia on the epithelial cells of the central canal in the spinal marrow in the young of mammalia only; and Bergmann and Bidder confirm the statement of Stilling, that the fibres proceeding from the sharp extremity of the above ciliated cells are in connexion with cells in the deep part of the spinal marrow. Bergmann, however, differs from Stilling in describing the layer in the fourth cerebral ventricle of the toad corresponding to the epithelial cylinders as being composed of spindle-cells, which send forth a short process towards the upper surface of the ventricle, and a long one towards the spinal marrow.

MUSCULAR SYSTEM.

On the Minute Structure of Involuntary Muscular Fibre. By J. Lister.†—The author seeks to establish the position held by Kölliker, and put forth by him in 1847, and confirmed by Reichert in 1849—that involuntary or unstriped muscle is composed of elongated, flattened cells, tapering at their extremities (called by him muscular fibre-cells), and containing at their central part a roundish or rod-shaped nucleus. This view of the composition of unstriped muscular fibre was controverted by Mazonn of Kiew in 1854, and by Professor Ellis of London, in a paper communicated to the Royal Society in

* Henle and Pfeuffer's Zeitschrift, 1858, p. 27, &c.
† Transactions of the Royal Society of Edinburgh, vol. xx., part iv., p. 519.

1856, who asserts that neither in voluntary nor in involuntary muscle is the fibre of the nature of a cell, but that it is composed of long, slender, rounded threads or fibrils of a uniform width.

The statements of Lister are based mainly upon observations made upon the sphincter of the iris, the skin, the contractile tissue of the vessels in the frog's foot, and the muscular fasciculi of the circular coat of the pig's intestine. He determined that in these instances the involuntary muscular fibre is "composed of slightly-flattened elongated elements with tapering extremities, each provided at its central and thickest part with a single cylindrical nucleus embedded in its substance." These muscular or contractile fibres, usually homogeneous when extended, "become altered during contraction by the supervention of highly-refracting transverse ribs, which grow thicker and more approximated as the process advances. Meanwhile, the rod-shaped nuclei appear to be pinched up by the contracting fibre until it assumes a slightly oval form, with the longer diameter placed transversely." The author particularly mentions the presence of thickish bodies, oval and pointed at their extremities, among the muscular fibres of the pig's intestine, which presented thick transverse ridges on their surfaces, each having a roundish nucleus. Every gradation existed between these bodies and the long, delicate contractile fibre-cell; and the author satisfied himself that these bodies are only contractile fibre-cells, the nuclei being rounded by their own contraction or by pressure of the long contracting fibres. The faint longitudinal striæ noticed by Kölliker in these fibre cells are attributed by Lister to a fine fibrous structure in the substance of the cells. The dotted appearance considered by Ellis as characteristic of involuntary muscular fibre, he looks upon as being (in the pig's intestine) exterior to the cell, and caused by little globules of a tenacious connecting fluid.

On the Structure and Growth of Transversely-striped Muscular Fibre. By Julius Budge, of Greifswald.*—After alluding, on the one hand, to the views of Schwann as adopted by Valentin, Kölliker, &c., according to which a muscular fibre consists of numerous fibrils, each of which is comparable to a string of pearls separated a short distance from each other, and on the other to those views of Bowman which are so well known in this country; the author proceeds to state that muscular fibres consist neither of longitudinal fibres nor of transverse discs, but that they can be divided in both directions, and appear at one time in one form, at another in the other, but more frequently in the longitudinal one. He then alludes to the views of Rollet† and Häckel‡ on the subject, and having premised that by way of anatomically decomposing the elements of muscular fibre for the purposes of observation, he uses a solution of chlorate of potash and sulphuric acid, in which he macerates the part for twelve or twenty-four hours, he goes on to add the results of his inquiries.

He found the muscle of the frog the best adapted for his wants, owing to the tissue being so readily acted on by the solution. In this way the tendinous parts are discriminated from the muscle by their being coloured yellow, and a very slight pressure—even that of dropping water—will suffice to separate the muscle into its single fibres, which seldom become torn, but are seen rounded off, being obtuse or more sharpened. The cross-stripes, nuclei, vessels, and even nerves, can sometimes be clearly observed.

On longer maceration the fibrils inside the fibres can easily be recognised, and the fibre itself becomes, after a short time, torn. The bundles separate more and more, until a vast number of single muscular elements lying along each other in all forms, are seen—such as isolated vesicles or granules, then

* Archiv für Physiologische Heilkunde, &c., Heft i. 1858, p. 71.
† Sitzung's Berichte der Wiener Akademie, 1857, p. 291.
‡ Müller's Archiv, 1857, p. 492.

groups of two, three, or four in a row, then others, in which they are arranged more or less transversely; others, again, lie in immediate connexion with a number of vessels, either in a longitudinal or transverse direction. The appearance as if of a fibre bifurcating is an illusion. The author then considers the discovery of Kölliker and Henle*—of certain granules inside muscular fibre. Budge will not say whether these are identical with those seen on separating the muscular fibre by maceration in the fore-mentioned fluid, but he states that on one occasion he was so fortunate as to fall in with the sarcolemma so emptied that a number of small granules were seen strewed on the inner wall along with the nuclei. This, however, only happens when maceration has existed for some days.

The nuclei appear to be in intimate connexion with fibres, so that on the entire inner surface of the structureless investment (sarcolemma) a copious anastomosing fibre-net with many nuclei was seen. The author then speaks of a string of nuclei extending along the axis of the sarcolemma in the muscle of old frogs, and alludes to the well-known occurrence of the row of nuclei in young frogs, birds, and mammals, along the axis of the muscles, the formation of which, in the case of adult animals, is so limited, that they only lie on the parietes of the sarcolemma. The author supposes that the nuclei may stand in close connexion with the small granules, and may originate them. In the nuclei a granular mass is contained, out of which possibly the granules, reminding one of the sarcous elements, may proceed. Should this supposition be confirmed, one must look on the sarcolemma, almost surrounded by vessels, as a matrix which receives the blood, and upon whose inner surface the granular structures are formed, out of which proceed the elements of the interior of the muscle. Hence it is that a piece of excised muscle cannot be reproduced, as the matrix of the individual fibre is destroyed. The above-described matter of research into muscular tissue has led, in the author's hands, to another interesting observation —i.e., as to the fact of the increase or not in the number of muscular fibrils during the growth of an animal.

After alluding to the prevailing opinion, that in the growth of the body the increased size of muscle is owing to the increase of the substance placed between the fibres, and especially to the inquiries of Harting,† Kölliker,‡ Deiters,§ &c., who assert that the number of future muscular fibres is determined from birth, he objects that, according to the usual method of admeasurement, owing to the smallness and close connexion of the fibrils, their relative size cannot with precision be recognised. He proceeds to state that, by reason of his observations, he has concluded that during growth entirely new muscular fibres are formed. He carefully counted the fibres in the gastrocnemius muscle of the frog, both young and old, and gives a table showing the addition of numbers acquired by the muscle. Then he proceeds to consider the method by which the new formation is attained, which may be, apparently, either a creative act, the new fibres proceeding from the existing ones, or a new formation out of blood-fluid from the capillaries. The author, however, has not observed sufficiently to give an answer to the query as to their method of origin.

GLANDULAR SYSTEM.

On the Glands at the Root of the Tongue. By F. Gauster, of St. Andrä.‖—A large portion of the communication is occupied in defending the views entertained by Kölliker, concerning the sebaceous glands at the root of the

* Zeitschrift für Wissenschaft. Zoolog., Band viii., 1856.
† Recherches Micromètr., 1845. ‡ Gewebelehre, p. 280.
§ Dissertatio de Incremento Musculorum, 1856.
‖ Moleschott's Untersuchungen, Band iv., Heft. ii., p. 134.

tongue and tonsils, in opposition to those of Sachs.* Although, in some respects, the author takes exception to the statements of the former, he concludes with the following observations on the ducts of the mucous glands in the tongue of the deer:—He says that these ducts in man take a direct or arched course at about a right angle to the surface of the tongue, and open by puncta; but in the deer, on the contrary, they open into large wide follicles whose diameter is from four to ten times that of the ducts, which are formed by fibrous connective-tissue containing no form-elements. The contents of these pouches or follicles are composed of a hyaline, homogeneous mass, projecting, on section, like a plug, with the properties of mucus. The pouches are formed far from the surface, and after winding about in various directions between the deeper-placed gland-lobules, gradually approach the surface, and thus at various points receive the ducts from neighbouring gland-lobules. Then running a longer or shorter distance near to and beneath the surface, and becoming thickly clothed with epithelium, they suddenly emerge free upon the mucous surface, or open into the follicles of the sebaceous glands.

The pouches are considered by the author as reservoirs for mucus, which is expressed from them by surrounding muscular pressure.

THE BLOOD.

On the Division of Blood Cells in the Embryo. By R. Remak.†—The author prefaces his observations by noticing his former objections to the "generatio equivoca" of cells maintained by Schwann, and his former statements made in 1841, which were supported by Kölliker and Gerlach, regarding the increase of embryonic blood-cells in birds and mammalia by division. This division of blood cells (and it is the same also with muscular fibre-cells) is only a single instance out of a series of such phenomena. From these and other examples of a like nature, Remak asserts the establishment of a complete reform in cell theory, being supported in his views by Leydig, Schultze, and Virchow, but opposed by Henle and Reichert. In the year 1856, the author made fresh observations upon the embryo of the chick. The method adopted he gives as follows:—The egg, between the third and sixth day of incubation, is to be opened at one side and laid flat on the other until the embryo rises to the top of the yolk. A bloodvessel must then be snipped, and a drop of the blood collected on dry glass, when a number of round or oval cells will be seen, having a constricted appearance, i.e., engaged in division. Should the colouring matter of the cell be less dense, and the illumination strong, then the nucleus will be seen in each half of a constricted cell, situated sometimes nearer and sometimes far from the constriction, and containing one or two nucleoli.

At times dilution of the fluid is requisite for a good observation, as of warm sugar and water, or a weak solution of bichromate of potash. Occasionally the cell-membrane engaged in division is so distended in its whole circumference, that the double cell appears as a single oval or round cell with nucleoli. If the drop of blood has been observed to become cold, or the division of the cell has proceeded too far, the double cell breaks at its place of division, and one might think that there were two cells adhering to each other. Of the double cell there appears to be two kinds, as observed in the recent state. In one there is only a slight constriction, giving to the cell the form of a pair of rolls of bread before separation; whilst in the other, there is at the place of constriction a fine dark transverse line, or a clear stripe, as if the colouring matter was here interrupted and separated by a clear substance projecting from the cell-membrane, and dividing the coloured contents. The cell-membrane then becomes

* Observationes de linguæ structurâ penitiori (Dissertation).
† Müller's Archiv, 1858, Heft ii., p. 178.

elevated on one side, the protoplasm still retaining its indentation, and we have a deceptive appearance of an endogenous cell formation. The nuclei and nucleoli can also be seen undergoing division in blood cells of the third and fourth day of incubation, at which early period more cells are dividing, the colouring matter is less dense, and the nuclei less concealed. There is no doubt that the division of the cells commences with that of the nuclei. At the beginning or middle of the third day, one sees at times only a few cells with double nuclei, whilst in almost all we have nuclei and nucleoli engaged in the act of constriction; and these are either double or three or four-fold. Twelve hours later many cells exist with double nuclei, and single or double nucleoli. All these phenomena are only preparative to the active increase of the cells which occurs at the fourth or fifth day, when the vessels are filled with blood for the purposes of the allantois. As a rule, the nuclei and nucleoli divide into two, but sometimes into four. In mammalia, the existence of four nuclei in one cell is frequent. Sometimes, cells divide into two parts of unequal size, but this is rare. It is, however, frequently so in the common snake, and not uncommonly so in the chick. Again, some cells grow into very large flattened oval discs, whilst others are engaged in division, causing great disparity in the size of the cells.

The division of cells appears to coincide with the increase of the blood, which mainly occurs between the third and eighth day, and after the twelfth day no normal division is observable.

Remak then proceeds to allude to his former mention of the existence of "biscuit-shaped" nucleated blood corpuscles in the chick, coloured red at one extremity, and to his doubts whether the biscuit-shaped blood-cells mentioned by Kölliker* were normal structures. He states that these doubts have been confirmed by later observations. He describes certain bodies which in the last week of incubation he met with, in which only one half contained a nucleus, both halves being connected by a colourless pouch-like part, which is in reality none other than the extended cell-membrane. These structures appear to arise by an abortive process of division, and this idea corresponds with the fact that cells with constriction exist from the sixth day, in which only one half contain a nucleus. These cells do not appear in all embryos or in all days of incubation, but chiefly in the last week, and appear attributable to an unusual diminution of temperature or to other evil influences.

Size of the Corpuscles, &c.—Henessy† states that the blood corpuscles at the under part of the blood-clot are smaller than in the upper part, and in blood-serum the corpuscles which have sunk are smaller than those swimming above. In inflammatory blood, the corpuscles are smaller and darker than in healthy blood, but on being dried they become like the normal dried corpuscles as to size and colour.

Blood-corpuscle-holding Cells.—Leydig and Berlin‡ agree with Remak in looking upon these bodies as masses of blood-coagulum, in which the corpuscles are in a condition of disintegration and crumbling down. As a proof, Leydig states that he had met with them in greatest abundance in the flesh of the tail of the fish in the neighbourhood of entozoa. Berlin§ found them in the coagulum of the vena portæ of a bird. In this case they could not have existed during life, and disappeared with the general cadaveric softening of the fibrin.

* Geweblehre, 1852, p. 21.

† Dublin Hospital Gazette, November, 1857, p. 347.

‡ Henle und Pfeuffer's Zeitschrift, 1858, p. 18.

§ Archiv für die Holland Beitrag. zur Natur- u. Heil-kunde, Band i. p. 356: quoted from the above.

Part II.—Pathological Micrology.

Tumours, morbid growths, excrescences, etc.

*Cholesteatoma of the Scalp.**—Dr. R. Volkmann, of Halle, minutely describes a tumour of this nature, which was removed from the head of a woman, aged sixty-six. The patient, when sixteen years old, had suffered from swelling of the cervical glands, with fever and gastric symptoms, and also erysipelas of the face and head, which symptoms returned three or four times. When thirty-eight years old, she perceived under the skin, at the posterior and upper side of the left parietal bone, a small knot, of the size of a millet-seed, which occasionally was somewhat irritable, and grew so slowly that it, in twenty-two years, only attained the size of a hen's egg. It was moveable, and smooth on its surface. When sixty-two years old, the patient again experienced erysipelas of the head, and whilst delirious she threw herself out of bed, striking and laying open the tumour. After this the tumour quickly increased in size, and extended over a large part of the skull, and the skin covering it began to ulcerate. Finally, the tumour reached from one ear to the other, and from the lambdoidal to the coronal suture, and its surface in places became covered by whitish-red convolution-like prominences, equalling a walnut or more in size, which were covered by a fine layer of granulations, or already had partially cicatrized.

In places the ulcerations were deep, the clefts being covered with a cheese-like grumous material, from which small pieces were at times thrown off. The tumour possessed considerable consistence and cartilage-like elasticity. Eventually, except on the upper surface exposed by ulceration, the tumour was surrounded by a thick, almost tendinous, investment of connective tissue, which sent thick processes down into the tumour, forming closed cyst-spaces and chambers, in which were embedded the tubers and lobes of the tumour. These tubers and lobes had great resemblance to clumps of atheroma which have been long preserved in spirits, and were seen to consist, on section, of a tolerably firm but crumbly material. The tumour had completely destroyed the soft parts, and there existed on the outer surface of the heavy thickened skull-cap a number of shallow pits and impressions containing portions of the growth. On longitudinal section, a multi-locular texture appeared, the separate nodules being separated by fibrous parietes, and varying in size from that of a millet-seed to that of a small cherry. They also varied in form, being generally round, at times egg-shaped, flattened, &c. In one place, near the centre, the intervening fibrous material was very abundant, to the exclusion of other elements. Many could easily be removed by the scalpel out of their position, showing a smooth yellowish surface, whilst others were retained in position by fine offshoots from the fibrous separations. On microscopical examination, all within the septa was seen to consist of large rounded or angular epithelial cells, chiefly containing fat and albuminous molecules, and often showing a nucleus. In every direction the cells rather assumed the concentric arrangement common in cancroid formations, and in the centre of such concentric formations an enlarged cell or a hollow space, filled with granular material, was frequently formed. These concentrically-disposed cells were never very flattened, or formed into a fibre-like texture, but always retained a more or less globular and more seldom angular form. Other large quantities of cholestearin crystals lay between them, and often also destroyed cells and cheese-like altered substance. Between the fibres of the strong septa collections of epidermis cells existed, showing the rapid growth of the formation. In one part near the centre of the tumour, numerous large vitreous globules, varying in diameter from 0·10 to 0·18 mm., and also large regularly-shaped polyhedral and very transparent cells, containing a round, glittering, hollow space, occupy about one-third of the cell, but no transitions were visible between the large,

* Virchow's Archiv, January, 1858, Band xiii. Heft i. p. 46.

almost structureless globules or vesicles and these formations. The author compares these formations to those of the cylindroma of Billroth, the pouched cartilage of Meckel, and the proliferous enchondrosis of Virchow. After giving various reasons why the growth should be classed among the cholesteatomata, or among the atheromata, and quoting Virchow and Wernher, the author determines to place it among the former group.

Atheromata. By Dr. F. Hartmann, of Tübingen.*—The author, after giving eight cases of this species of tumour, with all details, general and microscopical, infers, as Wernher did, that atheromata have their origin as solid tumours, and that there is a stage at which no traces of softening of their contents can be found.† He then proceeds to deliver objections to the view entertained by very many, that the atheromata are only altered sebaceous follicles of the skin, specially opposing the position maintained by Ribbentrop, and concurred in by Virchow, who asserts that this species of tumour has its origin from one or the confluence of more sebaceous follicles, and that the contemporary existence of these comedones forms a means of diagnosis of atheroma.

Hartmann contends that the fact of the tumour being solid originally, is adverse to the idea of its only being an enlarged follicle. He proceeds to describe the minute anatomy of the enlarged follicles, which possess almost universally the cyst thickened, and appear to be void of capillaries. Moreover, whilst the atheroma at an early stage is hard and mostly rounded, the enlarged follicle is soft and lax, and easily compressed.

On section, the contents of the atheroma observe a certain arrangement, whilst those of the mere follicle appear like a fortuitous heaping up of epithelial cells, the larger being softer than the smaller ones; and in none was observed any cartilage-like cavities of the contents, as in the case of the atheromata. In all the latter examined, large or small, the excretory duct was closed. After alluding to the small nucleated polygonal cells forming the contents of enlarged follicles, when of the smaller kind, he states that in those besetting the inner surface of the cyst no free fat is found, but that here and there fat is found in the cells; but in the larger follicles free fat and cholestearine is found, which may have the same origin as in atheromata. In one case detailed by him, however, where the atheroma was as small and young as it could be, fat was already formed, but no cholestearine. At a further stage cholestearine was found. He never found calcareous salts in the enlarged follicles, although he does not deny that at a further stage of their existence they may exist, but he has often met with those small round so-called amyloid bodies. Should the same circumstances, decomposition and exclusion from the air, attend an enlarged follicle containing cells, as are present in the case of atheromata, then the same products would be produced, and the final result would be a cyst with atheromatous contents, standing in the same relationship to atheromata proper, as the chlolestearine cyst does to the cholesteatoma. As respects the clinical history of the atheromata, more than one tumour is found as a rule to exist at the same time, and in different stages of development; but if an atheroma has been met with still solid, it has always been solitary. The author thinks that the supposed connexion between the *comedones* and this kind of tumour is controverted by the consideration that in the majority of cases no *comedones* exists along with them, and from the fact that so many people are subject to the frequent enlargement of the sebaceous follicles who are not subject to atheromata. The process of softening appears to depend upon fatty change occurring in the middle parts and advancing outwards; at a later stage, cholestearine crystals and calcareous salts are found. At first this degeneration proceeds tardily, but afterwards it keeps pace with the growth of the tumour. Should the tumour attain a large size, then we have a sac filled with thickish fluid, which however possesses a thin layer of consistent contents in immediate

* Virchow's Archiv, Band xii. Hefte 4 and 5, p. 431. † Ibid, Band viii. s. 237.

connexion with the cyst. On minute examination of a transverse section of an atheroma, next to the dark cyst will be seen a number of pale, clearly-nucleated cells. The nearest cells are not at all arranged, but gradually begin to observe a concentric lamination, and still further near the centre, fatty changes begin; this serves to explain the fact that the peri-cyst is the seat of the formation of new cells. On a closer examination of the inner sides of the peri-cyst, it is seen to be covered with small, partly rounded, and elongated cells, with one or more nuclei and often nucleoli. The cyst wall is the formative locality for cells by which the contents of the tumour is increased, and it is supplied with bloodvessels, often abundant and of large size. Cells, however, appear also to be formed in the contents themselves, at the various spots where net-like aggregation of cells exist. At these places a clear centre is often seen to exist, containing pale nucleated cells, and often only a clear granular mass. More outwards the cells are darker and granular, and gradually the nest-form is lost, and the cells elongate parallel to the concentric laminæ. These foci of new formation arise originally from the peri-cyst, and are gradually pushed inwards by new cells, but remain in connexion with the membrane, for on tearing it away the connexion by means of broad clear threads, very like elastic fibres, is visible. These fibre-like connexions may be the means for the conveyance of nourishment. Among other general observations, the author concludes by referring to the influences likely to induce the formation of these tumours, such as blows, &c., but refers to cases in which no hereditary tendency was known to have existed.

NERVOUS SYSTEM.

