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PART FIRST.

Analytical and Critical Reviews.

REVIEW I.

Clinical Researches on Disease in India. By CHARLES MOREHEAD, M.D., Principal of Grant Medical College, Professor of the Principles and Practice of Medicine, and of Clinical Medicine; also Surgeon to the Jamsetjee Jejeebhoy Hospital, and formerly Assistant-Surgeon to the European General Hospital at Bombay.—*London*, 1856. 2 vols., 8vo, pp. 687, pp. 766.

THE medical works which come under our notice are commonly more or less of two kinds,—one written as introductions to practice, more theoretical than practical, more specious—if the products of clever intellects, than useful, ephemeral in their origin, and as ephemeral in their end; the other, and the more rare, composed with a totally different intent, and of a totally different character, embodying the results of a large and long experience, making additions to our stock of medical knowledge, and becoming—and deservedly—like the works of Hippocrates and Aretæus amongst the ancients, of Sydenham, Laënnec, and may we not say of Bright, amongst the moderns, a permanent portion of the literature of our profession—treasuries of facts constituting the foundations of the philosophy of medical science. To remark that the work which we are about to review belongs to, or even assimilates to the latter class, is certainly bestowing on it a very high compliment; and yet, so much does it display of research, so much of original observation, with other qualities of a high order, that, as we believe, it fully justifies the opinion we have formed of it.

The title portrays well the character of the work. It is essentially practical and clinical, written, as we are informed by its author, after a continued service in India extending over twenty-five years, he during the time enjoying opportunities of observation of varied and great extent—a field of research such as could hardly have been obtained in any other country, and under circumstances specially favourable for bringing to maturity and giving to the public the results of his experience. Some of these circumstances it may be right to mention, and we shall quote from the preface, not in Dr. Morehead's own words, but in those of Dr. M'Lennan, Physician-General of the Bombay Army, and from a part of a Minute addressed by the latter to his colleagues in the Board of Education, and approved by them and the Government of Bombay, on the occasion of recommending an extension of leave of twelve months at home, expressly for the purpose of preparing this work. Dr. M'Lennan, assigning the grounds for this recommendation, states, that on Dr. Morehead's—

"First arrival in India, he served for two years with European, and for many years with native, troops, at different stations. He was then for two years in charge of the sanitary station of Mahableshwur; thereafter, for more than six years resident assistant-surgeon of the European General Hospital, Bombay, an institution in which the inmates are of very varied circumstances as to habits, position in life, nature of duties, and length of residence in India, &c. In that hospital are accommodated the newly-arrived European and the old servant of many years' Indian residence; the seamen of the Royal, Indian, and mercantile navies; the soldiers of all arms and both services, Queen's and Company's; the townsman, mechanic, clerk, male and female, adult and child, from most classes of life and many stations in the interior. The opportunity, therefore, for seeing variety of disease under great diversity of circumstances, is considerable. Dr. Morehead was likewise for six years surgeon of the Byculla Schools. In parts of 1843 and 1844, he was in Scinde, and had an opportunity of observing the state of health of Europeans and natives after the sickly season of 1843. He has been for nearly nine years surgeon of the Jamsetjee Jejeebhoy Hospital, and for six years has been engaged in teaching medicine and clinical medicine in the Grant Medical College; and the records of the clinical wards have been carefully preserved during the whole of this period. He has been twelve years secretary to the Medical and Physical Society, during which time there has been afforded him by the Medical Board the opportunity of becoming acquainted with the tenor of the medical reports and cases from all parts of the Presidency. In 1833, and again in 1853, Dr. Morehead had the opportunity of observing some of the hospitals and medical institutions in Madras, Calcutta, Colombo," &c. &c. (Preface, p. vi.)

After giving a list of the numerous papers contributed by Dr. Morehead, on the Diseases of India, the Physician-General proceeds :—

"Having thus detailed the sources from which Dr. Morehead's experience and fitness for the task which I have ventured to suggest have been derived, I may now add a few words as to the nature of that want which I propose he should supply; and here I honestly give it as my opinion, that till some work of the kind I suggest be brought forth, the efforts of the Indian Governments and their servants in medical education will be incomplete. At present, graduates and students of Indian medical colleges are without any book on practice in Indian disease, as now generally followed, or as requiring modifications to meet peculiarities of native habit and constitution. The duties of the clinical wards in the Grant Medical College have been so carried on and so recorded, as to constitute an important collection of facts and practice, which may be brought to bear on this want. The labour of collecting, digesting, and condensing for such a work will be considerable, and as it is valuable for Indian purposes, it should (it seems to me) receive support and encouragement from the Indian Government, which Dr. Morehead has so zealously and usefully served. I therefore trust my colleagues will support my proposition, and recommend, that after the expiration of the leave lately granted, Dr. Morehead may have for the above purpose another year in England on Indian allowances, and to count as service, with the right of returning to that place in the Grant Medical College over which he has so beneficially presided." (Preface, p. vii.)

This is a long extract; but, besides its bearing on the work before us, is it not interesting and instructive, as showing the nature of the Indian medical service, and its expansion? The last paragraph we have specially given, both for the excellent intent it displays, and the liberal spirit with which it has been adopted and acted on, to the credit of all concerned. The encouragement which the Indian Government has given to practical science—more particularly to medical science and its diffusion amongst the natives—is deserving of all praise; and how well has it been responded to by the Company's medical officers! What a satisfaction to know that every one of these, if deserving, is sure of reward—of retiring with more than a competency, still in the vigour of life; and if possessed of superior ability, exercised with zeal, sure also of earning, besides fortune, at least local distinction, by becoming either a secretary or member of a directing medical board, or a professor in a medical college, or an official naturalist;—in brief, by filling some appointment of the kind named in one or other of the many departments belonging to a great empire, the business of which, for its efficient performance, requires exact science, and of the kind included in the curriculum of a liberal medical education. Would that we could bestow the same commendation on our Home Government, for the treatment of its army medical officers; a service in which zeal is chilled by want of acknowledgment, merit by want of reward; justifying the remark in the spirited narrative of the siege of Kars, as to "the singular resemblance between the English and the Turks in their approbation [neglect] of military surgeons," and which, in both services, has never been more strongly displayed than in the war just concluded.

Now to our task,—the work of Dr. Morehead, which, were it not for the liberality we have been commending, probably would never have been undertaken, and surely not on the ample plan according to which it has been carried out, making it at the same time a record of facts for the augmentation of science and a handbook of practice for medical officers, and not only in our wide Indian empire, but in our colonies also, even more widely extended. We should be unjust to the author were we to withhold his description of it. Referring to the design sketched out by the Physician-General already quoted, he says:

"In performing this duty I have endeavoured to embody my experience in a connected form, and to illustrate my opinions by cases which have passed under my immediate observation and care; while, at the same time, I have not been inattentive to the views of other inquirers.

"My clinical researches have been directed to disease, as occurring both in Europeans and in the natives of India. I have aimed not merely to increase practical knowledge of the diseases usually termed tropical,—as malarious fever, hepatitis, dysentery; but also to show that affections—pneumonia, phthisis pulmonalis, pericarditis, Bright's disease—familiar to European observers, are sufficiently common in India, more particularly in some classes of the native community." (Preface, p. viii.)

In the Introduction he offers some general remarks on the character of

Indian diseases, their predisposing and exciting causes, and their treatment,—remarks well deserving of attention, as founded on experience, and as expressing briefly the principles of the system by which he has been guided, both in his practice and in his reasoning on theoretical views.

He holds that the prevailing diseases, except in the instance of the newly-arrived Europeans, are rather asthenic than sthenic, oftener chronic than acute; that their chief predisposing causes are malaria and cachexia,—the latter a word he prefers to the dyscrasia of Rokitansky; that their chief exciting causes are the same malaria, acting with greater intensity than when predisposing, and external heat and internal cold; that in their treatment consistently with their character, the antiphlogistic plan is seldomer required than the tonic and alterative; that when the abstraction of blood as a remedial measure is indicated, local, by cupping or leeches, is preferable to general by venesection.

In these prolegomena we see much to approve and little to dissent from. They all need further research; and we are sure, if properly entered on, according to the most approved methods of investigation, they will richly repay the labours of the scientific physician. Malaria may be taken as an example. The author justly remarks, that all we know of it is by its effects on the animal system. We may say, were it not for these effects, it would be to us a nonentity. On this account, ought we not to be specially careful in admitting its presence and operation, lest we attribute to it what may be more correctly owing to other circumstances? On the same account, ought not our conclusions regarding its properties to be derived from the largest possible induction? Dr. Morehead, we think, has hardly observed this rule. Amongst the propositions (eight in number) which he has laid down respecting malaria, there are two or three which are open to objection. We shall notice only the seventh, in which he adopts the belief that the noxious properties of malaria are lost in passing over a surface of water, even of small extent; and in adopting the conclusion that it is "attracted by, and clings to, the foliage of trees—thus rendering them a focus of the poison, but at the same time, a protection to tracts of country beyond." Facts we could mention, not according with these statements. The east wind, it is too well known, is not rendered harmless by crossing the intervening sea to our shores. No part of Ceylon is more salubrious than that portion of it which is skirted by a belt of the cocoa-nut palm, from a quarter of a mile to one or two in depth; and we were assured many years ago, that the inhabitants of Trincomalee, in the same island, had occasion to repent the cutting down of trees which had afforded a grateful shade round their dwellings, in an increase of malaria fever after their removal, contrary to the expectations on which they had acted.

Concerning atmospheric heat and cold, as agents productive of disease, we have little hesitation in adopting our author's views, especially his inference—and it is an important one—that a few degrees below the mean temperature are more operative as the predisposing and exciting causes of disease, than the highest degree of temperature exceeding the mean: which is easily explicable, keeping in mind the susceptibility of the natives as to cold, from their thin clothing, comparatively spare vegetable diet, and their relaxed cutaneous system—circumstances rendering them better fitted for bearing a high atmospheric temperature; whilst their dark colour (the darker the more they are exposed to the sun) affords protection, in a measure, from the sun's rays acting as radiant heat.

He remarks, in considering the causes of disease, that "one effect of elevated temperature on the animal system, is the less necessity for animal heat; . . . less demand for food, diminished metamorphosis of tissue, and decreased excretion." This we think just; but we are doubtful of the accuracy of his conclusion that, amongst the natives and the acclimatised Europeans living moderately, observing the ordinary rules of health, bilious complaints, implying an action of the liver vicarious of the lungs, are not more prevalent in India than in cooler climates, and, *à fortiori*, in other warm and tropical climates. From our own experience, we are led to the inference that such complaints are even less common in the West than in the East Indies—in the West, where there is a greater uniformity of elevated temperature, and where, were mere temperature concerned, it might be expected that the liver, in its vicarious function, would be more severely tasked. This difference we are disposed to attribute to the difference of diet: that of the East consisting of a larger proportion of low vegetable food, that of the West containing a larger proportion either of animal food or of vegetable food of a higher nutritive quality, and very much less of fatty or oleaginous matter—a matter which enters so largely into the composition of Indian curries—a preparation of food, in its infinite varieties, rarely absent from an East Indian meal. This peculiarity has not been adverted to by the author. It is deserving, we think, and we hope it will have, his attention in his further researches on Indian disease.

Fevers, very properly, are first treated of. They are included under the head of intermittent, remittent, and common ardent continued fever; and are followed by short sketches of the plague of the Levant, of yellow fever, of typhus, typhoid, and relapsing fevers of colder climates,—these introduced as supplementary, not founded, as the preceding, on his own clinical researches, and consequently not affording any new information, mainly given, it would appear, for the sake of comparison and completion.

The importance of the idiopathic fevers of India is denoted by their proportional frequency and fatality:—of the European troops of the Bombay Presidency, 61·3 per cent.; of the Madras, 27·838; and of the Madras native troops, 27·937, of the strength, are, on an average, annually affected with fever; whilst of the total deaths among the European soldiers in the Bombay Presidency, about 23 per cent. are from fever, and among the officers as high as 28·7 per cent. In the native population of the island of Bombay, during five years, the deaths from fever have amounted to 27,212, which is in the ratio of 40·26 per cent. of the total mortality. The liability of the natives to this class of diseases—little inferior to that of Europeans—is remarkable, especially when contrasted with some other races—such as the Malays, and more especially Africans, who may be considered in a manner exempt from malaria influence;—a peculiarity this, we may remark in passing, more deserving the attention of Government than it has yet received, inasmuch as it is capable of being turned to great advantage in malarious districts, at times when their military occupation may become necessary. In Ceylon, during the rebellion of 1818-19, and in the West Indies on several occasions, striking examples have occurred of a vast saving of life by relieving white by black troops, under the circumstances alluded to; and reflecting on the subject, we cannot but express surprise that neither Malays nor Caffirs (Africans) have been brought into the military service of the East India Company.

The author's account of intermittent fever affords a good example of his method—combining the practical and rational; careful observation of symptoms, and of the effects of the remedial means employed, with a logical and judicious reasoning on both; exemplifying by an ample record of cases, and concluding with statistical returns: a method which he follows more or less throughout the work.

The pathology which he adopts of intermittent fever is—"That in the cold stage, there is a sedative influence exercised by the morbific cause [malaria] on the action of the heart, and a tendency in the blood to move languidly, and be delayed in the capillary system of important internal organs;" but whether acting through the blood on the heart, or intermediately on the nervous system, or in any other way, as matter of speculation in want of adequate data, he wisely declines discussing. Not the least instructive portion of this chapter are his remarks on the influence of the disease and of malaria—of the frequent recurrences of the one and of continued exposure to the other—in producing "a cachectic state of the system, in which the nutritive processes of the tissues and of the blood are defective and perverted, and in which splenic and hepatic enlargement, and other local congestions of blood, tend to occur"—often leading to death by asthenia—i.e., functional exhaustion; and often, even oftener, to fatal bowel complaints, under the influence of cold acting on the mucous membrane of the intestinal canal.

The treatment he advocates is founded, we think, on just principles—being temporizing, expectant, and palliative, chiefly during the cold, hot, and sweating stages; and active only during the intermission, and then trusting mainly to quinine.

Speaking of the palliative means which he recommends in the several stages of the paroxysm, he makes the following excellent remarks:

"They do not aim at checking or materially cutting short these stages. We are not acquainted with any means that possess this power, but we must rest satisfied with an endeavour to control somewhat the deranged actions. We must be particularly careful in preserving the strength of the patient, not by the injudicious use of food which the system cannot assimilate, but by guarding against a too evacuant and depressing course of treatment.

"The excessive and injudicious use of bloodletting, of emetics, purgatives, mercurials, antimonials, is not only prejudicial by favouring the development of a cachectic state, but it also distinctly favours the recurrence of the paroxysm, and the protraction of the disease. Nor is it difficult to explain this. The malarious influence affects with greater severity, and clings with greater tenacity to, debilitated constitutions. It matters not whether the debility has been induced by medical treatment or by other causes. Under an increasing asthenia, tertians may be observed to become quotidian, and quotidians to become remittent; and I am satisfied that this unfavourable course has not unfrequently been occasioned by the increasing asthenia caused by too depressing a treatment." (Vol. i. p. 40.)

Adding ingenuously, in a foot-note:

"I write with this confidence, not merely from the negative evidence of success attending the opposite course of treatment, but from the positive evidence of having witnessed the evils I describe. I have before me cases, to be afterwards quoted, of my early practice in India, which prove these truths, and show that then they were not familiar to me."

Concerning the active treatment during the period of intermission, by the disulphate of quinine, the following is equally worthy of attention:

"The very earliest intermission should be taken advantage of, and quinine be at once exhibited. The best mode of using this remedy is to give it in doses, from four to ten grains, more or less frequently, according to the severity and obstinacy of the case. The nearer it is given to the period of the expected accession, the more efficacious it will prove to be. For example, if we are acquainted with the probable period of accession, the quinine may be commenced four hours before, and be repeated every second hour. Thus the third dose will fall to be given about the time of commencement of the expected paroxysm; then the quinine should be continued, in perhaps decreasing doses and lengthening intervals, for four or six hours after the period. If the paroxysm has been prevented, the quinine is to be resumed on the following day, in the same manner, and repeated on the third and fourth succeeding ones, but in decreasing doses and at longer intervals after the second or third day. If the type has been tertian, quinine may be given in smaller quantity on the intermediate day than on that of the expected recurrence." (Vol. i. p. 41.)

The dose of the disulphate which he prefers is from four to six grains, seldom using ten, and never to the extent to produce cinchonism, which he considers unnecessary. On the other antiperiodic medicines he places little reliance—such as the liquor arsenicalis, which, in most of his trials of it, he found little if at all efficacious; such as the sulphate of bibeerine, muriate of narcotine, a strong infusion of chiretta, and scruple doses of the Cæsalpinia Bonduccella.

We must pass over much that is valuable respecting intermittents and their complications with enlargement of the spleen and liver, with affections of the stomach and bowels, with cerebral disease, and others—such as bronchitis, pneumonia, rheumatism, scorbutus, pericarditis, asthma. Each has a separate section. They will all amply repay a careful perusal.

To select a single example, we shall give Dr. Morehead's view of the pathology and treatment of that complication which is most frequent—viz., enlargement of the spleen, a complication which, out of 243 clinical cases of intermittent fever, he found present in 91.

"With few exceptions," he states, "it is met with only in individuals who have suffered from recurring attacks of intermittent or remittent fever, or who, not having suffered from distinct attacks of fever, have long resided in malarious localities. Under both circumstances, the splenic enlargement is accompanied by a cachectic state of the system, a deteriorated condition of the blood." (Vol. i. p. 58.)

This state, this condition, he ingeniously illustrates by reference to the physiology of the organ and its presumed functions. The following are his indications of treatment, founded on experience, and, as he thinks, in accordance with the physiology and pathology as before given:

"1. To prevent the recurrences of intermittent fever, should they still continue to take place. 2. To remove the cachectic state, and improve the condition of the blood by the use of all means which tend directly to this end, and by avoiding all measures which tend to induce asthenia, or still further impoverish the blood." (Vol. i. p. 63.)

The remedies he trusts to are chiefly quinine and preparations of iron. The abstraction of blood, the employment of mercury and purgatives, he deprecates as more than hazardous, stating the grounds of his belief. Change of air he recommends in obstinate cases, when it can be had recourse to under favourable circumstances, and the avoidance of the dangerous risk of a land journey through a malarious district, deprived of medical aid,—a proceeding that has cost the life of many individuals.

Remittent fever, the most prevalent disease of the East Indies, as, indeed, it is of the West, and of the South of Europe, and the western coast of Africa,—occasioning the greatest mortality, the most perplexing and difficult to treat in its many varieties and complications, naturally has the special care of the author; and he describes it with no ordinary ability. No part of the work marks better his fitness for the undertaking he has engaged in, or will better repay a careful study.

He attributes remittent fever to the same morbific cause as intermittent —viz., malaria, either acting with greater virulence, or on individuals more predisposed. He considers it, too, identical in nature, differing from intermittent chiefly in degree. The treatment, also, which it requires, he holds to be very similar, and resting on the same principles.

These his views are perhaps as near the truth as the present state of our knowledge can justify. Yet, were we to express our own opinion of the etiology of the disease, we should prefer the conclusion, that the malaria, the materies morbi of the one disease, is rather similar to that of the other, than strictly identical,—the one allied or kindred to the other, like iodine and bromine; and we might extend the inference to the pathology of the two, and their general history. But this is speculative, and of little importance.

We shall not attempt an abstract of the several sections in which the author delineates the disease in its simple form, its varieties, complications, and their pathology. It may be better, limited as we are for space, to confine ourself to the subject of treatment.

Recapitulating the general principles of the medical treatment of intermittents, he explains how, as he thinks, they should be modified to meet the exigencies of the more formidable disease. We shall quote his words:—

"In intermittent fever, there is in general not much risk of injury to important organs during the stage of febrile reaction. Frequent recurrence of the paroxysm is not in general attended with immediate danger to life. It does harm by deteriorating the constitution.

"In remittent fever, on the other hand, there is more commonly risk of injury from the increased vascular action of the stage of exacerbation. Recurrences of the exacerbation are, therefore, attended with immediate danger to life from lesion of important organs or depression of vital actions. Hence, in the treatment of remittent fever, there is more frequently necessity for the reduction of vascular action by depleting means; but at the same time, much greater demand for discrimination and caution, for the evils of the injudicious use of depressant remedies are more immediate, more certain, and more serious. If such are the dangers which more or less attend upon the exacerbation of remittent fever, then the prevention of its recurrence by the efficient use of quinine given in the remission, is even more urgent than the same indication in the intermission of intermittent fever.

"If it be true, that at some periods of the exacerbation of remittent fever, there may be risk of injury to important organs from excessive vascular action calling for control by depletion, and that, at other periods, there may be danger to life from exhaustion, requiring the prompt use of stimulants and nourishment; if it be also true that the periods of exacerbation and remission are liable to vary in different cases, that it is most important to prevent the exacerbation, and that we possess the means of doing so,—then it follows that there cannot be successful treatment of remittent fever, justice to the sick, or loyalty to the profession of medicine, unless our visits be frequent and our watching attentive and well-timed." (Vol. i. p. 175.)

He corroborates this by contrasting the zymotic continued fevers of colder climates with remittent fever, both depending on a materies in the blood whose power in operation in the febrile action produced we are unable to stop; danger in both, but more in remittent fever than in the others, from excess of vascular excitement; in both danger from depression of vital actions, but with this difference, comparing the one with the other, that in remittent fever

"There are suspensions of the febrile condition, and there is an agent which, effectively used in the remission, tends to prevent the recurrence of the fever, and thus most materially to shorten the general course of the disease. In this (he justly and forcibly observes) lies the strength of medical practice in remittent fever. It has no place in the treatment of the zymotic continued fevers of colder climates." (p. 176.)