Two Cases of Pathological Development of Areolar Tissue in the Brain. By R. Leubuscher.*—In the first case, along with other unusual appearances, both lateral ventricles were very much enlarged, but the right one more so than the left, and filled with a thickish yellow fluid, which flowed out with difficulty, and after a time separated into a thinner and a thicker colloid-like mass. The middle and back part of this ventricle was occupied with a cystic formation whose upper investment was connected with the ependyma of the ventricles. It was surrounded by softened brain. On section, the cyst was seen to be fan-like, with many processes passing from its inner wall; and other similar formations communicated laterally with it by its fan-like parietes. The cysts were filled with fluid like that in the ventricles, and numbers of small cysts were suspended in the fluid. Beneath these cysts lay a roundish tumour, clearly defined from neighbouring parts, being $2\frac{3}{4}$ zoll. in length and $1\frac{3}{6}$ in breadth. This tumour and the cysts considerably pressed on and diminished the contiguous thalamus opticus. The growth consisted of a greyish-red flesh-like material, containing gelatinous fluid, which penetrated the substance irregularly, and enclosed white, soft, broken-down medulla-like parts. This outer substance consisted of a firm areolar tissue for the most part, being in places very easily split up, and in others more compact, and containing numerous large colloid-globules and free nuclear bodies. Many of these were filled with fat. Besides, there were very peculiar, large, irregularly-formed cells, with numerous nuclei, having processes anastomosing with each other, and many containing glittering colloid drops. Capillaries, with aneurismal bulgings, also existed, but no nerves.

In the white masses, however, nerves were seen disintegrated, and also amyloid and inflammatory bodies, with fatty detritus. No epithelium was found on the outer surface of the cyst-wall, but on the inner surface very delicate flat epithelium was seen; and also, in addition to the larger cysts, smaller growths arising from it, with vascular loops and network, existed.

* Virchow's Archiv, May, 1858, p. 494.

The cystic formations in the above case seemed to have arisen from the ependyma of the ventricles and choroid plexus. Possibly the areolar-tissue-corpuscles were originally the seat of the deposition of the colloid. The specimen was from a boy aged seven, who for some time had had a scrofulous disease of the knee, and suffered from headache, fever, and vomiting; he had also been the subject of hydrocephalus.

In the second case—that of a lunatic, aged forty, who had apoplexy and paralysis—general induration of the brain, and frequent cicatrices and relics of extravasated blood, were found in various parts of the brain, as well as recent effusion on the outer side of the right corpus striatum, beneath which was a thicker dense mass, occupying the under part of the right anterior lobe, and much of the middle one. In addition, yellowish-white spots of a soft, butter-like consistence, without any trace of extravasation of blood, were found over the centre of Vieussens, not abruptly limited, but passing diffusely into the healthy brain. The harder portions consisted of a fine fibrous tissue, here and there finely granulated, and beset with small nuclei and scanty granular cells and fat drops, from which the scanty vessels were free. Very slight traces of brain-fibres existed. On close inspection, the fine areolar fibrils were seen to proceed from large irregular cells, with one or more nuclei, and these cells had processes as high in number as twenty, dividing and anastomosing. The soft butter-like mass consisted of small cells, with one or two nuclei, numerous granule-cells and aggregations of granules, amyloid bodies, and free fat, with margarine crystals. A few uninjured brain-fibres existed. The large vessels of the brain were free from atheroma.

RESPIRATORY SYSTEM.

Lung containing small Calcified Enchondromatous Masses. By Professor Förster, of Göttingen.*—The specimens were two in number, of the size of a horse-bean, and situated in the middle of a thickened and dark part of the lung-substance. They possessed no positive capsule, and were very hard when broken, showing a shining surface, in which a somewhat concentric arrangement was visible. Microscopical examination of a very fine section exhibited a closely calcified foundation substance, containing large, round, dark, calcified cells. On removing the salts with acid, the matrix became partly homogeneous, partly fibrous, and cells were seen like large cartilage-cells with thick capsules and endogeneous elements, partly simple, partly compound. In a very few places only was non-calcified cartilage-tissue visible. These specimens had been sent to Förster by Hahn, of Hanover, and were removed from the body of a man in whom no other traces existed of enchondromata. The growth of the tissue had most probably been arrested by the calcification.

Ramifying Formation of Bone in the Lung.—Förster also describes† a specimen of the above, existing in Langenbeck's collection (after referring to a similar one described by Luschka.)‡ The lung was beset by fragmentary pieces of bone, varying in size up to half a line, more or less twisted and branching, like vessels. The ramifications were bony, or consisted of solid fibre-bands alternating with bony parts, and the fibres consisted of areolar and elastic tissue, which, when freed from bone, showed an arrangement as in arterial walls. The pieces of bone had not special medullary canals, but consisted of a compact outer layer, containing concentric lamellæ bone cells, and an inner network-tissue, with a homogeneous matrix and bone cells, the meshes containing areolar tissue, which sent out the above-mentioned fibrous bands. On the addition of sulphuric acid, areolar-tissue cells were seen in the place of

* Virchow's Archiv, Band xiii. Heft 1, p. 106. † Ibid., Band x. s. 500.
‡ Ibid., Band xiii. Heft 1, p. 105.

bone cells, but no elastic tissue; hence the whole product was not the result of ossification of vessels or bronchi, but of the interstitial pulmonary tissue. The author does not venture to determine whether all this was the result of inflammation or not.

MISCELLANEOUS.

Case of Amyloid Degeneration. By Dr. O. Beckmann.*—The subject of this affection was a man aged sixty, who had Bright's disease of the kidney and a large sore on the skin, in connexion with thickened periosteum and deposit of bone on the tibia. The heart was large and fatty, and portions of the mitral valve flap, which was thickened, gave the usual red colour on the addition of acetic acid and iodine, and the violet-red colour on the addition of sulphuric acid. The same reaction was observed in the fine vessels around the vesicles of the glands at the base of the tongue, tonsils, &c. In the thyroid gland were four or five tubera of the size of a pigeon's egg. Of one of these the sectional surface was homogeneous, of a bluish-grey colour and wax-like look; another contained an irregular cavity, filled with dark fluid blood, and both were surrounded by an investment of areolar tissue, forming a kind of network with very narrow meshes, containing chiefly fatty and occasional homogeneous colloid corpuscles. Between these cavities the material contained thick-walled vessels and corpuscles of various kinds. These parts also gave distinct amyloid reaction, as did the vessels of the highly-developed areolar-tissue-stroma of the gland between the tubera. Amyloid degeneration was also found in the small arteries and capillaries of the mesenteric glands, the glomeruli and arteries of the central parts of the kidneys, the vessels of the mucous membrane of the gall-bladders, and the fine vessels in most parts of the intestinal mucous membrane.

On the Changes undergone by Cartilage in Disease of the Joints. By Professor C. O. Weber, of Bonn.†—The author's observations, which are accompanied by interesting illustrations, have, among other points, special reference to the intra-cellular origin of pus, and the new formation of bloodvessels in inflamed cartilage, and bear a great resemblance on certain points to those made by His,‡ upon the cornea. This might be expected when one considers the properties, which exist in common, of the cornea and articular cartilage.

The author first enters upon the changes undergone by cartilage in acute suppuration and so-called caries of joints, whether arising in connexion with rheumatism or scrofulous inflammation. He considers the joints of the hand and foot as best suited for examination, as in them the disease generally has not so far advanced. By the comparison of numerous joints in various stages of disease, the following changes are seen to occur. In the first place, the membrane investing the cartilage loses its smoothness and brilliancy, and a few small, slightly-roughened islets of pale yellow look are formed. These multiply and run irregularly one into the other, so that only here and there is the natural opal colour of the cartilage visible. At the same time a delicate network of vessels grows forth from the synovial membrane around the edge of the cartilage, which at first may be removed like a thin veil. Later on, this fastens on the cartilage, as it were, with fine projections, which may at first be easily torn away, but afterwards so penetrate the cartilage that the vascular network can be no longer separated from the cartilage. Very often the vascular circle surrounds the cartilage like a ring, but later on in the form of a disc. It often also extends in a ramifying way, and frequently this ramification will send up its twigs on both sides into the opposed cartilage, and thus

* Virchow's Archiv, Band xiii. Heft 1, p. 94.
† Ibid., Band xiii. Heft 1, p. 74.
‡ Beiträge zur Histologie der Cornea, Borel, 1856.

adhesions are formed very like the false membrane of the pleural cavities. In fortunate cases these constitute the forerunners of future ankylosis; but in unfavourable ones, they become destroyed by fatty metamorphosis.

On perpendicular section, the deeper parts of the cartilage next to the base is often found healthy, and then the bone itself is sound. Should, however, the degeneration advance towards the bone, the cartilage may itself be entirely destroyed, owing to the growth and formation of cavities, as also by fatty degeneration, and then the granulations of the synovial membrane reach to the bone. If the bone have been previously diseased, we then have, in addition, an upward growth from this, of areolar and vascular tissue into the cartilage. In the other cases, the growth from the synovial membrane is unimportant, but on the contrary, we have springing from the diseased bone, granulations, which either lift off the cartilage so that it becomes merely a thin disc in a state of fatty degeneration, or become fused down with the already diseased cartilage into a soft lardaceous mass, beset with numerous vessels. In all instances the cartilage is finally absorbed, and in its place a granulating surface is formed. This generally becomes profusely covered with pus, which mostly makes for itself a path outwards through one or more fistulous openings, which are clothed with granulations. Very often, and especially if the vascular development proceeds quickly on both sides, and if it be very abundant and the cartilage be destroyed by fatty change rather than by suppuration, one finds the granulations on both articular surfaces intimately united with each other like the tufts of the chorion, giving rise to a kind of ankylosis. Subsequently the vessels degenerate in favourable cases, and shrink into connective tissue, and then by ossification a perfect ankylosis is effected. In some cases, especially if the bone adjacent to the diseased cartilage be softened by the growth of capillaries and areolar tissue into the medullary substance, by copious fatty formation in the medullary spaces and accompanying loss of bone tissue, the formation of granulations advances towards the diaphysis, the articular extremities become worn away, and there is, especially in the knee, a disposition to sub-luxation. Should the formation of granulations proceed regularly, we then have the so-called insensible exfoliation, inasmuch as only small bony lamellæ become here and there necrosed. If, however, it advances irregularly, it may happen that larger parts of the bone may become surrounded by granulations, and embedded in them as sequestra, which is particularly so in the case of the tarsus and carpus. Fatty changes and formation of fat cells may often be observed in the granulations as well of the synovial membrane as of the bone itself.

As regards the alteration in the cartilage cells, &c, the earliest observed appearance is a marked enlargement of the cartilage cells, and especially of those which adjoin the upper surface of the cartilage. The cells increase in number, and the nuclear corpuscles are seen divided and increasing, and forming groups of from two to six in number. The cells change with various rapidity, but eventually all the cells, as deep down as the bone, become diseased. Those cells near the surface attain in size to large vacuoles, filled with smaller cells by fusion of neighbouring spaces and destruction of intervening tissue. The foundation material disappears towards the surface, owing to the extension of the cell cavities, the cells collapse, and the smoothness of the cartilage is interfered with. The superficial surface seen from above, shows lacunæ and irregular pits filled with the young brood of cells; and a perpendicular section will show the remainder of the intervening substance projecting in the form of irregular dentate club- and thread-shaped processes. Then it is that the numerous cells inside the large spaces obtain the characteristics of pus, and, owing to the destruction of the walls, become free, and mix with the synovial membrane.

Very often also, along with increased growth of the cell, the contents become cloudy and granular, and even previous to the collapse of the cell the

nuclei may be converted into detritus, and often the outline of the lacunæ becomes indistinct as if dependent on a gelatinous liquifying of the intervening substance. Fatty degeneration may affect the cell-contents, the nuclei, and the matrix of the cartilage, but it always leads to destruction of the cartilage.

Along with these changes in the cells it may be observed that a granular, and occasionally a fibrous condition of the matrix is assumed, and always a tendency to splitting of the same, pointing out the way to vascularization, whether from the synovial membrane or the bone.

As respects the formation of bloodvessels in diseased cartilage, this is effected in two ways—firstly, by the formation of club-shaped processes, solid at the beginning, and composed of accumulated spindle-shaped cells, which proceed direct out of pre-existing capillaries, and grow beyond and over the cartilage in a fringe-like manner; and secondly, by the hollowing out of a branching network of young connective-tissue corpuscles. The latter method especially comes into play in the deeper vascularization of the cartilage.

As regards the origin of the solid cellular club-shaped processes, they are formed from the nuclei of the capillaries at the edge of the cartilage. Spindle-shaped nuclei or cells are seen to accumulate at the edge of a capillary loop, which quickly developes into solid bud-like processes, which again, on their part, push out similar processes, and thus form vascular ramifications, the original branch from the capillary becoming hollowed out into a cylinder.

Along with these changes we have originating a network of areolar-tissue corpuscles. A star-like cell increases in size, its nuclei increase, the star-like processes increase, and also intervening anastomoses; and out of such a net of budding connective-tissue cells a system of irregular vessels is formed, which, by final fusion with a neighbouring capillary vessel, becomes accessible to the flow of blood. The above-described growing cells especially affect the cavities in the cartilage tissue, in order to push their bud-like processes into them. Very often enlarged cartilage cells are surrounded by a group of areolar-tissue corpuscles in a circular manner, and it is possible that the latter may have proceeded from the increase and division of the cartilage cells, but it is difficult to determine whether the cartilage cells play any part or not in their changes. Not infrequently in the neighbourhood of the surface, the matrix of the cartilage possesses cartilage cells having great resemblance to the star-shaped areolar-tissue cells on the surface of the cartilage. Similar processes of vascular formation extend also from the bone, the cartilaginous matrix becoming cleft in a fibrous way, and granulations growing forth into the cartilage from the capillaries of the medullary canal in the form of solid cellular cylinders. In this way a gradual destruction of the cartilage occurs, and in its place luxuriant granulations, which either, as before said, lead to ankylosis or form that fungous investment of the articular surface seen in the worst cases. In the above-described cell-formation, moreover, many cells, as is the case in all granulations, do not subserve the organization of vessels, but undergo changes into so-called pus corpuscles, swelling out, their contents becoming cloudy and their nucleus corpuscles increasing. Tuberculization, shrinking of the cells, and granular detritus, are often found in the granulations.

The above description the author gives regarding the processes of *acute* inflammation. He then proceeds to treat of the *chronic* form of inflammation, the so-called "malum senile," alluding to the researches of H. Meyer,* Zeis,† and Förster.‡ He describes a case which he looks upon as a typical one, as showing every stage of the disease. This was that of a man, aged thirty-seven, who died of pneumonia. In him almost every joint was affected—the head of the femur and humerus showing the characteristic fungoid growth in various stages, and the foot, knee, elbow, and hand joints showing the earlier

* Müller's Archiv, 1849. † Nov. Acta. Acad. Leop. Carol. 23.
‡ Spec. Path. Anatom., s. 797.

stages of development. The first trace was found in the limitary cartilage near the bone. Here an osteoid layer was seen, and then complete bony formation, passing through all the steps found in naturally growing bone. Soon began the changes, also, in the tufts of the synovial membrane, which form red fringes, often of extreme length, and are stronger and more abundant in the reduplicated fold of the synovial membrane, and consist of delicate club-shaped projections, growing by lateral buds, with a capillary loop in the centre, surrounded by young areolar tissue. These increase by the outgrowth of an insignificant lateral loop of the vessel and of the corresponding lateral boss of the tuft into a long club-shaped projection. Very often spindle-shaped cells are found inside the projections, their nuclei coming out on the addition of acetic acid, and even also round large many-nucleated cells. The latter species of cell cannot be distinguished from cartilage cells, and commence on their own part an independent growth in the club-shaped process. The small pedicle may then contract, or even be torn off, and then the above-named growth lies loose in the joint. Such free bodies may again undergo change, and even go on increasing in size to a great extent. This appears to be by reason of nourishment received from the surrounding fluid, which is conveyed on from cell to cell. These free bodies may undergo ossification, both true and false, and also fatty degeneration.

Recurring to the condition in the earlier changes in the joints, the author describes the loss of brilliancy on the part of the cartilage, which assumes a velvety roughness, along with the occurrence of the prevailing growth of the synovial membrane. This becomes so great that the cartilage becomes in places split up to the bone into numerous firm fibrillæ, which eventually fall away, thus forming a cavity as if a piece had been bitten out. This cavity generally has a sharp edge, and its base, composed of the bone, is very injected. Presently, at the edge of the articular surface, new growths of bone appear, in the form of a wreath of wart-like osteophytes. After the bone becomes denuded the process advances rapidly, and the bone becomes necrosed, and the defect extends, chiefly owing to the motion of the joint, in a direction from before backwards. The exposed and new-formed bone becomes very vascular, and one sees a film of hardened substance protecting the medullary cavity. As the removal of the cartilage takes place where pressure chiefly exists, a ring of cartilage remains at the periphery. The newly-formed bone goes on, still being removed in the centre, whilst increasing at the edges. The synovial membrane still goes on also, forming fringes and often tubercular growths, and the moveable body may even become capsulated.

As regards the minute histological changes observable in chronic inflammation of articular cartilage, as before said, the earliest is ossification in the boundaries of the bone; then comes destruction of epithelial cells on the free articular surface, and then a striped condition of the matrix perpendicularly to the surface. Firstly, one sees a fine punctate cloudiness between the cartilage corpuscles. These cloudy parts coalesce, and there is formed a wavy fibrous cleaving between the capsules, extending from the surface downwards, and giving rise to a splitting of the cartilage into club and peg-shaped processes, which pass at their tips into broad branched fibres after the displacement of the cartilage corpuscles. The latter bodies thus lose their regular arrangement, increase in size, and fall out or undergo fatty changes, or finally grow luxuriantly, giving rise to roundish swellings in the club-shaped processes, nests filled with cartilage cells. Possibly they may, after displacement, grow further, and become the free bodies in the joint. With the increased splitting the cartilage becomes destroyed, until the bone is quite exposed. Whilst the above is going on, the cartilage near the bone has been undergoing ossification, that portion dwindling where pressure exists, and increasing at the periphery. This newly ossified part has the same histological elements as normal bone.

The author concludes by observations upon the origin, in certain cases, of pus corpuscles from within cells, as from cartilage cells in the case of articular inflammation, or as observed by His in the interior of the corpuscles of the cornea; the pus corpuscles in these instances not being formed around pre-existing nuclei, nor by the division of cells. This supposed method of origin of pus corpuscles is supported, according to the author, by the origin of spindle-shaped cells in the solid cellular cylinders observed by His in the cornea, and by Billroth in granulations.

HALF-YEARLY REPORT ON FORENSIC MEDICINE, TOXICOLOGY, AND HYGIENE.

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I. Toxicology.

Experiments with Bibron's Antidote to the Poison of Rattlesnake.—Mr. Louis De Vesey reports that, whilst at Contra Costa, on his way to Fort Tejon, in California, a boy was bitten by a rattlesnake. In a short time the wounded limb (the leg) was much swollen, and there was a good deal of pain in the part. The remedies prescribed were unavailing; the boy was completely prostrated, and the bitten limb, and even the body, were much swollen. The next morning, all hopes of recovery having been abandoned, Mr. Louis De Vesey was requested to prescribe for him. He immediately gave ten drops of the bromine mixture, diluted with a little alcohol and water, and four hours afterwards repeated the dose. Very soon after the first dose amendment commenced, and progressed rapidly after the second dose. Towards evening all swelling and pain had disappeared, and the next morning the boy was as well as if nothing had happened.

In the vicinity of the forts rattlesnakes are found of very large size; and desirous of further testing the power of the antidote, Mr. De Vesey procured three dogs, and subjected them to a series of experiments with it, after causing them to be bitten by the snakes. The first dog—a small but powerful animal—was bitten twice, in a very short period, in the throat; the characteristic symptoms soon supervened. Mr. Vesey then gave him ten drops of the mixture, and he almost immediately began to improve. The next morning he was well, but the swelling did not entirely disappear for two days. The other dogs yielded similar results; one dose of the antidote was always sufficient, except when the wound was inflicted by the "*crotalus lucifer*," which always inflicts wounds of more than ordinary severity. Two or three doses were generally necessary in bites from this species. In all, Mr. De Vesey performed seventeen experiments on these three dogs, and always successfully. He found, too, that dogs which had taken several doses of the bromine mixture were for some time afterwards incapable of being infected by the poison of the rattlesnake.—*The American Journal of the Medical Sciences*, April, 1858.

[There is now so much important evidence collected in favour of Bibron's antidote, that we feel bound again to direct special attention to it. If there is such an antidote, surely its virtues cannot be confined to the treatment of snake-bites alone. The composition of the antidote will be found in our last Report.]

Arsenical Poisoning.—Two Ounces of Arsenious Acid taken without fatal results.—Dr. W. C. Jackson, of Montgomery, Alabama, U.S., reports a most interesting case, in which two ounces of arsenic were taken without fatal results. The patient was a man of twenty-eight years of age. He was first

seen by Dr. Jackson at 1 P.M. on March 23rd, ten or fifteen minutes after he had taken the last dose of the poison, and one hour after he had taken the first portion. He had taken the poison at three distinct intervals; he did not complain; his pulse was a little excited. He was ordered twenty grains of sulphate of zinc, which he at first refused to take, but after some persuasion consented. It was also ordered that the hydrated oxide of iron should be prepared, and that two ounces of the subcarbonate of iron should be given after free vomiting. At 2 P.M. there had been slight vomiting, with some traces of the arsenic. The subcarbonate of iron was given, followed by albuminous drinks. By three o'clock he had thrown up some three or four drachms of the arsenic, in mass, and the subcarbonate had been repeated several times. The dose of sulphate of zinc was now repeated, and followed by mustard and water, which produced considerable effort and vomiting. When the stomach was emptied of its fluid contents, two ounces of the hydrated sesquioxide of iron were administered, and the same dose was ordered to be repeated every hour, with albuminous drinks in the intervals. At six o'clock there was great irritability of stomach, and a burning sensation in the stomach and throat. Another mass of the poison was ejected, as much as six or eight drachms. The irritability of the stomach continued until eleven o'clock, after which the patient rested well. The hydrated sesquioxide of iron was then discontinued. The patient from this time began rapidly to recover, and with the exception of a quickened pulse and slight headache for a day or two, exhibited no notable symptom.

From the careful evidence collected by Dr. Jackson, we feel no doubt but that at least two ounces of the poison were taken. The young man was under arrest at the time. The points of interest in a toxicological point of view are, as Dr. Jackson explains, the large quantity of arsenic taken; the fact that the greater part of the poison was retained in the stomach six hours; the freedom from pain; the absence of inflammation of stomach and bowels; and the complete recovery.

The mode in which the arsenic was taken is another point of great interest. The first and second portions were taken in a glass of wine, the remainder in water; and, not being soluble, the mass was scraped from the vessels in which it had been placed, and swallowed. The patient had taken no food that day, which renders the slow absorption of the poison the more remarkable.—*The American Journal of the Medical Sciences* for July, 1858.