In due course he discusses the treatment of remittent fever,—1, in its most tractable and common form; 2, in its severer form, its inflammatory —i.e., when attended with a greater degree of febrile excitement and cerebral and gastric derangement; 3, in its congestive form, connected with a depressed state of the vital actions of the vascular and nervous systems; 4, with a tendency to become continued, and then adynamic in character; 5, with badly-developed symptoms, and symptoms of unexpected collapse. *Seriatim*, also, he discusses its *modus medendi* in its complications,—such as cerebral affection, gastric irritability, jaundice, hepatitis, dysentery,—adding, in a section apart, some general remarks on bloodletting and the mercurial treatment, on cold affusion and wet-sheet packing, on purgatives, emetics, blisters, opiates, quinine, diet, and change of air.

We could wish to point out some valuable observations which occur, relative to the treatment of the disease in its several forms and complications, but our space forbids. We must not, however, pass over altogether the contents of the last-mentioned section, especially as it affords, as it were, the pith and essence of the whole, enhanced by a sound and enlightened criticism on the modes of treating remittent fever which have been in fashion at different times, and have been advocated more or less by distinguished authors. In giving his views, we shall use as much as possible his own words.

Of general bloodletting, he says:—

"In my observations on treatment, I have endeavoured to explain that general bloodletting is an expedient and useful proceeding—sometimes a very necessary one—in reducing the high vascular excitement of the early exacerbations of remittent fever in sthenic and lately-arrived Europeans, as well as in lesser degrees of excitement, when there co-exist in this state of constitution and stage of fever considerable determinations of blood in organs important to life. These conditions are seldom present except in European troops shortly after their arrival in India. The extent to which bloodletting should be carried in appropriate cases is a point on which the physician must exercise his discretion, keeping in view the ultimate advantage of effecting the advantage aimed at with as little loss of blood as practicable, and recollecting that the judicious removal of sources of irritation, the adoption of free ventilation, the well-timed use of emetics, cold affusion, tepid sponging, and antimonials, are all measures of considerable influence in lowering febrile excitement, and to which it is of very essential consequence assiduously to have recourse, with the view of lessening the necessity of large evacuations. In the treatment of remittent fever in Europeans some time resident in India, and in all classes of the native community, general bloodletting is, with few exceptions, an unnecessary proceeding; and when so, if used, it can hardly fail to be injurious." (Vol. i. p. 198.)

In corroboration of this doctrine, the author states various particulars, derived from his own experience and that of others.

On the use of calomel and the mercurial treatment, which he discusses very carefully and fully, he sums up as follows:

"For these reasons, then, I am of opinion that an endeavour to induce mercurial influence in remittent fever is erroneous in theory and of no value in practice. But the question is not thus easily disposed of. Not only is it erroneous in theory, and of no value in practice, but it is opposed to all rational theory, and very injurious in practice. If it be true that prostration of vital actions and deteriorated condition of the blood are pathological states to be much dreaded in remittent fever; and if mercury deteriorates the blood and favours prostration, on what principle of reasoning can it be supposed that induced mercurial influence can have any other than an injurious effect on remittent fever? I have on several occasions pointed out the tendency of malarious fever to produce a cachectic state of the system, and have endeavoured to enforce the importance of our adding as little as possible to this state of constitution by the remedial means we adopt. To all who, within the last twenty years, have had the opportunity of extensively observing disease in India, in all classes of the European community, the asthenic state, the dyspeptic symptoms, the injured teeth, the pains of sides and loins, the habitually foul tongue, the constipated bowels, the pale alvine evacuations, the depressed spirits, and the sense of sinking at the epigastrium—all clearly traceable to the abuse of mercury—must be familiar facts." (Vol. i. p. 206.)

He adds some valuable information respecting the ascertained effects of calomel on the dog, as ascertained by a series of experiments made by Mr. Murray in 1842, proving, contrary to the earlier and less extended trials made by Sir James Annesley, that its operation, in whatever doses given, is not sedative on any part of the *primæ viæ*, but is altogether irritant. Of cold affusion—that is, using water of a temperature about 80°, he speaks favourably, used timely and with discretion; but not so of the wet-sheet packing, which he considers hazardous, and generally to be avoided. The use of purgatives Dr. Morehead recommends in moderation, given early, and chiefly during the remissions, and in asthenic cases in combination with quinine. After the first two or three days, he deprecates their employment, as irritating the intestinal canal, and disposing to dysentery. Respecting emetics he offers similar cautions.

Blisters he approves, with the intention of controlling local capillary derangement (or, as he sometimes uses the expression, determination of blood), when the stage appropriate for topical bloodletting is passed; the stage of remission he points out as most proper for their application. On the use of opiates he offers, and certainly not needlessly, many cautions:

"I assume (he says) that opium in remittent fever is thought of only when there is restlessness and want of sleep; and that it can be used with safety only in the early stage, when there are not symptoms of marked determination to the brain, and when the pulse is of good volume and soft, and not much above 100." (Vol. i. p. 233.)

He adds other rules, thus concluding:

"Whenever in remittent fever the pulse is towards 120, feeble and compressible; whenever there is wandering delirium, a slight drowsiness, the exhibition of a full opiate is a measure of danger, more particularly towards the close of a febrile exacerbation. In other words, whenever in remittent fever the tendency to death by asthenia or by coma is well marked, a full opiate will expedite the fatal result." (Vol. i. p. 235.)

Respecting quinine, he gives much information, practical and historical, showing how, during the last twenty years, it has been gradually gaining ground in repute and extension of use in India, since juster views of the pathology of the fevers of the country have been formed, and they have ceased to be confounded with, and treated on principles derived from, the practice in the zymotic fevers of colder climates. We could have wished that in his notice of the gradual adoption into practice of this heroic medicine, he had made mention of what had been done in the West, where, as may be seen from the researches of Dr. Blair on yellow fever, it has had a most extensive trial with excellent results. It would appear from documents in the office of the Inspector-General of Hospitals in Barbadoes, which we have had an opportunity of consulting, that quinine was employed even earlier in the West than in the East Indies. It was first used in St. Lucia, in December, 1824, in a case of obstinate intermittent—one that for months had resisted "every medicine that could be thought of," till trial was made of the new remedy, under the action of which it yielded in one day, and without a recurrence.

On diet and change of air, the last topics under the head of general treatment that the author enters upon, his observations are such as might be expected, judiciously cautious. He recommends, as before in the instance of obstinate intermittents, change of air, if necessary, and safely available. He justly remarks that "the importance of placing fever patients, whenever practicable, in a pure and temperate atmosphere cannot be overrated." As to diet, he points out the error of postponing "the use of nutritious food till the signs of prostration are urgently present."

The section following that on the treatment of remittent fever, entitled, On certain Obscure Phenomena probably related to Malaria; and on Adynamic Remittent Fever, infectious in character, observed at Pali and elsewhere, is, as relates to the first subject, very deserving of the attention of the medical inquirer, and especially of members of the profession exercising their calling in malarious districts; and as relates to the second, it cannot fail to interest those who study the diseases of climate, the habitats of diseases, and appreciate the mystery of their several abiding places. We must pass them over, and also the subjects of the following sections, comprising the common and ardent continued fever of India, the fevers of children in India, and the hospital statistics of fever given in a tabular form; and though unwillingly, we must exercise the same forbearance regarding the contents of the succeeding chapter, relating to Plague of the Levant, Yellow Fever, and Typhus, Typhoid, and the Relapsing Fevers of Colder Climates, to enable us to reserve space for diseases which have come immediately under the observation of the author, and which from their wide spread are of general importance.

Under the head of Eruptive Fevers, the author gives his experience on small-pox, measles, scarlatina, erysipelas, followed by brief mention of hooping-cough and cynanche parotidea. The two latter, it would appear, are of rare occurrence, and consequently of little importance. The same remark applies to scarlatina and erysipelas; the latter, as an idiopathic affection, the former, also rare and even of doubtful existence in its genuine form, that is, identical with the scarlatina simplex, anginosa, and maligna of European countries. Small-pox and measles are better known in India, and on these the author gives fuller information. From his statements, according with the statements of others, the former disease, it may be inferred, is not less dangerous than in cooler climates, and is as effectually guarded against by vaccination, the practice of which hitherto has been but little enforced. Measles, as described by Dr. Morehead, though of less frequent occurrence than in Europe, appears to be as serious in its effects; indeed, the recorded mortality from it in India exceeds that in Europe, in the ratio of about 4·6 to 3 of those attacked. Though the details he gives of both these diseases are not without interest, the results of his observations are not sufficiently novel to require to be particularized. It is satisfactory to find that his confidence in vaccination is unshaken, and that, after careful inquiry, he considers it as powerfully preservative against small-pox as inoculation is against a second attack of the disease; which altogether accords with our own belief, formed chiefly in Malta during the epidemic prevalency of the disease in 1830-31. At that time, out of a population of 105,367, as many as 8067 were attacked, and as many as 1172 died, the great majority of whom were unprotected by vaccination; and yet, though there was free communication between our troops and the natives, only 10 of the latter, out of a force of 2219, contracted the disease, and of these 2 only died, one of whom had had small-pox before, and the other had been vaccinated.* According to the rules of the service, every soldier on his enlistment is vaccinated, unless there be sufficient evidence of his having had small-pox, or of a previous vaccination having taken effect.

On cholera, to which a chapter is devoted, the author gives much valuable information. His account of the disease, which has had his attention for many years, is excellent—truly practical and rational—written with a caution the result of a lengthened experience, and with

* See an account of this epidemic by Dr. John Davy, in his Notes on the Ionian Islands and Malta, vol. vii.

views enlarged by the study of the best authors on the subject. We shall notice briefly some of the more general conclusions at which he has arrived, and nearly in the order in which they occur.

1. He lays it down that the cause of cholera is as yet undetermined, and that the data at present collected are inadequate for the solution of the problem. 2. That in India it has not its seasons of preference. 3. That if the spread of the disease be due to human intercourse, it is very limited indeed. 4. That attention to scrupulous cleanliness and ventilation around the cholera sick, and the placing them wide apart, are, in hospital arrangements, of the first importance. 5. That chilling cold and wet, as in the instance of malarious fevers, are probably determining causes.

Considering the pathology of the disease, he has come to the conclusion that the general and capillary circulation of the blood, and all the actions of the system depending on them, whatever the morbific cause—whether acting first on the blood or on the ganglionic nervous system—are more or less arrested in cholera, with which are in harmony all the morbid appearances that are met with previous to the secondary stage, the stage of reaction, often accompanied by inflammatory action, and in the fatal issues productive of corresponding lesions of tissues.

Under the head of Treatment, the following are some of his results:— 1. That when cholera is prevalent, or even apprehended, all cases of diarrhœa should be carefully attended to; and at the same time, great caution should be observed in the use of purgatives, of mercurials, antimonials, or other intestinal irritants. 2. That a simple opiate is the best remedy for the premonitory diarrhœa. 3. That when the cholera discharges are established, opium alone is not to be trusted: the disease then is to be treated on the mild palliative plan. 4. That the administration of acetate of lead and other astringents should not enter into this plan, their effects being doubtful. 5. That general bloodletting is injurious. 6. That the hot bath, emetics, hot saline enemata, rubefacient liniments, saline injections into the veins, the inhalation of vapours, galvanism, cold affusion, and wet sheet, are all either decidedly noxious, or of such a doubtful efficacy as not to warrant their employment.

His recapitulation of the practical conclusions to which he has been led, we shall give in his own words, strongly recommending it to the attention of our readers, and regretting that our limits do not permit us to give a fuller account of this important part of his work.

"These conclusions," he says, "may be shortly re-stated under the following heads:—

"1. In cholera epidemics, there is a proportion of cases ushered in by premonitory diarrhœa, which, if early treated by simple means, are frequently curable, and the cholera attack is prevented. In some instances, however, the diarrhœa is not checked by treatment, and cholera becomes developed.

"2. Cases of cholera occur—common in the early Indian epidemics, but more in the later ones—in which the state of collapse is moderate in degree. In these the tendency is to recovery, not to death; but restoration is materially favoured by judicious, moderate medical treatment.

"3. When collapse is considerable, then we have a condition somewhat analogous to the cold stage of ague, or the initiatory fever of small-pox—a state [illegible] varying in intensity [illegible] we can pretend to [illegible] as possible for [illegible] while we at the same [illegible] may be injurious, not only [illegible]

[illegible] of the various [illegible] assistance, and [illegible] is more certainly the [illegible] hours. When the [illegible] danger is increased; [illegible] be modified, by [illegible] before us, [illegible]

[illegible] general principles, [illegible] in all forms of [illegible]

[illegible] medical treatment —[illegible] still often [illegible] statistical data, as [illegible] drawn from such a source are very [illegible]

[illegible] diseases in their [illegible]—in its severer [illegible] treatment; and that, [illegible] the mortality which [illegible] causes and to prevent their [illegible] the medical profession should [illegible]

Dysentery, which the author next treats of—defining it as "inflammation more or less extensive, more or less acute, of one or other or all of the constituent parts of the mucous membrane of the large intestine,"—is certainly one of the most important diseases of India, and if we estimate it by its fatal effects, in importance [illegible] fevers. This is shown by the following table:

	Ratio per cent. of admissions from dysentery and diarrhoea to strength.	Ratio per cent. of deaths from dysentery and diarrhoea to treated.	Ratio per cent. of deaths from dysentery and diarrhoea to aggregate mortality.
European troops, Bombay Presidency	[illegible]	[illegible]	32·441
European troops, Madras Presidency	[illegible]	[illegible]	—
Native troops, Madras Presidency	[illegible]	[illegible]	—
European [illegible], Bombay [illegible]	—	[illegible]	26·1
European officers, Bombay Presidency	—	—	5·7
[illegible] hospital	—	[illegible]	21·8
General population of Bombay	—	—	18·50

(Vol. i. p. 439.)

After adverting to the analogy in structure between the mucous membrane and the skin, and the probable analogy of their diseased conditions, he enters on the morbid anatomy of dysentery, which he arranges under the following heads:—

"1. The morbid appearances presented by the mucous membrane of the large intestine.

"2. The complication of inflammation of the mucous membrane of the large intestine, or [illegible], with peritoneal inflammation, general or partial.

"3. Tumefaction in the region of the caecum, or sigmoid flexure of the colon.

"4. Displacements of the colon.

"5. Complications of dysentery with morbid lesions of stomach or small intestine.

"6. Complication of ulceration of the large intestine with abscess of the liver.

"7. The co-existence of enlargement of the mesenteric glands with dysentery." (Vol. i. p. 442.)

The above indicates a comprehensive view of the subject, and is well adapted, with the cases introduced by way of illustration, and the author's commentaries on them, to convey a just idea of the many forms of dysentery. So various, indeed, are the morbid appearances. and different, as to justify a preliminary remark which he makes, that, were the lining membrane of the colon as open to inspection as is the outer covering of the body, the diseased affections of the former, now generalized under one name, would probably have assigned them as many distinct names as are employed in the description of the diseases of the skin.

In treating of the etiology of dysentery, he again refers to the analogy above mentioned, taking occasion to express doubt that the disease is ever produced by a specific poison like that which occasions inflammations of the skin—such as the eruptions of small-pox, measles, scarlatina; or such as erysipelas; or some of the squamous, vesicular, and pustular eruptions.

The causes of dysentery he divides into exciting and predisposing. The chief of the former he holds to be cold and wet, under imprudent exposure, acting on constitutions of low capacity for generating animal heat, peculiar to the tropics. The predisposing causes he assigns are more complicated and more obscure: amongst them he ranks highly cachectic states of the system, and malaria. He concludes his discussions on them with the remark, that he prefers considering malaria a predisposing rather than an exciting cause, inasmuch as—

"The cold season of all the hill climates of India will excite dysentery in cachectic individuals, irrespective of the conditions of malaria generation; whereas the view that malaria is itself the exciting cause of dysentery, will tend to condemn all those hill climates in which the conditions of malaria generation are apparent." (Vol. i. p. 533.)

Besides the causes referred to by the author, there is, we believe, another, and which we are rather surprised he has not adverted to—viz., unwholesome water—water containing remains of decomposing animal and vegetable substances, and probably some living organisms, which act as irritants on the large intestine. We have known the disease more or less constantly persisting, so long as the troops in garrison in one of our West India Islands were supplied with water rendered impure in its course; and its ceasing altogether so soon as the same water was kept free from impurities by being conveyed through a well-constructed aqueduct. And it is notorious in the same islands, that the disease is most rife during a period of drought, when from scarcity of water the inhabitants are compelled to use water from ponds and other stagnant collections.

Under the symptoms of the disease, the author does not attempt to specify its several varieties, and assign them names: practically, he considers the following questions more important:

"Is it recent or advanced? Does it engage much or little, and what part, of the mucous membrane of the large intestine? Is it idiopathic, or co-existing with remittent fever? Is it simple, or combined with hepatitis, peritonitis, or other disease? What is the state of constitution: is it sthenic or phlogistic, or likely to be the subject of erysipelatous inflammation; is it asthenic from former disease, deficient food, or elevated temperature; or is it tainted with malaria, scorbutus, struma, syphilis, mercury, or retained excretions? What is the condition of the mucous membrane: simply reddened, or thickened, or ulcerated, or sloughing?" (Vol. i. p. 553.)

And these are the considerations by which he is guided in the description of the symptoms, for which we must refer to the work itself.

His account of the latency of the disease in its early stage is particularly deserving of the attention of the young military surgeon; as are also his remarks on some other important matters, such as the effects of certain remedies in modifying the symptoms—especially the qualities of the discharges; the impropriety of attaching importance to tenesmus as a diagnostic symptom, inasmuch as it only indicates inflammation or irritation of the lower portion of the rectum; or of considering symptomatic fever a necessary accompaniment of its early stage, or purulent discharge of its chronic stage.

In the treatment of the disease, he proceeds on the principle that "it must vary according to the stage of the inflammation, and the state of the constitution of the individual affected;"—in the early stage, diseased action being to be arrested; in the advanced and ulcerative stage, processes of repair being to be established: the one indicating the use of bloodletting, general and local, mercurial preparations, purgatives, ipecacuanha, and opium; the other, astringents, tonics, alteratives, opium. These several means he discusses at large. We can only notice those conclusions at which he has arrived which are of most importance.

Bloodletting, Dr. Morehead holds, requires to be used with great discrimination, especially general bloodletting; local, by leeches he prefers, excepting in particular cases. Calomel he considers of great service in the early part of the disease, given at bed-time, ten grains with a grain and a half or two grains of ipecacuanha and the same quantity of opium, followed the next morning by from half to an ounce of castor oil, and repeated twice or thrice, according to circumstances; and even continued if the dejections be pale and scanty, the abdomen full, and not much reduction of strength; the indication in view being to excite free secretion from the liver and the small intestine, without aggravating the excited state of the large intestine. He deprecates, and we think justly, the treatment of dysentery by large doses of calomel, on the idea—the illusive idea—of its being a sedative, he believing that in large doses it is commonly the contrary in its effects, and injurious. He is opposed, too, to affecting the system by mercury—the system, when under the influence of mercury, being predisposed to dysenteric attack, particularly in the natives of India. Ipecacuanha he holds in estimation as a dysenteric remedy, and generally applicable either alone or combined with blue pill, or in some cases with opium. With Sir John Pringle, he refers its good effects to its laxative quality. He gives it in doses of from six to three grains, combined with blue pill from five to ten grains, and extract of gentian from four to ten grains, every third, fourth, sixth, or eighth hour, continuing it steadily till amendment takes place. He thinks that the manner in which the combination acts is analogous, but in a less degree, to that of calomel and purgatives—viz., by maintaining a moderate secretion from the liver and small intestine, favouring the return of the deranged circulation of the large intestine to its normal state. Opium he highly approves, believing it, "in certain combinations and doses," applicable to, and useful in, almost every condition of the disease;—for instance, as given with calomel at the commencement—with ipecacuanha, blue pill, and extract of gentian in the more advanced stages—and alone, or in union with tonics and astringents, after the disease has existed for some time, and is only to be recovered from by a process of repair. He considers the doubts that have been raised against it, and is satisfied that they are not well founded, an opinion in which we cordially agree with him. We have found it indeed an heroic medicine in some of the worst cases of disease—those admitted into hospital from the field during a harassing campaign, in the ulcerative stage; thus, given in one-grain doses every hour, the effect was often excellent, and always so when it exercised no hypnotic influence. Even in these doses, and commencing with them, we did not find that it had any constipating, but rather a laxative, tendency.

We must pass over the other medicines mentioned by the author, though not unworthy of remark. His observations on the treatment of the disease in its chronic state, when associated with cachectic states, whether arising from malaria or a scorbutic diathesis, are specially deserving of attention; as are also the rules which he lays down respecting diet and change of climate. The last, change of climate, is indeed indispensable in obstinate cases, such as resist treatment in India and other hot climates. In such cases, a change to a cooler climate has commonly a wonderful effect, and has been the means of saving life to a great amount amongst our troops. We can speak of this effect from our own experience; and what is remarkable, we have seen men who had been treated with large doses of mercury in India, and this without salivation being produced at the time, becoming severely salivated on gaining strength with improving health at home under a tonic plan of treatment, without taking a single additional particle of mercury. The same men, in their passage round the Cape of Good Hope at an unfavourable season, had become the victims of rheumatism with severe periostitis.

Hepatitis, which follows dysentery, has, as might be expected, the author's careful attention; indeed, no part of his work is more elaborated, more in detail, or enriched more by illustrative cases.