Poisoning by an Arsenite of Copper, with Experiments.—Dr. Edward Schaefer, of Gratz, relates the following case: A girl, aged fourteen, a maker of flowers, took designedly half an ounce of arsenite of copper. She was seized with severe symptoms, and in an hour and a half was brought into hospital under the care of Dr. Schaefer. On his arrival he found that vomiting had set in; the vomited matter deposited an evident green sediment. She had sunken eyes, and trembling of the limbs; she complained of great thirst, a burning sensation about the throat, and malaise; she spat up mucus streaked with blood; the gastric region was tender, and touching it gave great pain; the pulse was 132. At a later period pain set in in the calves of the legs. Vomiting was encouraged by the administration of lukewarm water; hydrated oxide of iron was given, in tablespoonful doses, every five minutes, and afterwards at greater intervals. The first doses were vomited, the rest were retained. The vomited matters were subjected to analysis by Schneider's method. The matters, treated for half an hour with common salt and sulphuric acid, yielded a distillate, which treated by Marsh's test gave a distinct arsenical crust. After two hours the patient was not worse. As she had passed no urine for four hours and a half, the catheter was used, and urine was drawn off. In this fluid a considerable quantity of arsenic was found. With the exception of headache, no other symptom occurred on the first day. She gradually improved, and on the fourth day had regained her appetite; but on taking food she had

a sensation of pressure in the region of the stomach. On the fourth day a diuretic was given. Arsenic was found every day in the urine; but how long it was thus eliminated after she left the hospital, could not be ascertained. She was dismissed cured on the ninth day. Dr. Schaefer endeavoured to ascertain how much arsenic was contained in the poison taken by the girl, and how much was excreted in the fæcal matters during the first five days, so as to be able to draw some conclusion as to the effect of hydrated oxide of iron as an antidote to arsenious acid. In 100 parts of the commercial arsenite of copper there were found—of oxide of copper, 30·46; of arsenious acid, 54·36; acetic acid, 9·13; foreign substance, 6·25=100, 20. The girl had taken 9·51 grammes of arsenious acid; the fæcal matters obtained for analysis contained 4·23 grammes of arsenious acid. It is inferred that the remainder passed off in the matters eliminated by vomiting and in the urine. Dr. Schaefer also investigated the following questions:

1. When does arsenic appear in the urine after taking arsenical preparations, and how long does its elimination continue after the medicine has been discontinued?

2. Can arsenic be found in the urine after its endermic application?

3. Is arsenic retained in the bones for a longer period, as tribasic arseniate of lime, or is it eliminated from them as from other tissues?

4. Is arsenic found in new growths, and in the scales of eczema and psoriasis?

1. A paraplegic man was taking daily twelve drops of Fowler's solution; of the urine passed four hours after, the first portion contained arsenic. In another instance the urine was removed by the catheter; one hour after the first dose of Fowler's tincture, arsenic was found in the urine. Arsenic was also found in the urine an hour or two after its administration, in cases of epithelial cancer, cancer of the breast, eczema, psoriasis, and syphilis. Dr. Schaefer concludes from these inquiries, that arsenic may be detected in the urine one hour after the taking of ten drops of Fowler's solution. As the result of twenty-four observations, he also concludes that there is no fixed period for the last appearance of arsenic in the urine, but that it is dependent on the quantity administered and on the length of time during which it has been given. The arsenic is more rapidly eliminated when diuretics have been taken.

2. After the application of an arsenical powder to an ulcer on the foot, the urine passed at the end of half-an-hour contained traces of arsenic, but the simple application of arsenic to the skin gave only a negative result.

3. To a patient suffering from carcinoma of the breast, and unfit for operation, two drachms of Fowler's solution were given, in doses of five drops daily; the last dose was taken three days before death. Dr. Schaefer removed a thigh-bone from the body after death, and after carefully cleansing it from the soft parts and stripping off the periosteum, he subjected it to concentrated hydrochloric acid. After three days' maceration at the ordinary temperature, the solution was diluted with water, and distilled in Schneider's apparatus. The distillate yielded distinct evidences of arsenic by Marsh's test. Arsenic acid is found instead of arsenious acid in Fowler's solution when it has been made for some time, and this was the case in the present instance. The arsenic, it is inferred, must therefore have been retained in the bones for a longer time as a tribasic arseniate of lime, through the isomorphism between the arseniates and the phosphates. Dr. Schaefer was the more confirmed in this idea by observing that while the bones yielded a copious arsenical deposit, the carcinoma, rich in bloodvessels as it was, examined at the same time, gave only a small trace.

Dr. Schaefer repeats the statement, that in Styria many persons are, from various causes, addicted to the use of arsenic. A servant woman, aged thirty-five, a native of his (Dr. Schaefer's) district, came under the care of Professor Rzehaczek, with gangrene of the left leg. She said that some time previously she had the care of horses, and used to give two horses a handful of "arsenic"

daily. It is a superstition that, in order that horses may thrive, the person who has care of them must also eat arsenic, so she took every day a pill of arsenic (*Hüttenrauch.*) In consequence she had frequent vomiting and pain in the abdomen. After she had taken arsenic for two years, the vomiting became more frequent, and she wasted. She had a dry, hard, desquamating skin, and a remarkable wasting of the whole subcutaneous cellular tissue. Amputation of the thigh was performed on account of gangrene of the leg; she had left off taking arsenic eight or ten weeks previously. Dr. Schaefer first analysed the soft parts surrounding the bones of the leg, and found no arsenic; nor did he find any in the bones. The arseniate of lime must therefore have been removed, or the arsenic must have been eliminated from the bones at the same time as from the other tissues. It is yet a desideratum to examine the bones in a case in which the use of arsenic shall have been left off for four weeks.

4. Dr. Schaefer does not seem to have arrived at any satisfactory results in regard to the fourth question.—*Zeitschrift der k. k. Gesellschaft der Aerzte zu Wien*, 8 März, 1858.

Arsenical Poisoning. Arsenic in Subnitrate of Bismuth.—Dr. Rogers, of the Philadelphia College of Physicians, has directed the attention of the College to a case of a very singular character in a medico-legal sense. A woman was being tried for administration of arsenic. The trial was cut suddenly short, and the prisoner released (although it was proved that she had procured arsenic, and other circumstances tended to create the suspicion that arsenic had been criminally administered), on account of an accidental impurity having been discovered by Dr. Rogers in the medicine which the attending physician had administered to the deceased. During the trial the Court called for a copy of the prescriptions which had been ordered by the physician; and subnitrate of bismuth being amongst them, Dr. Rogers directed his efforts to determine how far that substance was pure. He was unable to procure a specimen of the identical subnitrate which had been administered, but obtained a number of samples from individuals throughout the city, and prepared, many of them, from different manufacturers. Of the specimens thus obtained, some of which were imported, all were found on examination to contain arsenic, with the exception of two. The quantity obtained was small, too small to be presumed to have any material effect except in long-continued and frequently-repeated doses. Moreover, it was not shown, in the case in question, nor was it probable, that enough could have been taken in that way to produce the vomiting and purging, much less the fatal result. It was to be remembered, also, that the symptoms attributed afterwards to the poisoning, had begun and continued for some time before the bismuth had been taken. The stomach and intestines of the deceased contained a very small quantity of pulpy mucus, and the evidence of arsenic was well marked; the stomach and bowels contained arsenic. Despite these circumstances, the fact of the discovery of arsenic in the medicine was considered sufficient to invalidate the evidence against the accused, and warrant her immediate acquittal. In the discusion which followed the reading of Dr. Rogers's communication, several fellows stated that, although they had been accustomed to prescribe bismuth for several years, and often for considerable periods, no symptom of arsenical poisoning had ever been observed.—*American Journal of the Medical Sciences*, July, 1858.

Arsenic in Paper-hangings.—A dispute has been going on during the past six months in reference to the possibility of the air of a room, the walls of which are covered with paper-hangings containing arsenic, becoming charged with arsenic, and injurious to health. Dr. Halley, Mr. Dugald Campbell, Dr. A. Taylor, Mr. Paul, and others, have joined in this debate; and the *Times* and other general papers, as well as the scientific journals, have given room in their columns for the contest of authors. We have read through all that has been written, and cannot but infer from the evidence that the evil results said to

arise from the use of paper-hangings containing arsenic are not established. The following series of inquiries by Mr. F. A. Abel, published in the 'Pharmaceutical Journal,' seem to us as the most conclusive of all on the point at issue. Two experiments were made by Mr. Abel, in the first instance, in a room the walls of which were covered with paper exhibiting a large pattern of a light green colour. The surface of the paper was not glazed, and the coloured portions were found to have on their surface about two-tenths of a grain of arsenic per square inch. In the first experiment the room was kept thoroughly closed for about thirty-six hours; a portion of its atmospheric contents were then drawn by an aspirator through a solution of nitrate of silver, and then through a tube containing asbestos, moistened with ammonio-nitrate of silver. In the second experiment air was passed through these tests for a longer period than in the first (about five hours), and three gaslights had previously been kept burning in the well-closed room for several hours. Not a trace of arsenic was discovered by a careful examination of the tests at the close of these experiments.

In another experiment, a large glass tube (three feet six inches in length, and two inches in diameter) was filled with slips of arsenical paper-hanging, placed in a small room in a position where it could be raised to a temperature of 90° Fahr., and connected with a similar apparatus to that employed in the former experiments, containing solutions of nitrate and ammonio-nitrate of silver. Air was then drawn through the apparatus uninterruptedly for one week, by means of Johnson's aspirator, three gaslights being kept in the closed room, at intervals, for several hours. The air passed through the apparatus at the rate of one cubic foot in two hours. In a fourth experiment, air was passed through the apparatus as before, for twenty-four hours; the tube containing the paper was kept warm, and the air was made to pass through a strong solution of sulphurous acid before it reached the tube. In a fifth experiment, the end of the tube at which the air entered was connected with a large flannel suspended immediately over a burning gas jet; hot air, mingled with the products of the combustion of gas, was thus made to pass over the paper for three days. This experiment was repeated, the air being passed uninterruptedly for four days. In a seventh experiment, hot air was passed through the tube, as in the fifth and sixth experiments, for nine days, the coloured surface of the arsenical paper having first been thoroughly roughened by friction, so as to promote the mechanical detachment of the arsenical pigment by the current of air, and to favour to the greatest extent the possible volatilization of arsenic. In an eighth experiment, strips of arsenical paper were pasted together back to back, with paste in a state of decomposition. Air collected over the gas-flame was passed over this paper for a period of nine days; the tube containing the paper was not heated until after the second day.

The result of all these experiments was, that not a trace of arsenic could be detected in the solutions in any one of the experiments; neither were any particles of arsenic carried over mechanically.

But in order to furnish indisputable proof that the green arsenical colour employed in the manufacture of paper-hangings is not affected by air even when in a finely-divided and perfectly unprotected condition, six hundred grains of finely-powdered emerald green were uniformly dispersed through a quantity of cotton-wool, sufficient to fill compactly a tall jar of about a gallon capacity. A tube connected with the test apparatus, and plugged with cotton-wool, was passed to the bottom of the jar, and air was drawn through the apparatus continuously for one week, the jar which contained the emerald green being maintained at 90° Fahr. during a portion of the time. Not a trace of arsenic was found to be volatilized at the conclusion of this experiment.—*Pharmaceutical Journal*, May 1st, 1858.

Poisoning by Camphor.—A case in which camphor acted as a poison is put on brief record by Dr. T. M. Woodson. On March 27th, 1858, a negro girl,

aged ten years, obtained improperly a bottle containing tincture of camphor, and drank a portion. The amount taken is not very certain, but the tincture, from which she drank twice, contained seven and a half grains of camphor to the drachm. The first symptoms excited were those of wild delirium; she ran up-stairs, and then fell headlong down. When Dr. Woodson saw her, two hours after the dose had been taken, he found her in the wildest fury, screaming at the top of her voice, running violently against surrounding objects, and having to be under the guardianship of two persons, to prevent personal injury; she talked constantly and incoherently, and did not know those about her; her extremities were cold, her head hot, her pupils contracted, and her pulse at eighty-four, but small and feeble. She had vomited a short time before, and the vomited matter smelt strongly of camphor, which led first to the discovery that the camphor tincture had been swallowed, a little negro boy admitting on inquiry that he saw her drinking from the bottle of camphor tincture. Dr. Woodson produced free emesis with ipecacuan and antimony, and dashed cold water on the head. The delirium was quieted, but she then fell into a state of profound coma, from which she could not be aroused; the pulse fell to sixty, and became full; the breathing became slow and stertorous; the pupils underwent dilatation; the extremities became warm; there was slight tetanic rigidity of the jaws, and the eyes were drawn forcibly inwards. The coma lasted four hours, when she awoke with irritable stomach and staggering gait; the latter symptom remained several days. She ultimately did well. On being questioned, she admitted that she had partaken of the camphor tincture. Dr. Woodson thinks this case as unique in some respects in toxicology, but refers incidentally to another case, upon the authority of Professor G. B. Wood, in which fatal symptoms resulted in an infant of eighteen months old, from a dose of ten grains of camphor.—*New York Journal of the Medical Sciences*, July, 1858.

[The case is rare and valuable, but a few others have been recorded, as well as numerous comparative experiments by Orfila and Scudery. The symptoms induced in all cases by camphor administered in poisonous doses, are very uniform, and in strict accordance with those observed in Dr. Woodson's patient.]

Summary.—In addition to the papers and cases referred to already, there are many others of importance published during the past six months to which we can do little more than give brief reference.

Elimination of Poisons.—In the 'Glasgow Medical Journal' for July, Professor Easton has an admirable paper, entitled 'General Observations on the Elimination, Catalysis, and Counter-action of Poisons.' In his conclusion, and by way of summary, the learned Professor remarks that the phenomena of saturnine poisoning illustrate in a very striking manner some of the general propositions to which he has alluded. First, there is the initial act of elimination performed by Nature herself. This is partial in its operation, and though generally sufficient to depurate the blood of the extraneous element, is quite inadequate to remove it farther. Secondly, the very incompetency of the natural effort to complete elimination, while all-protective to the vital fluid, inflicts serious injury on a limited portion of the tissues. Thirdly, in order to repair the damage which these tissues have sustained by their union with the saturnine compound, a catalytic agent is required to unbind the connexion, and there cannot be the slightest hope of success from strychnia, galvanism, or any other medicinal appliance which is not capable of exerting the necessary catalytic influence. Fourthly, the iodide of potassium exerts the necessary power, and with it the poison is eliminated along the channel of the kidney.

Poisoning by Laudanum.—In the 'Gazette des Hôpitaux' for the 3rd of July, a remarkable history of poisoning by laudanum is recorded. In the night

of February the 3rd, a man and his wife, residing in Strasbourg, took about two ounces and a half of laudanum for the purpose of suicide. Their room was not entered until three o'clock on the following day. The man then had been dead some time, but the woman showed signs of life. She was taken to the hospital, and while being conveyed vomited a yellow liquid having the smell of laudanum. The *interne* on duty at the hospital found the woman offering the following symptoms:—She lay on her back; the features were immovable and calm; the eyes were open, and strangely fixed; the pupils were contracted, but dilated a little when the lids were depressed; the limbs were rather stiff; sensation was deadened, but not quite lost. She did not move or groan, but when shaken and interrogated, she answered all questions by the same words, "I know nothing of it." Her speech was slow, and well accentuated, but the voice was weak and without *timbre;* the pulse was thread-like, 110, regular; the breathing was reduced to four inspirations per minute; the temperature of the body was 98° Fahr. in the axillæ; the skin was cyanotic; the jugular veins distended. Emetics were given, and acted. For twenty-four hours she lay in much the same state; the heart-beat could be felt, and the sounds were well accented, but there was a venous pulse in the distended jugulars. The air penetrated the lungs, and the vesicular murmur was well marked at each inspiration. The urine was passed involuntarily. Papules about the size of pins' heads were observed in large numbers over the clavicles and chest. Infusion of coffee was given twice a day, and a glass of Malaga wine every hour. Gradually the woman recovered. On the 6th of February the inspirations were nine in the minute; on the 8th they were thirteen; on the 10th, fifteen; on the 11th, eighteen. The cyanosis had now disappeared, and the jugular pulse had stopped, movement had become free, and the features had relaxed. On the 18th the patient was able to sit up, but she remained sluggish, with paralysis of the right side. After a few weeks she sank into a state of complete exhaustion, with obstinate constipation and incontinence of urine; she died with bedsores. M. Schützenberger, who reports this interesting case, adds some short but very able comments.

Deaths from Chloroform.—Three deaths have recently occurred from chloroform—one in Paris, two in England; and one is reported in America, which occurred in 1856. The accident in Paris happened to a soldier of the Imperial Guard, on the 27th of May, in the military hospital. The patient was a strong man, and apparently of sound constitution. The intended operation was removal of the testes. The chloroform was inhaled from a folded compress, containing lint, and for a time the inhalation proceeded regularly. After two minutes, signs of consciousness to pain being still present, the chloroform was continued, when suddenly the patient sat up, threw up his arms, every feature of his countenance expressing suffocation, and immediately he fell back lifeless. At the post-mortem both lungs were found to contain miliary tubercle, and in the right lung there was detected the signs of a large cavity. The further history of the case will be found in the 'Journal of Practical Medicine and Surgery' for June, vol. xxix. The cases in England occurred, one at Epsom, on August the 27th, intended operation, tooth extraction; the other at Towcester. The first has not yet been correctly reported, but the patient, a young woman, breathed the chloroform from a napkin, and the death was sudden. The second case will be found described at some length in the 'Medical Times and Gazette' for September 11th. Mr. Watkins administered the chloroform from a cotton handkerchief, for the purpose of examining an injured toe, in a boy. The patient for a time resisted the chloroform, but seemed ultimately to come kindly under its influence. Mr. Watkins was now about to examine the toe when the patient gave one or two stertorous inspirations. The narcotic was withdrawn, but the change was apparent. The lips became livid, the pulse fell, and life was extinct. The

case in America will be found described in the 'American Journal of the Medical Sciences' for July, by Dr. W. A. Hammond. The patient was a soldier, and the chloroform was given, as in Mr. Watkins' case, to enable the surgeons to examine into the nature of an injury. Tincture of chloroform, made by mixing one part of chloroform with two parts of absolute alcohol, was used. It was inhaled from a tubulated bell glass, held to the mouth of the patient. The inhalation was continued eight minutes. Suddenly the eyes turned up, the muscles relaxed, vomiting came on, and the pulse stopped in an instant. It was found at the post-mortem that the patient had fatty degeneration of the heart. The inference as to the fatal issue in these cases is that, owing to the manner in which the chloroform was exhibited, an overdose of the vapour was inhaled.

Phosphorus Poisoning.—The 'American Medical Monthly Journal' for June contains a short summary from the labours of MM. Chevallier, Seur, and Abel Poirier, on the dangers arising from phosphorus matches. The number of cases of poisoning by phosphorus matches, recorded by the authors as coming under their notice between the years 1824 and 1858, are, cases of suicide, 18; of homicide, 21; and by accident, 12. Many other deaths occurred from phosphorus in other forms, to say nothing of the evils arising from chronic poisoning amongst the manufacturers of the match. To prevent these evils, the reports suggests that the fabrication of chemical matches out of ordinary phosphorus should be interdicted, and that in its stead red phosphorus should be used, either by the employment of the formulæ in which the red phosphorus enters into the paste, or by making use of Lundstrom's Swedish process, which consists in the use of a paste which cannot be inflamed except by friction on a small board covered with red phosphorus. The friction surface of the board in this process can be replaced by slips of paper, which, like postage-stamps, are gummed on one side, and coated with amorphous phosphorus on the other.

A Precipitant for the Alkaloids.—The 'Pharmaceutical Journal' for Sept. has a note describing a new precipitate for the alkaloids, by M. Sonnenschein. The precipitant is phospho-molybdic acid. This acid forms with ammonia, in acid solutions, a remarkably insoluble compound, and comports itself in a similar manner with those compounds which are analogous to ammonia—the nitrogenized organic bases—consequently forming a good reagent for their detection. It is thus prepared: Molybdate of ammonia is precipitated by phosphate of soda; the yellow precipitate having been washed, is diffused through water, and heated with sufficient carbonate of soda to dissolve it. The solution is then evaporated to dryness, and calcined to drive off the ammonia. In case any of the molybdic acid should be reduced by this operation, the residue must be moistened with nitric acid and again calcined. The dry mass is then dissolved in cold water, the solution strongly acidulated with nitric acid, and water added until ten parts of the solution contain one of the salt. The liquid, which is of a golden yellow colour, must be preserved from ammoniacal fumes.

It precipitates the solutions of all the alkaloids, with the exception of urea, when a mere trace only is present. The precipitates are yellow, generally flocculent, insoluble in water, alcohol, ether, and the dilute mineral acids, with the exception of phosphoric acid. Nitric, acetic, and oxalic acids, concentrated and boiling, dissolve them. These compounds are decomposed by the alkalies, certain metallic oxides, and the alkaline salts, which separate the alkaloid.

To give an idea of the sensibility of this new reagent, it may be stated that the 0·000071 gramme of strychnia gives an appreciable precipitate with one cubic centimetre of the solution of the phospho-molybdic acid.

Poisoning by Conium Maculatum.—Dr. Skinner relates, in the 'Liverpool

Medico-Chirurgical Journal' for July, the histories of four children who were poisoned by eating the leaves of the conium maculatum. The cases are full of interest, and the more so because the symptoms observed differed in many respects from those ordinarily attributed to conium. Dr. Skinner thinks he is justified in considering conium as a pure narcotic poison, void of acridity, and not specially affecting the functions of the spinal cord, its effects being exactly similar to those produced by morphia and its salts, excepting that it dilates the pupil, and the stertor is not so well marked. This view is corroborated by the post-mortem appearances found in the fatal cases.

Chronic Lead Poisoning.—In the same Journal, Dr. Fearnside relates three cases of chronic lead poisoning, in which symptoms of delirium and coma, and in one instance conditions allied to catalepsy and ecstasy, were presented. There were in all these instances evidences of derangement in the muscular system; but in two of the instances the mental derangement is the most striking fact. Death occurred in one of these cases, and the post-mortem revealed that the brain generally was softer than natural, and that both the grey and white portions were somewhat modified in colour, having acquired a dirty pale, yellowish hue. Dr. Fearnside's paper is one to which the medico-legal inquirer will often refer in the future.