The manner in which he connects the pathology of the disease with the physiology of the organ, is ingenious, novel, and instructive, tempting us to give the passage explanatory of it, though long for an extract:

"Which are the capillary vessels of the liver concerned in the morbid action to which we give the name inflammation? The answer is, I apprehend, sufficiently clear. If the pathological doctrines at present received as to inflammation be correct—viz., that it is an altered state of the nutritive processes of the part affected, depending upon something faulty in one or other of the conditions of normal nutrition,—then the capillaries concerned in inflammation must necessarily be only such as circulate, in their normal state, *arterial* blood for purposes of nutrition. The capillaries of the hepatic artery are the nutrient vessels of the solid structures of the liver, and consequently those alone which can be directly engaged in the inflammatory processes of these structures. On the other hand, the portal capillaries circulate venous blood for purposes of secretion, and have no concern, as we believe, with the nutritive processes of the organ; they are therefore not *directly* engaged in inflammation. Now this is not a question of mere curiosity. Firstly, if we regard the small capacity of the hepatic artery capillaries in comparison with those of the portal vein, we have, under the view that the former are those concerned in inflammation, an explanation of the fact that the bulk of the organ is little increased, compared with that to which it attains in congestion—a deranged state in which the capacious portal capillaries are directly implicated. Secondly, this view helps to explain how it is that frequently the secreting function of the liver is not deranged in hepatitis. Thirdly, it tends to remove that difficulty which practical writers on hepatitis have more or less experienced in reconciling the results of clinical observation to therapeutic theory. It has been urged that to give mercury with a view to its cholagogue action in hepatitis, is contrary to that general therapeutic principle which teaches that the special stimulants of secreting organs are contra-indicated in the active inflammation of these organs. But this principle—doubtless true when the secreting capillaries and the inflamed capillaries are the same, and carrying arterial blood—is surely without application in the instance of the liver, if we believe that the secreting capillaries and the inflamed capillaries are altogether distinct. Further, if we hold that the hepatic artery capillaries finally communicate with the portal, then to quicken the portal capillary circulation by increasing secretion from its blood seems, in theory, a good way of lessening the stagnation in the capillaries of the hepatic artery." (Vol. i. p. 598.)

The author distinguishes three stages of hepatic inflammation. The first, that of vascular turgescence, with increased redness and some softening; the second, that of exudation, with effusion of lymph into the parenchyma; the third, that of lymph degeneration ("degeneration into pus"), and the formation of hepatic abscess. A good division, and we think unexceptionable; though we cannot go along with him in his pyogenic view, that the pus of the abscess is a mere degeneration of lymph. Call it puritoid matter, such as we know to be derived from the softening of lymph, and may be obtained by slow coction even out of the body, and the objection ceases.

In considering the etiology of the disease, Dr. Morehead refers chiefly to atmospheric influences, vicissitudes of temperature and high atmospheric temperature. He does not even allude to diet and modes of living, which we are of opinion, as already expressed, are not without influence in the production of liver complaints in India. In confirmation, we may refer to Mr. Macnamara's paper on Fatty Degeneration of the Liver, &c., in Bengal, of which a summary has been given in the July number of this Review; and in further confirmation, we may mention that as far as our experience extends, hepatic abscess is of more frequent occurrence amongst troops east of the Cape, when living well in barracks, leading an indolent life, than when in the field, subjected to greater vicissitudes of temperature, undergoing severe fatigue, many privations, and often restricted to a spare and poor diet. We agree with the author that dysentery is hardly to be viewed as one even of the causes of hepatitis, much less as a principal cause in the way promulgated by a late author; and we can refer to the same experience in confirmation of this also, in so much, that in the field, when troops are actively employed, dysentery is commonly exceedingly prevalent, and abscess of the liver is of rare occurrence. The frequent coexistence of the two under ordinary circumstances—both of them common complaints in India—is no more, perhaps, than might be expected.

In describing the symptoms of hepatitis, the author offers some excellent remarks, very deserving of attention—especially those tending to show that the disease may exist, and occasionally run into suppuration, without any well-marked symptoms—at least, till low hectic sets in—neither pain of side, nor pain in the right shoulder, nor vitiated biliary secretion, nor enlargement, and other physical signs, the more common attendants on hepatitis, being always and necessarily present.

The treatment described is varied according to the stages of the malady, and is founded on much the same rational principles as the treatment recommended in dysentery. Calomel, Dr. Morehead is decidedly of opinion, should be altogether avoided, as soon as there is any suspicion of the formation of an abscess. We must refer to the work itself for the details. The cautions given respecting the management of hepatic abscess are very judicious.

On the other diseases of the liver, of which cirrhosis and jaundice are the most important, the observations of the author are less extended, and offer less of novelty. The same remark applies to the diseases subsequently treated of—of rare occurrence in India, or rarer there than in colder climates—such as peritonitis, ileus, gastritis, and dyspepsia, Bright's disease of the kidney, diabetes, pneumonia, phthisis pulmonalis, organic disease of the heart and aorta, delirium tremens, cerebral disease, tetanus, blood diseases, comprising pyæmia, leprosy, elephantiasis, scurvy, general dropsy; followed and concluding with an appendix containing articles on the meteorology of Bombay, an account of experiments made with calomel on dogs, and a note on the supposed uses of the bile in the function of digestion.

The account of the diseases just enumerated occupies more than two-thirds of the second volume. Under the head of each, information will be found of much value to Indian practitioners, and not without interest to medical inquirers at home, especially those who are engaged in the study of the influence of climate on the constitution of man, and the morbid tendencies which, in connexion with climate, and diverse modes of living, different races exhibit. Having entered so fully in the analysis of the more important diseases treated of by the author, we must pass over those we have just enumerated, referring—and we do so with confidence—such of our readers who would wish for any information respecting them, to the work itself.

We cannot finally lay down the pen without expressing the satisfaction we have derived, and not only from the matter—the contents of these volumes—but also from the style of their composition,—at once clear, simple, and correct. And we have had a like feeling produced by finding throughout their pages a liberal criticism exercised, or an acknowledgment made of the labours of others in the same field, accompanied by generous notices, and we have no doubt just eulogiums, of professional brethren, especially the deceased, who have contributed to the diffusion and advancement of medical science in India. It is pleasing and refreshing to think of the manner in which this science is exercising a beneficial influence in the Eastern world, not limited to

India, but extending even from the Bosphorus to beyond the Ganges, even to "the Celestial Empire;" and whilst in its immediate action serving the cause of humanity in the divine office of relieving human suffering, in its indirect and reflex action promoting the introduction of the exact sciences, and a humanizing and elevating philosophy. Military glory—a bad sign, if history be true—is at present in the ascendant; let us hope, though it be against hope, that better times will come, when glory of a purer kind will be appreciated,—that due to the real benefactors of their fellow men, connected with the peaceful arts, and nowise associated with desolating, impoverishing, and cruel war, and when it shall no longer be said, *Laudatorem iis dignum esse solum modo tempus.*

REVIEW II.

Transactions of the American Medical Association. Vol. VIII. *Philadelphia,* 1855. 8vo, pp. 763.

THE American Medical Association is composed of representatives annually elected by all the Medical Societies, Colleges, and Hospitals in the United States; these representatives, at the expiration of their year of office, becoming permanent members. Its meetings are held annually, at different places. At each annual meeting, individuals and committees are appointed to prepare reports upon scientific subjects specially referred to them, to the best of which prizes are awarded.

The present volume of 'Transactions,' in addition to the minutes of the eighth annual meeting of the Association, the President's address, and list of office-bearers and members, &c., contains ten Reports on different medical subjects, of which the following are the titles:

1. Report on the Diseases of Missouri and Iowa.
2. Report on the Hygrometrical State of the Atmosphere in Various Localities, and its Influence on Health.
3. Deformities after Fractures.
4. Report on the Diet of the Sick.
5. The Pathology, Causes, Symptoms, and Treatment of Scrofula.
6. Report on the Means of Preserving Milk, &c.
7. Report on Dysentery.
8. The Effects of Alcoholic Liquors in Health and Disease.
9. Sketch of the Caustic Pulverizer.
10. Statistics of Placenta Prævia.

Considering the bulk of the volume, and the fact that the American Medical Association is, according to a recent American writer,* the most important of all the medical societies in the United States, we are somewhat disappointed at the small amount of original matter which some of these Reports contain. There is an evident want of original investigation, which is so characteristic at the present time of the Transactions of the leading medical societies in England and on the Continent. To some of the Reports, however, these remarks do not apply; such, for instance,

* Edinburgh Medical Journal, p. 115. August, 1856.

as the first, third, fourth, and tenth. These four papers are of considerable value, and a brief analysis of some of the more important facts contained in them, we propose to lay before our readers.

I. *Report on the Diseases of Missouri and Iowa.*

Missouri and Iowa are two of the most western States in the Union, lying on the right or western bank of the Mississippi, and between 36½° and 43½° north latitude. The principal observations recorded were taken in the town of St. Louis, the capital of Missouri, built on the right bank of the Mississippi, at an elevation of about 400 feet above the level of the Gulf of Mexico. Numerous tables of observations, extending over several years, illustrate its meteorological peculiarities. From these it appears that the annual fall of rain in eighteen years was 42·12 inches, of which 22·86 inches fell in the five months from April to August inclusive. The temperature during the hot months (May to September) might rise to 100° Fahr. in the shade; the mean temperature in July, the hottest month, being 78°; while in December and January, the thermometer often fell to zero, the mean temperature being seldom above freezing point (32°). One of the most remarkable peculiarities of the temperature, was the high range of variation in the course of twenty-four hours, which in the cold months might amount to 40°, and even in the hottest, might reach 25°. The population of St. Louis has increased from 30,000 in 1841, to 127,000 in 1854. The average mortality for eight successive years, before the outbreak of cholera in 1849, and for 1853, in which there was no cholera, was 34·6 per thousand; but during the five cholera years, 1849-50-51-52, and 54, it rose to 59·7, and in 1849 alone was 106·2. The principal diseases noted as producing the mortality, besides the cholera, are diarrhœa, dysentery, intermittent, remittent, typhus, and typhoid fevers, phthisis, and other pulmonary affections. Cholera prevailed principally during the three hottest months, May, June, and July, the largest number of deaths occurring in July. The total number of deaths from cholera, during the five cholera years, amounted to 8380, of which 4317 occurred in 1849 alone. The number of males who died of cholera greatly exceeded the number of females. Of 781 cases in 1851, 488 were males, and 293 females; and of 789 deaths in 1852, 486 were males. Deaths from cholera were also far more numerous in persons above forty-five years of age, than in those under this age; and the "resistance to death" was greatest in subjects from ten to twenty years of age.

The authors of the Report adduce many arguments in support of the contagious nature of cholera. Thus, in 1851, at St. Louis, "as in previous years, the disease first manifested itself among European immigrants, who arrived in the city by New Orleans. Cases of the disease occurred among these immigrants on the steam-boats from that port." (p. 91.) Again, the deaths during the first month all "occurred amongst recently-arrived immigrants." (p. 91.) Similar observations are recorded with regard to the years 1852 (p. 153) and 1854 (p. 225). From St. Louis, cholera was traced into the surrounding districts along the great rivers and most frequented routes. The Report asserts "wherever a much-frequented route is newly opened, thither will the disease march, and thus gain access to detached communities, which, without this means of communication, might have continued exempt from its ravages." (p. 120.) Numerous proofs of this assertion are adduced. Thus, in the State of Missouri, cholera, during the several years, was "in a great measure confined to the towns on the river banks." In 1849, cholera made its first appearance among the Indian tribes on the Upper Missouri, being imported "by the crowd of emigrants in that year, who opened that route to California." (p. 120.) Again, numerous instances are mentioned of provincial towns in Missouri in which the first cases of cholera occurred in the persons of immigrants from infected districts. (p. 123.)

As regards the localization of the disease in St. Louis, it was found to prevail mostly in badly-drained, badly-ventilated, over-crowded localities; and among those persons who were "imprudent in their habits of living."

A remarkable instance is recorded, showing the length of time during which the formites of cholera may remain in a house. From a certain house

"A family removed, to avoid the pestilential neighbourhood, one of the members having died by cholera. The removed members were exempt from the disease so long as they remained away from the deserted house. In some weeks, their alarm having subsided, and the health of the locality having improved, two of the family returned to the house to supervise its being cleaned, previous to its being again occupied. On their return to their temporary residence, at a distance from their former infected dwelling, both of them were attacked with cholera, one on the night succeeding the visit, the other on the third day after." (p. 232.)

The following statements show the number of deaths in proportion to the number of persons attacked. In 1851, out of 169 cases admitted into the St. Louis Charity Hospital, 89 died, or 52·6 per cent.; and of 128 cases admitted into the City Hospital, 79 died, or 61·7 per cent.

We have no information as to any peculiarities in the mode of treatment.

The whole Report confirms the opinion which is now pretty generally acquiesced in by the profession in England and on the Continent, that cholera *may be* propagated by human intercourse, or in other words, is contagious. Whether contagion be the sole or even the principal means of its propagation, remains to be decided, but that cholera is contagious few will now venture to deny. Many observations made in our own country during the recent epidemics; the admirable Reports by Löberg, Kierulf,* and others, of the cholera in Norway; and the very interesting researches of Dr. Alison of Edinburgh, and Dr. Budd of Bristol, recorded in the first volume of the new 'Edinburgh Medical Journal,'† should suffice to convince the most sceptical. It seems astonishing that in India, the birthplace and head-quarters of the disease, the doctrine of contagion is almost universally repudiated by our professional brethren. "All our experience," say the editors of the 'Indian Annals of Medical Science,' "is opposed to the doctrine of contagion;"‡ and this opinion we know to be the one which in India is generally entertained. This difference of opinion on the part of those who have such ample opportunities of observation, we think may admit of explanation in the fact, that in India all the predisposing causes of cholera are in constant

* See British and Foreign Medico-Chirurgical Review, p. 102. July, 1856.
† pp. 481, 668, 1112. ‡ Vol. i. p. 456. 1853.

operation, more especially prolonged heat, decomposing organic matters, a more or less debilitated state of the constitution, and excitable condition of the nervous system, &c.; and hence, no sooner is cholera imported, than it spreads with such rapidity as to resemble an epidemic invasion. Cases of undoubted contagion, however, are not wanting in India. Mr. Barry, a surgeon in the Bengal service, has recorded an outbreak of cholera which occurred at Gowalparah, in Upper Assam, in 1853.* In this instance, the cholera was evidently imported into a healthy station by a body of Sepoys coming from an infected locality; every case of the disease could be traced to communication with the sick, a large number of attendants on the sick were seized, but those who separated themselves escaped in every instance.†

As regards continued fevers, the two forms pointed out by Louis in France, and Dr. Jenner in this country, are also met with in America. Thus we find:

"In general the term *typhoid* is applied to continued fever when complicated with enteritic lesions, the term *typhus* being retained to nominate that form in which cerebral lesions predominate without enteritic lesions, and in which there occasionally occur implications of the pulmonary organs." (p. 106.)

The following important observation is made in reference to the typhoid form:—"Cutaneous eruptions, either of petechiæ, vibices, or 'taches rouges,' are frequently seen in the disease, but are by no means its invariable accompaniment." (p. 110.)

In many of the cases of continued fever, the abortive treatment by quinine was adopted. The result is contained in the following somewhat unsatisfactory paragraph:

"We have seen the fever successfully subjugated in its early stages by quinine; we have, on the other hand, seen the disease evidently exasperated by it. We have known the quinine prove injurious in the early stage of the disorder, and very promptly efficient in the same cases at a later period."

Under the head of periodic fevers, a remarkable instance is mentioned, in which the *partial* drainage of a lake produced such a hotbed of malarious fever as to render the surrounding district, previously comparatively healthy, uninhabitable; but which complete drainage and desiccation restored to its original condition. (p. 209.)

An epidemic of scarlet fever at St. Louis in 1853 is recorded, remarkable for the large number of cases proving fatal in the early stage. Anasarca was observed to supervene most frequently on the decline of the mild cases. Some observations also by a Dr. Engelman are mentioned, with the object of showing that an epidemic of scarlatina may gradually pass into one of measles, and that there is a transition form, partaking somewhat of the characters of both, but which protects the system from a recurrence only of itself, and not of the other two.

* Indian Annals of Medical Science, vol. i. p. 448. † Ibid.

III. *Deformities after Fractures.* By Frank H. Hamilton, M.D., of Buffalo, New York.

The author premises his paper by observing that, although most hospital reports show the result of the treatment of fractures in as far as they prove fatal or are cured, yet there are no tables which indicate the "value of the cure;" or, in other words, the presence or amount of resulting deformity. He goes on to state that deformities after fractures, even in the hands of the most experienced, are far more frequent than is generally supposed, and that a contrary belief has originated in a want of careful examinations and measurements. Notwithstanding the assertions of Dr. Hamilton that his remarks are applicable to the surgery of other countries as well as of America, and although we have no precise statistics to bring forward to prove the contrary, we are hardly prepared to admit, that on this side of the Atlantic, fractures are so frequently followed by deformity as he asserts. We are reminded on this occasion of a remark which much surprised us, and which serves as a sort of corollary to the above. The recent American writer to whom we have already had occasion to allude, states that general practitioners in that country "are frequently subjected to the annoyance of prosecutions for mal-practice, and most of these have been in cases of fracture." These observations are not made with a view to disparaging Dr. Hamilton's paper, which we consider of great interest. The inquiry which he has instituted is well deserving of being followed up by surgeons in this country; for certainly, if his statements are correct, the art of treating fractures has "not attained that degree of perfection which surgeons have almost universally claimed for it."

Detailed statistics are given in the Report as to the result of treatment of fractures of the ossa nasi, septum narium, superior and inferior maxilla, and clavicle, which fractures all surgeons know to be more or less frequently followed by some deformity. No mention, however, is made of fractures of the bones of the extremities, deformities in which are of far more importance than in other parts of the body, but, as we believe, of much less frequent occurrence. It is to be hoped that this defect will be supplied in a subsequent report.

Of fractures of the nasal bones, 22 cases are mentioned.

In 9 there was deformity, but no treatment.

3 died from severity of other injuries.

Of 10 subjected to treatment, in 7 there was permanent deformity, and in 3 only complete restoration.

The author confirms the observations of Malgaigne as to the extreme rapidity of union of these fractures, repair taking place without any provisional callus.

Seven cases of fracture of the septum narium are mentioned, all of which were followed by deformity. A surgeon was consulted in five of the cases, but no treatment was adopted in any.

Of fractures of the superior maxilla, 6 cases are recorded. In 2, death resulted from the severity of the injuries; in all the remaining 4, more or less deformity remained. One of the principal causes of this deformity was a depression of the malar bone, and the author suggests a mode of elevating it by means of a screw levator screwed firmly into the bone, after making an incision through the soft parts. In the absence of an instrument for the purpose, he adds, "a joiner's gimlet might answer tolerably well"!! There are few surgeons, we hope, even in America, who would have recourse to such a procedure.

Of fractures of the inferior maxilla, there are 18 cases:—1 died; in 2, result was not known; in 1, fracture not united after seventy days; in 4, permanent deformity, but slight; in 10, no deformity.

These results appear on the whole successful.

Of fractures of the clavicle, there are 14 cases recorded as incomplete, and 39 as complete. Of the former, in 7 the result was perfect, and in 7 there was deformity; and of the latter, there was deformity in 32, and union without deformity only in 7. The amount of displacement varied from a quarter of an inch to an inch, and the inner fragment was almost always found above and in front of the outer. Numerous suggestions are made as to the treatment of this fracture; and a new contrivance, invented by the author, is described.

IV. *On the Diet of the Sick.* By Charles Hooker, M.D., of New Haven, Connecticut.

At the commencement of the Report, several rules are laid down for the diet of the healthy. Various diseased conditions are mentioned as resulting from a too fluid diet, and among others, upwards of 30 cases of purpura hæmorrhagica, "which have been connected with the habitual excessive use of drinks." Some interesting and important observations are made on the relations between scrofula and an oleaginous diet. On this subject, the author's observations for many years have led him to the following conclusions:

"1. Of all persons between the ages of fifteen and twenty-two years, more than one-fifth eat no fat meat.

"2. Of persons at the age of forty-five, all, excepting less than one in fifty, habitually use fat meat.

"3. Of persons who, between the ages of fifteen and twenty-two, avoid fat meat, a few acquire an appetite for it, and live to a good old age, while the great proportion die of phthisis before forty-five.

"4. Of persons dying of phthisis between the ages of fifteen and forty-five, nine-tenths at least have never used fat meat."

These observations confirm the views which are maintained by Dr. Bennett and others, that in phthisis there is a deficiency of the oily ingredients of the tissues, and that hence the most rational treatment consists in the administration of oleaginous ingesta, in such forms as to be most easily assimilated. Dr. Hooker also confirms the observation which has been made in this country, that patients "who have never used fat meat —to whom, indeed, it is absolutely disgusting—will readily take cod-liver oil;" but adds: "the few patients who have phthisis after a habitual use of fat meats, are little, if any, benefited by cod-liver oil."

Hence, in estimating the probable good effects of a cod-liver oil treatment, it would seem advisable to take into consideration the previous habits of the patient, as regards the use of oleaginous food. Several cases are detailed of advanced phthisis, which were cured.

The author insists strongly upon the injurious effects of tobacco in producing dyspepsia, maintaining that it acts not only as a narcotic, but also by increasing and wasting the saliva. This statement we readily corroborate. We have ourselves seen several cases of aggravated dyspepsia attributable solely to the excessive use of this weed, and remediable by a removal of the cause.

In the treatment of typhus, Dr. Hooker enforces the injunctions of the late Dr. Graves, of Dublin, to guard against the patient dying of starvation. He recommends that small quantities of solid food, such as panada, dry toast, or even a little meat, along with small doses of quinine, should be repeatedly given. Under this treatment, he lost only 8 out of 195 patients; and adds that, at the Connecticut Hospital, with this mode of dieting, the mortality from typhus is only four per cent. There can be little doubt of the propriety of guarding against starvation in typhus, but the cases alluded to must have been of a mild type. At the London Fever Hospital, where small quantities of nutriment (such as beef-tea, milk, &c.) are repeatedly given, the mortality during the two years 1854 and 1855 was almost twenty-two per cent.

X. *Statistics of Placenta Prævia.* By J. D. Trask, M.D., of New York.

This paper is the most important of the series. To it was awarded the prize of the Association. The author has collected in a tabular form, with great labour and care, 353 cases of placenta prævia, his object being to show the result of the treatment of this complication of labour by the plan recommended by Dr. Simpson, of Edinburgh—viz., the complete separation and extraction of the placenta, as compared with that from the more ordinary treatment by turning.

Most of our readers are no doubt familiar with Dr. Simpson's paper as first published in the 'Edinburgh Monthly Journal' for March, 1845, and reprinted in the first volume of his 'Obstetric Memoirs.' In this paper, Dr. Simpson has shown that, in placenta prævia, the mortality to the mother under all the previous modes of treatment, was 180 out of 654 cases, or 1 in $3\frac{6}{10}$ths; and from turning alone, 1 in $2\frac{9}{10}$ths; whereas out of 141 cases in which the placenta became spontaneously detached, only 3 deaths occurred, or 1 in 47, which could in any way be attributed to the complication in question. He has therefore recommended that, in certain cases of placenta prævia, the placenta should be artificially detached.

Dr. Trask's statistics are, on the whole, confirmatory of Dr. Simpson's; but he points out, and we think with justice, that it is hardly fair to estimate, like Dr. Simpson, the results of *artificial* separation as the same as those of *spontaneous*. The cases which he has collected show a great difference between the two.

Dr. Trask's cases are classified in three tables. The first table includes 251 cases, in 200 of which turning was adopted. Of these 200, 59 died, or 1 in $3\frac{4}{10}$ths.

The second table embraces 36 cases in which the placenta was spontaneously separated and expelled. In 29 the result is mentioned, and out of these there were two deaths, both of which were caused by diarrhœa (one eight, and the other twelve days after delivery), so that here, not a single death could be referred to the placenta prævia.

In the last table are given 66 cases, in which the placenta was artificially separated and extracted. Out of 60 of these cases there were 13 deaths, or 1 in $4\frac{6}{10}$ths. The author, however, adds, that the cases in the last table embrace "a considerably larger proportion of severe cases than are ordinarily met with."

The mortality to the child after artificial separation, as ascertained by Dr. Trask, corresponds with that given by Dr. Simpson. Thus, in Dr. Trask's cases, it was 1 in $1\frac{15}{32}$nds; and in Dr. Simpson's, 1 in $1\frac{35}{78}$ths.

The author concludes by recommending the treatment by artificial separation in similar cases to those in which it was originally proposed by Dr. Simpson.

REVIEW III.

1. *Leçons de Physiologie Expérimentale appliquée à la Médecine, faites au Collège de France.* Cours du Semestre d'hiver 1854–55. Par M. CLAUDE BERNARD.—*Paris*, 1855.

Lectures on Experimental Physiology applied to Medicine, delivered at the Collège de France. Winter Session 1854-55. By M. CLAUDE BERNARD.

2. *Sur le Méchanisme de la Formation de Sucre dans le Foie.* Par M. CLAUDE BERNARD. (In 'Comptes Rendus de l'Acad. des Sciences,' tom. xli. pp. 461–469, No. 13, Sept. 24, 1855; and in 'Annales des Sciences Naturelles,' Quatrième sér., tom. iv. pp. 109–119.)—*Paris*, 1855.

On the Mechanism of the Formation of Sugar in the Liver. By M. CLAUDE BERNARD.

3. *Saccharine Matter, its Physiological Relations in the Animal Economy.* By FREDERICK WILLIAM PAVY, M.B. (In 'Guy's Hospital Reports.' Second Series. Vol. viii. pp. 319–344.)—*London*, 1853.

4. *Researches on the Nature of the Normal Destruction of Sugar in the Animal System.* By FREDERICK WILLIAM PAVY, M D. Lond. (In 'Guy's Hospital Reports.' Third Series. Vol. i. pp. 19–37.)—*London*, 1855; and, in a condensed form, in 'The Proceedings of the Royal Society of London,' for May 3rd, 1855. Vol. vii. pp. 371–376.

5. *Sugar in the Animal Economy in Health and Disease.* The Harveian Society's Prize Essay for the year 1856. By JAMES L. BRYDEN, M.D. Edinb., H.E.I.C.S. Not yet published.

ABOUT two years ago, we devoted a short article to Claude Bernard's contributions to physiology. The most important of his discoveries is unquestionably that of the formation of sugar in the liver, and the 'Physiological Lectures,' which we now propose to notice, are for the most part devoted to this subject, and to its bearings on the physiology of diabetes. They are twenty-five in number, and were delivered in the Collège de France, in the winter of 1854–55. The phrase "experimental medicine," which was adopted many years ago by Magendie (Bernard's predecessor in the chair), expresses, perhaps, better than any other, the nature of the course, which is usually devoted to some one or more subjects which, in the opinion of the Professor, require special experimental elucidation.

Until very recently, it was regarded as an established fact, that the vegetable kingdom alone had the power of forming sugar, and that any sugar found in the blood, urine, &c., of animals, must have had its origin in the amylaceous or saccharine portion of the food. Experiments of unquestionable accuracy have, however, demonstrated that the animal organism has also the power of forming sugar, altogether irrespectively of the nature of the food; that sugar exists normally in the blood, in a certain part of the circulation—namely, from the hepatic veins to the pulmonary capillaries, in both carnivorous and herbivorous animals; and that the quantities of sugar which we find in these two great classes of animals, do not present any sensible differences.

"In man, and in all animals, there is a sugar-producing organ, and this organ is the liver; and as all secreting organs are impregnated with the product of their secretion, as the kidney is impregnated with urine, the testicle with spermatic fluid, the pancreas with the pancreatic juice, and the salivary glands with their different varieties of saliva, so is the liver impregnated with sugar; and it is the only organ of the body which in the normal state presents this peculiarity. To convince ourselves of this, we have only to take the tissue of any freshly-killed animal, to pound it and boil it with a little water, and to search for sugar by the ordinary means in the (filtered) liquid decoction."*

In performing this experiment, it is necessary to make the filtered fluid pass through animal charcoal, in order to decolorise it, and then again to filter it, before we apply the ordinary tests; but this being done, we obtain ready evidence of the presence of glucose or grape-sugar by Trommer's test (the reduction of oxide of copper), by boiling with liquor potassæ, and by fermentation.

The presence of sugar in the liver, and in no other organ of the body, is a fact that has been established by Bernard, by observations on a large number of animals in almost every department of the zoological scale of beings.

In order that the experiments made on man should correspond with those instituted on animals, Bernard was obliged to confine his observations to cases of sudden death in healthy persons. He examined the livers of five executed criminals, of a man who was killed instantaneously by a gun-shot wound, and of a diabetic patient who died suddenly from pulmonary apoplexy.

The following are his results, arranged in a tabular form:

	Age.	Weight of liver in grammes.	Sugar in 100 parts of liver.	Ditto in the whole liver.
Criminal (A)	45	1300	1·79	23·27 grammes.
Criminal (B)	45	1330	Sugar present: quantity not determined.	Not determined.
Criminal (F)	—	1175	Ditto.	Alcohol obtained by fermentation.
Criminal (V)	22	1200	2·142	25·704
Criminal (C)	—	1175	Alcohol obtained by fermentation.	
Gun-shot case ...	30	1575	1·10	17·10
Diabetic case	—	2500	2·30	57·50

In the first three cases, the person had taken no food since the preceding evening; in the others, digestion was going on. It should further

* Bernard. Leçons, &c., p. 51.

be mentioned, that in the gun-shot case, the liver was not examined for two days after death, and decomposition, which destroys the sugar, had commenced.

With immense labour and unwearying powers of work, he has obtained evidence of the presence of sugar in the liver, throughout almost the whole animal scale. He has found it in mammals (of which at least seventeen different kinds were examined, besides man), in birds (sixteen different kinds), in reptiles (ten different kinds), in osseous fishes (twelve different kinds), in cartilaginous fishes (three different kinds), in molluscs (eight different kinds), and in articulate animals. The relative quantity of sugar in the liver varies little when the system is in a normal condition; it very seldom exceeds 4 per cent., the mean being from 1·5 to 2 per cent. in mammals and birds; while in reptiles, fishes, and molluscs it is somewhat less. It is unnecessary to notice the experiments by which he distinctly proves that the sugar which exists in the liver is identical with that which occurs in diabetic urine; and we pass on to his demonstration that this sugar is secreted in the liver, and that it is in no way connected with the nature of the food.

"The most simple proof seems to be afforded by withholding all amylaceous and saccharine food from an animal, and observing if sugar still continue to exist in the system. This experiment has been made upon a great number of animals (dogs), which we have fed exclusively on flesh for six and even for eight months. When, at the end of that period, the animals have been killed, we have found 1·9 per cent. of sugar in the liver, which is as much as occurs in dogs that have been kept on a mixed diet.

"Birds of prey, owlets, taken in their nests, and fed exclusively on raw bullock's heart for three months, were then killed; their livers always contained sugar in the normal quantity (1·5 per cent.), while the other tissues presented no trace of this substance."*

The above experiments suffice to prove the persistence of sugar when no amylaceous or saccharine matters can by any possibility be introduced into the system; and the case of the young owls completely overthrows the view that has been maintained against Bernard—namely, that the sugar may have been localized and hoarded up in the liver from saccharine food taken during *une alimentation antérieure.* But the main demonstration is based on the relative analyses of the blood of the portal vein as it enters, and the blood of the hepatic veins as they emerge from the liver. A dog, after fasting thirty-six hours, was fed freely with boiled sheep's head, and three hours afterwards, when digestion was in active progress, was instantaneously killed (by division of the medulla oblongata) in the presence of the class. The blood collected from the portal vein, before its entrance into the liver, gave no trace of sugar; while, on the other hand, the blood of the hepatic veins contained a considerable quantity of sugar, as was proved both by Trommer's and the fermentation test. This experiment has been fully confirmed by the comparative analyses of these two kinds of blood, instituted by Professor Lehmann with special reference to the sugar question, and laid before the Académie des Sciences a few weeks after the delivery of this lecture. Lehmann found that the portal blood never contained the least traces of sugar,

* Leçons, p. 69.

either in dogs when fasting, or in dogs living on flesh; but when they were fed on boiled potatoes, the portal blood did contain sugar in such small quantity that its amount could not be determined. Minute quantities were also found in the portal blood of two horses. The blood of the hepatic veins, on the other hand, always contained sugar *en forte proportion*, as is shown in the following tabular view of his collective observations:

Animal.	Food.	Quantity of sugar in the solid residue — Of portal blood.	Quantity of sugar in the solid residue — Of hepatic blood.
Dog	... fasting for two days	none ...	... 0·746 per cent.
Dog	... ditto	ditto ...	... 0·638 „
Dog	... ditto	ditto ...	... 0·804 „
Dog	... flesh	ditto ...	... 0·814 „
Dog	... ditto	ditto ...	... 0·709 „
Dog	... ditto	ditto ...	... 0·946 „
Dog	... boiled potatoes	traces ...	... 0·981 „
Dog	... ditto	ditto ...	... 0·854 „
Horse	... bran, hay, and chopped straw ...	0·055 per cent.	... 0·635 „
Horse	... ditto	0·052 „	... 0·983 „

Were it necessary, we might quote memoirs by Leconte, Moleschott, and others, affording further demonstration of this fact; but Lehmann's analyses appear so decisive, that we shall proceed without delay to the further consideration of Bernard's lectures. We have already alluded to the view held by some of his opponents—that the sugar is merely localized in the liver, just as mercury, copper, and arsenic are found in that organ long after the administration of the salts of those metals. He now proceeds to demolish this objection by the following experimental proof. It has been already shown that young birds of prey fed exclusively on flesh from the period of their hatching, contain from 1·0 to 1·5 per cent. of sugar in the liver; and the presence of this constituent in the liver of the unhatched chick is very readily demonstrated. If we perform similar experiments on the mammalian fœtus, we arrive at the singular conclusion, that the "glycogenic function only commences at a special period of intra-uterine life, and that the saccharine matter augments in proportion as the animal approaches the time of birth." Bernard exhibited to his class a fœtal calf at four or five months, in which the liver presented sugar, while in the liver of a corresponding fœtus at about two months no sugar could be detected; and he has made numerous other experiments, with similar results, not only on fœtal calves of various ages, but on the human fœtus, and that of the rabbit, goat, sheep, and guinea-pig.

He gives the following results regarding the per-centage of sugar in the liver of the fœtus of various animals:

Human fœtus at six months and a half ...	0·77 per cent.
Fœtus of calf at from seven to eight months	0·80 „
Fœtus of the cat at the full time	1·27 „

But there is other and even stronger evidence that the sugar is not localized, but produced in the liver. Far from remaining and being hoarded up in that gland, the sugar is undergoing perpetual destruction and renovation, and we can induce its disappearance (by preventing its re-formation), if we cause an animal to die slowly, as, for instance, by dividing its pneumogastric nerves. Similarly, when the function of the liver is disturbed by severe, and especially by acute, diseases, the formation of sugar is often arrested, and none is found in the liver after death; when, however, death supervenes rapidly—that is to say, when the nutritive faculties have not been suspended for any length of time—sugar may usually be found.

Lastly, not only is the hepatic sugar not dependent on *une alimentation antérieure*, but its amount is in no way connected with the nature of the animal's diet. Two dogs were fed solely on flesh, three on bread and meat, and two on amylaceous or saccharine food, and they were all killed at as nearly as possible the same period of digestion; and the results of the chemical examination of their livers fully bore out this conclusion.

We now come to the question of how the sugar is formed in the liver.

"We have to consider a gland which gives origin to two products—to the sugar which enters the blood, and to the bile which is given off outwardly. What relation exists between these two concomitant phenomena? or are they independent of one another? Can we suppose, for example, that the albuminous matters of the blood on coming in contact with the hepatic cells, break up into two products—a hydrocarbon, which is to form sugar, and a nitrogenous product for the formation of bile? If this were the case, these two products would be formed simultaneously; but the experiments which have been made seem to indicate that the sugar is not formed at the same moment as the bile, and that there is a sort of alternation between these two formations, one of them appearing to be arrested when the other attains its greatest intensity."*

Comparative anatomy confirms the view that these two secretions are independent of one another. Numerous experiments have been made by Bernard on the common grey slug (*Limax flava*), whose liver always contains sugar, and which lives almost exclusively on wood-lice and grubs, and is therefore an animal-feeder. In these animals the order of succession of the digestive phenomena has been carefully watched. After these slugs have been fasting for some time, their stomach and intestines are found to contain a little bile, but no trace of saccharine matter. Shortly after taking food there is a secretion of acid gastric juice, but this mixture contains no trace of sugar. When, however, the dissolved food has passed almost entirely from the stomach into the intestine, a colourless saccharine fluid enters the stomach by the ductus choledochus, which opens near the pyloric extremity. As the intestinal absorption proceeds, the secretion of this saccharine fluid in the liver becomes more abundant, and at length not only fills the stomach, but also the ductus choledochus; and by its backward pressure causes a very distinct and remarkable dilatation of the liver. This general distension of these organs soon diminishes, in consequence of the absorption of the fluid, which seems to be effected solely by the walls of the stomach, scarcely any of it passing into the intestine; and when this secretion has almost disappeared, the ductus choledochus begins to pour forth a fluid which gradually becomes less saccharine and more coloured, till at length, towards the end of the digestive process, pure bile, altogether devoid of

* Leçons, &c., p. 90.

sugar, is effused, such as is found in the stomach of the slug after fasting; and the turgescence of the liver then disappears. Here, then, we have ocular demonstration of the independence of these two functions of the liver. Bernard believes that in the articulata, and especially in insects, he has made out the anatomical distinction between that portion of the liver which is to produce bile, and that which is to secrete sugar; but we regard this evidence to be less trustworthy than that yielded by the slugs.

From these observations, Bernard attempts to give a hypothetical explanation of the minute structure of the liver in the vertebrated animals; his view, that there are distinct cells for the secretion of each product, does not, however, appear to be satisfactorily established. Like the other secretions, that of sugar is constantly oscillating between certain limits. Before proceeding to show the causes of these oscillations, he demonstrates the absence of sugar in the different secretions and excretions—in the saliva, the urine, and the bile; and hence he infers that its retention within the organism indicates that it must serve some special purpose; and as, further, it does not exist in the blood of different vessels in anything like the same proportions, it must obviously be in great part destroyed. As it is being constantly formed in the liver, and yet never exceeds a certain limit, at least in the physiological state, it is obvious that there must be a perfect equilibrium between its formation and destruction.

We shall trace the course taken by the sugar from its starting-point—the liver—onwards:—

"Secreted by the hepatic cells, the sugar passes with the blood of the capillaries into the hepatic veins, and from thence into the vena cava ascendens. It is at the point of discharge of the last-named vessel, that the blood is the most strongly saccharine; it then becomes mixed with the blood from the lower parts of the body, and passes up to the right auricle, where the sugar undergoes a new dilution from its admixture with the blood of the vena cava descendens. From the right auricle it passes into the right ventricle, and thence to the lung. In the whole of the route from the liver to the lung, the blood is constantly saccharine, but the amount of sugar varies extremely, and is least at the greatest distance from the liver. In the lung, the sugar, being brought into contact with the air and mixing with the whole mass of the blood, sometimes completely disappears.

"These two organs, then—the liver and the lung—stand in an inverse relation to one another, in so far as the saccharine matter is concerned. In a fasting animal, for example, the blood which arrives at the liver contains no trace of sugar, while that which leaves it is distinctly saccharine. Inversely, the blood which arrives at the lung contains sugar, while that which leaves it contains no traces of this constituent. The sugar, in this physiological state, remains hidden between the liver and the lung, and this is the reason why its existence and formation within the animal body were not earlier discovered. The analysis of blood drawn from superficial veins would fail to detect it under these conditions."*

There are, however, physiological conditions under which sugar may be found in the blood beyond the lungs. During digestion, the liver, in place of merely receiving the returned blood of the mesenteric vessels, &c., additionally receives the whole of the soluble matters absorbed by the capillaries of the portal vein—a quantity twice or thrice as great in some

* Leçons, &c., pp. 105–6.

cases as when the animal is fasting. This organ consequently becomes engorged with blood, and considerably enlarged. The usually slow circulation now becomes singularly active, and the wave of blood which thus enters it probably displaces most of the sugar which had been previously formed, and projects it into the general circulation. Independently of the augmented activity due to the afflux of blood, the liver is also stimulated by the nervous system under the influence of the natural excitation induced by the digestion of food. For these reasons, the activity of the glycogenic function increases with the augmented flow of blood to the liver, and in the course of four or five hours after the commencement of intestinal digestion, the production of sugar in the liver attains its maximum of intensity. Hence—although, as has been previously shown, the nature of the food exerts no influence on the production of this sugar — the period of digestion exercises a very evident influence. Some time, then, after the ingestion of food, and during three or four hours, the production exceeds the destruction of sugar, and there is a temporary excess of this substance in the organism, and that portion which escapes being consumed in the lungs, passes onwards into the arterial system. "At this period of digestion, we find sugar in all the vessels of the body, both arteries and veins; we even find it in the renal arteries, but in too small quantity to pass into the urine." In about six or seven hours after the meal, the excess of sugar in the blood begins to disappear, and the equilibrium between its production and its destruction begins to be restored.

There is, however, one liquid in the animal economy into which, according to Bernard, sugar always passes, even when it only reaches the general circulation in very small quantity,—namely, the cerebro-spinal fluid. He has constantly detected sugar in it (we presume by the reduction of copper only, as he makes no reference to his tests in this instance) in dogs, cats, and rabbits, both when fasting and during digestion—a fact which accords with the observation made several years ago by Magendie, that this fluid is one into which substances introduced into the blood pass with the greatest facility. If, however, food be withheld from an animal beyond a given time, the sugar can no longer be detected. In reference to this subject we ought to observe that M. Bussy,* who has carefully examined the cerebro-spinal fluid which escaped from a man with a fracture at the base of the cranium, and likewise this fluid in the horse and the dog, found that although it reduced the oxide of copper, it could not be made to undergo fermentation; and as other organic substances (leucine and allantoine, for example) possess this reducing power, the reduction-test alone cannot be relied on as affording certain evidence of the presence of sugar. Messrs. Paget and Turner† have recently attempted to determine whether sugar was actually present in these cases, and the latter gentleman examined three separate portions of the cerebro-spinal fluid, obtained by puncturing a spina bifida in a child, several days intervening between the removal of each portion. The three specimens corresponded in giving no indication of grape-sugar,

* Bulletin de l'Acad. de Médecine. Dec. 1852.

† Proceedings of the Royal Society of London, June 15th, 1854, vol. vii. p. 89.

except with Trommer's test—Moore's, Maumené's, and the fermentation-test yielding negative results.

Similar oscillations of the "glycogenic function" to those which we have described, occur in an exaggerated form in diabetic patients; and the preceding observations elucidate the cases of intermittent diabetes described by Rayer, Traube, and others, in which the urine of digestion is saccharine, while no sugar can be detected in the urine at other periods.

The circumstances modifying the secretion of sugar are next considered.

It would be interesting to ascertain, if it were possible, the effect of various changes in the hepatic tissue on the secretion of sugar. In fatty liver, induced artificially in ducks, Bernard found, to his surprise, that the quantity of sugar was increased rather than diminished. Most local alterations of the liver—such as cysts, hydatids, tumours, &c.—appear only to have the effect of diminishing the secreting mass of the liver, for in the immediate neighbourhood of these lesions we find sugar in the ordinary proportions.

Before noticing the influence of different kinds of food upon the production of hepatic sugar, he investigates the effect of entire abstinence from food of any kind on the glycogenic function. Four dogs of the same age, and as nearly as possible the same weight, were selected for this experiment.