Poisoning by a Salt of Cadmium.—The 'Journal de Médecine et de Chirurgie Pratiques' for March gives the outline of three cases, published by Dr. Sovet, in which dangerous and peculiar symptoms resulted from the absorption of a powder used for the cleaning of plate. Giddiness, dyspnœa, vomiting, purging, extreme prostration, and cramps in the legs, were the leading symptoms. The patients all recovered. On inquiry, it was found that carbonate of cadmium was the only metallic salt that entered into the composition of the powder.

A case of poisoning (not fatal), by a pectoral syrup known in France as "Briant's syrup," is recorded in 'L'Union Médicale' for June 22nd.—The medical evidence in the Eaglesham poisoning case is very fully given in the 'Edinburgh Medical Journal' for April and August. The report is concluded in the number for August, and has added to it an editorial summary. An important medical question, brought forward in the examination of the scientific witnesses, related to the possibility of the spontaneous production of prussic acid during the decomposition of healthy animal textures. The case will often be referred to by the legal tribe in support of the view that such production is possible.—In the 'Journal of the Society of Arts' for August 27th, there is a copy of a report to the Commissioners of Inland Revenue, by Mr. Phillips, chemist to the Commissioners, on "Arsenic in Paper-hangings." Mr. Phillips' experiments, like those of Mr. Abel, though not so conclusive, have led to the same conclusion—viz., 1. That a small bulk of air, allowed to remain for a considerable time in contact with a large surface of arsenical paper, and at a temperature of 80° Fahr., is not contaminated with the slightest trace of arsenious acid. 2. That the products of the combustion of gas do not facilitate the liberation of arsenious acid from the surface of green paper. 3. That arsenious acid is not volatilized from such paper, except at temperatures too high for human endurance.

Majendie was of opinion that after death from hydrocyanic acid, the muscles were but little sensible to the effects of the galvanic stimulus. In the 'Medical Times and Gazette' for Sept. 11th, a case is reported which is opposed to this view. A woman, the wife of a photographic artist, was brought into St. Thomas's Hospital, apparently lifeless, with the history that she had poisoned herself by drinking a solution of hydrocyanic acid. Dr. Clapton, one of the resident medical officers, applied galvanism, not with the effect of restoring animation, but with the result of exciting the most vigorous muscular action. The muscles of the face quivered, and those of the back and extre-

mities were thrown into such powerful action that the corpse was raised into the sitting posture. It appeared, in fact, that there was an excess rather than a reduction of muscular sensibility.

II. Wounds and Injuries.

Case of Gun-shot Wound by a Suicide.—A young woman, aged eighteen, was admitted to the Bellevue Hospital, under Dr. J. R. Wood, at two p. m., on March 22nd, 1858. Two hours previous to the admission she had attempted suicide with a large revolver, the barrel of which she had held close to her chest, and discharged. At the time she was in a standing position, with no dress on except her chemise. She fell, in a state of unconsciousness. Dr. Wood found that the ball had entered the body one inch and a quarter to the left of the mesial line of the ensiform cartilage of the sternum, just beneath the last costal cartilage, and two inches and a quarter distant from the left nipple. She held the pistol in her left hand, and it was slightly directed to the left side when it was discharged. Dr. Wood, on searching for the ball, found that it was lodged in the back. He extracted it by incision. The incision was two inches and one-eighth to the left of the spinous process, and between the eleventh and twelfth ribs, being seven-eighths of an inch further from the median line, and about one inch lower than the anterior opening. On admission she seemed to be suffering from severe shock. Auscultation revealed nothing abnormal in the thorax. Water dressings were applied to the wounds, and anodynes administered. Some symptoms of peritonitis followed, but ultimately the patient recovered, and left the hospital on the 8th of April. At the time of admission, and for some time after, she was in an excited mental condition, in consequence of a "love affair," which was the cause of the suicidal attempt.

The point of most interest in this case relates to the course taken by the ball. Dr. Wood is of opinion that the bullet did not pass directly through the body, as the sites of its entrance and exit would indicate, but that it passed round superficially, without penetrating any viscus or injuring the peritoneum. As there followed no ecchymosis or tenderness of the side, Dr. Wood modified his opinion, by supposing that the ball passed less superficially than he at first supposed. He attributed the peritonitis to the contusion produced by the pistol being held so close to the body at the time of its discharge.—*American Medical Monthly*, May, 1858.

III. Miscellanea.

Conveyance of Syphilis from an Infant to a Wet-nurse. Trial for Damages.—A very singular trial has recently occurred before the Civil Tribunal of the Seine. In January, 1857, a woman, named Madame Dumas, became wet-nurse to an infant, four months old, of Madame Perriraz. A medical man examined the nurse, and declared her healthy. The child had on its mouth sores, and on its body some pimples; to these, however, the medical men did not at the moment attach much importance, and the nursing commenced. The disease increased in the child, and in about a fortnight the nurse became affected—pustules appeared on the breast, and soon afterwards the general indications of syphilitic disorder manifested themselves. The child died of syphilitic angina, and the nurse, apparently first apprized by her medical adviser of the disease she had incurred, brought an action against Madame Perriraz. The Court gave judgment in favour of the nurse, stating that, whereas it was proved that

the disease was transmitted to Madame Dumas by the infant—whereas the result had been an injury to Madame Dumas—whereas this injury was such that the health of Madame Dumas was injured for ever, it was expedient for the Court to grant the amount of damages demanded—viz., 7680 francs and costs.—*Journal de Médecine et de Chirurgie Pratiques*, Mars, 1858.

Extraction of a living Child from a dead Mother.—Dr. G. W. Thornton, in the 'Cincinnati Lancet and Observer,' records a remarkable instance of the above fact. A mother died suddenly during labour, from some unascertained cause. Forty minutes after the death of the mother Dr. Thornton arrived. He found that the membranes had been ruptured, that the head was in the cavity of the pelvis, and that the vertex had presented at the inferior strait. Dr. Thornton raised the head, passed a hand into the flaccid womb, turned the child, and extracted it as far as the head without delay; at this point it momentarily hung, until assisted by the fingers in its mouth. Thus, forty-five minutes elapsed from the last expiration of the mother to the complete extraction of the child.

The child did not breathe, and was of a bluish colour, but a slight ticking sensation could be heard over the heart. The fauces were cleared, and Marshall Hall's ready method was diligently practised for half an hour, when a convulsive inspiration took place. The method was persevered in, and respiration was established. The child—a boy of average size—sucked vigorously at the bottle, and lived for three weeks and two days; its death probably resulted from want of sufficient care.—*American Journal of the Medical Sciences*, April, 1858.

The Medico-legal Relations of Pæderasty.—Dr. Tardieu has published an important memoir on pæderasty or sodomy, founded principally on the physical examination of 205 individuals, and on a series of twelve observations made by him at the depôt of the prefecture of police in Paris. After some preliminary remarks, the author states that pæderastic prostitution has reached in France an almost incredible extent, and has received an organization, the principal object of which is extortion of money by threats (*chantage*). Sharpers, speculating on the degraded tastes of certain individuals, ensnare them under the pretence of gratifying their secret passions, and fleece them without mercy. Accompanying these men, who are usually on the look-out, are young boys, corrupted by them, and receiving their wages; these boys they enrol, and rule over, and style them the tools which they use to attract their dupes and seize their victims. These miserable children, sometimes allured from honest labour, more frequently picked up in low haunts, are sent out every evening into unfrequented places, where they know how to pick up their prey readily. When they have succeeded in gaining the attention of a passenger, the individuals accompanying them show themselves suddenly, and, assuming the quality and language of police agents watching over public morals, end by extorting a bribe. More than this, the same practice is carried on in houses, and a rich capture may be the foundation of a profitable connexion.

Female and pæderastic prostitution are frequently carried on together. Some mistresses of brothels keep both sexes; and a prostitute once stated, in a public inquiry, that two-thirds of the men who frequented the house came there for boys. Other disgusting facts are referred to, and an instance is recorded in which a man offered his wife to the embrace of boys in return for the infamous pleasures which he enjoyed with them.

Many sodomites set no limits to their practices, and their bodies bear double evidence of their vicious habits. M. Tardieu gives the following classification of the habits in 205 cases: Exclusively passive, 99; exclusively active, 18; both active and passive, 71; not noted, 17.

Juvenile sodomites, who are stigmatized by the name of *tantes*, have their hair curled, their face painted, their waist narrowed, their fingers, ears, and chest loaded with jewels; their whole person exhales a most penetrating odour of perfume; they carry in the hand a handkerchief, flowers, or some piece of needlework.

M. Tardieu has in many instances been struck with the miserable aspect, the impoverished constitution, and the morbid pallidity of pæderastic prostitutes. He has very frequently recognised the propriety of the appellation *cracked chest* (*casse-poitrine*), given to some of them; and he has observed that the exhaustion of the physical and intellectual powers has led to pulmonary consumption, paralysis, and insanity.

The mouth has a peculiar appearance in some who descend to the lowest grades of infamy. In two cases, for instance, the mouth was awry, the teeth very short; the lips thick, turned inwards, and deformed—completely in accordance with the vile use to which they had been put.

The signs of active habits of sodomy are for the first time described by M. Tardieu. The penis is generally very slender; sometimes, though rarely, very large; in either case, the departure from the normal size is excessive in one direction or the other. The form of the organ is remarkable. When it is small and slender, it narrows considerably from the base towards the point; this is the most common shape. When the penis is much enlarged, the glans is constricted, and becomes tapering, and sometimes immoderately elongated. The penis is twisted on itself, so that the meatus urinarius is turned obliquely to the right or left. This twisting and change of direction sometimes reach a very great extent, and appear more marked in proportion as the penis is enlarged. Another appearance presented by this organ, which has been met with many times in the inmates of prisons by M. Jacquemin and M. Tardieu, consists in a globular dilatation of its extremity, the glans being enlarged and, as it were, flattened.

Of these malformations of the penis, some, as the narrowing, constriction, and elongation of the glans, correspond exactly to the infundibuliform disposition of the anus. The torsion and change of direction of the organ are also to be explained by the peculiar motion required for effecting an entrance into the resisting anal orifice.

The characteristic signs of passive sodomy are—excessive development of the buttocks, infundibuliform deformity of the anus, laxity of the sphincter, effacement of the anal folds, warty excrescences, extreme dilatation of the orifice; incontinence of fæces, ulcerations, fissures, blennorrhagia of the rectum, syphilis, and foreign bodies introduced into the anus. The signs of a recent attempt are well marked, according to the degree of violence used, the size of the parts, the youth of the victim, and the absence of previous vicious habits.

Setting aside the hypocritical protestations and equivocations of some sodomites, most submit readily, and even voluntarily, to examination. But a common practice with them is to contract the nates. They can thus prevent the funnel-shaped anus and relaxed sphincter from being at once seen; but it is merely necessary to make them suddenly change their position, or to place them on their knees on the edge of a chair in a leaning position, or simply to prolong the examination so as to tire out the contracted muscles. In cases where the funneling of the anus is not well marked or is wanting, the relaxation of the sphincter must be ascertained by the introduction of the finger. There may be externally a narrow contractile ring; but beyond this, the lower part of the rectum will be found greatly dilated. In other cases, the enlargement of the orifice, and its inability to retain the contents of the bowel, will be apparent at a glance, from the filthy appearance presented.

A question here arises, whether these appearances may not be produced by

certain pathological states of the rectum and anus, or by surgical operations performed on these parts. M. Tardieu states, that the persons examined never fail to allege these as causes; and hence it is important to ascertain whether the form, situation, and extent of the appearances presented can lead to an exact appreciation of their cause. The possible coincidence of ordinary diseases with habits of sodomy complicates the question; and, in general, it is only a probability that can be arrived at, rather than a formal conclusion.

Traces of violence are rarely met with, except in women or male or female children, who have been the victims of criminal attempts. The inflammation, redness, heat, painful itching, ecchymosis, excoriation and laceration of the anus, contusion and irritation of the sexual organs, especially of the urethra, the impeded gait, the agitation, and the fever which sometimes arises, can leave no doubt. The surgeon should establish a comparison between the lesions observed in the patient and the size of the organs of the person accused, in whom he will find the signs of pæderastic habits, both active and passive. If syphilis has been produced by sodomy, its development, progress, and different phases will have to be followed.

Several medico-legal questions regarding pæderasty, are set forth by M. Tardieu.

1. *Do traces of pæderastic habits exist?*—The results of a medical examination are either (*a*) negative; (*b*) characteristic of active habits; (*c*) characteristic of passive habits.

(*a*) When there is no material trace to raise the least doubt in the mind and conscience of the surgeon, he should distinctly express a negative conclusion. But if a direct examination of the organs does not remove every motive for suspicion; if the surgeon fears to be contradicted by alleged facts, and even by the accumulated proofs of a flagrant offence,—reserve is necessary. It must be said in such cases that it is possible that in certain persons the vicious habits may leave no traces impressed on the physical conformation.

(*b*) In seeking for the signs of active habits, the surgeon must bear in mind the changes which the male organ may undergo in dimension and in form from its normal condition. He must not forget that a tapering form and pointed extremity are connected with the slender penis; while the large penis is characterized by a twist on itself, a change in the direction of the meatus urinarius, and elongation of the glans, with constriction at its base.

(*c*) The evidence of passive habits does not consist only in the funnel-shaped anus; it is derived from a totality of appearances, not all of equal importance separately, but of great importance when taken together.

2. *Can syphilis be communicated by sodomy?*—It is not uncommon to find at the edge of the anus or orifice of the rectum, whether in the male or in the female, a very characteristic chancre; and, in the person accused, a specific ulcer at an exactly corresponding point of the extremity of the penis. Such facts are of the greater value, as the circumstances under which, in the adult, primary symptoms are manifested in the anus without unnatural connexion, are very rare.

3. *Has assassination been preceded or favoured by unnatural acts?*—The body of the victim and the person of the murderer must both be examined. In the victim, the position in which the body has been discovered, must be taken into account. Almost always it will be in bed; or, if there have been a struggle, it will be thrown on the floor adjoining, naked or slightly clothed. The murderer will almost always belong to the degraded class, and will present most distinct signs of passive sodomy.

The excuse of ordinary diseases or surgical operations as the cause of the appearances presented has already been referred to. Many sodomites allege that they are married, or that they keep mistresses, presuming on the common

notion that sexual connexion is incompatible with unnatural habits. But M. Tardieu says he has repeatedly observed sodomy to be practised by married men, as well as by those who associate with harlots.—*Annales d'Hygiène Publique et de Médecine Légale*, Janvier, 1858.

QUARTERLY REPORT ON PATHOLOGY AND MEDICINE.

By Edward H. Sieveking, M.D.

Fellow of the Royal College of Physicians, Physician to, and Lecturer on Materia Medica at, St. Mary's Hospital.

On Paralysis of the Cerebral Nerves through Affections of the Base of the Brain. By Dr. Ziemssen. (Archiv für Path. Anat. und Physiol., Band xiii. Hefte 2 and 3.)

The author, after speaking of the difficulties surrounding the diagnosis of diseases of the nervous system, strongly advocates the employment of galvanism according to the method of Duchenne for the purpose of determining whether we have to deal with a centric or an eccentric affection. He agrees with this observer, that in destructive processes affecting the brain, the electro-muscular contractility remains normal, whereas the electro-muscular contractility is diminished and gradually extinguished when the peripheral nerves are injured Dr. Ziemssen relates two cases in support of his views. In the first the exudation resulting from chronic meningitis caused compression and consequent atrophy of the right facial and oculo-motor nerves; the galvanic current applied during life to the muscles supplied by these nerves, caused no contraction whatever. In the second cause, traumatic injury gave rise to hæmorrhage at the base of the brain, with paralysis of the left facial, right oculo-motor and abducens, followed by paralysis of the left abducens, and partly also of the left oculo-motor nerves. The patient in this instance recovered; from the temporary loss of the power of reacting to the galvanic current, and the gradual return of the reaction, it was concluded that hæmorrhage had taken place external to the basis, and that the gradual absorption of the effused fluid was proportionate to the restoration of contractility on the application of the galvanic stimulus.

On the Development of the Larvæ of Dipteræ in the Frontal Sinuses and Nasal Fossæ of Man, at Cayenne. By Dr. C. Coquerel, Surgeon to the Imperial Marine. (Archives Générales, Mai, 1858.)

Five cases are reported by Drs. St. Pair and Chapuis, the medical officers attached to the Convict Hospital at Cayenne, in which the most violent symptoms, followed in three by death, were produced by the immigration and subsequent multiplication of a diptera into the nasal and frontal cavities. In most of the cases, several hundred larvæ were evacuated by ulceration and necrosis of parts investing the cavities. The cases all present symptoms so closely resembling one another, that it will suffice to give one in detail.

Goujon, a watchman, was admitted into the Hospital of Cayenne on the 5th September, 1855, complaining of intense pain in the supra-orbital region and in the right side of the face; there was neither tumefaction nor change of colour. On the following day there was severe headache, the other symptoms remaining. On the 8th of September, the right side of the face was swollen, and a sanguinolent fluid was discharged from the nasal fossæ. On the 9th, the whole face presented an erysipelatous swelling, with œdema of the eyelids and

of the upper lip; the skin was stretched, hot, and shining; on the bridge of the nose a tumour of a purple hue appeared, from which, on being incised, black, fœtid blood escaped. The patient having sat down, a larva was discharged by the nose. An injection of warm water into the nasal fossæ brought away about a dozen larvæ resembling the first. Delirium ensued; the head was bent back; the pulse hard and quick; the skin hot, with much thirst; the swelling of the face extended to the forehead, while the tumefaction of the eyelids entirely concealed the ball of the eye. Several injections of a solution of chloride of soda were made, and about twenty larvæ removed with the current. On the night of the 9th, the symptoms previously mentioned became more severe; local and general bloodletting, revulsive remedies applied to the intestinal tube and the extremities, cold affusion to the head and injections into the nares, failed to produce any amelioration. Death ensued on the 10th September, six days after admission.

Autopsy.—The tissues covering the nasal bones were gangrenous, and discharged a black and fetid liquid. The mucous membrane of the nasal fossæ was inflamed in its whole extent; about ten more larvæ were found crawling on the pituitary membrane. On a level with the middle passage the mucous membrane was red, thick, and softened. On opening the cranium, the meninges were found of a dark red, gorged with blood, which was particularly the case at the base of the brain. The ventricles contained a sanguinolent fluid; the brain on incision showed a large number of red dots.

In his remarks on the treatment, Dr. Coquerel lays great stress on the importance of injections, though he admits the extreme difficulty of removing the animals, especially after inflammation of the mucous membrane has set in; it does not appear that the plan of trephining the frontal sinuses which he recommends has been adopted. He advises the most energetic antiphlogistic treatment to be employed at the same time, a proceeding for which we can see no warranty, so long as the irritant cause is not dislodged.

None of the patients know how the larvæ were introduced, though it is probable that they were the issue of eggs deposited in the nasal fossæ. After being warned to prevent the entrance of insects, one of the convicts caught a fly which was about to enter, and it appears that this was a member of the tribe Lucilia, belonging to the order Dipteræ. Dr. Coquerel gives to the Cayenne variety the name *Lucilia Homini-Vorax*, and describes it thus: Length, nine millimètres (0·35 inch); yellow palpæ; the head and mandibles of a fallow colour, covered with a golden-yellow down; a very large head, broader at its base than at its junction with the thorax; the latter of a deep blue, with a purple reflex; on each side of the thorax, and in its middle, a transverse band of blackish-blue, the middle one being narrower than the others, and separated from them by a golden-yellow line of little brilliancy, and presenting a few purple reflexes. The abdomen was of the colour of the thorax, with purple reflexes accompanying each segment. The feet were black, the wings transparent, slightly opaque, especially towards the base, with black veins.

Dr. Coquerel does not regard the insect in question as a parasite, but considers the development of the larvæ in the site in which they were found as purely accidental. He quotes several analogous cases to those forming the subject matter of this paper, from authors who had observed similar occurrences in Europe.

On Hæmorrhage in the Cerebellum. By Dr. J. B. Hillairet. (Archives Générales, Fevrier, Mars, Avril, et Mai, 1858.)

The author remarks upon the comparative rarity of hæmorrhage in the cerebellum, which he estimates as occurring in the ratio of 1 to 30 or 35 of cerebral apoplexy. He quotes the prevailing doctrines regarding the symp-

tomatology of the disease, and then passes to the analysis of some detailed cases which have fallen under his own observation, and that of other medical men, making a total of 26. Several of these cases are given in detail, and are valuable contributions to neuropathology. The author divides the disease into two varieties—a slow and chronic form, and an acute and rapid form. In the *former*, the attack is preceded by occipital headache of greater or less duration, accompanied by vertigo and formication, or by habitual drowsiness. These symptoms generally indicate the precursory softening of the cerebellum. After a time there is a severe attack of giddiness and loss of power, with repeated vomiting, and either hemiplegia or paraplegia may supervene. Paralysis is absent in more than half the cases; facial paralysis is very rare. At the commencement there rarely is complete loss of consciousness; there is a tendency to coma and to complete relaxation (*résolution*); these symptoms go on increasing from hour to hour, until death ensues. Still, in the complete coma the patients are capable of being roused to understand and reply to questions addressed to them. Intelligence seems to be preserved, because, even if the patients do not reply, they open their eyes, and show by signs that they have understood the inquiry. In this condition the pupils are generally much contracted and immovable; there is occipital or general headache; convulsions and contractions may, but rarely do, occur: these indicate other lesions besides those of the cerebellum. The whole duration, from the time of the attack to death, does not exceed 120 hours. This form of the disease appears to be the least frequent.

In the *second* or acute form, the prodromata resemble those of the first, but are much less frequent; the patient is attacked suddenly and violently, is struck down not to rise again; he loses all consciousness for a time, then recovers it, and is able to understand questions addressed to him; he again relapses into complete coma, followed by absolute immobility, loss of sensibility, stertor, and death. Partial or general contractions or convulsions are very rarely observed. Vomiting also occurs less frequently in this form, though often enough to deserve to be mentioned. The pulse intermits, is small and hard, the expression heavy and stupid, the mouth drawn on one side. This second form is very brief—it does not exceed ten hours in duration.

There are between these two forms different degrees of intensity, but not presenting any characteristic features. The author has found 4 or 5 cases belonging to the first, and 21 belonging to the second class.