"During the first days of abstinence the secretion of sugar goes on to a considerable extent; for in a dog that had fasted thirty-six hours I found 1·255 parts of sugar in 100 of liver; and in another dog that fasted four days, there was 0·93 of sugar in 100 parts. On the following days, the quantity of sugar that is formed diminishes more rapidly till the animal has lost four-tenths of its weight, and is past recovery. I have never found sugar in the tissue of the liver of dogs, rabbits, or guinea-pigs, that died from starvation. . . . . The time necessary for the total stoppage of the production of sugar in the liver under the influence of abstinence, varies with the age and size of the animals, with their class, species, and power of resisting inanition. Amongst the vertebrata, birds most rapidly lose the power of forming sugar in the liver. Thus, in from thirty-six to forty-eight hours sugar ceases to be found in the livers of small birds, such as sparrows. Next to birds come mammals, especially young ones. I have experimented in reference to this point on rats, dogs, cats, and horses. In the rats and in rabbits, from four to eight days sufficed; and in dogs, cats, and horses, from twelve to twenty days sufficed to cause the complete disappearance of sugar from the liver. . . . . Reptiles and fishes differ from warm-blooded animals in resisting for a much longer period the effects of abstinence, and in the slower disappearance of sugar from the liver. Thus, toads, adders, and carp exhibit very evident indications of sugar in the liver after four or six weeks' abstinence. . . . . As the sugar disappears, the respiration, which is intimately connected with its destruction, becomes slower."*

These observations on the effects of abstinence do not, however, apply to hybernating animals during the period of their winter sleep, as has been distinctly shown by Valentin, in his memoir "On the Existence of Sugar in the Liver and other parts of Hybernating Animals," published in volume xiii. of this Review.

The influence of a fatty diet is remarkable. Two dogs were fed on fat

* Leçons, &c., pp. 129–131.

bacon from which all the lean had been removed, and on hog's lard, for three and for eight days respectively, and the singular result was obtained, that under the influence of this food there was a positive diminution of the sugar in the liver, such as would have occurred if the animals had been rigidly fasting, the sugar in these cases amounting to 0·88 and 0·57 per cent. Bernard explains this result in the following manner: the fundamental principles of all foods are reducible to three classes—the albuminous, the farinaceous or saccharine, and the fatty kinds of food; of these, the last alone do not pass through the liver, but are absorbed directly by the lacteals, and hence they do not affect the portal blood.

In order to investigate the effect of a nitrogenous diet, two dogs were fed solely on gelatine and gelatinous matters for some days. Under the influence of this food the quantities of sugar found in the liver were as nearly as possible normal (being 1·33 and 1·65 per cent.), although one of the dogs had been kept without any kind of nourishment for four days before the experiment commenced. "Hence," says Bernard, "it is the nitrogenous element which serves to form the sugar, and chemistry confirms that which physiology indicates; for Lehmann has proved that the portal blood in traversing the liver loses a certain quantity of its nitrogenous principles, and that the fibrin is distinctly diminished." As in the case of animals fed solely on nitrogenous food, neither the intestine nor the portal blood contains sugar, we can arrive at no other conclusion than that the sugar which is found under these conditions is the result of the action of the liver on the albuminous principles which have entered the portal blood.

The effect of a farinaceous diet was then observed upon two dogs—a subject of special interest, "in consequence of the care taken by all physicians to exclude every trace of starch and sugar from the diet of their diabetic patients." The first dog, after being starved for four days, was fed for six days on starch and water; the second dog, without any preliminary starving, was fed for three days upon a mixture of mashed potatoes, starch, and sugar, with a little water. In the liver of the first dog there was 1·25 per cent., and in that of the second 1·88 per cent. of sugar —numbers which do not materially differ from those which are yielded during a gelatinous or mixed diet. In a physiological state, then, the ingestion of amylaceous or saccharine matter does not augment the quantity of sugar in the liver, and consequently in the animal economy generally; although in cases of diabetes the use of these substances commonly causes a great and immediate augmentation of the sugar in the urine.

The influences of various diseases, of temperature, age, &c., on the glycogenic function of the liver, are considered in a subsequent lecture. It appears, from Bernard's researches, that severe diseases, whether acute or chronic, but especially if acute febrile symptoms are present, very rapidly put a stop to the production of sugar in the liver; and this is the reason why sugar is so often sought for fruitlessly in the livers of hospital patients. When a diabetic patient is seized with another disease, the same thing holds good. The sugar is no longer secreted, or, at all events, the secretion is much diminished, and the urine is no longer saccharine. When, however, the secondary disease abates, the sugar re-appears, Bernard notices a singular case of this kind that fell under his own observation. A diabetic woman had a chronic affection of the bowels, which occasionally assumed an acute form, when she suffered from colic and diarrhœa; whenever the last-named symptoms appeared, the urine ceased for the time to be saccharine. In this patient he saw the sugar vanish and re-appear five or six times under these circumstances. In the last stage of diabetes, when phthisical symptoms have supervened, and the digestive functions are much disturbed, the sugar often disappears from the urine; and this may be taken as a sign that death will speedily occur.

The influence of external temperature on the hepatic functions was determined experimentally on guinea-pigs and rabbits. On exposing these animals to great cold, so as to reduce the temperature from the normal standard of about 100° to 68° Fahr., the hepatic sugar entirely disappears in the course of two hours; while on placing them in a surrounding medium of a temperature rather above the bodily heat—as, for instance, in a stove at 113° Fahr.—there is an exaltation of the functions of the liver, which, however, is more marked in relation to the secretion of bile than to that of sugar. But if the temperature he raised to 120° or 130°, an opposite effect is produced: the sugar disappears, and the animal dies in from one to two hours, without a trace of saccharine matter in its liver.

Neither age, sex, pregnancy, nor lactation, seems to exert any special influence on the formation of sugar in the liver.

We now proceed to consider the uses and final destiny of the hepatic sugar in the organism.

As farinaceous and saccharine food does not increase the amount of the sugar that is found in the tissue of the liver, it is in the highest degree probable that the sugar derived from the food plays an entirely different part in the animal economy from the hepatic sugar. Bernard finds that, if we take two dogs, and feed one exclusively on flesh and the other exclusively on amylaceous matters for some days, and then kill them, the watery decoction of the liver will be perfectly limpid in the first case, while in the second it will be "turbid, opalescent, and of a milky appearance." The two fluids will be found equally to abound in sugar, but the latter also holds in suspension an emulsive matter—a mixture apparently of a fatty and of a protein body; and from this he infers that the sugar yielded by the food does not pass in the form of sugar into the blood, but that it is converted by the liver into fat. We have not space to notice the various arguments or the experiments by which he supports this view; and we will merely remark that Lehmann, who has published a very elaborate criticism of Bernard's Leçons, in vol. lxxxviii. of Schmidt's 'Jahrbücher,' regards the arguments as inconclusive, and the experiments (which are in direct opposition to those of von Becker, who worked under Lehmann's direct superintendence) as fallacious. The question as to what becomes of the alimentary sugar is still involved in considerable obscurity. We turned to Dr. Bryden's Harveian Prize Essay, to see if he had thrown any new light upon it, but we do not find that he has made any experiments on the subject; and his conclusion, "that it is as lactic acid, or more probably as lactates—the base being derived from the biliary and other secretions—that the variety of sugar which we have been considering enters the circulation," is, we think, by no means satisfactorily established. It is, indeed, in a great measure based on an isolated observation in page 278 of the third volume of Lehmann's 'Physiological Chemistry,' which can hardly be taken as a fair expression of that eminent chemist's views on the subject; for in his latest work we find the following sentence, which tells most decisively against Dr. Bryden's theory:—"From the augmentation in the amount of sugar in the blood after the ingestion of sugar, it follows that the greatest part of the glucose is absorbed in an unchanged state; a fraction of the sugar is, however, always converted into acids."* Nor does Dr. Pavy at all remove our difficulties on this point: he merely tells us that, from experiments performed in M. Bernard's laboratory, it appears that the liver—

"Exerts some modifying influence on the sugar which is traversing its capillaries, and which has been absorbed from the food, by which it is transformed from vegetable into animal sugar or glucose, and thus rendered more apt for being subsequently destroyed by the processes of animal life."†

We must confess that we do not clearly comprehend the differences between animal and vegetable sugar to which Dr. Pavy refers; and we have chiefly noticed these discrepant opinions with the view of directing further inquiry to the subject.

We now proceed to give a sketch of Bernard's views regarding the destruction of the hepatic sugar in the blood. The theory of the oxidation or combustion of the sugar in the lungs is first discussed. If the sugar were actually destroyed in the lungs by coming in contact with the oxygen of the air, any cause that disturbed the due performance of the respiratory functions—as a more or less perfect occlusion of the air-passages, or the inhalation of air mixed with certain vapours, as those of ether or chloroform, or the respiration of air poor in oxygen—would prevent the destruction of the sugar, and allow it to pass into the general circulation, and consequently into the urine. M. Reynoso found that, under the influence of ether, the urine became temporarily saccharine, and at once referred the result to the deficient oxidation of the sugar in the lungs. The fact is correct enough, but Bernard shows, by the following singular experiment, that it admits of an altogether different explanation: If a dog or a rabbit be taken for experiment just after the digestion of a meal has been fully accomplished, and if we draw blood from its jugular vein, this blood will contain no appreciable traces of sugar; if, however, we compress its abdomen so as to exercise a certain pressure on the liver, or if we excite violent contractions of the abdominal muscles and of the diaphragm by carefully closing the nostrils for some minutes, and then draw blood from the jugular vein, we shall find that the fluid is saccharine. The compression of the liver causes a sudden and excessive effusion of sugar into the blood, and the passage of a portion of it into the general circulation and the urine. Bernard believes that, in Reynoso's experi-

* Handbuch der Physiologischen Chemie, p. 265. Leipzig, 1854.

† Guy's Hospital Reports, third series, vol. i. p. 20. As in one or two cases Dr. Pavy makes statements regarding Bernard's views that are not strictly in accordance with those expressed in the Leçons, we think it right to mention that the article from which we quote was written previously to the publication of Bernard's volume.

ments, the presence of the sugar was due to the muscular efforts and contractions of the animal, although ether and chloroform exert a special action in this respect, to which we shall presently allude. At first he seems to have inclined strongly towards the oxidation theory, and this view was confirmed by his finding a saccharine fluid in the urinary bladder of the fœtus of the calf, at the fourth or fifth month; but on examining the urine at different stages of fœtal existence, he discovered other facts that could not be explained by this theory. He found that in the calf the glycogenic function of the liver does not commence before the fourth month of fœtal life; but, contrary to his expectation, he also found the urine of the fœtus, at a very early stage, to be highly saccharine when the liver does not yield a trace of sugar. On the other hand, the tissue of the liver contains an increasing quantity in proportion as the period of gestation approaches, and hence we might naturally infer that the urine would become more saccharine as the period of birth drew near. But this is not the case: the urine of the fœtal calf, at the sixth or seventh month, ceases to contain sugar,* although it is then secreted in the organism, and is found in large quantity in the liver. Bernard next examined the direct influence of oxygen on the destruction of the sugar in blood obtained from the hepatic veins, but to his surprise he found that the sugar was not decomposed, even after five or six hours, in blood saturated with oxygen; while it was decomposed with considerable rapidity by nitrogen and hydrogen, and in still shorter time by arseniuretted hydrogen. These experiments with the gases, however, are in reality of less weight than Bernard supposes, because, as Lehmann has observed, many of them have a tendency to promote the decomposition of the blood.

The next theory, or rather hypothesis, is, that the destruction of the sugar in the organism is due to the combustion of the sugar with the co-operation of an alkali. Bernard makes various experiments, similar to those of Lehmann and v. Becker,† and arrives at the same conclusion—namely, that sugar, when injected into the jugular vein, in association with potash or its carbonate, is not destroyed in greater quantity than under ordinary circumstances.‡

There are only two ways in which organic matters can be destroyed, either by a process of oxidation or of fermentation. As oxidation fails to account for the phenomena in question, we must fall back on fermentation, which, as we know, is the main agent in a host of transformations in both the vegetable and the animal kingdoms. The conditions necessary for alcoholic fermentation are absent in the organism; and if they are artificially supplied—if, for instance, a mixture of sugar and yeast be injected into the veins of an animal—death is the certain result. The destruction of the sugar in this manner is consequently impossible; but our author believes that under the influence of the extreme division which it undergoes in the blood, it may be converted into lactic acid by

* These observations accord with those recently published by Dr. W. D. Moore, who failed to detect sugar in the urine of the human fœtus at the full period.

† See Lehmann's Physiological Chemistry, vol. iii. p. 234.

‡ Still further evidence on this point may be found in a memoir by Poggiale, On the Action of Alkalies on Sugar, in the Comptes Rendus, tom. xlii. p. 198. Paris, 1856.

a simple molecular change, in which the oxygen plays only a secondary part. The researches of Dr. Pavy throw considerable light upon this subject, and strongly corroborate Bernard's view.

"In experiments which the author (Dr. Pavy) has now several times repeated, he injected blood removed from the right side of the heart of an animal—and therefore normally containing sugar—through the capillaries of the artificially inflated lungs of another; and found that as long as the blood retains its fibrin, there is as much destruction of its sugar as would take place in the living animal; but that where the fibrin has been separated from the serum and corpuscles, the sugar ceases to be influenced by the presence of oxygen, or ceases to disappear during the process of artificial respiration. It would hence appear that something besides mere contact with oxygen is requisite for the destruction of sugar. But in other experiments he has found that oxygen is nevertheless a necessary agent concerned in the process of transformation observed during the arterialization of the blood that has not undergone spontaneous coagulation. It would therefore seem, in fact, that oxygen acts secondarily on the sugar, through the medium of the fibrinous constituent of the blood; that it exerts some changes upon this azotised principle, which are capable of inducing the metamorphosis of sugar. . . . If the molecular changes occurring during the decomposition of an azotised substance be capable of converting sugar ($C_{12}H_{12}O_{12}$) into lactic acid ($C_6H_6O_6$), why should not the molecular changes occurring during the building-up or elaboration of this same nitrogenized compound effect the same? Indeed, we have seen that the process of destruction is carried on to a certain extent in the systemic capillaries, and more especially in those of the chylopoietic viscera, where the molecular changes of nutrition are also correspondingly carried on with greater activity than elsewhere. So that analogy and experiment would tend to show that the physiological destruction of sugar is owing to a process similar to fermentation, induced by the molecular changes occurring in the nitrogenized constituents of the animal during life. And in accordance with this, we find lactic acid present in the system, and largely separated from arterial blood by the muscular tissue and the secerning follicles of the stomach. As regards the lactic-acid fermentation, it is well known that the presence of an alkali favours, while that of an acid retards, the process. In two experiments on animals, the author injected carbonate of soda and phosphoric acid into the circulating current, and observed, in the case of the latter, that sugar immediately accumulated in the blood."*

In proceeding to notice the uses of sugar—which he does in his twelfth lecture—Bernard observes that it is difficult at first sight to perceive what part this substance plays in the organism. As it is constantly produced in the liver from a certain epoch of intra-uterine life to the death of the animal, we cannot doubt but that it must have important functions to fulfil. Some physiologists maintain, that by its destruction it developes the heat necessary to support the animal temperature; but this is a mere supposition, not only unsupported by evidence, but in direct opposition to the fact observed by Bernard, that the greatest heat is produced during the formation of sugar in the liver, and not during its destruction, the blood which leaves that organ by the hepatic veins being found to exhibit a higher temperature than the blood in any other part of the body.† The actual uses of sugar in the animal economy are so fully described by Lehmann, in the third volume of his 'Physiological Chemistry,' pp. 216–221, that it is unnecessary for us to advert to them here. Most of

* Proceedings of the Royal Society, vol. vii. pp. 373–4.

† Bernard found that in a dog the temperature of the portal blood entering the liver was 102°·92 Fahr., and that of the hepatic veins 103°·64, while the aortic blood only raised the thermometer to 101°·66. (See Leçons, &c., p. 200.)

these uses are altogether ignored by Bernard, who has discovered that it has a new use, *d'une bien plus grande importance.* He believes that he has proved the presence of sugar to be as necessary for the development of animal as of vegetable tissues; in short, that the action of sugar is essential in the development of organic cells generally. As we do not think that he has succeeded in establishing his case, we shall not describe the experiments which led him to entertain this idea. We shall, however, take this opportunity of noticing more fully the observations which he has made in reference to the occurrence of sugar in the organism during fœtal life (to which allusion has been made in p. 30), inasmuch as it was the theory regarding the importance of sugar in relation to the development of the tissues, that led him to inquire whether sugar did not play an important part, and was not to be detected chemically, in the animal body when its evolution was proceeding with the greatest energy—namely, during intra-uterine existence. His observations on the occurrence of sugar in the fœtal tissues are scattered through various parts of his volume.* The following are his chief results in connexion with this subject:

1. If fœtal lungs or muscular tissue (either voluntary or involuntary) be placed in water at a temperature of 60° or a little higher, a very considerable quantity of lactic acid is developed; whereas, if corresponding adult tissues are similarly treated, ammoniacal products are formed, and the water presents an alkaline reaction. By taking means to arrest the lactic fermentation, undoubted evidence of the presence of glucose in those tissues may be obtained both by Trommer's test and by fermentation. We have not as yet been able to isolate the substance from the lungs or muscles which gives rise to the sugar; but we know that it exists in these tissues in a state insoluble in water, alcohol, or ether; for not only do the above-named tissues yield no sugar to those fluids, but after soaking in these menstrua they still yield sugar and lactic acid. When the tissues are once fairly developed, generally about the fifth month of intra-uterine life (in the fœtal calf, whose period is the same as that of the human fœtus), this property diminishes, and at about the eighth or ninth month, when the muscular elements are definitely formed, the production of sugar in these tissues entirely ceases.

2. While sugar can be discovered in the lungs and muscles, it cannot be detected in the glandular or nervous systems, in the skin, or in the bones; and (contrary to what might have been expected) the liver, which, when the functions are duly localized, becomes the great glycogenic organ, is, during the earlier period of embryonic life, as free from sugar as the other glandular structures. In the fœtal calf, it is not till about the fourth or fifth month that sugar in small quantity begins to appear in the liver, but from that period the quantity of hepatic sugar increases with the age of the fœtus.†

3. Until about the middle of intra-uterine life, the saccharine matter which is formed in the pulmonary and muscular tissues is not converted into lactic acid (or otherwise destroyed) almost immediately after its

* See Lecture XI. pp. 230–232; Lecture XII. pp. 248–254; Lecture XX. pp. 380–389; and Lecture XXI. pp. 393–403

† See Leçons, &c., p. 82.

formation, as occurs in adult life, but enters into the circulation, and thus makes its way into the urine; and the urine accumulating in great quantity, fills not only the bladder, but the allantois, which communicates with it, and thus the allantoic fluid also becomes saccharine. The liquor amnii also contains sugar (but in much smaller quantities), which must either have got there by endosmosis, or must be due to a small quantity of urine having been expelled from the bladder through the urethra.

4. Independently of its use (according to Bernard's view) in connexion with the development of the tissues, he shows, by ingenious experiments, that its presence in the blood prevents the infiltration of that fluid into the tissues, and promotes the circulation generally. In the liquor amnii the sugar undergoes a kind of viscous fermentation, which gives to that fluid its well-known glutinous character; so that, as Bernard observes—

> "The advocates of final causes might perceive here a secondary use of sugar during intra-uterine life; having first prevented the imbibition and infiltration of the young and delicate tissues by the liquor amnii, it becomes converted before the period of parturition into a viscid substance, capable of lubricating the passages and facilitating the escape of the fœtus."

The mechanism of the formation of sugar in the liver next claims our attention. That a secretion may be produced, it is an established law that two conditions are imperative—namely, 1, the blood; and 2, a glandular organ to which the blood goes. It is a doctrine generally accepted by the physiologists of the present day, that the glandular organ furnishes nothing to the secretion, but that its tissue (or, at all events, certain of its cells) exerts a catalytic action on the elements of the blood as it traverses the organ. In accordance with this view, Lehmann has afforded us a very satisfactory explanation of the origin of the sugar in the liver. On comparing the composition of the blood of the portal and the hepatic veins, he found that the saccharine blood of the hepatic veins contains less fibrin and less hæmatin than the non-saccharine blood which enters the liver by the portal vein. He then proved, by a very ingenious chemical process, that pure crystallized hæmatin might be resolved into glucose conjugated with a nitrogenous substance; and from this he infers that the liver most probably disintegrates this, and perhaps some other constituents of the portal blood, in a somewhat similar manner, into glucose and a nitrogenous substance which enters into the composition of the bile. Since the publication of his Leçons, Bernard has, however, been led to give up Lehmann's explanation, and has been driven to the belief, from certain experiments which he has recently made, that *it is not in the blood, but in the hepatic tissue itself, that we must search for the substance which precedes and directly gives origin to the sugar.* The subject is one of such importance that we give the main points of the leading experiment.

A dog that had been fed exclusively on flesh for several days was killed seven hours after a meal; the liver was carefully removed, without injury or the slightest delay, and a current of cold water was injected with considerable pressure into the portal vein before the blood had had time to coagulate, and continued for forty minutes. The liver gradually assumed an exsanguine appearance, and the water which jetted out of the hepatic veins, from being bloody and saccharine, became colourless, and entirely free both from sugar and from albuminous matters. The liver was afterwards submitted to the external action of a current of water; and on boiling a small portion, it was found that its decoction did not give the slightest trace of sugar. On exposing the organ for twenty-four hours to the action of the atmosphere, it was found that this structure, which on the previous day was completely free from sugar, now contained it in abundance.

This experiment clearly proves (says Bernard) that in the healthy liver there are two substances,—(1), sugar which is very soluble in water, and which is carried off during the injection into the portal vein; and (2), a substance so slightly soluble in water, that it remains fixed in the hepatic tissue after the blood and sugar have been removed by the prolonged washing. It is this latter substance which, in a liver left to itself for a sufficient time, becomes converted into sugar by a kind of fermentation.

This formation of sugar generally terminates in about twenty-four hours, and if after that time we again inject and wash the liver, so as to remove all the newly-formed sugar, we seldom find that any more is produced, the substance yielding it being doubtless exhausted; the formation is, however, effected more rapidly when we increase the surfaces which are in contact with the air, by cutting it in slices, and moistening them with water.

We shall devote the short space that still remains to us to the consideration of "artificial diabetes."

We find, as might be expected, minute details regarding the mode in which Bernard performs his celebrated experiment of inducing artificial diabetes, by pricking a certain point of the medulla oblongata either of a herbivorous or a carnivorous animal; but until we read these lectures, we were not aware that he had extended this experiment in the manner described in the following paragraph:—

"When we prick the mesial line of the floor of the fourth ventricle in the exact centre of the space between the origins of the auditory and pneumogastric nerves, we at the same time produce an exaggeration of the hepatic [saccharine] and of the renal secretions; if the puncture be effected a little higher, we very often only produce an augmentation in the quantity of the urine, which then frequently becomes charged with albuminous matters, while if the puncture be below the indicated point, the discharge of sugar alone is observed, and the urine remains turbid and scanty. Hence it appears that we may distinguish two points, of which the inferior corresponds to the secretion of the liver, and the superior to that of the kidneys. As, however, these two points are very near to one another, it often happens that if the instrument enters obliquely, they are simultaneously wounded, and the animal's urine not only becomes superabundant, but at the same time saccharine."*

The urine becomes saccharine in from one to two hours after the operation, but seldom continues so for more than a day.

It is gratifying, and, we must confess, somewhat surprising, to find that M. Bernard's animals do not seem to suffer much pain or inconvenience from his scientific investigations. After performing the experiment which has just been described, he observes that the rabbit merely "*semble être un peu étonné sur le moment, mais il remettra assez rapidement.*"

* Leçons, &c., pp. 339-340.

As we have seen it implied that this singular discovery—that by pricking a certain point of the nervous system we could render an animal diabetic—was due to a mere happy chance, we shall give in a very few words Bernard's account of the manner in which he was led to it. Assuming it as a recognised fact that all secretory organs are influenced by the nervous system, which can either augment or depress their secreting functions; and bearing in mind Magendie's experiment, in which, by exciting the lachrymal branch of the fifth pair, he caused an abundant flow of tears, and that they ceased to flow when the nerve was divided; Bernard was led to investigate whether a somewhat corresponding experiment could not be performed on the liver. Indeed, half the experiment was ready prepared for him, for he had previously ascertained that the secretion of sugar in the liver is stopped when the pneumogastric nerves are divided, and it only remained to try the inverse case, and to see whether irritation or stimulation of the nerves would cause an increased secretion of sugar. With this view he galvanized the nerves, but without obtaining the expected result. He then recollected that in experimenting upon a totally different subject — namely, upon the functions of the fifth pair of nerves—he had observed that when, instead of dividing them within the cranium, as he intended, he only pricked the nervous centres at the origin of the nerve, the secretions (tears and saliva), which would have been stopped if the operation had been properly performed, were actually increased to a considerable extent. This led him to attempt to prick the origin of the pneumogastric, and to observe if an analogous effect would be produced to that which he had seen manifested by the secretions which are under the influence of the fifth pair. His very first attempt was successful, and in the course of an hour, the rabbit on which he operated became diabetic, both the blood and the urine being charged with sugar.

Bernard's original theory, that the secretion of sugar was under the direct influence of the pneumogastric nerve, was, however, erroneous, as he shortly discovered; for he found that if before irritating the floor of the fourth ventricle he first divided the pneumogastric, sugar still appeared in the urine. He now believes that the nervous influence on the liver is transmitted by reflex action through the ganglia of the sympathetic. After laying it down as a law that a ganglionic apparatus pertaining to the great sympathetic always exists between the organ which receives the reflex action and the nervous centres which propagate it, he maintains, in relation to the glycogenic functions of the liver, that the starting-point of the irritation is the lung, which is always receiving on its surface the impression of the air; this impression or sensation is perceived by the extremities of the pneumogastric nerves, which are distributed over the lungs, and is thus conveyed to the medulla oblongata, from whence it is propagated by the spinal cord and by filaments of the great sympathetic nerve to the liver. We regret that we cannot find room for a sketch of the anatomical details, or the various ingenious experiments by which he arrived at the above conclusion.

Other means of producing artificial diabetes are subsequently described. It appears that any agents or conditions that cause a suspension of the functions of animal life, while the purely nutritive or organic functions remain intact, induce the diabetic state. In this way the celebrated Indian poison, *Curare,* and apoplexy induced by a severe blow on the skull, have been shown by Bernard to produce saccharine urine. Moreover, local irritation of the liver may augment the glycogenic functions. M. Harlay (who is probably better known in this country as Dr. Harley) injected irritating substances—such as a dilute solution of ammonia or ether—into a branch of the portal vein, and after some time found sugar in the urine. Hence it is not impossible that abnormal matters may be sometimes absorbed from the intestines by the mesenteric veins, and produce a similar effect; and we may thus probably explain the fact that Leconte always found sugar in the urine of dogs slowly poisoned with small doses of nitrate of uranium. To somewhat similar causes we may also refer certain cases of diabetes produced by contusions in the region of the liver—such, for instance, as one mentioned by Bernard, in which a man became diabetic from receiving a kick in the right hypochondrium from a horse. The sugar disappeared when the patient recovered from his contusion, but he continued to pass an excess of urine.

We regret that our limited space prevents us from noticing, even in the briefest manner, Bernard's experiments upon the influence of the spinal cord on the formation of sugar, or his lectures on the application of his physiological discoveries to the pathology of diabetes; and we cannot conclude this short sketch of the 'Leçons de Physiologie Expérimentale' without assuring our readers that in the preceding pages we have merely given them an average sample of the physiological riches with which this volume abounds.

Dr. Bryden's prize essay contains an excellent *résumé* of nearly all that is known on the sugar question, and there are several points which he has illustrated by original observations and analyses. He is, as far as we know, the only British observer who has succeeded in confirming Reynoso's statement, that the internal use of arsenic and quinine gives rise to saccharine urine. A small volume "On the Physiological Relations of Sugar" would be a welcome addition to our medical literature; and we hope soon to see Dr. Bryden's essay (or, at all events, the more important chapters of it) in print.

Review IV.

The Obstetric Memoirs and Contributions of James Y. Simpson, M.D., F.R.S.E., Professor of Midwifery in the University of Edinburgh, &c. Edited by W. O. Priestley, M.D. Edinburgh; and Horatio R. Storrer, M.D., Boston, U.S. Vol. II.—*Edinburgh,* 1856. pp. 819.

The present volume of Dr. Simpson's obstetric writings, although nearly equal in point of magnitude to its predecessor, contains fewer materials for either analysis or criticism. Of the entire work, upwards of a third is occupied with papers on the subject of anæsthesia, many of which are now possessed of historical interest only; whilst the remainder consists for the most part of a series of disquisitions upon the physiology and pathology of the products of conception, which, although replete with interesting facts and ingenious speculations, are rather adapted for specific reference than for critical analysis. Our notice of the volume will therefore be restricted to comparatively small portions of it only ;—to such papers as are of a practical rather than of a speculative character ;—and these will be found more especially in those sections of the book which treat of the pathology of the puerperal state, and that of infancy and childhood.

Two papers on the subject of puerperal fever first claim our consideration, of which the first is devoted to a consideration of the analogy which subsists between it and "surgical fever ;" and the second, to its communicability and mode of propagation. It is believed by the author that puerperal and surgical fever are assimilated to each other in the following respects:—1. In the anatomical conditions and constitutional peculiarities of those who are the subjects of them. 2. In the pathological nature of the attendant fever. 3. In the morbid lesions respectively left by either disease; and 4. In the symptoms which accompany each affection. We subjoin the principal facts which are alleged in support of each of these analogies.

I. The anatomical conditions of the puerperal patient after delivery, and of the surgical patient after an operation, are represented as being in many respects the same. In both there is a wound or solution of continuity; in the latter case, on some external part of the body, in the former, on the internal surface of the uterus, caused by the separation of the placenta and the exfoliation of the decidua. On the surface of both of these wounds numerous arteries and veins open; both are repaired by the exudation of organizable lymph, and the reparation of either is liable to be complicated with various constitutional states of the same kind. Both may be followed by symptoms of shock or collapse; both have generally a subsequent limited degree of febrile action; and in each case the wound is liable to deviate from the standard mode of reparation, for their secretions may alter morbidly, or they may become the seat of an excess of inflammation or ulceration, or of phlebitic suppuration and its consequences. In both cases air occasionally enters by the mouths of the veins which open upon the free surface of either wound; from both, dangerous hæmorrhage, both primary and secondary, is liable to occur. Both are occasionally, though very rarely, followed by delirium, tetanus, and other nervous complications; and in like manner, but much more frequently, they are apt to be followed by that form of combined febrile and inflammatory action which we term surgical fever in the surgical patient, and puerperal fever in the puerperal patient. In short, it is averred that the two species of wounds are subject to the same local pathological deviations, and liable to be attended with the same pathological constitutional effects and complications. II. The analogy in the pathological nature of puerperal and surgical fever is chiefly based upon the doctrine that the real source and cause of both is to be found in a toxæmia or morbid state of the circulating fluid. After commenting upon the insufficiency of the theories which were formerly held regarding the nature of puerperal fever, one of which viewed it as an idiopathic fever *sui generis*, the other as essentially a local inflammation, upon which the fever was dependent, the author refers to the experiments made by Gaspard, Cruveilhier, Castlenau, and others, upon the lower animals, as showing that by the direct injection of pus and other morbid secretions into their veins, a train of symptoms during life, and a series of lesions observable after death, may be produced, having a very strong analogy to those of puerperal fever. In harmony with this doctrine, he then points out various facts which show that both in the puerperal and surgical patient such conditions exist as facilitate the infection of the general circulation—such as by the absorption of pus and other morbid matters from the uterine and surgical wound through the orifices of open veins, by the inoculation of morbid and contagious matters through the abraded vaginal surface, and by the supervention of any accidental inflammation. Accepting, then, this theory upon these grounds, the author maintains that it affords the best explanation of the relations which subsist between the general febrile action and the internal inflammations which are respectively met with in each case. III. In support of the analogy in regard to the internal pathological lesions left by puerperal and surgical fever, the author gives various tables drawn up by Chevers, Dugès, and Tonnellé, and deduces from them the following conclusions:—1. That both diseases generally leave upon the dead body ample evidence of the occurrence before death of acute and often extensive internal inflammatory action. 2. That the internal inflammatory lesions are seldom limited in the same case to one organ or texture only, but two or more different viscera or surfaces are usually observed to have been either the simultaneous or successive seats of inflammatory action; and the different parts thus attacked are sometimes very distinct and distant from each other. 3. The internal viscera or textures which are the first and principal seats of inflammation are often far removed from the original wound or lesion, particularly in those cases in which the wound or lesion is in the head or extremities; whilst, however, in the case of the obstetric patient, various causes appear to localize inflammatory action upon the uterus, uterine appendages, and peritoneum in puerperal fever, and more especially from their being the immediate seat of injury and lesion in the act of parturition. IV. Lastly, in speaking of the analogy in the symptoms of puerperal and surgical fever, the author remarks that there is almost no disease which varies more than puerperal fever does in different cases, and that the same variability holds good in regard to surgical fever. When, however, the disease is fully marked, the symptoms, he observes, are sufficiently striking and similar in each—the more marked consisting of rigors, a pulse varying in strength, but always of great frequency, an altered and frequently darker or almost icteric hue of surface; the skin sometimes hot and dry, sometimes bathed with perspiration; local pains and functional derangements, anxiety and general prostration, laboured or hurried respiration, and often at last rapid sinking, with or without delirium.

Such are the chief grounds which are alleged in support of the analogy that subsists between puerperal and surgical fever; and admitting the full force of the considerations upon which it is founded, and the ability with which the question has been argued by our author, we must yet be permitted to express our dissent from some parts of the argument, and to state the grounds upon which it is founded.

With regard, then, to the analogy which exists between an amputated stump and the interior of the uterus after childbirth, we may observe that this had been long ago insisted upon by Cruveilhier, upon grounds very similar to those now alleged by our author, and yet the parallel has very generally been felt to have been overdrawn. What analogy, indeed, except the most distant, can possibly exist between the physiological process of childbirth and the proceeding by which an important limb is arbitrarily severed from the body? In the former case there is a gradual preparation for the changes which are about to take place; whilst in the latter there is none. The uterine wound after parturition is limited to that caused by the separation of the placenta and decidua. It is, therefore, essentially superficial, and confined to the mucous membrane of the uterus; whereas that caused by amputation involves a division of the muscular, cutaneous, vascular, and other structures of the limb; and whilst the vessels torn across by the separation of the placenta are extremely friable, elastic, and retractile, as we have particularly noticed in separating the placenta of the bitch; those divided by amputation are far otherwise. Now these circumstances, no less than the contractions of the uterus after childbirth—which, by diminishing the size of the uterine wound, and maintaining a closed state of the torn uterine vessels, must very much lessen the liability to hæmorrhage and other dangers which are so apt to follow upon amputations, and proportionately diminish the analogy which exists between the two conditions.

Nor is it evident that the advancement of puerperal and surgical pathology can be best secured by assimilating, under the generic terms of puerperal and surgical fever, all the varied forms of febrile and inflammatory disease which are so liable to occur respectively after childbirth and operations. Is there, indeed, any special form of fever consequent upon either, so constant and precise in its nature, as to justify these particular appellations? Or is it not rather the case that, in connexion with, or as a consequence of, both, many different forms of febrile and inflammatory disease are liable to supervene, the precise nature of which will vary under different circumstances and in different cases? Now we apprehend that these questions must be answered in the affirmative of the latter; for if we look alone to the statistical tables quoted by Dr. Simpson, we find a series of lesions given, incidental to childbirth and operations, so numerous and dissimilar, that it is impossible to group them together under one common appellation without confounding all rules of nosological classification. Peritonitis, enteritis, pneumonia, pleuritis, bronchitis, laryngitis, diphtheritis, pericarditis, arteritis, phlebitis, meningitis, cerebritis, cystitis, &c., represent in one table the inflammatory lesions which are sometimes consequent upon operations; and an equally formidable list in another represents those which are liable to occur after childbirth. We repeat, that it is impossible to assimilate or group together all these several maladies under one common designation, without an entire disregard of all nosological distinction.

But, admitting them to be nosologically distinct and different, the question arises, are they nevertheless identified in a common origin? or, in other words, can we admit the doctrine contended for by our author, that all forms of puerperal fever and inflammation, as also all forms of fever and inflammation supervening upon operations, have a common origin in a morbid or vitiated condition of the blood? Now, speaking rather of puerperal fever, which this paper is more particularly intended to elucidate, and reviewing the question in its several bearings, we feel bound to assert that this doctrine is altogether untenable; that the fevers consequent upon childbirth are of a very varied and dissimilar character, some having their origin as certainly in lesions of the solids, as others have their origin in a vitiation of the fluids of the body.

It is not, however, necessary, nor would it be consistent with the scope of this article, to discuss at any length the abstract doctrine of fever; nor do we propose to enter upon it farther than may be sufficient to indicate very generally the grounds upon which we are led to dissent from the doctrine propounded by our author, that the cause of puerperal fever is to be sought for, in all cases, in a morbid condition of the blood. Believing that fever is essentially a disease of the nervous system, we are led to look for its origin, not so much in one as in many modes of causation, and to conclude that its type and character will vary with the nature of the various causes by which it may be induced. Hence the origin of new forms and types of fever in different seasons and at different epochs—the specific differences in fevers arising from different specific causes—the difference observable in the puerperal fever of different epidemics—and the discrepancy in the writings of those who have observed it in different seasons or under different circumstances. At one time the inflammatory, at another the febrile, type prevails; in one epidemic the sthenic, in another the asthenic; but to argue hence that all puerperal fevers are and must be either idiopathic or symptomatic, sthenic or asthenic, would be very greatly to exceed the legitimate bounds of observation and induction; and admitting fully the blood origin of puerperal fever in many cases, we are yet constrained to believe that it may and does frequently arise independently of any primary vitiation of the blood.

Whatever the causes or the varieties in the type of puerperal fever, it is in the character of the attendant lesions, more perhaps than in anything else, that its chief peculiarity consists: in the presence of fever complicated with inflammatory lesions of the pelvic and abdominal viscera.—"The most fatal disease to which lying-in women are subject," observes Dr. Gooch, "is known under the names of puerperal or childbed fever, puerperal peritonitis. Its essential symptoms are, pain and tenderness over the abdomen, with a rapid pulse. It begins a few days after delivery, with pain of the abdomen, shivering, succeeded by heat, and a quick pulse. As the disease advances, the milk becomes suppressed, the belly tumid, and the breath short; when it terminates fatally, it does so commonly about the fifth day, but often in less than half that time. On opening the abdomen, the morbid appearances are not uniform; but the most common and remarkable are, a copious effusion of lymph and serum on the surface and in the cavity of the peritoneum. Thus it is a fever essentially complicated with an affection of the peritoneum. A better name than puerperal fever, or puerperal peritonitis would be that which I have placed at the head of this paper—peritoneal fever; for it would express the fact, that an affection of the peritoneum is an essential accompaniment of the disease, without defining what that affection is, because it is not uniform."* Dr. Lowder, after extensive reading and observation, found that the pathognomonic symptoms of puerperal fever were very few, and reducible to the following—fever, intense pain of the head, and intense pain of the abdomen; so that we are justified in asserting that, however the disease may differ in different cases, whether it be sthenic or asthenic, irritative, inflammatory, or typhoid, that it is in the disposition to such abdominal and pelvic lesions that its chief peculiarity as a fever consists; and hence the question arises how, upon the grounds we have assumed, can the origin of such lesions be explained. Now, undoubtedly in certain cases such complications may arise from the absorption of inflammatory, putrid, and other noxious secretions from the maternal passages, as also from causes tending generally to vitiate the blood—but not necessarily or exclusively; for, through the influence of the nervous upon the vascular system, such inflammatory lesions may arise from the operation of various other causes acting upon the nervous centres, and through these exciting and maintaining a state of preternatural activity of the heart and circulation, in which it will happen that the vessels of the uterine organs and the capillary, rather than the larger, will yield to the increased force of the circulation, allow themselves to become abnormally distended, and so the phenomena of inflammation to arise. Thus, in one series of cases we observe puerperal fever to occur in connexion with irritative disturbance of various organs—such as the mammary glands, the intestinal canal, the brain, or the spinal cord; and such irritative disturbance may, through the influence referred to, adequately give rise to fever, with all the attendant lesions which characterize the puerperal. For inasmuch as all causes of irritation, wherever existing, are immediately felt in the central parts of the nervous system, and through the sympathies subsisting between it and the vascular, tend to excite the latter to preternatural action, it will follow, on such febrile action taking place, that the vessels of any organs that may have been relatively weakened by antecedent actions or other causes, will yield more than others to the general impulse of the blood, allow themselves to become abnormally distended, and thus inflamed. Now, such debility, and consequent disposition to inflammation, is common to the vascular system of the uterus and its appendages after labour, as a consequence of the antecedent actions of pregnancy and parturition, and those necessarily going on in the uterine system subsequently to childbirth; and hence it is that they are so prone to yield preternaturally, and to become the seat of inflammation, in any febrile movement that may be casually excited after labour. In another series of cases, we observe puerperal fever to arise from the operation of various causes upon the nervous system, of a general and non-specific character—such as over-fatigue, over-excitement, and exposure to cold; and here, as in the former case, a febrile movement being excited through the sympathy subsisting between the nervous and the vascular systems, the uterine vessels may allow themselves to become abnormally distended, and so the first stage of inflammation to be established. In a third series of cases, we observe puerperal fever to arise from the direct consequences

* An Account of some of the most important Diseases Peculiar to Women, pp. 1–2.

of mechanical injury, such as contusion or mechanical injury of the uterine organs during labour; and here the nervous and the vascular systems are incited to preternatural and abnormal action through the medium of inflammatory irritation acting upon the peripheries of the nervous system. And lastly, it is not to be denied that puerperal fever is often traceable to the operation of causes which directly tend to vitiate the blood—such as various epidemic and endemic influences, noxious miasmata, specific contagions, and unhealthy discharges absorbed from the maternal passages, which, equally with the former, may give rise to fever, with all the attendant lesions which distinguish the puerperal. These causes, however, equally with the others, act primarily upon the nervous centres, and establish the phenomena of fever and inflammation through the medium of the sympathy subsisting between the nervous and the vascular systems; and here, as in the former case, the capillaries of the uterine system are predisposed to, and become the seat of, inflammation as a consequence of the antecedent actions of pregnancy and parturition, and the necessary changes consequent upon labour.

Such, to our mind, are some of the several ways in which puerperal fever may arise; and if our views are correct, it must follow that it cannot in all cases be referred to any one cause, however powerful or frequently in operation, even as that of vitiated blood. From general causes acting upon the nervous system, we may deduce not only the occurrence of fever, but of fever attended with the most prominent lesions which distinguish the puerperal. And with regard to minor differences in the type of the disease, as observed in different persons and under different circumstances, we may observe that they are often capable of an explanation by a reference to the intensity of the operation of the same cause, and the peculiarities of individual constitutions, rather than by any essential difference in the nature of the exciting cause. Thus, the more powerfully the nervous system is injured or impaired by the offending cause of the attack, the greater will be the prostration and the less marked the reaction; and hence, in such cases, fever of varying type rather than inflammation will be the consequence; whilst, on the other hand, the less powerful the injurious impression on the nervous system, or the greater the resisting power of the individual, the more decided will be the reaction, and the greater the tendency to inflammatory complications. Moreover, in proportion as particular parts of the capillary system are prone to yield, from antecedent weakness or other causes, to the reactive force of the circulation rather than the capillaries generally, will the disease partake of the inflammatory rather than the febrile type. Bilious, cerebral, gastric, pulmonary, and other complications, would appear to have reference to the predisposition of particular organs to morbid action, consequent upon antecedent weakness or derangement. Persons, for instance, naturally subject to bilious derangements may be supposed to have the hepatic functions more readily disordered than any others, under the operation of a general disturbing cause; and so the occurrence of antecedent disease or debility of any organ—pulmonary, gastric, or cerebral—will predispose such organ to inflammatory action in the course of any febrile movement which may be casually excited: it being an admitted physiological fact, that the weakest part feels most that which affects the whole; and hence it happens that the capillary vessels of organs relatively weak yield most to the force of the circulation, under the influence of any such febrile or preternatural excitement.