A minute analysis of the individual symptoms, which deserves to be specially consulted, is given, and the series of the author's articles terminates with a summary of all his conclusions.

Dr. Hillairet admits that there is often much difficulty in determining a diagnosis between cerebral and cerebellar apoplexy, on account of the intimate relation of the functions of the two parts. He lays much stress upon the presence of vomiting as a symptom of cerebellar apoplexy, which he thinks other neuropathologists have done wrong in attributing to cerebral apoplexy.

Again, consciousness is never so completely or enduringly destroyed in cerebellar as in cerebral apoplexy, for though the patient may be comatose, he is capable of being roused. Relaxation of the extremities accompanies cerebellar apoplexy; the patients are unable to support themselves in the erect position, but when lying down they can elevate their legs. When hemiplegia occurs, which it does in one-third of the cases, it is crucial, except in the face, for in 26 cases Dr. Hillairet has only once found deviation of the angle of the mouth. The special senses are unaffected, nor are there any convulsions.

A Case of Perforation of the Pericardium by Echinococci. By C. A. WUNDERLICH. (Archiv für Physiol. Heilkunde, Jahrgang, 1858. Zweites Heft.)

A labouring man, aged twenty-two, had always enjoyed good health, with the exception of an attack of peritonitis following a blow on the abdomen when sixteen years old. In June, 1857, he observed that his trousers were getting too tight for him across the belly, without other unpleasant symptoms. A week later he was attacked with colic, diarrhœa, headache, vertigo, and thirst; rigors followed, and he was brought to the hospital. The heart and lungs appeared healthy, but the diaphragm was pushed up to the fifth rib; there was high fever. The abdomen was tender, and the hypogastric region covered with a venous plexus; one prominence was observed in the epigastrium, and another in the cæcal region, due to moveable tumours. The former yielded a hollow percussion-sound, the latter one resembling the vibration characteristic of hydatids; the tumours were not more tender than the rest of the abdomen. The total evidence spoke rather in favour of the presence of cancer than of hydatid cysts. The tumours grew rapidly, the tenderness increased, the fever persisted, and icterus supervened, with severe epistaxis and hæmatemesis. Some improvement took place after the middle of the ensuing July, and the patient in August began to leave his bed; the idea of the cancerous nature of the tumours therefore was abandoned. On the 22nd September there was a temporary relapse, and on the 28th September severe pain in the abdomen, and dyspnœa, with great tenderness of the upper tumour. The patient recovered again somewhat, but the symptoms fluctuated more or less till the 18th October, when there was a sudden fall of temperature of the body, contracted features, cold sweats, small, slow pulse, quick breathing, increasing collapse, and death on the 20th October. We only note the prominent points observed in the autopsy twenty-nine hours later. In a pulmonary artery of third order, of the inferior right lobe, there was an echinococcous cyst of the size of a pigeon's egg; the remains of echinococci were found in the branches given off from this artery. Pleura healthy; pericardium distended up to the second rib, containing four ounces of a purulent fluid. The parietal layer was thickened and covered with yellowish-red villi; the visceral layer was $1\frac{1}{2}$ line thick; the heart reduced in size, its tissue pale and very friable. At the base of the pericardium there was a perforation with thin, smooth edges, which was covered by the heart, and which passed through the diaphragm, establishing a communication between the cavity of the pericardium and the epigastric tumour; the perforation was blocked up by a small echinococcous cyst which had got wedged into it. The left lobe of the liver was almost entirely replaced by a large hydatid tumour of the size of a child's head, contained numerous subdivisions with echinococci; otherwise, there was no marked derangement in the liver. The upper third of the spleen was occupied by a hydatid tumour of the size of a fist; in the retro-peritoneal space between the diaphragm and the stomach were three similar tumours of the size of apples; six were also found, from the size of a walnut to that of an apple, in the omentum. Between the psoas and the posterior surface of the cæcum was one of the size of a fist; a cylindrical one, three inches long and one broad, lay across the hypogastrium; above fifty were scattered over the mesentery, and two lay under the serous investment of the vermiform process. The intestinal mucous membrane was normal, there was no ascites, and nothing marked about the kidneys.

On the Lesions and Pathological Phenomena caused by the Presence of Lumbrici in the Biliary Ducts. By Dr. E. A. BONFILS. (Archives Générales, June, 1858.)

After combating Cruveilhier's opinion, that intestinal worms can be introduced into the biliary ducts only after death or during the death struggle, Dr.

Bonfils analyses the 23 cases which he has collected, in which lumbrici were discovered in the ductus communis choledochus, in the gall-bladder, or in the hepatic duct; in 2 cases the lumbrici were perfectly fresh and still living; in 1 the worm was dead and slightly altered, was of a pure white, and softened; in 1, reported by M. Forget, a lumbricus occupying the ductus communis and the ductus hepaticus was perfectly fresh, while another occupying an abscess in the right lobe of the liver was softened and macerated, evidently having been long dead; in 1 case a lumbricus formed the nucleus of a biliary calculus. The symptoms varied much in the different cases, but the author considers that the presence of the following circumstances justifies the conclusion that we have to deal with the presence of a lumbricus in the biliary ducts: the sudden appearance of morbid phenomena, without appreciable moral or physical causes, of considerable intensity, characterized by very violent pain, combined with deep colour of the skin, vomiting, &c., similar to the symptoms accompanying calculus in the biliary passages; a rapid disappearance of all phenomena on the discharge of the worm; the concurrence of these symptoms, unassociated with general colicky pains (*coliques extérieures*), are regarded by the author as indicative of a lumbricus being the foreign body which has entered the biliary ducts, and having thus arrested the passage of the bile.

Prurigo Pedicularis and Bronzed Skin. By Dr. Hardy. (L'Union Médicale, July 31st, 1858.)

The patient who was the subject of this observation in the Hôpital St. Louis, laboured under pulmonary phthisis, of which he died; but, besides the prurigo, for which he was admitted to the hospital, he presented gradually increasing bronzing of the skin, affecting chiefly the trunk. After death the whole left lung was found infiltrated with tubercles, and some tubercles were also discovered in the right lung; the suprarenal capsules were abnormally developed, and exhibited small yellow granulations on the surface; the left one was more voluminous than the right, and on section showed a genuine enveloping membrane, well distinguished by its yellow colour from the red subjacent tissue; it contained several tubercular granulations, which were easily picked out with the scalpel; a tubercle of the size of a pea, and numerous granulations of smaller size, were imbedded in the tissue of the organ. A similar condition, though not equally marked, existed in the right capsule. Except a few tubercles in the pelvis of the kidneys, these organs presented no notable change.

A Case of Progressive Muscular Atrophy, with Remarks on its Nature. By Dr. Baerwinkel. (Vierteljahrsschrift für die Praktische Heilkunde, xv. Jahrgang, 1858, Dritter Band.)

Dr. Baerwinkel, after giving the details of a case of muscular paralysis, and entering into an analysis of the views of authors entertained on the subject, adduces the arguments for the explanation which he himself offers of the phenomena. They are summed up in the following aphorisms. The primary derangement takes its origin in the sympathetic centres, in consequence of which the nutrition of the muscles is impaired and paralysis induced; as a result of the continued inactivity of the muscles, atrophy ensues, and fatty degeneration of the motor nerves takes place. The softening of the spinal cord, which is by no means a uniform occurrence, may be explained in the same way, by referring it to an extension of the pathological process to those fibres of the sympathetic which preside over the nutrition of the cord, or it is

a mere coincident complication. Probably the focus of the affection is to be sought in the spinal ganglia, and especially in those occupying the lower part of the cervical and the thoracic region.

We submit this explanation of the phenomena observed in the disease to our readers, without by any means endorsing it, as neither the facts nor the arguments adduced by the author appear to us sufficiently conclusive on the point.

Ecthyma Simplex, or Pustular Eruption on the Arms of Veterinarians from contact with the Vagina in Cases of Protracted Labour. By JOHN GAMGEE, Lecturer on Veterinary Surgery. (Edinburgh Veterinary Review, July, 1858.)

In the course of the protracted labour of a cow, Mr. Gamgee, Mr. Sarginson, a student of veterinary surgery, and a dairyman, had occasion to introduce their arms into the vagina of the animal, with a view to extracting the calf. All suffered from the appearance of a painful pustular eruption, which was particularly severe in the case of Mr. Gamgee, who describes it thus:—On the evening of the day on which the ineffectual attempts were made to deliver the cow, he felt a strong itching sensation on the right arm; "my left," he says, "was not altogether exempt, and on looking carefully, I found that the skin was the seat of a diffuse rash. Next morning the redness had augmented on both arms, but chiefly on the right, and the pruriency was replaced by pain. On the Tuesday evening, an abundance of small circumscribed pimples had formed, which suppurated on the Wednesday. By the afternoon of the 25th they were well formed pustules. The pustules were large, distinct, and surrounded by a red areola. Both arms were painful, the axillary glands slightly swollen, and I suffered somewhat in health. On the right, the pustules were very numerous, exceeding one hundred, but less so on the left." A lotion of diacetate of lead and opium relieved the pain, "but some of the pustules attained the size of a large pea, others became encrusted with a brown scaly scab, and others on bursting left behind a cicatrizing sore. I was tormented for six weeks, when the only indication of the pre-existing eruption was the brown appearance of the skin where the pustules had existed." Some weeks later a carbuncle formed on the inside of the right arm, just above the elbow, which healed slowly. The description is accompanied by an excellent coloured plate, the original of which was by Mr. Cruickshank. Mr. Gamgee collects cases from various sources, showing that the occurrence of the pustular eruption under similar circumstances is not unusual, and he also quotes an instance that has been reported by Dr. Murphy, proving that similar contact with the human female may produce analogous results.

Mr. Gamgee regards the poison capable of producing such effects as of a distinct class, resulting from an exhausting process, both mental and constitutional; he compares it to the poison of puerperal fever or of dissection wounds. For his arguments we must refer to the paper itself.

On the Pathology of Rheumatism. By FRANCIS T. BOND, M.B. (Midland Quarterly Journal, April and July, 1858.)

Dr. Bond analyses the prevailing doctrines regarding the intimate nature of rheumatism, and objects, with regard to the lactic-acid theory, which may be said to be the one most generally prevailing at present, 1, that lactic acid has not been shown to be in excess in the blood of rheumatic patients; 2, that, even supposing it to be present in excess, it would be difficult to trace the

connexion between this circumstance and the exudations in and about the different fibrous structures of the body; 3, that other acids being in excess in the secretions, and therefore possibly in the blood, they may be as much the cause of the phenomena as lactic acid; 4, that, in regard to the theory attributing the disease to suppression of the cutaneous excretions, it is doubtful whether it is preceded by greater suppression than the prodromata of all inflammatory diseases bring with them; and 5, that the extreme tendency to sweating which occurs during an acute attack of the disease, may be much better explained by another theory.

In order to establish a theory of rheumatism, Dr. Bond next analyses the phenomena of the disease, and finds that fatigue, exposure to cold, mental emotions or some other depressing agent, exercise a paramount influence in its production; febrile symptoms making their first appearance, followed by local affections in some fibrous tissue. A hyperinotic condition of the blood exists from the first, and the excessive fibrin having a special affinity for the fibrous structure, is specially deposited in and about them: hence the joints and the valves of the heart become the chief seats of the local affection. The preference shown in different cases for particular joints depends upon their greater weakness, or upon their labouring under some abnormal condition, upon the principle enunciated by Mr. Paget, that the depressed nutrition of a joint makes it more liable than any other part to be the seat of inflammation excited by the diseased blood. Dr. Bond's theory, then, reverses the order in which the different constituents of the diseases are commonly supposed to stand. Instead of regarding the hyperinosis merely as an effect of the reaction of the local disease upon the system at large, he considers it to be the primary source of the exudation, the causative agent of the latter, without which it could never exist. The increase in the urinary and cutaneous secretions, and the greater amount of urea, uric acid, lactic, phosphoric, and other acids in them, the author attributes to the metamorphosis of the fibrin; these substances being the products of the degradation of fibrinous matter, "the relations of urea and uric acid to highly-nitrogenized matters—as exhibited by the experiments of Lehmann, by the recent manufacture of urea by oxidizing albuminous substances by M. Béchamp, and by the general excess of these excreta in the hyperinotic states of the blood, combined with that of lactic acid, to the muscular juice as determined by the researches of Liebig—amply corroborate this statement as far as these three bodies are concerned; the others, from the smallness of their amount, may be put out of consideration."

Dr. Bond considers the sources of an excess of fibrin in the system to fall under three heads—1, as a result of imperfect primary assimilation; 2, as a result of a metamorphic process, normal in nature, but extreme in amount; 3, as a result of defective elimination of the fibrin by the excretory processes provided for the purpose.

Having said thus much, we must refer our readers for the conclusions which the author draws as to treatment to the paper itself; we will merely add, that his theory possesses a great resemblance to that propounded by Mr. Toynbee, a short time back, at the Medico-Chirurgical Society, shortly after the publication of the first part of Dr. Bond's paper.

On the Normal Course of certain Typical Diseases. By C. A. Wunderlich. (Archiv für Physiolog. Heilkunde, Jahrgang, 1858, Heft 1.)

Professor Wunderlich considers the thermometer (applied to the oral cavity, the armpit, or rectum) as the best means of determining the type of a disease, because its indications are but little liable to be influenced by accidental circumstances; thus, the character of the pulse, though presenting considerable

uniformity in different morbid states, varies too much, and is too dependent upon accidental circumstances, to allow of our using it for the purpose of establishing the types of disease by its aid. The author has not been able to determine any definite laws regarding some acute diseases—as acute articular rheumatism, peritonitis, pleurisy, and pyæmia, nor for chronic diseases generally; but he has found that most febrile affections run a definite typical course, which may be represented by the curves obtained by connecting the daily fluctuations of temperature. But even in these typical forms of disease variations occur, which depend upon the bearing of the individual, and upon influences to which he is subjected. But there are certain influences which are so uniform in their effect that this amounts to a law, and thus a new type is established; this is the case with regard to vaccination in its influence upon variola, venesection upon the course of pneumonia, or calomel upon typhoid fever. This knowledge of the typical course of a disease has a practical value, inasmuch as the diagnosis may be established by looking at the curve; the deviations from the normal condition are recognised, and the exacerbations, as well as the commencement of cure, are rendered visible. Moreover, when the typical course of a disease has been demonstrated, a look at the curve will protect from all illusions regarding our therapeutic achievements, while it affords a safe means of judging of really beneficial effects produced by remedial agents; the unusual reductions or diminutions of the course being shown irrefragably in the altered curve. Professor Wunderlich passes successively in review his observations on the following diseases:—Ephemeral fevers, which he does well in restoring to nosology; quotidian, tertian, and quartan agues; measles; erysipelas; scarlet fever; variola; typhoid and typhus fever; and several forms of pneumonia. Without reproducing the curves themselves, which give an ocular demonstration of the rising and falling of the thermometer in the various diseases mentioned, it would be futile to attempt giving the details of Professor Wunderlich's elaborate and valuable paper. The following general remarks will further indicate the kind of results which he has arrived at by this method of investigating disease. In some forms, the mode in which the disease commences is characteristic; the increase of temperature may be very rapid or slow, the maximum being reached in a definite period, or there may be a uniform rate of increase. The period in which the fever arrives at its full development affords numerous important points; the absolute elevation of temperature is determined in part by the special form of disease; on the other hand, it indicates its degree. The duration of the maximum point is even of more consequence than the absolute maximum, and the larger or less variation between morning and evening, and the number of days on which the temperature retains nearly the same elevation, are also points of importance. The duration of maximum elevation varies somewhat, but its duration must be regarded in the main as forming a characteristic feature of the individual type. In some diseases definite fluctuations occur during this period. The termination of the process, and the return to the normal condition, which the author terms defervescence, exhibit a very regular type, which is characteristic for every form of disease; the phenomena of defervescence are even more precise than those of the commencement and elevation of the morbid process, and serve still better to mark the nature of the disease.

On the Non-Periodical Fluctuations in the Frequency of Pneumonia, especially during the Years 1836–1856. By Dr. W. Ziemssen, Lecturer at the University of Berlin. (Vierteljahrsschrift für die Praktische Heilkunde, 1858, Band ii.)

Dr. Ziemssen presents us with the results of very extensive statistical

inquiries, embracing the mortuary registers of twenty-two localities scattered over different parts of the world. The variation of frequency differs very considerably; the average mortality from pneumonia in different places in England, Germany, France, Switzerland, Denmark, and America being 1·53, the maximum mortality in the same places from this disease was found to be 2·00, the minimum 1·08 per cent. On the other hand, the variation in the same place from year to year is very considerable; the author calculates that, assuming the mortality from pneumonia to be called 100, the annual fluctuation must be regarded as 19 per cent. This fluctuation does not appear to depend upon the influence of climate. From an examination of our Registrar-General's Reports, Dr. Ziemssen gathers that the fluctuation of the mortality from pneumonia in London does not vary as much as diseases which occur epidemically—as measles and hooping cough; while it varies much more than that produced by diseases which result chiefly from constitutional and general vital conditions. On examination of the curves representing the mortality of the twenty-one years (from 1836 to 1856), we find that the years 1838, 1840, 1842, 1844, 1847, 1849, 1851, 1853, and 1856, are characterized by a very high position in the scale, 1847 being the year in which the highest point was attained. The years 1836, 1839, 1841, 1845, 1846, 1848, and 1850, were the years in which the mortality was lowest. The fluctuations occurring during these twenty-one years may be regarded as a pneumonic wave, rising from the year 1836 to 1838, and then falling to 1839; again rising to 1840, and falling in 1841; a third elevation occurs, reaching its climax in 1842, and gradually sinking till 1846; the next elevation in 1847 is followed by a slow descent till 1850; a fifth rise takes place in 1851; a sixth, with the climax, in 1853 and 1854; and a seventh, of which the highest point was in 1856. We can only find space to dwell upon one or two of the author's remarks.

The year 1847 was a generally pneumonic year; of nineteen European localities of which the author has obtained the statistics, twelve attained their absolute maximum during this year; they comprised Great Britain, France, Germany, Scandinavia, and Eastern Russia; none of the localities show a diminution during this year. It appears from a statement by Dr. Rigler, which does not however possess absolute statistical accuracy, that during the same year pneumonia attained an unusual height in Constantinople; and again, according to Dr. Stratton, that the same was the case among the North American Indians.

With regard to the relative mortality of the two sexes, it appears that females bore the exact ratio to the general variations that characterized the different years. On examining the different ages, greater fluctuations were found to occur in childhood than in adult life; and it also happened that a pneumonia epidemic prevailing among children scarcely affected grown-up people; while, *vice versâ*, an unusual number of pneumonias might affect adults, and not be equally apparent among the children of the same locality.

In a brief review of the other diseases prevalent at the same time, Dr. Ziemssen concludes that the prevalence of pneumonia bears no proportion to the prevalence of other inflammatory disorders; and that, therefore, to determine the epidemic constitution, it is advisable to classify diseases, not according to nosological divisions, but according to the organs affected.

The whole paper is one of great value, and deserves to be specially studied by those who wish to appreciate the bearing of statistics upon the geography and history of diseases. Before concluding this notice, we would also draw attention to another careful essay by the same author, on the periodical fluctuations of pneumonia during the different seasons of the year;* in which he shows that, while considerable variations take place in different localities as compared with one another, each locality presents great uniformity as to the comparative frequency of pneumonia at certain seasons.

* Archiv für Physiologische Heilkunde, Jahrgang, 1857, Drittes und Viertes Heft, p. 393.

The Diseases to which Men engaged in the Manufacture of Sulphate of Quinine are subjected. By M. CHEVALLIER. (Archives Générales, July, 1858.)

In the Academy of Sciences, M. Chevallier presented a memoir on this subject, the main points of which are summed up thus :—

1. The workmen who are occupied in the manufacture of sulphate of quina are liable to a cutaneous malady, which may be extremely severe, and which compels them to cease from their work for a fortnight, a month, or more.
2. Some of the workmen are unable to continue the work, and are compelled to renounce it altogether.
3. M. Zimmer, a manufacturer of sulphate of quinine at Frankfort, has noticed that the workmen who were engaged in pulverizing bark in his manufactory were liable to a particular kind of fever, which he designates cinchona fever. It is sufficiently painful to compel some of the workmen affected to give up the occupation.
4. This fever has not been noticed in France.
5. The prophylactic measures by which the cutaneous affection can be warded off are not yet known.
6. The disease attacks not only the workmen engaged in the manufactories, but also persons who are exposed to emanations from the manufactories of the sulphate.
7. Temperate and intemperate workmen are both liable to be attacked.

QUARTERLY REPORT ON SURGERY.

By JOHN CHATTO, Esq., M.R.C.S.E., London.

I. *On the Theory of the Production of Hernia.* By Professor ROSER. (Arch. für Physiol. Heilk., 1858, pp. 60—70.)

THE views here advanced have already been published by Professor Roser, seventeen years since; but as they have not excited the attention he believes they deserve, he reproduces them with the advantage of being able to add, that all subsequent investigation during so long a period of an active career, has only confirmed his conviction of their truth.

Debating with a celebrated professor of surgery, he declared that a sudden production of a hernia was impossible, according to the laws of mechanics. The professor maintained the possibility, inasmuch as he had examined persons who had shown no signs of hernia, and yet, after violent exertion, its presence had become manifest. This, Roser regards not as observation, but as a *post-hoc* conclusion. It is well-known how difficult (or when small, impossible) it is to detect an empty hernial sac. We only recognise it when the intestine has entered it, and we can feel the impulse on coughing, &c.; but when such entrance is prevented by various circumstances, the most we can say is, not that no hernial sac, but that no hernia, is present. Why is it not possible, as Scarpa and Cloquet have shown, to produce a hernial sac on the dead body by the use of violent compressing power, aided by apertures made in the tendinous walls of the abdomen? First, because the peritoneum is not sufficiently elastic to bear the necessary extension; and, secondly, because compression of the soft and fluid contents of the abdomen acts by hydrostatic law; and although the peritoneum may be stretched, it is not thrust out as it is found to be in hernia, and as it may be to some extent by the finger locally applied.