The length at which we have entered upon this question forbids our noticing in detail the various causes by which the blood may be infected or vitiated after childbirth, so as to give rise to some of the more specific types of puerperal fever. Dr. Simpson is of opinion that the infecting cause is generally, if not always, an inflammatory secretion, just as is the inoculable matter of small-pox, cow-pox, and syphilis; and he argues against its being a mere product of putrefaction. We regard this, however, as a too partial and limited view of the nature of the causes from which epidemic, endemic, and some of the more specific forms of puerperal fever take their rise. These we believe, on the contrary, to be dependent upon the introduction into, or generation in, the system of poisonous principles evolved from organic matter in certain states of retrograde decay. We admit that all types and forms of decay do not furnish the specific products upon which the origin and dissemination of specific forms of fever and disease depend. But decay and putrefaction are but generic terms applied to various retrograde changes in the elements of organic matter, and are liable to be variously modified, retarded, or accelerated by a variety of circumstances. Moreover, regarded as the grouping of the atoms of organic matter into simpler and less complex combinations, we can readily conceive how different groupings of such atoms should be possessed of different physical and physiological properties and effects upon the living body. Let us revert, in illustration, to the different physical and physiological properties of oxalic acid and sugar, of otto of rose and sulphuretted hydrogen, of oil of turpentine and essence of lemons—and yet how similar their elementary composition! Now, if in these cases the same atoms differently blended yield products so manifestly different, it is easy to understand how the very complex atoms of organic matter, in the course of their retrograde metamorphoses, should, under the influence of various physical causes, enter into different combinations, some of which may be noxious, and others inocuous, to the human body. Thus it is notorious that, whilst the products of organic decay generally are not the specific cause of typhus, that typhus nevertheless prevails most commonly where such decay abounds; and so it is with cholera and other zymotic diseases: from which it would appear probable that the products of some particular, or perhaps specific, form of decay were their causes respectively. And inasmuch as the blood constantly teems with organic matter in a state of retrograde change, it is evidently possible that, under the influence of certain atmospheric constitutions or conditions, pernicious atomic groupings may take place within the body when the normal force tending to resist such changes is inadequate to prevent them; and hence that the causes and phenomena of specific fever, whether puerperal or otherwise, may be sporadically developed. Inflammatory products in some cases, and putrefactive in others, being equally possessed of noxious properties, and such as received into, or generated in, the organism, and applied through the medium of the blood to the nervous centres, may produce the phenomena of irritative, inflammatory, and febrile disturbance—each assuming more or less specific types or characters, according to the peculiar properties of the poisonous principle in operation.

We proceed to a consideration of the papers On Puerperal Arterial Obstruction and Inflammation, and On Tetanus following Lesions of the Uterus; and here we may observe, that little is left us to offer but an analytical notice—the subjects treated of being equally novel and original, and such as have hitherto received but little notice from the profession in relation to the puerperal state.

Inflammation and obstruction of the arteries in the puerperal female would appear to be producible by a variety of causes, and more particularly the following:—1. By the separation of old or organized cardiac vegetations, and their subsequent transference into the arterial canals, as more particularly pointed out by Dr. Kirkes, Virchow, and others; 2. By the passing forward into the current of the circulation of recent fibrinous masses formed in the cavities of the heart or larger arterial vessels; 3. By local arteritis; 4. By laceration of the internal coats of the occluded vessels; and 5. By morbid materials carried from the systemic venous system, and lodged in the pulmonary artery or its branches.

In illustration of the first mode of causation, 5 cases are reported of arterial obstruction, in all of which vegetations were found on the aortic valves; and loose bodies, having the same physical appearance and structure as the cardiac vegetations, were discovered on the obstructed arteries. It is argued, that the cause of arterial obstruction in these instances could not be local arteritis, inasmuch as the symptoms of arterial obstruction occurred suddenly and almost instantaneously, and the obstructed artery in some exhibited no post-mortem evidence of thickening, or previous inflammatory disease. And further, it is contended that the separation of vegetations from the heart is rendered highly probable by a variety of considerations: for, 1, The vegetations, whether sessile or pediculated, are often loosely attached, being easily removed after death by the handle of the scalpel; 2, The valves to which they are adherent are parts constantly in motion; 3, Currents of blood are ever rushing over them with considerable force; and 4, When once separated, they will be carried along until, meeting at last with a vessel whose calibre is smaller than their bulk, they become impacted; or they may become arrested where a larger vessel divides into the branches, each of which is smaller than the detached vegetation. The second cause of arterial obstruction is that in which recently-formed coagula are projected from the heart into the general circulation. Fibrinous polypi have been found after death in the cavity of the heart; and it is observed that they are specially likely to be formed in the interior of the left ventricle when the latter is anywhere mechanically rough or irregular, as from the presence of globular polypi in its cavity, vegetations on the valves, or endocarditic inflammation of its lining membrane, and more especially when the blood is super-fibrinated, as happens in the puerperal state. In illustration of the third cause of obliteration of arteries in puerperal patients—that arising from local arteritis—a case is referred to as having occurred in the practice of Dr. Duncan, in which acute gangrene of both lower extremities had come on in a patient who had been confined only two weeks. On dissection, no disease was discovered in the heart, its walls, valves, or cavities. But the aorta was found blocked by a firm fibrinous exudation, which descended along the iliac arteries, and in some situations was closely adherent to the arterial walls, the coats of the obstructed arteries being at the same time much thickened. The fourth cause of arterial obstruction in puerperal patients is laceration of the internal coats of the artery,—a subject which is illustrated by a reference to the writings of Dr. Hodgson and Professor Turner, and a case published by Dr. Oke, of Southampton. In this the patient had uterine hæmorrhage, terminating in abortion. Three days afterwards, her left arm had become cold and insensible, and the tips of the fingers discoloured. No pulsation could be felt in the limb. The action of the heart and the respiration were natural. The tips of the fingers became gangrenous, and dropped off, but the gangrene proceeded no further, and the arm recovered its natural plumpness. The patient is still alive, and no heart affection can be detected. The fifth cause of puerperal arterial obstruction is a diseased condition of the blood, tending to obstruction of the pulmonary artery and its branches by morbid materials coming from the systemic venous circulation, and passing through the right side of the heart. In support of this doctrine, a variety of cases are given in which obstruction of the pulmonary artery was met with in connexion with and as a sequence of phlebitis and phlebitic concretions in the uterine, pelvic, or other systemic veins. Other cases are, however, given, in which no antecedent venous inflammation existed, and it is surmised that some cases of sudden and unexpected death in the puerperal mother may have its origin in this pathological condition. With regard to the general causes capable of giving rise to arterial obstruction and inflammation in the puerperal state, the author is inclined to refer them to certain abnormal peculiarities in the blood of the puerperal female, such as the redundancy of fibrin common to this period, the amount of effete matter thrown into it, consequent upon the retrograde metamorphoses or disintegration of the uterus, and the new materials formed in it for the formation of the milk. These constitute a state of blood, it is argued, which, under the influence of a variety of accidental causes, inducing fever or interrupted excretion, may favour the development of arterial obstructions. The symptoms of such lesions will of course vary according to the artery obstructed, and with the function of the part to which the artery belongs. Hence the results of arterial obstruction are very different, according as the occluded artery belongs to organs connected with the head, chest, or abdomen, or is an artery belonging to one of the extremities of the body. Our knowledge of the former series of cases is limited to the possible occurrence of symptoms of paralysis, loss of vision, and *ramollissement* in regard to the brain, and distressing disturbance in the actions of the heart and lungs, with possibly gangrene of the latter organs in regard to the lungs when the arteries respectively of the brain and lungs are concerned. With regard, however, to arterial obstructions of the limbs, the following symptoms are liable to occur in the affected extremity:—1. Arrest of the pulse below the site of obstruction. 2. Increased force of pulsation in the artery above the site of obstruction. 3. Fall in the temperature of the limb. 4. Lesions of the motor and sensory powers in the limb in which the artery has become obstructed, giving rise to paralysis, neuralgia, &c. 5. Gangrene below or beyond the seat of arterial obstruction.

The foregoing must be regarded as a very condensed summary of the principal point mooted in one of the most original and interesting papers which has recently been published in obstetric pathology; and we cannot doubt but that it will prove the forerunner of many valuable investigations on the same subject. On the present occasion any critical remarks would be altogether out of place; but we cannot help directing attention to the light thrown by this communication upon the kindred subject of obstruction and inflammation of veins. Thus, it would appear that of the five modes in which obstruction and inflammation of arteries are alleged to take place, in one only are they assumed to depend upon idiopathic inflammation of the coats of these vessels; and this doctrine is so little supported by the circumstances of the case adduced in support of it, that additional evidence will be required before it can be admitted by the profession. In nearly all the other modes the obstructing cause is shown to be some morbid condition or constituent of the blood; and such was the doctrine affirmed by Dr. Mackenzie, after a lengthened investigation, in regard to the causation of the obstruction and inflammation of veins;* nay, further, the several conditions referred to by him as being the most favourable to the production of such lesions of the veins in the puerperal state, are precisely those alleged by Dr. Simpson to be the most powerful in the production of the same lesions of the arteries. So far, then, the researches of Dr. Mackenzie and those of our author may be regarded as mutually supporting each other, and point clearly to the direction in which further investigations may most profitably be made.

The researches of Dr. Simpson have led him to the conclusion, contrary to general belief, that internal injuries or lesions of the uterus, both in the unimpregnated and puerperal states, are sometimes followed by tetanus in an acute and fatal form; and he refers to a series of twenty-five cases as showing that traumatic tetanus does supervene occasionally as a secondary obstetrical disease in the same way as all medical authorities acknowledge it to supervene occasionally; and still more frequently as a secondary surgical disease. These cases tend to show that tetanus may follow—1, lesions of the unimpregnated uterus, as well as the lesions left in the uterus and maternal canals; 2, by abortion; and 3, by parturition at the full time. The nature, causes, and treatment of tetanus thus supervening are the subject of some interesting remarks which are appended to the cases. Referring to the general fact, that the existence of an injury or wound upon the external parts of the body is by far the most common cause of tetanus, it is affirmed that a similar state of lesion exists upon the interior of the uterus—viz., that caused by the separation of the decidua and the rupture of the organic attachments of the placenta to the uterus. Hence it is remarked, that obstetrical tetanus has in this respect an exciting cause essentially similar to surgical tetanus; and that the reason that this state of lesion of the interior of the uterus does not more frequently give rise to tetanus, is simply that the organ is chiefly supplied with nerves from the sympathetic system;

* Researches on the Pathology of Obstructive Phlebitis, and the Nature and Proximate Cause of Phlegmasia Dolens: Medico-Chirurgical Transactions, vol. xxxvi. p 169 et seq. See Medico-Chirurgical Review, vol. xii. p. 71.

tetanus being an affection far more easily excited by lesions of parts supplied with nerves from the cerebro-spinal system, than by lesions of parts supplied with nerves from the sympathetic system. Further, it is argued that as we have in obstetric pathology evidence almost amounting to certainty that the analogous super-excitable state of the cerebro-spinal system of nerves, which gives rise to eclampsia or puerperal convulsions, is generally produced by the existence of a morbid poison in the blood, it is possible that the generation of a special blood poison at the site of the wound or elsewhere, may sometimes in the same way give rise to obstetrical and surgical tetanus. It is worthy of remark, that in some of the cases quoted the tetanic attack followed upon exposure to cold, a common exciting cause of ordinary tetanus. Hence it may be concluded that the more immediate causes of obstetrical tetanus are, 1, the uterine lesion following abortion and parturition; 2, a morbid condition of the blood consequent upon the absorption of a special blood poison generated at the site of the wound; and 3, exposure to cold, or rather to currents of cold and damp air, especially if the person immediately before this exposure had been over-heated or perspiring. With regard to treatment, it is observed that no kind of local treatment to the seat of the original uterine lesion could be well applied, or would probably be of any avail if applied; and as to constitutional means, that the following are probably the most important:

1st. The greatest possible quietude and isolation of the patient from all irritation, corporeal or mental, during the course, and for some time even after the resolution, of the disease.

2nd. The special avoidance of painful and generally impracticable attempts at opening the mouth in order to swallow, but sustaining the strength of the patient, and allaying thirst, by enemata, or by fluids applied to the general surface of the body.

3rd. If there is any well-grounded hope of irritating matters lodged in the bowels acting as an exciting or aggravating cause, to sweep out the intestinal canal at the commencement of the disease with an appropriate enema.

4th. To relax the tonic spasms of the affected muscles, and diminish the exalted reflex excitability of the spinal system, by sedatives or antispasmodics; with the prospect of either directly subduing this morbid reflex excitability, or of warding off the immediate dangers of the disease, and allowing the case to pass on from an acute and dangerous attack to a subacute and far more hopeful and tractable form of the malady.

In connexion with this latter indication, the author refers to the employment of various sedatives and antispasmodics; and after pointing out the inutility of most of them, he speaks more hopefully of the antispasmodic action of chloroform sustained for many hours or even days. He dwells upon the safety of its continued employment, and gives a case which occurred in the practice of Professor Laurie, of Glasgow, in which it was successfully exhibited.

Passing over the section On the Physiology and Pathology of the Products of Conception, as containing papers of less practical importance —many of them, indeed, appertaining rather to the domain of anatomy and physiology than practical midwifery: as, for instance, the very elaborate essay On Hermaphroditism, which occupies 141 pages, and others On the Diseases of the Placenta, which have been fully treated of by Dr. Barnes in some recent numbers of this Journal,—we come to a series of papers On the Pathology of Infancy and Childhood, of which one on the external use of oil in the prevention and treatment of scrofula and phthisis is probably the most interesting. We shall examine this paper less with reference to the special curative powers of oil thus externally applied in these affections, for this appears at present to be a question *sub judice*, than its employment generally as a therapeutical agent, the value of which, in various cases, we have had many opportunities of observing.

Among the nations of antiquity, as in some Eastern countries at the present day, the external application of oil to the human body formed an important item of the medical art. By the former it was used both as a hygienic and curative agent, and the rules by which its application was regulated constituted a distinct branch of healing, known as iatraleptic medicine. The writings of Aretæus, Celsus, Pliny, Galen, and others, contain many passages in which its efficacy is spoken of; and among other remedies, we find it recommended in various diseases—such as fevers. eruptive diseases, gout, palsy, lethargy, tetanus, cholera, melancholy, dropsy, &c. The sacred writings also contain various references to this practice, so that we cannot doubt that in ancient times considerable importance was attached to it. The same would appear to be the case among many Eastern nations at the present day, in which it forms an almost necessary concomitant or adjunct of the bath, and is supposed to be possessed of many healing virtues. Among others, maintaining a moist state of the skin, lessening the irritation produced by acrid perspiration, and preventing an excessive transpiration of aqueous matter from the body. The reader will find many interesting remarks on this subject in a paper published by Mr. W. Hunter, in the second volume of the 'Edinburgh Journal' for 1806, the perusal of which originally led us to make a trial of the practice; from which we are enabled to speak favourably of it in many cases, and more particularly the following: 1st. Febrile affections generally, in which the skin is either preternaturally dry or morbidly suffused with perspiration. 2nd. The remittent fevers of children connected with irritative, congestive, or inflammatory conditions of the gastro-intestinal mucous membrane, and those which are so apt to follow some of the eruptive fevers, such as measles and scarlet fever, upon exposure to cold, or any casual interruption of the cutaneous functions. 3rd. Diseased states of the skin of a scaly character, such as the several forms of psoriasis and lepra, in which, with the addition of a little liquid tar or creosote, the external application of oil is signally beneficial. Lastly, strumous affections generally, and more especially those in which the mesenteric glands are obstructed, and in which the skin for the most part is either morbidly dry or preternaturally relaxed. In these and some other morbid states of the economy we have personally witnessed the beneficial effects of the inunction of oil, and we are therefore disposed to receive with considerable confidence the evidence of Dr. Simpson in support of its prophylactic and curative powers in consumption and scrofula.

The origin and results of his investigations may be thus briefly stated. When on a professional visit, in October, 1852, to Galashiels, in Roxburghshire, his attention was incidentally directed by Dr. Macdougal to the healthy state and robust appearance of the operatives at the large woollen manufactories in that town, and to the fact that they were strikingly exempt from consumption and scrofula—an exemption which they attributed to the free external application of oil to their bodies, which occurred in various parts of the manufacture of woollen fabrics. This casual observation appeared to him so interesting in itself, and possibly so important in the consequences to which it might lead, that it seemed to him a matter of moment to ascertain, *first*, if the same relative immunity from phthisical and strumous disease had been observed among the workers at other woollen factories in Scotland; *secondly*, if this immunity were attributable to the external application of oil; and *thirdly*, if the employment of external inunction, when resorted to as a prophylactic or therapeutic means, were capable of acting beneficially upon the body, and could be applied practically in the prevention and treatment of consumption, scrofula, and other affections. We cannot enter at length into the evidence adduced on the subject of these several questions, but may observe that it is tolerably conclusive as to the fact that the operatives in large woollen manufactories are remarkably exempt from consumption and scrofula, and that this exemption is regulated by the more or less "oily" nature of the departments of work in which they are engaged in the mills; so that they in general markedly improve in appearance and health when set to work at the more oily processes, and often as markedly decline after leaving them. With regard, however, to the prophylactic and curative powers of systematic oil inunction as a medicinal measure in tubercular disease, we can only repeat that at present this is a question *sub judice.* No cases are given by Dr. Simpson which support this conclusion, and all that can be said is, that he has used many ingenious arguments in its favour. Time and further experience must determine this question; and as it is one of considerable importance, we venture to append the rules given by Dr. Simpson for external oil inunction, with the propriety of which our own experience leads us entirely to concur:

"The oil selected ought to be bland and inodorous, like olive or salad oil, and it should be applied moderately warm. Its application is thus rendered far more agreeable to the feelings of the patient; the danger of chills is avoided; and the act of absorption is increased by an elevated temperature. 2. A considerable amount and duration of friction should be used either by the patient or his attendant, or by both, in order to rub in the oil as much as possible, and thus promote the completeness of its absorption. 3. The oil and friction should be applied to the whole cutaneous surface of the trunk and extremities, but especially to those parts of it where the skin is thin and the function of absorption greatest, as the sides, the flexures of the limbs, the insides of the thighs, &c. 4. The average quantity of oil requiring to be used at each inunction is about a large wineglassful. 5. In cases in which it is an important object to introduce the oil into the system as freely and rapidly as possible, the inunction of it may be practised twice or oftener in twenty-four hours, especially with children; but the best time for a single daily oil inunction is immediately before retiring to bed, as the imbibition of any free oil left on the surface may afterwards go on during the night; and to save the bed-clothes, the patient should sleep in a dress of flannel, linen, or other material that stretches beyond the feet. 6. In order to maintain the full absorbing action of the skin in conjunction with the practice of oil inunction, occasional warm sponging or bathing of the whole cutaneous surface with tepid water, or with a weak solution of soda and water, should be employed, either immediately before an inunction or several hours subsequently to one. 7. It is to be remembered that the cutaneous absorption of oil is usually, though not always, comparatively more slow and difficult, and hence the practice itself is so far more disagreeable for two or three weeks after the inunction is first begun than subsequently; and consequently that less oil disappears and more friction is required in the beginning of the practice than afterwards."

We come, in the next place, to the series of papers On Anæsthesia, which form so considerable and important a constituent of the volume, and with which, more than perhaps anything else, the name and reputation of our author are identified. Of these papers generally it will be sufficient to observe, that whether regarded with reference to their historical, physiological, or obstetrical value, they will ever possess the highest interest, and claim the attention of the profession, not only as portraying a new phase in obstetric practice, but as containing also the history of the discovery and introduction of the best anæsthetic agent with which we are acquainted. With many of the subjects treated of in this series, however, we have little to do in the present article. The objections which formerly existed to anæsthesia in midwifery have, in a great measure, passed away; numberless facts have attested its value and safety, and the only question now remaining to be considered is the best means of averting the occasional dangers with which its employment is attended. It is to this alone that we shall address any observations which we have to offer upon the subject, and in doing so will leave out of consideration the many speculative views with which it has been encumbered, confining ourselves, as far as possible, to a brief statement of our own practical experience in regard to it.