Professor Roser advances these two propositions, that femoral hernia arises

from the dragging out (*Herauszerrung*) of the peritoneum, and that external inguinal hernia, or more properly its sac, is almost always congenital. First, with respect to *femoral hernia*, the dragging out of the peritoneum is brought about by nodules of fat, which, appertaining to the sub-serous tissue, are firmly attached to the peritoneum. These nodules slide between the fibres of the *septum crurale*, thrust them asunder, and lead to their disappearance. The anterior part of a nodule passes out under the plica, covered only by *fascia superficialis*, and increases in size. Its movements are favoured by its pyriform shape, and by the motions of the body, and the peritoneum following it, a sac is gradually formed. In all the instances of commencing hernia the author has had the opportunity of examining, he has found such fatty nodule at the apex of the sac. We cannot expect to find this in old or large herniæ, as it would disappear under pressure. Professor Linhart, of Würzburg, the only anatomist who has of late years investigated the subject of hernia, has confirmed these views, stating that he believes that traction exerted on the peritoneum exerts far more influence in the formation of hernia than the pressure exerted by the contents of the abdomen.

External Inguinal Hernia.—The valvular character and oblique course of the inguinal canal, should have formed a sufficient reason for rejecting the ordinary theory of the formation of this hernia. All subsequent investigation has convinced Professor Roser that his statement made long ago, that this hernia is almost always congenital, is correct. He has found that the *hernia vaginalis funiculi*—i.e., an open state of the upper part of the vaginal process—occurs much oftener than is supposed. It has been found in almost all the children the subjects of inguinal hernia, whom he has examined; and he has frequently met in adults hernial sacs so long and narrow, that they could only be regarded as incompletely-developed vaginal processes of the peritoneum, into which intestine had not passed. He has also found the anatomical signs, detailed in his former treatise, exhibiting the congenital nature of the affection. Other co-existing anomalies of the peritoneal formation are often met with. Beside the descent of the testis, there is a descent of the cœcum and sigmoid flexure, and disturbances of these often occur at the same time. As the author has found in almost all the outer inguinal herniæ that he has examined, such grounds for considering them congenital, he has come to the conclusion that the bulk of cases regarded as accidental do not merit the appellation, inasmuch as the sac has been in existence prior to birth. He refers in confirmation of his views to Camper's statement, that of 63 full-timed children in whom the testis had descended, the vaginal canal was obliterated only in 7, it being open on both sides in 34, on the right side alone in 14, and on the left side in 8. So likewise Professor Engel, whose investigations are now publishing in the 'Wien Wochenschrift,' states that in children at birth, or during the first fourteen days afterwards, the vaginal canal is found oftener obliterated, or at least considerably shorter, on the left than on the right side—a fact agreeing with the preponderance of hernia on the right side. He found the canal entirely closed at birth in ten per cent. After fourteen days no trace of it could be found on the left side in 30 per cent., while it remained open on both sides at the end of fourteen days in 60 per cent. In the adult the presence or the remains of the vaginal canal was observed in 31 per cent. of the bodies examined, on both sides in 37·5 of these, and on the right side alone in 62·5.

Outer Inguinal Hernia in the Female.—The author long since proposed the question to Professor Meyer of Zurich, whether women were not liable to a similar descent of the peritoneum as men; and that writer, in a paper in Müller's 'Archiv,' has shown that in the female as well as the male fœtus, a projection of the peritoneum into the inguinal canal does take place. Its metamorphosis, however, occurs much earlier, as soon as the fifth month; and

it is also less considerable, and therefore less liable to disturbance than in the male sex.

Internal Inguinal Hernia.—This, Professor Roser observes, may, in some cases, have a fatty origin, like femoral hernia; but he believes that other cases arise from a local protrusion of the peritoneum, in aged and relaxed subjects, in whom a partial atrophy of the *fascia transversalis* has taken place. This is a very different thing, however, to the sudden production of hernia usually admitted. He believes with respect to internal inguinal hernia, that the fact that it occurs almost exclusively in aged men is not generally known. It takes place indeed much oftener than most authors admit; and may be almost said to be as frequent in old men as femoral hernia is in old women. The much greater narrowness of the ring is the reason it does not occur often in old women.

Umbilical Hernia.—This is the only hernia which is produced according to the old theory, viz., by a *vis a tergo*, and even here the author suspects that in some cases it may arise from a congenital protrusion of the peritoneum, remaining from the fœtal state. As this hernia differs in the mechanism of its formation from other kinds of hernia, so does it in the remarkable fact of its being generally spontaneously curable, as may be seen from the small number of cases met with in the adult, compared with the large number occurring in infancy. In aged and fat individuals, accidental hernia of this form is, however, frequent.

Originally commencing his investigation with a purely scientific object, Professor Roser has since found that it has an application to legal medicine, the question not unfrequently arising, whether a hernia has been produced in consequence of acts of violence in scuffles, assaults, &c., and the culprit risking to be dealt with too harshly in consequence of the off-hand way in which the affirmative is pronounced.

II. *On a Peculiar Form of Gangrene which appeared at the Dresden Hospital in the Autumn of* 1857. By Dr. Zeis. (Deutsche Klinik, 1858. Nos. 18 and 19.)

The surgical division of the Dresden Town-Hospital had suffered severely from pyæmia during the winter of 1856-7, but only one case occurred during the ensuing summer. The patients being very few in number in the summer, ample opportunity had been offered for whitewashing, scouring, and other means of purification. The autumn of 1857 was dry, there being a great prevalence of east winds, and a high condition of the barometer. Towards the end of October some of the patients (the surgical division still having but few) having granulating wounds, complained of great headache, prostration, and loss of appetite and sleep. The tongue was usually found dry in the middle, and exhibited a grey-streaked coating resembling that often seen in typhus. Then ensued violent fever, the pulse rising to one hundred and twenty to one hundred and thirty, and remissions always taking place in the morning. The fever was not proportionate in all to the local changes to be described; but it was observed to continue as long as these continued increasing, and to cease as soon as these came to a stand-still. Commonly, this happened about the third day, but in some cases not until the fifth or sixth day. At first, the attendants thought they had to do with a typhus, especially as the local changes did not usually take place until some time after these general disturbances.

A small portion of the granulating surface, that usually nearest the edge of the wound, became inflamed and very painful; and at one or more spots the granulations disappeared, their basis becoming yellowish and hollowed out. The appearance was significantly likened to an ulcer in a granulation, or to the gnawing of a mouse. This process gradually extended over the whole surface

of the wound, except in one case. When it had reached this extent, both the inflammation at the edges and the pains began to diminish, the remains of the destroyed granulations being strewed over with pus, and separating in large gangrenous shreds. After these were discharged new granulations arose, which usually soon manifested a disposition to cicatrize; but in some instances the destructive process again occurred, although with diminished force. In some cases the gangrenous process was not confined to the destruction of the granulations, but extended to the subcutaneous cellular tissue, which was also discharged in large shreds. The disease then resembled the ordinary hospital gangrene, but in a mild form only, for in but one case did the gangrenous process extend beyond the cutis.

Some particulars are given of the ten instances that occurred between the 27th of October and the 14th of December, these being, seemingly, all the cases of serious granulating wounds that occurred during this period in the surgical division. Patients suffering from slighter ailments, as panoris, bubo, and trifling wounds, escaped.

III. *Excision of the Lower Four-fifths of the Radius.* By Professor Carnochan. (American Journal of Medical Sciences, vol. xxxvi. pp. 89—92.)

This is another of Dr. Carnochan's interesting cases of excision of the bones of the arm. "It might, with apparent reason, be supposed," he observes, "that exsection of the long bones, involving the articular extremities, should be followed by serious impairment of the functions of the corresponding articulations. I have, however, already shown, by the cases I have recorded of exsection of the ulna and of the radius, that the functions of the elbow-joint, as well as of the wrist-joint, can be preserved, notwithstanding the entire removal of either of the bones of the fore-arm. The following case of exsection of the lower four-fifths of the radius has also been attended by a successful result, as regards the preservation of the functions of the radio-carpal joint and the hand."

This case occurred in the person of a woman, aged thirty-one, the disease of the bone dating back for several years. The bone having been laid bare by longitudinal and transverse sections, was found to be healthy some little distance below the bicipital tuberosity, and was here easily divided. During its separation from its attachments at the radio-carpal articulation, the humeral artery was compressed, the interosseous artery and a small branch only requiring tying. Care was taken to avoid injury to the vessels, nerves, and tendons at the wrist, and after all bleeding had subsided the tendons were duly arranged in their relative positions. The operation was performed April 9th, 1857, and by May 14th the wound had healed except at a small point. Seen again in June, 1858, both the functions of the hand and its sensibility were found to be unimpaired, the patient performing her household duties nearly as well as before the operation. The bone removed exhibited the various results of severe inflammatory action on the osseous tissue.

IV. *Statistical Report on Forty-six Operations for Stone in the Bladder.* By Dr. Eve. (North American Medico-Chirurgical Review, July, p. 756.)

Since 1841, Dr. Eve, of Nashville, has operated in 46 cases of urinary calculi. In 38 chloroform was employed, and without any ill effect. The deaths were 4; but as 1 took place from dysentery, when the wound was nearly healed, these should be reckoned as 3, or 1 in 15½. In 35 of the 46 cases the recovery was speedy. There were 44 males and 2 females. Of the patients

27 were under fifteen, 15 were adults, and 4 were from sixty-one to seventy-seven years of age. The operations performed were the bilateral in 40, the lateral in 1, the vaginal in 1, the high operation in 1, dilatation of the female urethra in 1, and lithotrity in 2.

Dr. Eve describes his usual mode of operating as follows: "My staff has at the upper part of its groove an opening large enough to admit the beak of the lithotome, and which is there so contracted as to prevent the escape of the cutting instrument until it arrives near the extremity, and has consequently entered the bladder. It acts, in other words, as a safe and certain director of the lithotome. It guides it with unerring precision into the bladder, and thus prevents the rectum being wounded, or the cutting instrument passing into the space between these two organs. I have also added a slight modification to the double *lithotome cachée*. I found its shoulder too large, especially for children, to enter freely upon the staff when in the urethra. To prevent laceration of this canal I have had two small blades put upon the shoulders, so that the instrument may cut its way into the bladder.

"In operating I make an inverted ⅄ incision in the perineum. Thus, with a scalpel, beginning at the bulb of the urethra, an incision three-quarters of an inch in length is made to the median line, then the knife turned to the left, to terminate about midway between the anus and tuberosity of the ischium. A similar leg to the inverted ⅄ is then made on the other side; but with its cutting edge turned up, and arriving at the median line, the point of the scalpel is entered to the staff in the urethra, upon which the lithotome is now conducted into the bladder. The staff being removed, and the half-turn given to the lithotome, the blades are expanded, and the bilateral section is made in withdrawing this instrument, which will be found to correspond very nearly with the external inverted ⅄ incision previously made. The advantage of this external incision over the crescentic one of Dupuytren, is the facility with which it may be made, and the urethra opened upon the staff. I make as small an opening in the prostate as will permit of the extraction of the stone."

V. *On Impermeable Stricture.* By M. Charles Phillips. (Bulletin de Thérapeutique, tome liv., p. 457.)

M. Phillips terminates a series of papers upon this subject with the following conclusions: 1. The transformation of tissues produced by urethritis may completely obliterate the canal. 2. Complete obliteration takes place more frequently after traumatic action than after simple inflammation. 3. It is always complicated with urinary fistulæ. 4. Complete obliteration is perfectly distinct from stricture termed impermeable. 5. This latter always allows a certain portion of urine to pass, either at more or less close intervals, or continuously drop by drop. 6. Wherever urine can pass, a bougie may be always introduced, on condition of our proceeding slowly, patiently, and with full confidence in the power of the instrument. 7. Perforation is the basis of the treatment of complete obliteration. 8. If the obstacle is situated in the straight portion of the urethra, it should be attacked by a trocar, the finger being able to follow and guide this through the tissues. When the obstacle is situated in the curved portion, we should first introduce a grooved canula into the perineal fistula, which may served as a guide to the trocar passed by the meatus. 9. If retention of the urine is produced by stricture, catheterism should never be performed by means of a metallic instrument. Filiform bougies should be employed, which should be introduced slowly, and after a few minutes withdrawn. Each time a little urine is discharged, with relief to the patient; and when his suffering becomes abated, we may fix the bougie, and the whole of the urine will be discharged over it. 10. If the introduction of the bougie is

for the time impossible, and the retention becomes insupportable, supra-pubic puncture of the bladder should be resorted to. 11. If retention is complicated with infiltration of urine, and the introduction of the bougie cannot at once be accomplished, the supra-pubic puncture should be made, as should be large incisions into the perineum. After a few days the tissues will have become sufficiently disgorged to admit of new attempts at catheterism. Not being now pressed by the patient's sufferings from retention, we may proceed slowly and cautiously, and we shall traverse the stricture erroneously believed to be impermeable.

VI. *On the Application of Sutures to the Bladder in the High Operation for Lithotomy.* By Dr. Lotzbeck. (Deutsche Klinik, 1858. No. 15.)

Dr. Lotzbeck, of Tübingen, observes that although the high operation has of late been more frequently performed than formerly, it has not obtained that admission into practice which its advantages might have led us to expect. Professor Günther's statistics of the high operation have sufficiently shown that the fear of urinary infiltration has been exaggerated. The object of this paper is to advocate the employment of the suture of the bladder, a practice revived of late, and opposed by Bardeleben, Pitha, and others, upon mere *à priori* reasonings. To them may well be opposed the practical experience of Prof. Bruns, of Tübingen, who has employed sutures in numerous cases, without finding much difficulty in the execution of the operation or danger in the result.

A case is given in illustration. A large stone being found in the bladder of a child eleven years old, it was determined to remove it by the high operation. After the bladder had been filled with lukewarm water, a vertical incision was carried two inches above the symphysis, while two lateral incisions, half an inch long, detached the pyramidales from their insertion in the pubis. Professor Bruns has on several occasions operated by a transverse in place of a vertical incision; but when the stone is large, he finds that such incision cannot be made long enough without injuring the inner crus of the outer ring, and thereby causing a risk of future hernia. Indeed, the author knows of a case in which a double hernia followed such incision of the two *crura interna*. The opening into the bladder and removal of the stone (which weighed nine drachms and a half) were rapidly accomplished. The bladder was somewhat raised by a hooked forceps, and four points of suture were inserted into its walls (avoiding the mucous membrane) by a strongly-curved needle, one end of the sutures being cut close and the other brought out externally. The keeping the external parts of the incision well apart was much facilitated by an instrument resembling the dilating forceps used by Trousseau in tracheotomy, the help of an assistant being thus dispensed with. In order to be certain that the edges of the wound in the bladder were closely applied, it was filled with water injected by the urethra, and not a drop escaped by the wound. After the operation was completed, an elastic catheter was left in the urethra, which allowed of the easy discharge of the urine. Not a drop of this fluid issued by the abdominal wound, nor of the water that was frequently injected by the urethra to cleanse out the bladder. The sutures came away easily on the sixth or seventh day, and on the tenth the catheter was entirely removed, having been occasionally so for some days previously. The urine could now be held for a long time, and discharged in a good stream. The cicatrization of the wound in the abdomen took place in the course of three weeks.

VII. *On Rupture of the Ligamentum Patellæ.* By M. Binet. (Archives Générales, tome xii. pp. 687—718.)

In this paper M. Binet collects together most of the cases that have been published in France, adding to them some that have come under his own observation. Without following him into the details, we mention some of the results of the investigation. The rupture has usually occurred at the instant of a fall or a false step, in consequence of the unexpected and excessive contraction of the muscles; but it has also been brought about by great voluntary effort. M. Binet divides his cases into two series—supra-patellar, or rupture of the tendon of the triceps, and infra-patellar, or rupture of the ligamentum patellæ. Of the *supra-patellar ruptures* the author has collected accounts of 24 cases, 14 of which were produced by the endeavour to prevent a fall forwards or backwards. In one case which came under his own notice, the rupture took place during a rotatory motion of the trunk from behind forwards, and from right to left, upon the leg planted on the ground. The cases of *infra-patellar rupture* were 23 in number, the accident being produced by a false step in 8 instances, and occurring in 6 during a fall.

As is the case with most traumatic lesions, these ruptures are much more frequent in males than in females, so that 40 out of the above 47 cases were observed in men. With respect to age, it is observable that most of the infra-patellar ruptures have occurred in young persons, the contrary being the case in the supra-patellar rupture. This difference may probably in part be attributed to the fact that in adults and young persons complete flexion of the leg on the thigh is possible, which is not the case in the aged; and it is found that during excessive flexion the rupture takes place much oftener below than above the patella. Double infra-patellar rupture has not yet been observed, but 3 cases have occurred of supra-patellar rupture taking place on both sides. The supra-patellar rupture is never quite complete, the fibres of the vasti, and especially of the vastus externus, retaining their attachments. The infra-patellar rupture is almost always complete.

The right leg seems to be somewhat more liable to these ruptures than the left. The supra-patellar rupture usually takes place very near the patella, and the infra-patellar rupture generally occurs near the upper or lower insertion of the ligament. The amount of separation is less in infra- than in supra-patellar ruptures, since in the latter it may reach seven or eight centimetres, while in the former it scarcely exceeds two. The diagnosis is usually easy when the rupture is complete, but when only partial the lesion may be confounded with fracture of the patella without displacement. As far as known, this accident has never terminated fatally; but the recovery of the use of the limb requires several months, and even then the cure is often only partial, a certain amount of lameness frequently remaining.

For the treatment, M. Binet prefers the simple application of the starch bandage, as recommended by Velpeau. A common bandage is applied from the foot to the hip, pushing the patella upwards or downwards as it passes the knee and fixing it with moderate pressure. Over this the starch bandage is applied, and kept on for thirty or forty days. It is then either renewed or replaced by an ordinary bandage. The stiffness which remains after union must only be overcome by gentle passive movements.

VIII. *Hemeralopia treated by Azotized Vapours.* By M. Deval. (L'Union Médicale, 1858, No. 78.)

M. Deval relates a case of hemeralopia, occurring in a lad ten years and a half old, and dating about ten days. No cause whatever could be assigned for its production. Quinine, valerian, and opium, were resorted to on account of the

periodical character of the affection, in the absence of any definite indication; but as these proved of no avail, M. Deval determined to try the effect of a remedy much praised of late. This consists in exposing the eyes to the vapour arising from a decoction of ox liver, the application taking place for ten or fifteen minutes morning and evening. Great was his surprise to find the hemeralopia entirely gone after the second of these applications, although they were continued awhile longer as a matter of precaution. The employment of the remedy dates as far back as 1762, when Dupont described it as used in the garrison at Strasburg. The vapours arising from the livers of sheep or calves have since been employed with advantage by Stoeber and others, and quite recently by several Italian practitioners.

IX. *On a rare Form of Fracture of the Clavicle.* By M. Robert. (L'Union Médicale, No. 79.)

In the adult, fractures of the clavicle are almost always oblique, the periosteum being torn and the fragments displaced. Sometimes, however, in the adult, but oftener in the child, the fracture may be transverse, without laceration of the periosteum or displacement. This was the case with a miserable-looking lad, aged sixteen, brought to M. Robert at the Hôtel Dieu, his left clavicle having come in contact with a table during a fall. There was no displacement, and the bone presented quite its normal appearance, there being neither ecchymosis, projection, nor depression. On passing the fingers along it, however, with a gentle pressure, a painful spot, with a slight mobility at it, was felt; and whenever the long thin bones, such as the fibula, ribs, or clavicle, have been exposed to violence, and no deformity is observed, pain limited to a very small portion of their course is quite sufficient to give rise to the belief in the existence of a fracture. To search here for crepitation would only rupture the periosteum still entire, and thus increase the gravity of the case. All that was required was to keep the arm against the chest, and caution the patient against using it—a caution of importance, for there being no displacement and but little pain, the patient, believing the bone not broken, might easily employ the limb dangerously.

In a few days the tissue surrounding the fractured point became inflamed, and the swollen periosteum formed a projecting ring uniting and maintaining the fragments in contact; confirming entirely the diagnosis derived from the localized pain and slight mobility first observed. Cases come to the hospital a week or a fortnight after the accident, exhibiting merely this circular ring, the nature of which may be yet more difficult to understand, as the patients have often forgotten that a fall or a blow has taken place. It is well to be aware of this, as the projection may be mistaken for a periostitis or an exostosis. It is easily ascertained to be a ring constituting a provisional callus, by imparting to the bone movements which are easily perceived, the ring being as yet only in its fibrous or cartilaginous condition. It gradually ossifies, and is replaced by a small definitive callus. Ordinary fractures of the clavicle are hardly ever accompanied by a provisional callus, this only being found when the periosteum is preserved entire.

X. *A New Mode of Treating Purulent Ophthalmia.* By M. De Condé. (Annales d'Oculistique, tome xl., pp. 15—22.)

M. De Condé, a Belgian military surgeon, dissatisfied with the present treatment of this disease, which seems to be very prevalent still in the Belgian army, lays down a new plan, based upon the following considerations:

1. The greatest danger to the eye arises from the contact of the upper lid, which, inflamed and swollen, floods its surface with an acrid and corrosive pus. The excessive heat of the eyelid, the internal surface of which is rough and unequal, interferes with the nutrition of the cornea, while the acridity of the pus leads to its softening and destruction. 2. It is admitted that it is of importance to prevent the contact of inflamed mucous membranes by the interposition of an isolating body, such as charpie or wadding. This is seen in vaginitis, balanitis, and fissure of the anus. 3. This body, which may alone produce great amelioration, or even in some cases a removal of the disease, may exert a powerful effect if impregnated with an active agent. In this way lint, soaked in a concentrated solution of acetate of lead, and placed between the glans and the prepuce, will cure gonorrheal balano-posthitis within forty-eight hours. 4. Cod-liver oil exercises a powerful action in disease of the mucous membranes, modifying and then suppressing their secretions. It strengthens the fibrous tissues of the eye and the cornea, and tends to prevent ramollissement. It is especially in ulceration and chronic ramollissement of this membrane that this double action is perceived. 5. The red precipitate ointment (four parts to fifteen of lard and fifteen of linseed oil) is an excellent substitutive agent, sufficing alone to arrest the disease when employed early. It is the best remedy for cutting short the ophthalmia of new-born infants. 6. A solution of the chloride of lime (thirty parts to two hundred of water) is an energetic modifier, neutralizing with certainty the virulence of the secretions. 7. Lastly, perchloride of iron exerts an instantaneous hæmostatic effect upon the hæmorrhagic mucous membrane, and an indubitable modifying influence upon the mucous secretion.

As an isolating body, M. De Condé, after trying various substances, gives the preference to wadding, as forming at once an inoffensive application, capable of imbibing and favouring the flow of the secretions, and of being impregnated with medicinal agents. A slip of the wadding somewhat longer than the transverse diameter of the eyelids, is brought opposite the palpebral aperture, and gently pushed up beneath the upper eyelid by means of the little finger or a large probe. Some cases are related as examples of the manner in which a disease often so tedious in its progress, may upon these principles be rapidly cured.

XI. *On the Absorption of Abscesses.* (Gazette des Hôpitaux, 1858. No. 62.)

M. Chassaignac brought this subject recently under the notice of the Paris Surgical Society, stating his belief that purulent collections are never absorbed, at least the solid globular portions, although the serum may be sometimes so removed. Abscesses said to be so removed were in fact non-purulent collections or lymph. M. Richard pointed out the error of supposing that pus-globules were not absorbable because they were solid, inasmuch as tumours, effusions of blood, &c., are known to undergo such absorption. It is by no means rare to find small axillary or peri-mammary abscesses becoming absorbed; abscesses the size of a pigeon's egg, will in chronic farcy disappear in twenty-four hours; and congestive abscesses not unfrequently heal without any aperture having been made. M. Huguier thinks everyone must have seen examples of the spontaneous disappearance of abscesses, a by no means rare circumstance in inguinal bubo. M. Verneuil referred to a case that came under his care, in which an undoubted abscess, as large as the fist, situated in the middle of the thigh, entirely disappeared after three weeks' local application of iodine. In another case, two abscesses in the axilla were opened, while two others, just as manifestly fluctuating, being left to themselves, were spontaneously absorbed. In a case of hygroma, M. Morel-Lavellée obtained

a few drops of fluid by means of an exploratory puncture. This was shown to be pus, and the rest of the fluid being left in the collection, flying blisters were applied, and entire absorption was induced. M. Chassaignac observed that this was the only demonstrative fact advanced in the discussion, and he must wait for others to be convinced. At present he does not believe that properly-characterized abscesses are capable of absorption. Many cases are, it is true, mentioned by authors, but with insufficient proofs. Fluctuation and other symptoms mentioned are not sufficiently rigorous ones. Thus, in a case of angio-leucitis, in which he opened two collections apparently exactly alike, one contained pus and the other plastic lymph; and from some inguinal bubos he has opened, he has only removed such lymph. M. Broca, however, desired to know what M. Chassaignac designates as "collections of plastic lymph;" for if he means those masses of yellowish concrete, semi-solid matter found in the centre of commencing bubos, M. Broca can assure him that this substance, which is susceptible of absorption, contains an enormous quantity of pus globules. M. Cullerier declares, that since he has treated bubos by the method recommended by Dr. Sirus-Pirondi—viz., blistering, followed by tincture of iodine, he has obtained a cure by absorption of the pus in nine-tenths of his cases. Even when the blister does not succeed, it still exerts a great influence upon the absorption of the globules; and if the bubo has afterwards to be opened, pus containing a large proportion of serum is discharged.

QUARTERLY REPORT ON MIDWIFERY.

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I. Anatomy and Physiology of the Female Generative Organs.

1. *Researches on the Erectile Organs of Woman, and on the Muscular Tubo-Ovarian Apparatus in their Relations with Ovulation and Menstruation.* By Dr. Charles Ronget. (Brown-Séquard's Journal de Physiologie, No. 2, April, 1858.)
2. *On the Duration of Pregnancy; and Menstruation during Pregnancy.* By Elsässer. (Henke's Ztschft. 1857.)
3. *Menstruation in Austria.* By Fred. Szukits. (Ztschft. d. Wien. Aerzte, 1857.)
4. *An Experimental Inquiry into the Effect upon the Mother of Poisoning the Fœtus.* By W. S. Savory. 1858.

1. Dr. Ronget begins by insisting upon the observance of the distinction between congestion, or turgescence, and erection. Erection implies a peculiar vascular arrangement; thus it is wrong to describe the congestion of the Fallopian tubes as erection. His observations confirm those of Valentin, and coincide in refuting those of Müller, as to the mode of termination of the arteries in the erectile tissue of the penis. He shows that the arterial diverticula, described by Müller under the name of helicine arteries, have only a purely artificial existence. No more than in any other organ do the arteries of the cavernous bodies terminate in culs-de-sac. There, as elsewhere, these vessels pour their blood into canals communicating with the veins. Nor does any artery open freely in the centre of the areolæ of the corpus spongiosum. The arteries of erectile organs have this peculiar appearance—namely, that they do not divide into dichotomous branches, but are provided with tufts of vessels separating from the trunk, to the number of three to ten, by a common pedicle. These do not end in diverticula; they traverse freely the large

sinuses of the central part of the corpus spongiosum and bulb, and penetrate, after multiplied divisions and anastomoses, into the muscular trabecula. The veins and capillaries, which perform the principal part, adapt themselves, by enormous dilatations and anastomoses, forming true retia mirabilia. Such being the true character of erectile bodies, Dr. Ronget was soon enabled to trace similar organizations in the uterus and in the ovary.

The utero-ovarian artery is not distributed equally in all the parts of the organ; whilst throughout the length of the neck the branches given off are rare and scarcely flexuous, at the level of the body, in the neighbourhood of the insertion of the Fallopian tubes, the artery divides suddenly in from twelve to eighteen tufts of arteries twisted into spirals, most frequently of astonishing regularity, but so numerous and pressed together, that in some preparations they quite cover the lateral angles of the uterus. Along the inferior border of the ovary, the tubo-ovarian trunk furnishes another series of ten or twelve branches, which spring, all immediately after each other, from the upper border of the artery, and directly after their origin divide, twist together, and interlace, exactly like the arterial tufts of the root of the cavernous bodies; and lastly, penetrate the parenchyma of the ovary, where they again form spirals.

As for the venous system of the uterus, the vast plexus which is situated at the sides of the vagina and uterus establishes a free communication between the properly-called erectile organs. This plexus has for canals of discharge, at its inferior origin the pudic veins, in the middle the uterine veins, at its termination the ovarian veins. It presents two principal enlargements—the one, placed behind the pubic arch, is frequently prolonged between the rectum and vagina, forming a ring round the vagina; the other enlargement, much more voluminous, covers in great part behind the extra-vaginal portion of the neck of the uterus, upon which it is moulded.

No proper erectile structure exists either in the Fallopian tubes or around the vagina.

The erectile property of the uterus is demonstrated in the following manner. If the pelvis be placed in a hot bath, and an injection be thrown into the ovarian veins, so as to completely fill the spongy bodies of the uterus and ovaries, it will be seen that the body of the uterus rises in the axis of the neck, lifting itself up in the pelvic cavity, and executing a movement quite analogous to that of the penis in erection. This state lasts while the injection distends the erectile bodies. This change of position is accompanied by a distinct change of volume and form. The uterus becomes more convex, both anteriorly and posteriorly. At the same time the walls of the uterus separate from each other, in the same manner as Gunther and Kobell showed the urethra to do. In the ovary, analogous phenomena are observed. The Fallopian tubes undergo no change of form, volume, or position; but the ovary is lifted up by the tension of the venous plexuses, whilst the spongy body, which supports it like a kind of receptacle, swells, and seems to grow, like the bulbs of the vestibule at the moment of erection.

At this point a difficulty encountered Dr. Ronget. An essential element of erectile tissue is muscular fibre. This is incontestably present in the uterus. But where is it in the ovary? He sought for a solution of this in comparative anatomy. Another question presented itself. In the presence of the anatomical observation which proves that the Fallopian tube is not erectile, the only plausible hypothesis advanced to explain how the fringe which floats freely at a distance from the ovary, can at the period of menstruation traverse the intervening space to receive the ovule, falls to the ground. In a series of comparative researches, Dr. Ronget succeeded in showing that there is a muscular apparatus more or less complex, but everywhere presenting the same general disposition which presides over the expulsion of the ovule from the ovary, and its transmission in the oviduct or the Fallopian tube. The phenomenon of

ovulation is accomplished according to the same laws, by the same agents, in the inferior vertebrata, in mammifera, and the human species. In the latter case only, the presence of vascular erectile formations in the body of the uterus determines, as a secondary consequence of ovulation, menstrual hæmorrhage.

2. Elsässer records his observations on the duration of pregnancy, made in the Stuttgard Institution. He is of opinion that no single method of computing the length of pregnancy can be taken as trustworthy; neither the date of conception, nor menstruation, nor the first fœtal movements. By the reckoning from the day of conception, out of 260 mature children, only 23, or 8·8 per cent., were born on the 280th day; 166, or 63·8 per cent., before, and 71, or 27·3 per cent., after this day. The greatest number of births (126 = 48·4 per cent.) fell between the 271st and 280th days. Of the recorded cases, 140 were first births; of these, gestation lasted in 14 exactly 280 days; in 96 a shorter time, and in 39 longer. In 111 pluriparæ, 9 lasted 280 days, 70 less, and 32 longer. The sex of the child appeared to exercise no influence. In reckoning from the commencement of the last menstruation, out of 175 deliveries, 12 = 6·8 per cent., took place on the 280th day; 43 = 24·5 per cent., before; and 120 = 68·5 per cent., after. The greatest number of deliveries fell between the 280th and the 290th days. Thus, by the reckoning from the beginning of the last menstruation, 93 per cent., and by the reckoning from conception, 91·1 per cent. of all the cases lasted a less or longer time than 280 days. The weight of the children is in no constant relation with the length of the gestation.

During pregnancy, menstruation appeared once in 8 women, twice in 10 women, three times in 12 women, four times in 5 women, five times in 6 women, eight times in 5 women, and nine times in 2 women.

3. Dr. Szukits has made an elaborate investigation into the menstruation phenomena of Austrian women. It admits of comparison with the similar researches of Brierre de Boismont for France. The foundation of the statistical tables given is the examination of 2275 women. He examines the influence of town and country life, of social condition and race. In Vienna, the 665 women taken were mostly of the working classes. The mean age of first menstruation was 15 years 8½ months. In the greatest number (one-third of the whole), menstruation commenced in the 15th year. In Paris, the accession of this phenomenon is one year earlier. Out of 1610 country women, the mean menstruation-age was 16 years 2½ months. The greatest number began in the 15th year, the next greatest number in the 16th, then the 17th, 18th, and 19th years. Amongst the Lower and Upper Austrians, the mean menstruation-age was 16 years 3 months; amongst the Bohemian women, 16 years 2 months; the Moravians, 16 years 3¾ months; amongst the Hungarian women residing in Vienna, about 15 years; amongst the Silesians, 16 years 1½ month; the Bavarians, 16 years 10 months. As to the influence of class, Szukit's observations agree with those of De Boismont. He found the mean age for the middle classes to be 15 years 2 months; for artisans 15 years 10 months; for labourers 16 years 1½ month. Luxury hastens the advent.

4. Mr. Savory's experiments, on account of their physiological bearing, were adverted to in the last Physiological Report; we recur to them because of their obstetric interest.

Majendie had injected very active poisons into the vessels of the cord, directing them towards the placenta; but he had never seen the mother suffer from the effects of them. Experiment therefore seemed to give a negative result. And Mr. Savory cites the opinions of Dr. Kirkes, as representing the

current idea amongst physiologists, to the effect that the placenta serves for the absorption and conveyance of materials drawn from the mother's blood into the bloodvessels of the fœtus, "no idea seemingly being entertained of a converse process. Moreover, the view taken of that portion of the fœtal blood transmitted to the placenta, appears to be exclusively that of *renovation* or *aeration*, by coming into contact with the oxygenated blood of the mother, nothing being said as to *re-absorption* into the maternal system." [It is right to interpose that this is not altogether a correct view of the state of the question. The emunctory function of the placenta, the interchange by the fœtus of excretory for nutritious matters, has been clearly recognised. The very analogy of the placenta to a lung implies this. The placenta must carry off what the lung carries off—i.e., carbonic acid and other impurities. The Reporter, in articles published in this Journal in 1855-6, on 'The Diseases of the Placenta,' extended the analogy further, by showing that the placenta was the physiological lung, liver, kidney, and skin of the fœtus, the mother's blood receiving all effete matters.] Still, the direct experiments of Mr. Savory are especially valuable. His general scheme was to narcotize a pregnant bitch or cat with chloroform; to open the uterus, and carefully to isolate one or more fœtuses from the mother, retaining connexion by the cord only, and to inject, by puncturing the chest or abdomen with an Anel's syringe, a few drops conveying a definite quantity of strychnine. Every precaution was taken to prevent access of strychnine to the mother by any other mode than by circulation in the umbilical vessels. It was observed that—first, the fœtuses so treated became affected by tetanic spasm; secondly, that the mother became similarly affected; thirdly, that the remaining fœtuses, not injected, were not affected by the poison; fourthly, the influence of the chloroform was not extended to the fœtuses, which always remained lively and active.

The deduction from these experiments is, that the mother may be poisoned through the fœtus. The failure of the strychnine or chloroform circulating in the mother's blood to influence the untouched fœtuses in return, is supposed to be explained by the speedy death of the mother. [Obstetric experience, however, does not appear to prove that there is much danger of intoxicating the fœtus by administering chloroform to the mother.—Reporter.]

II. Pathology of the Female Organs of Generation.

1. *On Exfoliation of the Epithelial Coat of the Vagina, producing Casts of that Canal, with Remarks on the true Form of the Vagina.* By Dr. A. Farre. (Dr. Beale's Archives, No. II. 1858.)
2. *Liquor Amnii containing much Urea.* By Dr. Beale. (Archives of Medicine, No. II. 1858.)
3. *On the Pathogenesis of Hydrorrhœa Gravidarum.* By Professor Braun. (Ztschft. d. Wien. Aerzte, April 26th, 1858.)
4. *Removal of an Ovarian Tumour in which the Ecraseur was used.* By John L. Atlee, M.D. (North American Med.-Chir. Rev., July, 1858.)
5. *Fibroid of the Uterus weighing Sixty-two Pounds.* By Dr. Binz. (Deutsche Klinik, July, 1857.)

1. Dr. Farre records three cases in which membranous casts discharged by the vagina were shown by their form and miscroscopical examination to be shed by the vagina, and not by the uterus. The first consisted of a thin double layer, of a somewhat slight, yet tough and parchment-like membrane, of an opaque white colour, and smooth, almost lustrous or pearly, surface. The two membranous layers of which it was composed were in close apposition, and were bounded by a peculiarly sharp, thin border, like the marginal folding of

the uncut pages of a book. The size of the entire cast was more considerable than the cavity of the uterus in the usual unimpregnated state. The angles that should correspond with the points of entrance of the Fallopian tubes showed not the slightest trace of an aperture, and nowhere was there any of that cribriform appearance produced by the pores of the uterine glands, which is so characteristically shown upon the inner surface of true dysmenorrhœal membranes; whilst the outer surface was not rough, but smooth and lustrous. The whole was found to consist of broad, flattened, nucleated cells of pavement-epithelium, and was entirely destitute of the histological characters of the uterine mucous membrane.

In another case, the preparation of which is in the Museum of King's College, the membrane also consisted of a sheath of dense opaque epithelium, but the outer surface instead of being smooth was everywhere indented, so as to form numerous pits and depressions running in oblique lines, and exactly representing the course of the vaginal rugæ. When the preparation is laid open and viewed from within, the furrows upon the reverse surface are seen to be converted into rugæ, having the ordinary arrangement of the columnæ rugarum upon the inner surface of the vagina. This specimen has more of the cylindrical form than the former one, and at its upper end is a depression corresponding with the cervix uteri. The chief differences between this and the former specimen are, that the cast is here evidently that of a narrower and more tubular canal, and exhibits the rugæ which are wanting in the former, differences which Dr. Farre has no doubt depend upon the circumstance that the one is the cast of the vagina from an unmarried, and the other from a married person in whom the surface of the vagina had become smooth by unfolding and obliteration of the vagina.

The third specimen was from a lady who had borne three children, who had complained of severe pain, with sickness, and a peculiarly crawling sensation in the vagina at the menstrual periods. The surface of this specimen was smooth and shining, like the first, and of the same dense white parchment appearance. It possesses the cylindriform character of the second specimen, and the peculiar crescentic border. It had no cribriform markings. It consisted of nothing but tesselated epithelium. At the upper end was a cup-like depression, having in its centre a transverse cleft, exactly corresponding in size and position with the two lips of the os uteri.

Dr. Farre observes that these membranes, being actual casts of the vagina, may help to correct the conventional notions of the form of this canal which the ordinary representations are apt to give. If the vagina is examined *in situ*, just as Kohlrausch has represented it, it will be found to be a short flattened canal, the anterior and posterior walls of which are in mutual contact; measured along the anterior wall from the median tubercle of the vaginal orifice to the margin of the anterior lip of the cervix uteri, it commonly does not exceed *two*, or at the utmost *two and a half* inches, while the length of the posterior wall, from the hymen, or the entrance of the canal, to the extremity of the cervix, where the peculiar crescentic fold occurs, does not ordinarily exceed *three* inches. The width ranges from one inch to an inch and a quarter, the broader part being at the upper recess or fornix.

The vagina deprived of its epithelium may be compared to the red and raw tongue of a person suffering from gastric or intestinal irritation.

2. The constitution of liquor amnii is replete with interest. The analyses have been scanty. Dr. Beale relates a case of a patient in the eighth month of pregnancy, from whom about seven pints were drawn off. The specific gravity was 1·006. The deposit was flocculent, and consisted principally of epithelial cells and oil-globules from the surface of the skin of the fœtus. A few circular cells, probably derived from the bladder, and some particles of dark green and

brown colouring matter (meconium), were also present. The following was the result of the analysis:

Water	987·00
Solid matter	13·00
Urea	3·50
Albumen and salts	9·50

In another case Dr. Beale found, in liquor amnii drawn from a woman in the eighth month, a number of casts of uriniferous tubes, scarcely half the diameter of those found in the adult.

Dr. Beale observes that in the earlier months of pregnancy the liquor amnii is much richer in albumen than in the later months. It is not unreasonable to suppose that during the later months, when the greater part of the albumen has been absorbed, and it was no longer required as a nutrient material, that the urine of the fœtus should become mixed with it. There would be no danger of the re-absorption of the urinary constituents by the fœtus, as it is covered with a thick layer of vernix caseosa, which contains much oily matter. That urine is secreted by the kidneys at least as early as the eighth month, was proved by Dr. Robert Lee in a case in which the secretion from these organs had accumulated in the obstructed ureters. Uric acid and urea were detected in it by Dr. Prout. [Dr. M'Clintock had six specimens of liquor amnii subjected to examination for urea; the result was negative. In all these cases albumen was found in the urine of the fœtus in very considerable quantity, so that from these and other analyses he was led to infer that the urine before birth differs from that after birth in these two remarkable circumstances —viz., 1, the *absence* of urea; 2, the *presence* of albumen.—Reporter.]

3. Professor Braun communicates an observation in illustration of the pathogenesis of hydrorrhœa gravidarum. It has been conjectured that the watery fluid occasionally discharged during pregnancy must proceed from the sac of the fœtal membranes either by rupture or transudation. Professor Braun confirms the opinion of Nägele, that the origin of hydrorrhœa is the inner wall of the uterus. The case he records is as follows:—On the 17th of June, 1857, a healthy girl, aged twenty-three, came under treatment at the Clinique, for frequent and profuse watery discharges from the vagina during her pregnancy. By careful examination, the regular structure of the fœtal membranes was determined; they burst at the proper time, discharging about six pounds of liquor amnii. The child was premature, and had pemphigus; it died next day. The membranes were ruptured nowhere but at the point that gave exit to the child. On microscopical examination with Dr. Wedl, Dr. Braun found on the convex surface of the placenta a delicate cellular membrane hanging here and there in shreds. This membrane (which it is to be presumed was not the remains of the normal decidua) Dr. Braun considers to be evidence of an exudation process, and to explain the origin of the so-called false waters.

4. Dr. John L. Atlee, alluding to his experience of ovariatomy, says—"In all he could trace the disturbing effect of the presence of the silk ligature around the pedicle, as well as of those used to restrain hæmorrhage. With the écraseur to sever the pedicle, and the use of the silver suture and silver ligature, when ligatures are necessary, I am satisfied that a very large share of the dangers attending the operation will be obviated." Dr. Atlee records a case in which he divided the pedicle of an ovarian tumour by the écraseur with complete success. This tumour (in a woman, aged sixty) had expanded the broad ligament, and encroached upon that portion of the peritoneum between it and the colon, so as to leave the pedicle but one inch long, four inches broad,

and highly vascular. The whole tumour being firmly held up, the pedicle was tightly grasped by the forefinger and thumb of the right hand. The chain was passed round it above, and very close to the tumour. The instrument worked, the pedicle was divided in six minutes and a half; no oozing of blood was observed from the stump. The tumour weighed seventeen pounds and a half. The abdominal wound was closed by four silver sutures. [No mention of a silver or other ligature being applied to the pedicle.] The patient recovered favourably.

5. Dr. Binz's case of enormous fibroid tumour of uterus. A woman, aged forty-one, died in the clinic at Bonn. For eighteen years she had led an irregular life, and finding her abdomen enlarge, concluded she was pregnant, although the catamenia had not ceased. She had once been pregnant, and had aborted after repeated ill-usage from her husband. From this period she observed her abdomen to swell, and she suffered often from attacks of peritonitis. During the last three years these attacks had ceased, until her last illness, when they returned with great severity and caused her death.

Autopsy.—About fourteen pounds of yellow watery fluid escaped from the neighbourhood of the stomach. On opening the abdomen, a tumour of immense size showed itself, covered with largely-developed vessels, and largely adherent to the peritoneum. Its surface was covered with purulent exudation, and ulcerous spots. The omentum covered it and adhered to it. It laid under the transverse colon, and pressed the intestines over to the left. The tumour was found connected with the uterus; it was covered by the muscular fibres, and at the place of union were meshy spaces filled with blood. The microscope showed that this product was a fibroid, in whose centre was a thick cheesy mass of tuberculized tissue. The long measurement was 46 cent., the transverse 32 cent.; largest circumference 125 cent.; weight 62 pounds.

III. Labour.

1. *On the Prevention of Laceration of the Perineum.* By Mattei. (Vierteljahrsschrift f. p. Heilk., 1858.)
2. *Eclampsia at the Eighth Month; Recovery; Delivery of two Children United by the Sides.* By Dr. Boursier, of Creit. (L'Union Médicale, June, 1858.)

1. Dr. Mattei gives the following views on the means of preventing laceration of the perineum. It is especially necessary that the head pass the vulva in a favourable direction. This can only happen when it passes with the necessary degree of flexion. Whilst the occiput passes under the pubic arch, the face has not yet quitted the pelvic outlet; first when the upper part of the neck comes under the pubic arch, can the extension of the head (or the separation of the chin from the breast) begin. If the distension of the perineum begins too early, the head must pass the vulva with unfavourable diameters—namely, with the great oblique, or great or straight diagonal diameters. Such a passage easily causes laceration. Hence it is the task of the physician to prevent a premature distension by the head. This he effects by placing two fingers between the labia, or in some cases between the pubic arch and occiput, so as to bring the head downwards and outwards, at the same time laying the other hand on the hinder part of the perineum, upon which the face is lying, and pushes this upwards. This manœuvre is to be executed during the pains, which will thus protrude the head forwards in the requisite arc. A very simple means of expediting the birth of the head, consists in compressing firmly the distended perineum with the whole hand. This resembles the squeezing-out of

the kernel from a cherry. On the passage of the shoulders care must also be taken lest the two shoulders pass together.

2. The case of eclampsia and double-birth of Dr. Boursier is especially interesting. A primipara, subject to hysterical attacks, was at the eighth month of gestation. A month ago she had complained of headache, for which she had been bled. Eight days before this she had been seized with considerable œdema of legs and hands. It was remarked, also, that her eyes were very bright. At time of observation, eclampsia had come on; she had had five severe attacks, with coma. Largely bled, and delivery attempted. A foot was felt presenting; this was seized, and also a second; they were right and left; but after an hour and a half of extractive effort, the pelvis could not be brought down. Dr. Boursier now felt a soft body situated at the junction of the two legs. After further violent exertion, he brought the two thighs out of the vulva, and a portion of the soft, smooth, blackish mass he had before felt. He then perceived a third leg behind. An extensive adhesion between the two fœtuses was now found. The fourth leg was freed; the two trunks descended, as well as the black mass, the size of a hen's egg, which had been torn, and gave exit to some intestinal coils. During this the fits increased. The delivery was accomplished by passing a loop of a napkin over the chest of the most forward child. By dragging strongly on this backwards, the head was delivered; and lastly, the second head was born. Several attacks of convulsions occurred afterwards; but she recovered favourably.

The Anatomical Relation of the Fœtuses.—Both boys, of middle size, well formed in all the non-adherent parts; they are slightly inclined towards each other. The adhesion is formed in the soft and bony parts of the abdomen, so as to constitute only one abdominal cavity. In the central part of the common belly the walls are formed by the expansion of the umbilical cord, which has the form of a funnel, whose upper part, containing the vessels, is fixed to the sternal union; below, this membrane is turned backwards, and is inserted on the iliac crests. This funnel has the size of a hen's egg, and contains a large part of the intestinal organs. The cord is single, very large, and has only one vein. The chests are normal, and contain four lungs. The hearts are large, and placed forwards on a level with the sternums. There are two stomachs, small, each containing a spoonful of yellowish mucosity. A spleen is attached to each. There are four kidneys and two bladders. The consequences of the fusion are two diaphragms united by their corresponding sides, preserving their points of attachment to the ribs. They possess a single liver, evidently composed by the union of two organs, for there are two gall-bladders. It is large, and occupies the central part of the umbilical pouch. The greatest anomaly consists in the atrophy of the intestine of the left child, which is reduced to the form of a cord (hollow). The rectum, colons, cæcum, small intestine, are very distinct. This intestine has no communication with the stomach. The intestine of the right child would have acted for both. The left one would only have had the trouble of digestion.

MEDICAL INTELLIGENCE.

The Medical Act.

On Monday, the 26th of July, 1858, the third reading of the Bill intituled 'An Act to Regulate the Qualifications of Practitioners in Medicine and Surgery,' established a national organization of the Medical Profession in Great Britain. None will be disposed to maintain that the organization thus initiated is the most perfect and satisfactory that could be brought about; but if we take into consideration the ancient claims and prerogatives of so many medical corporations, the power and influence of younger corporate bodies, the different wants and requirements of countries not altogether identical, though associated under one executive government, we may well regard the achievement of the unanimity evidenced by the Medical Act as a sign of the true civilization of our countrymen. Whatever difference of opinion there may be with regard to individual provisions of the Bill, we now possess the undoubted advantage of a forum in which the best means for advancing the interests of our common profession may receive due discussion and attention. It is to be hoped that in the selection of the representatives of the individual corporations in the Council, the merits of the candidates will be weighed rather by the breadth of their views and the interest they take in the advancement of the profession as a body, than by the warmth of their advocacy of class interests. Let not the bickerings of Hall, College, and University be carried into the new Council, but let every member of the Council bear in mind that he represents a larger constituency than the members of the corporation that elected him. By no other means can the medical profession as a body secure to itself the respect of the general public—in no other way shall we be able to obtain for ourselves or our successors that share in the highest honours of the State which legitimately belongs to us, but which has hitherto been withheld from us by the legislators of this country, because we have not been able to exercise that influence and to show that powerful and united phalanx which our services to the State, our intellectual status as a body, and our numbers, justify our exhibiting.

May the new Council commence its career of independence by selecting from the ranks of the profession a President who has its interests deeply at heart, and who, from his position as a man of science and his tact and knowledge of the world and of business, may command respect both in and out of the Council. There can be no real necessity for selecting the President beyond the pale of the profession. We ought to have profited by the many sad lessons that we have learnt, not to be guilty of so great an error, and not to let our first act as an united profession prove to the world the small account in which we hold our own rank. No other profession would act so; and if the suggestion is met with the remark that the President of the General Council must be a member of the Privy Council, the simple

reply is that if we have no Privy Councillors in the profession, the creation of the new office will be an ample excuse to sticklers for precedent for commencing a new and more enlightened system. We shall augur ill or well of the prospects of the Medical Council, as they assert their dignity and that of the profession or not, by the choice of a President. This will be the first step in the right or wrong direction. The time must come when we shall be represented by one of ourselves in the Councils of the Queen, and now we have an opportunity which may not offer again for a long time, of securing the position without trenching upon any prerogative or running against any precedent. This is the first act by which we shall test the spirit of the new corporation. It would be useless to offer other suggestions where so much is vague and uncertain. The General Council have a great field before them; may they enter upon their duties with the consciousness of a high aim, and let it be the duty of the profession at large to acknowledge the difficulties of their position and to show ample allowance for shortcomings, provided they be not the shortcomings of a truckling and pusillanimous spirit.

If the profession is now true to itself, the time cannot be far distant when its national organization, by aid of the new Act, will render a representation of the medical profession in Parliament necessary and unavoidable. We have long desired that this should be, and we know that men of independent character and high position can be found whose private means enable them to become our spokesmen in the legislative assembly of the kingdom. It is a noble and laudable ambition that would lead a man to take such duties upon himself. There is wide scope for the exercise of his powers in a medical point of view in the House of Commons. The ignorance of the honourable House on medical matters is proverbial, and the time has passed when sanitary legislation could be left to the hap-hazard of local boards of health or vestries. Our interests, too, as a profession, require to be asserted and defended, so that many reasons urge us all to promote the parliamentary representation of the medical profession. Whatever else may be requisite, these two points should now be our watchword, and inscribed on the banner under which, if need be, we are prepared to do battle: a Medical President of the General Council, and Medical Members of Parliament.—Quod felix faustumque sit!

The Restoration of Persons apparently Drowned.

In a pamphlet recently published,* Dr. Silvester argues that the postural method of Dr. Marshall Hall for the restoration of persons who have been asphyxiated, does not displace more than a fraction of a cubic inch of air from the lungs, that the expansion of the thorax is limited to that which results from the mere elasticity of the tissues compressed during the rotatory efforts,

* The True Physiological Method of Restoring Persons apparently Drowned or Dead, and of Resuscitating Stillborn Children. By Harry R. Silvester, B.A., M.D., formerly Student in Human and Comparative Anatomy in the Royal College of Surgeons of England. London, 1858.

and that the contraction and expansion are confined to one side of the chest. He points out other objections, such as that the contents of the stomach and œsophagus may pass into the windpipe, or that the patient's face may be bruised and his neck twisted by the prolonged movements, to which may be added that ribs are sometimes fractured, as we have ourselves witnessed. The author proposes to make the first effort of the asphyxiated individual an *in*spiratory one, by using the arms as handles to open and close the chest.

To determine the effect produced on the contents of the thorax, by the proceeding advocated by Dr. Silvester, he introduced a glass tube into the trachea of a corpse; this was connected by a flexible tube with a glass horse-shoe tube containing a small quantity of coloured fluid, which was maintained at the same level in both legs. "The height of the column having been first carefully noted, the arms of the subject were raised and steadily extended upwards by the sides of the head so as to draw up the shoulders, and put the pectorals on the stretch, elevate the ribs, and consequently enlarge the cavity of the chest. The result was, that the fluid in the bent tube rapidly fell, and so considerably as to recede high up in the leg of the instrument nearest the body, that is to say, the tendency to a vacuum produced in the chest drew the air into the lungs; the shoulders and arms were next pressed down upon the sides of the chest, and immediately the fluid rose as much above its usual level in the further leg of the apparatus as it did in the foregoing experiment."

Dr. Silvester has tested the effect of the rotatory plan by the same instrument, which demonstrates that while a small quantity of air is expelled from the thorax by compression, only so much can be drawn in as will occupy the space created by the elasticity of the ribs. He concludes—"1. That by his mode of procedure the actual capacity of the chest was increased, and air drawn into the lungs by the constrained action of the muscles of respiration upon the moveable walls of the thorax. 2. That expiration was produced by pressing the arms and shoulders down upon the sides of the chest."

The author points out that his method is characterized by an actual enlargement of the cavity of the chest, owing to elevation of the ribs above their ordinary or natural level; this he maintains is not effected by the Marshall Hall plan.

Dr. Silvester's suggestion certainly is based upon sound anatomical and physiological principles; we therefore hasten to place it before our readers, and request them to put it to the test, without at the same time neglecting such other precautions with regard to the restoration of warmth by friction and dry clothing, of drawing forward the tongue to prevent the larynx from being closed, and the like, as the author very properly points out.

In connexion with Dr. Silvester's investigations, we may mention the results of an inquiry instituted by the National Life Boat Institution, to determine the value of Dr. Marshall Hall's suggestions, and the opinions entertained by the profession regarding the comparative merits of the plan adopted by the Royal Humane Society and of Dr. Marshall Hall's plan. To the queries issued by the Institution at the suggestion of Mr. Gorham, of Aldborough, three hundred replies were received, eleven of which represent medical bodies. The total number in favour of Dr. Marshall Hall's plan was two hundred and fifty-four; the number in favour of the Royal Humane Society's plan, ten; the number of those who recommend the addition of some of the Royal Humane Society's rules to those of Dr. Marshall Hall, twenty; number whose replies are undecisive, twelve; number who decline to offer an opinion, four; number who have successfully practised Dr. Marshall Hall's plan on drowned persons, seven; number who have successfully practised Dr. Marshall Hall's plan on still-born children, some of them in several instances, twenty-two.

The large number of medical men who have approved of Dr. Marshall Hall's plan in preference to the older one, which sought mainly to restore the circulation, while the former aims at a primary restoration of respiration, has in-

duced the National Life Boat Institution materially to modify their directions for the restoration of persons apparently drowned, and in the main they have adopted the principles of Dr. Marshall Hall's system; it is therefore unnecessary to dwell upon them, as the "Ready Method" is now sufficiently well known. We hope, however, to hear that Dr. Silvester's suggestions will receive attention, as they appear, *à priori*, to embody some valuable practical advice, and to deserve to be fairly put to the test.

The New C.B.

Among the many medical men who have of late years performed deeds of heroism unequalled by any gallantry ever shown on the field of battle, three names stand out with peculiar brilliancy—Thomson of the Alma, Turnbull of China, and M'William of the Niger. The first two sacrificed their lives in the execution of their voluntary task; the third, thanks to a vigorous constitution, resisted the pestilential influences through which he piloted his vessel with the death-stricken crew, and survived silent perils as great as those which surround the soldier in the din of battle. A tardy reward at last recognises the services of Dr. James Ormiston M'William, who in the beginning of the month of September was gazetted a Companion of the Civil Division of the Order of the Bath. So long a time has elapsed since the deed was done which justifies our coupling the name of Dr. M'William with the two martyrs, Thomson and Turnbull, that it may be well to remind some of our readers that in 1841 an expedition was despatched from this country to explore the Niger, with Dr. M'William as chief medical officer. Three weeks after entering the river, two of the three steamers which ascended it were compelled, by the disastrous fever which broke out, to return to the sea, leaving the *Albert* to carry civilization singlehanded to the heart of Africa. The malaria, however, was too powerful; and a few weeks later, none but the surgeon and geologist were possessed of sufficient strength to perform any duties on board this vessel. It fell to their lot to work a ship, whose complement of men was one hundred and nine, through all the intricacies of the river to the open sea. The weariness and anxiety of those long days can scarcely be conceived, except by those who have experienced similar events. If State rewards are not altogether a sham and a snare—if it be right that heroism should be honoured for its own sake, and decorations not become an appanage of a class—then is Dr. M'William truly one of those whose claims to knighthood, as a recognition of eminent services, are undeniable; for he has indeed shown himself *sans peur et sans reproche.*

It is well for the country that such rewards are the least of the incentives which stimulate men to noble deeds; for if the State were served with the alacrity with which it recognises services rendered, these would soon dwindle to a minimum.

Rejoicing as we do in every distinction worthily conferred upon our medical brethren, we take this opportunity of placing on record in our pages the names of the following medical officers who have recently received the Victoria Cross for distinguished bravery:

"78th Regiment. Assistant-Surgeon Valentine Munbee M'Master; date of act of bravery, 25th of September, 1857. For the intrepidity with which he exposed himself to the fire of the enemy in bringing in and attending to the wounded on the 25th of September, at Lucknow. (Extract from Field-Force Orders of the late Major-General Havelock, dated 17th of October, 1857.)

"90th Regiment. Surgeon Anthony Dickson Home, September 26th, 1857; for persevering bravery and admirable conduct in charge of the wounded men left behind the column, when the troops under the late Major-General Havelock forced their way into the Residency of Lucknow, on September 26th,

1857. The escort left with the wounded had, by casualties, been reduced to a few stragglers, and being entirely separated from the column, this small party, with the wounded, were forced into a house, in which they defended themselves till it was set on fire. They then retreated to a shed a few yards from it, and in this place continued to defend themselves for more than twenty-two hours, till relieved. At last, only six men and Mr. Home remained to fire. Of four officers who were with the party, all were badly wounded, and three are since dead. The conduct of the defence during the latter part of the time devolved therefore on Mr. Home, and to his active exertions previously to being forced into the house, and his good conduct throughout, the safety of any of the wounded, and the successful defence, is mainly to be attributed.

"Assistant-Surgeon William Bradshaw, September 26th, 1857. For intrepidity and good conduct when ordered with Surgeon Home to remove the wounded men left behind the column that forced its way into the Residency of Lucknow on September 26th, 1857. The dooly-bearers had left the doolies, but by great exertion, and notwithstanding the close proximity of the Sepoys, Surgeon Home and Assistant-Surgeon Bradshaw got some of the bearers together, and Assistant-Surgeon Bradshaw, with about twenty doolies, becoming separated from the rest of the party, succeeded in reaching the Residency in safety by the river bank."

BOOKS RECEIVED FOR REVIEW.

Epilepsy and other Convulsive Affections; their Pathology and Treatment. By C. B. Radcliffe, M.D. Second Edition. London, 1858. pp. 383.

Annual Report of the Royal Edinburgh Asylum for the Insane. Edinburgh, 1858.

Second Annual Report of the Medical Officer of Health to the Vestry of St. James's, Westminster. 1858. pp. 76.

Treatise on the Principles and Practice of Veterinary Surgery. By William Haycock. Part I. London, 1858. pp. 160.

On the Causes of Idiocy. By Dr. L. G. Howe. With an Appendix. Edinburgh, 1858. pp. 78.

The Dwellings of the Poor. By Henry Leach, M.A. London, 1858. (Reprint.)

Notes on the Surgery of the War in the Crimea. By George H. B. Macleod, M.D. London, 1858. pp. 439.

The Use of the Microscope in Clinical Medicine. By Lionel Beale, M.B. No. III. London, 1858.

Archives of Medicine. Edited by Lionel Beale, M.B. No. II. London, 1858.

Sanitary Science. By W. T. Robertson, M.D. London, 1858. pp. 32.

A Manual of Photographic Manipulation. By Lake Price. London, 1858. pp. 256.

The Liverpool Med.-Chirurgical Journal. July, 1858.

Urethro-Vaginal Fistules. By N. Bozeman, M.D. Montgomery, United States, 1857. pp. 59.

Second Annual Report on the Sanitary Condition of St. Pancras, Middlesex, during the Year 1857. By T. Hillier, M.D.

The Ophthalmoscope. By Jabez Hogg. London, 1858. pp. 106.

Edinburgh Veterinary Review and Annals of Comparative Pathology. Published Quarterly. July, 1858, No. I. Edinburgh.

American Medical Monthly. June, 1858.

Notes on the Existence of the Castor Fiber in Scotland. By C. Wilson, M.D. Edinburgh, 1858. pp. 40. (Reprint.)

On the Transmission of Diseases between Men and the Lower Animals. By W. Lauder Lindsay, M.D. (Reprint.)

On Sulphate of Cinchonia. By F. Peyre Porcher, M.D. (A portion of an America Journal.)

Papers relating to the Sanitary State of the People of England, communicated to the General Board of Health. By E. H. Greenhow, M.D. London, 1858. pp. 164.

On the Pathology of Rheumatism. By F. T. Bond, M.B. 1858. (Reprint.)

The Atlantis, a register of literature and science. No. II. July, 1858.

On the Function of the Yellow Spot of Sömmering in Single Vision. By T. Hayden, F.R.C.S.I. Dublin, 1858. (Reprint.)

On the Sickness and Mortality in the French Army during the Campaign in Turkey and the Crimea. By Gavin Milroy, M.D. pp. 23.

On some severe Forms of Disease arising from the Retention of Decayed Teeth. By J. C. Clendon. 1858. (Reprint.)

Pathologie und Therapie der Muskellähmung. Von Dr. H. Friedberg. Weimar, 1857. pp. 349.

Health and Disease. By Benjamin Ridge, M.D. London, 1858. pp. 624.

On Dislocations and Fractures. By Joseph Maclise, F.R.C.S. Fasciculus V. London, 1858.

North-American Medico-Chirurgical Review. July, 1858.

American Medical Monthly. July, 1858.

Charleston Medical Journal. July, 1858.

Twelfth Report of the Commissioners in Lunacy. 1858.

The New York Dental Journal. Vol. I. No. 1. New York, 1858.

Forhandlinger ved de Skandinaviske Naturforskeres syvende möde. I. Christiania, den 12–18 Juli, 1856. Christiania, 1857. pp. 34.

Diskussion i det norske medicinske Selskab i Christiania angaaende Spedalskheden. Christiana, 1857. pp. 176. (Reprint.)

Norsk Magazin for Lægevidenskaben. Anden Række. Bind xi. Hefte 11 and 12, Bind xii. Hefte 1–4.

The Histology of the Supra-renal Capsules. By Geo. Harley, M.D. London, 1858. (Reprint.)

On Gun-shot Wounds of the Thorax. By George Lawson, F.R.C.S. London, 1858. (Reprint.)

The Retrospect of Medicine. Edited by W. Braithwaite. Vol. xxxvii. Jan.—June, 1858.

The Half-yearly Abstract of the Medical Sciences. Edited by W. H. Ranking, M.D., and C. B. Radcliffe, M.D. Vol. xxvii. Jan.—June, 1858.

The Restoration of Persons apparently Drowned. (A paper reprinted from the Journal of the National Life-boat Institution.)

The British Army in India: its preservation by an appropriate clothing, housing, locating, recreative employment, and hopeful encouragement of troops. By Julius Jeffreys, F.R.S. London, 1858. pp. 393.

The Journal of Psychological Medicine. Edited by Forbes Winslow, M.D. July, 1858.

Die Physiologie der Thymusdrüse in Gesundheit und Krankheit. Von Alexander Friedleben, M.D. Frankfurt a. M. 1858. pp. 336.

Schiff, Lehrbuch der Physiologie. 2 Heft. Lahr, 1858.

Obstetric Morality. By Fleetwood Churchill, M.D. Dublin, 1858. (Reprint.)

Gazette Hebdomadaire. Tome V., Nos. 33, 34, and 35.

On the Comparative Use of the Ergot and the Forceps in Labour. By B. F. Barker, M.D. New York, 1858. (Reprint.)

On the Nature, Cause, Statistics, and Treatment of Erysipelas. By Peter Hinckes Bird, F.R.C.S. London, 1858. pp. 60.

A Treatise on the Pathology of the Urine. By J. S. W. Thudichum, M.D. London, 1858. pp. 429.

A Review of the Anniversary Discourse, by J. Marion Sims, M.D., on Silver Sutures. Philadelphia, 1858. pp. 20. (Reprint.)

On the Restoration of Motion by Forcible Extension and Rupture of the Uniting Mediums of partially Anchylosed Surfaces. By B. Brodhurst. London, 1858. pp. 39.

Anatomy, Descriptive and Surgical. By Henry Gray, F.R.S. The Drawings by H. V. Carter, M.D. London, 1858. pp. 750.

Lectures on the Diseases of Women. By C. West, M.D. Part II. Diseases of the Ovaries. London, 1858. pp. 247.

Fever in Agricultural Districts. By H. W. Acland, M.D. Oxford, 1858. pp. 23.

The true Physiological Method of Restoring Persons apparently Drowned. By H. R. Silvester, M.D. pp. 20. (Reprint.)

Medical Report of the Royal Lunatic Asylum, Aberdeen, 1858.

On the Temperature of Man within the Tropics. By John Davy, M.D. From Philosoph. Transact. London, 1845 and 1850.

ERRATUM.—In the July Number of the 'British and Foreign Medico-Chirurgical Review,' at p. 111, line 15 from foot, *for* years, *read* months.

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