Death from chloroform has been affirmed to take place in a variety of ways—from coma, or a suspension of the nervous and sensorial functions generally—from asphyxia, or a primary suspension of the respiratory functions—from syncope, or a primary suspension of the heart's action. We do not doubt that it *may* occur in each of these ways, but in a practical point of view it is necessary to distinguish between the more ordinary and the more exceptional modes in which it may take place; because a knowledge of each is important to a right understanding of the precautionary and curative measures to be adopted. Now in ordinary cases, in which there is no peculiar idiosyncrasy of the patient, and no undue haste or rapidity in the administration of the agent, we apprehend that the sequence of events tending to fatal anæsthesia is the following:—1st. A suspension of the functions of the ganglia related to common or general sensibility; 2ndly. A suspension of those related to volition; and 3rdly. A suspension of those related to the respiratory movements. The functions of sensation, volition, and respiration being successively annulled in the order here stated. Such, we apprehend, in ordinary cases, is the progressive tendency of anæsthesia to a fatal result; and if so, it points to the importance of administering the drug very moderately in the first place, and carefully watching the respiratory movements in the second. In certain exceptional cases, however, death may take place in one of the following ways—first, from a sudden and injurious impression upon the brain and nervous centres, simultaneously annulling the functions of respiration and circulation, together with those of sensation and volition, as in the case of violent nervous shock or concussion. In some such cases this result may be due to the concentrated manner in which the chloroform is inhaled; but in others it would appear to be connected with a peculiar idiosyncrasy or susceptibility of the patient or her nervous system; and we have more particularly observed it in weakly anæmic females. One case will illustrate this peculiarity. An extremely anæmic and highly sensitive young lady expressed a desire to take chloroform in the course of her first labour. Not more than a teaspoonful was put upon a handkerchief, and this was placed at a moderate distance from her face to be inhaled. Notwithstanding these precautions, however, she fell back in a state of alarming syncope almost immediately after the first inhalation; both respiration and the heart's action had ceased; her face was deadly pale, and some seconds elapsed before animation returned. Now it is impossible, we conceive, to doubt, on the one hand, that in this case the temporary suspension of the respiratory and circulatory functions—which, if prolonged, would have proved fatal—was due to the sudden impression of the vapour of chloroform upon the nervous centres; or, on the other, that this was intimately connected with some peculiar idiosyncrasy of the patient, such as we have referred to. In other cases, however, we have reason to believe that death may commence primarily at either the heart or the lungs. In the former case, by syncope, from a kind of paralysis of the organ, occasioned by the circulation of the vapour of chloroform through the coronary vessels; in the latter, by asphyxia, induced either by the pungency of the vapour exciting irritation or spasm of the glottis, and so preventing the entrance of air into the lungs, or from the air admitted being so highly charged with the vapour of chloroform as to be irrespirable, or prevent, by its high specific gravity, the exosmosis of carbonic acid from the blood. These cases, however, we regard as the exceptional rather than the usual modes in which death takes place. They would moreover appear to be occasioned by the incautious use of the agent, and are attended with the manifestation of symptoms, such as struggling, suffusion of the face, &c., which, if properly attended to, would enable us to avert serious consequences. We repeat, it is our conviction that in the great majority of cases in which death follows the administration of chloroform, it is due to the suspension of the respiratory functions through the narcotic or benumbing influence of the agent upon the brain and nervous centres, and more particularly upon the ganglia immediately subservient to respiration; and we would add, that we are supported in this opinion by the following facts: —1st. That in a great number of experiments and observations made upon the lower animals, the heart was found to be irritable and contractile for some time after respiration had ceased; and 2ndly. That many were restored to life from an apparently hopeless state of suspended animation by steadily and perseveringly maintaining the respiratory movements by rhythmical compression of the chest. Nay, more, it fell to our lot to have occasion to test the value of this proceeding in the case of a lady in whom respiration and the heart's action had both stopped from the incautious administration of chloroform, and with the most perfect success. Our practical conclusion, therefore, would be, that safety in the administration of chloroform is to be mainly ensured by giving it, in the first place, most moderately, cautiously, and well diluted with atmospheric air, until the nervous system has, as it were, become accustomed to its influence; and 2ndly. By observing closely its action upon the brain and nervous system, the heart's action, and the respiration, until we are satisfied that no peculiar idiosyncrasy exists on the part of the patient to endanger its employment. Assured upon these points, the next precautionary measure to be adopted is to watch closely the respiratory movements during the further use of the agent, as the most certain key to any threatened danger, resorting immediately to artificial respiration whenever the function appears to be embarrassed or suspended. As already stated, we believe that this can be best accomplished by the persevering employment of rhythmical compression of the chest, so that the contained air may be expelled on compression, and a fresh supply drawn in by the elastic recoil or expansion of the walls of the chest when the compressing force is removed. This proceeding requires at least no accessory or complicated apparatus; it can be instantly resorted to; and looking to its success in the several cases in which we have tried it, we have great confidence in recommending it to the notice of the profession.

With these observations we conclude our notice of the second volume of Dr. Simpson's obstetric works. It has been necessarily partial and fragmentary; but in making our selection of the topics to be discussed, we have dwelt chiefly upon those which were of a practical nature, and therefore most likely to interest the practical reader. It would be impossible, in an article like the present, to give anything like a complete exposition of the many subjects treated of in it; and we must therefore refer our readers to the work itself for a just appreciation of the many interesting facts and laborious researches which are embodied in its pages. It is difficult to over-estimate the value of such writings, whilst we are unwilling, at the same time, to indulge in the language of flattery or adulation; but dispassionately considered, and impartially estimated, we venture to believe that their intrinsic merits are such as will carry their author's name down to the latest posterity as one of the most zealous and indefatigable cultivators of the obstetric art.

Review V.

1. *Report of the Committee for Scientific Inquiries in relation to the Cholera Epidemic of* 1854. Presented to both Houses of Parliament by command of her Majesty. 8vo, pp. 129. *Appendix to Report of the Committee for Scientific Inquiries in relation to the Cholera Epidemic of* 1854.—*London.* 8vo, pp. 322.

2. *Report of the Commissioners appointed to Inquire into the Causes which have led to, or have aggravated, the late outbreak of Cholera in the towns of Newcastle-upon-Tyne, Gateshead, and Tynemouth.* Presented to both Houses of Parliament by command of her Majesty.—*London,* 1854. Folio, pp. 580.

3. *Report made by Dr. Milroy to the Colonial Office on the Cholera Epidemic in Jamaica*, 1850–51, &c. Ordered by the House of Commons to be printed, May 11th, 1854.—*London.* Folio, pp. 147.

4. *Report on the Cholera which attacked the Fleet in the Black Sea in August*, 1854, *more particularly as relates to her Majesty's ships "Britannia," "Albion," and "Trafalgar."* Printed by order of the Right Hon. the Lords Commissioners of the Admiralty.—*London.* Folio, pp. 15.

5. *Reports relating to the Sanitary Condition of the City of London.* By JOHN SIMON, F.R.S., Surgeon to St. Thomas's Hospital, and Officer of Health to the City of London.—*London*, 1854. pp. 312.

6. *Report on the Cholera Epidemic of* 1854, *as it prevailed in the City of London.* By JOHN SIMON, F.R.S., Medical Officer of Health to the City of London, &c.—*London*, 1854. pp. 18.

7. *Third Annual Report of the Commissioners for Administering the Laws for Relief of the Poor in Ireland under the Medical Charities Act*, 14 *and* 15 *Vic. cap.* 68. Presented to both Houses of Parliament by command of her Majesty.—*Dublin*, 1855. pp. 381.

8. *Report of the Commissioners of Health, Ireland, on the Epidemics of* 1846 *to* 1850. Presented to both Houses of Parliament by command of her Majesty.—*Dublin*, 1852. pp. 79.

9. *Appendix* (*C.*) *to the Report of the General Board of Health on the Epidemic Cholera of* 1848 *and* 1849. *Abstract of Report by* JAMES WYNNE, M.D., *on Epidemic Cholera, as it prevailed in the United States in* 1849 *and* 1850. Presented to both Houses of Parliament by command of her Majesty.—*London*, 1852. pp. 93.

10. *Report upon the Sanitary Condition of New Orleans.* By E. H. BARTON, M.D. (Forming an Appendix to *Report of the Sanitary Commission of New Orleans on the Epidemic Yellow Fever of* 1853. Published by authority of the City Council of New Orleans.) pp. 249.

11. *Untersuchungen und Beobachtungen über die Verbreitungsart der Cholera, nebst Betrachtungen über Massregeln, derselben Einhalt zu thun.* Von Dr. MAX PETTENKOFER, Professor der Medizinischen Chemie an der Universität zu München.—*München*, 1855. pp. 374.

Investigations and Observations on the Propagation of Cholera; with Remarks on the Precautions necessary to Arrest its Progress. By MAX PETTENKOFER, Professor of Medical Chemistry at the University of Munich.

12. *Report on the Cholera Outbreak in the Parish of St. James's, Westminster, during the Autumn of* 1854. Presented to the Vestry by the Cholera Inquiry Committee, July, 1855.—*London*, 1855. pp. 175.

13. *Report on the last Two Cholera Epidemics of London, as affected by the Consumption of Impure Water.* Addressed to the Right Hon. the President of the General Board of Health, by the Medical Officer of the Board.—*London*, 1856. pp. 35.

14. *Report on the Outbreak of Epidemic Cholera in Brecon in* 1854. By PRESTWOOD LUCAS, M.D.—*London*, 1855. pp. 27.

15. *On Animal Decomposition as the Chief Promotive Cause of Cholera.* By HENRY HARTSHORNE, M.D. (From the 'Medical Examiner,' August, 1855.)—*Philadelphia*, 1855. pp. 12.

16. *Memoir on the Cholera at Oxford in the year* 1854. By HENRY WENTWORTH ACLAND, M.D., F.R.S., F.R.G.S., Physician to the Radcliffe Infirmary, and Lee's Reader in Anatomy in the University of Oxford, &c. — *London*, 1856. 4to, pp. 172.

IN a former article* we proposed to inquire into the history and origin of cholera, with the twofold intention of placing prominently before our readers those facts in its history which seem to indicate an indigenous rather than an exotic origin to this pestilence, and of pointing out the circumstances under which it has prevailed. Upon the subject of contagion we did not enter, because its propagation by means of human intercourse, in the manner in which small-pox, scarlatina, or measles are propagated, has never been generally believed, and its history is adverse to the supposition that its spread as an epidemic is in any considerable degree, if at all, attributable to human intercourse. After stating reasons which lead us to infer that cholera is not altogether new to Great Britain, and adducing some instances which seem to prove that it has, at least occasionally, arisen irrespective of the introduction of a poison from abroad, we next proceeded to inquire into the circumstances under which epidemic outbreaks have occurred. These were found to be referrible to two chief heads: "seasonal or meteorological conditions," and "localizing causes." Of these necessary factors in the causation of cholera, termed by Dr. Barton, the "two blades" of "the shears of fate," we had space only for the consideration of the former. As regards this country, it appeared that certain meteorological phenomena which, in the aggregate, borrowing an idea from the older physicians, we termed the pestilential constitution of the year, have mostly accompanied outbreaks of cholera. Allowing for difference of climate and situation, it was also found that the atmospheric conditions under which cholera has usually prevailed abroad, have been almost identical. These conditions were found to be, a somewhat variable but elevated temperature, a still and peculiarly oppressive state of the atmosphere—more oppressive than the simple elevation of the thermometer can account for—conjoined with a certain degree of moisture. Such climatic conditions are rarely, if ever, confined to a limited locality. Situation may aggravate them; lowness of level, or the ill-arrangement of streets or blocks of buildings may add to their force; but in general, the result of such local circumstances upon local climate excepted, when these seasonal conditions exist in one place, they must be likewise present in many others. Yet of such places as partake in the same seasonal and meteorological influences, some usually escape an epidemic visitation at the very period when others in the immediate vicinity are suffering severely from its presence. Even in the same town, whilst the inhabitants of some streets or courts are being decimated, those dwelling in others not far distant altogether escape; or, as frequently happens, the inmates of certain houses suffer severely, whilst their neighbours are entirely spared.

* British and Foreign Medico-Chirurgical Review, vol. xvii. p. 286.

Thus, from Mr. Simon's Official Reports to the City Commissioners of Sewers, we learn that the cholera epidemics of 1849 and 1854 fell with unequal force upon different localities.* For example, the cholera-mortality in 1849 was 19 in the 10,000 in the north-west sub-registration district of the City of London Union, and 47 in the 10,000 in the Cripplegate sub-registration district. These districts are at the same level with each other, and with the adjoining Hackney-road division of Bethnal-green, in which the mortality was as high as 110 in each 10,000 persons.† A minuter investigation shows still more remarkable differences; for in certain districts of the City the mortality from the several species of alvine flux was in the same year extremely small, in others excessively large. Thus, in Cordwainers', Coleman-street, and Aldersgate-within Wards, out of resident populations estimated as amounting to more than 7800 persons, there occurred only 4 deaths from cholera; whilst in a band of two or three hundred yards' width northwards from Blackfriars Bridge, "in the parallelogram which lies along the main road from Stonecutter-street to Bridewell Hospital, were 76 deaths; . . . in the little clump of houses forming the angle of Farringdon-street and Holborn-hill, were 17 deaths; . . . in a square space behind twenty-seven shopfronts in Fleet-street were 57 deaths; . . . . and lastly, in the small parish of St. Ann's, Blackfriars, the deaths were at the rate of 25 to every thousand of its population." The mortality from cholera in the City in 1854 varied from 8·90 per 10,000 inhabitants in the north district of West London, and 8·57 per 10,000 in the north-east division of the City Union, to 23·32 per 10,000 in Cripplegate. Differences these sufficiently remarkable, and evidently not referrible to the epidemic constitution of the atmosphere alone, but to be explained only by the presumed existence of some special circumstances in the localities themselves or their inhabitants. Moreover, seasons presenting all the characters which conjointly form what we have termed the pestilential constitution,‡ have without doubt existed very often when there has been no accompanying pestilence. During the century and a quarter that England was free from epidemic pestilences, many such seasons must have occurred; and in tropical climates they exist in ordinary years. In his fourth recapitulatory proposition, Dr. Barton says that the atmospherical cause of pestilence is annually more or less present at New Orleans, yet neither yellow fever nor cholera are annual visitants to that city. Another co-efficient, at least, is therefore required in order to give character and energy to the seasonal conditions which favour the development of cholera. This is what has already been alluded to as the terrene element of Dr. Barton, and corresponds with what have been termed the localizing causes of cholera. That it is strictly local is further evidenced by the fact that an analysis of the history of cholera-epidemics shows them to be most frequently made up of a succession of partial local outbreaks, and this not only as regards different districts, but even the same place. On the other hand, it has often occurred that the pestilence has lingered in some few favourite haunts throughout the entire course§ of an epidemic; and

* Reports, pp. 231, 167, and 96.

† Registrar-General's Report on the Mortality of Cholera in 1848–9, p. clxvii.

‡ British and Foreign Medico-Chirurgical Review, vol. xvii. pp. 296–7.

§ See Dr. Acland's Memoir, p. 29.

now and then, after visiting a place at the commencement of a visitation, it has returned to it again, after an interval of complete immunity, before its close.

The tendency of cholera to return at a subsequent visitation to the same towns, parts of towns, and even houses, which had been formerly affected by it, affords additional proof, if it be necessary, that local circumstances have at least great influence in determining its seat. For example, the earliest case of cholera in Chelsea in 1848 is said to have been in White Hart-court, and there it continued to exist until the end of the epidemic in 1849. The first case in 1854 was in the same place, perhaps also in the same house, for deaths occurred in the same house in both visitations. A very similar fact is presented by Augusta-court,* in which the three earliest fatal cases of cholera in Chelsea occurred in February 1832, and which being revisited in 1854, continued to furnish victims to the pestilence throughout the entire duration of the outbreak. Kent-street† and Mint-street, Southwark, which were severely visited at an early period of the last epidemic, were also amongst the first seats of cholera in 1832. We also learn from Dr. Acland's very valuable 'Memoir on the Cholera in Oxford in 1854,' that, with one exception, every yard and street in St. Thomas's parish, which had been attacked by cholera in 1832 and 1849, was revisited in 1854. (p. 39.) Thus, whatever other conditions may be necessary to the development of cholera, it is evident that some local circumstance plays a very important part in its evolution. Into the nature of this local element we now propose to inquire, with the aid of the works before us, using at the same time such facts as have fallen under our own observation. As our purpose, however, is not to suggest a theory of the causation of cholera, and then to select only those facts which seem to afford it countenance, we must prepare the way for this investigation by first of all referring briefly to the chief theories that have been suggested to explain the operation of local conditions in cholera; and secondly, by examining the several circumstances which have been set down as "its determining local conditions."

The existence of local causes of insalubrity is almost universally considered necessary for the evolution of a cholera-epidemic, although very great diversity of opinion exists as to the part they bear in the production of the pestilence. By most persons, the unwholesome conditions to which the dwellers in unhealthy districts are habitually exposed, are believed to produce a low tone of the general health, and proclivity to disease, which disable them from resisting the exciting cause of the epidemic. Dr. Carpenter‡ surmises that these influences, and also other causes of a more personal nature, produce a condition of the blood itself which predisposes it for zymotic action, the precise character of which depends upon the nature of the exciting cause with which it is brought into relation,—the special poisons of small-pox, scarlatina, typhus, or cholera, for example, being each capable of exciting

* Cholera Gazette (published by authority of the Central Board of Health), p. 205. London, 1832.

† Loc. cit., pp. 121, 156, 218.

‡ British and Foreign Medico-Chirurgical Review, vol. xi. pp. 161–177.

its peculiar fermentation in blood already charged with organic compounds in a state of retrogressive change. Without entering upon the general question so ably argued by Dr. Carpenter, we may be permitted to doubt whether cholera be a zymotic disease in the sense here intended, whether the action of its exciting cause be not rather simply toxical, and, if fermentation has any share in its production, whether this does not occur externally to the organism, and produce rather the exciting cause than the disease itself. Moreover, that persons arriving from a pure atmosphere, and in sound health, have so frequently shown themselves peculiarly prone to suffer from a brief exposure to the epidemic influence, is altogether at variance with the opinion that the supposed condition of the blood is a necessary predisposition for cholera.

The opinions propounded by Drs. Barton and Pettenkofer, whilst they differ materially from each other, are just the converse of those already referred to, since they seem to infer a simply toxical action on the part of the poison of cholera. Dr. Barton considers epidemic pestilences as the direct consequences of the co-operation of certain meteorological conditions with a local cause. This local cause he believes to be "filth, moisture, and stagnant air," and especially the emanations arising from extensive upturnings and exposure of a soil impregnated with the results of organic decomposition. The efficacy of such disturbances of the soil in the production of outbreaks of epidemic disease, is illustrated by a chart exhibiting the mortality per thousand of New Orleans for each year from 1787 to 1854, together with the presumed cause of the excessive mortality which occurred in several of these years.* This very remarkable and valuable document clearly shows how a large mortality, and especially a prevalence of epidemic pestilence, has uniformly accompanied any extensive disturbances of the soil for the construction of canals, pavements, or other public works. In 1832, the most fatal year of the series—when the deaths from cholera amounted to 78·78 in each 1000 persons living, and the gross mortality to 147 in 1000, or upwards of one-seventh of the entire population—there had been extensive digging for the foundation of a street in the preceding autumn, followed in the year itself by similar diggings for the formation of a canal and pavements, regardless of the season. In the latter part of October, 1848, two canals were cleaned out, and two acres of ground were excavated, with the removal and exposure of upwards of 336,000 cubic feet of earth, for the foundation of the new Custom House in the heart of the city. This work lasted until August, 1849, and during the period of the exposure of this mass of soil, saturated with the impurities of the swampy city, there was a severe epidemic of cholera, with a mortality of 3600. In fact, in every year in which an epidemic outbreak, whether of yellow fever or cholera, has occurred, a similar exposure of the soil had previously taken place, the precise form of epidemic being, in Dr. Barton's opinion, determined by the meteorological phenomena of the season. Thus, Dr. Barton evidently considers cholera to be caused by a poisonous miasm, and believes this miasm to be altogether of indigenous origin. Dr. Pettenkofer, on the other hand, believes that the introduction of a ferment from without is necessary for the production of cholera, but thinks that

* Report on the Sanitary Condition of New Orleans, pp. 280, 447, 461.

this ferment can only act where it meets with suitable local conditions. Whilst Dr. Carpenter believes that the foulness of localities taints the blood of persons exposed to inhale their emanations, and thus produces in them a personal predisposition for zymotic disease, Dr. Pettenkofer is of opinion that the special "leaven" sets up a zymosis, or series of decompositions, in the impure soil itself, and that the special poison of cholera is a miasm generated by this earthy fermentation. Whilst he considers the presence of a special ferment as essential to the production of a cholera-epidemic, he also insists upon the existence of certain local peculiarities. These consist in a damp subsoil, sufficiently porous to be penetrable by the decomposition products of human and animal excrement. It is only in such soil, thoroughly impregnated with this peculiar organic matter, that the special cholera poison is generated. Hence Dr. Pettenkofer says the susceptibility or insusceptibility of towns for a cholera-epidemic is in exact proportion to their "soil relations." The difference between the mortality from cholera in the upper and lower terraces of London is hence attributable to the dry gravelly soil of the former, which naturally allowed all the matters for decomposition to gravitate towards the moist closer soil of the lower levels, where it underwent a much slower decomposition. Entertaining the belief that cholera has never prevailed epidemically upon rock, Dr. Pettenkofer readily accounts for the supposed fact on the ground that the excrement cannot penetrate into the soil, and that the rock neither takes up nor gives off moisture. Single cases may, he says, occur in towns or houses whose foundation is rock, but epidemics never; referring to some alleged cases which seem opposed to this opinion, he says the exceptions are more apparent than real. In this respect, however, Dr. Pettenkofer is mistaken, for cholera has occurred sufficiently often on rock to prove that at least the porous soil to which he attaches so much consequence, is not a necessary element in the production of cholera. Arguing from this presumed fact, Dr. Pettenkofer confidently asserts that we must abandon all idea of the air and water as the nidus of cholera, and seek for it in the soil alone.*

The ferment supposed by Pettenkofer to be necessary to set up the peculiar decomposition of which the cholera poison forms one of the products, is the matter of the dejections of cholera patients. His notion is, that the cholera-germ-bearing excrement which spreads itself in the damp porous soil, already impregnated with fæcal matters, produces, by means of the fine division which it there undergoes, such a modification in the process of putrefaction and decomposition, that, in addition to the gases usually formed, a cholera miasm is produced which becomes diffused through the atmosphere of dwellings, in common with other exhalations. Thus, although the cholera miasm is formed in the ground, the air is the vehicle for its transmission to the patient. Dr. Pettenkofer adduces several instances in which he supposes cholera to have been imported by means of the dejections of persons suffering either from diarrhœa, cholerine, or cholera, for he views these diseases as mere varieties, and infers that if the dejections of cholera patients be capable of originating the pestilence, those of persons suffering from either of the milder complaints most probably produce the like result. The most circumstantial account

* Pettenkofer, pp. 104, 110, 37, 38.

of the introduction of cholera by the supposed means given by Dr. Pettenkofer, refers to the convent prison of Ebrach. Here both the male and female prisoners were attacked, whilst the officials, a company of soldiers quartered there as a guard, and the inhabitants of the adjoining village, entirely escaped. On inquiry, it was found that the first person attacked by cholera was a prisoner who, passing through Munich on his route to Ebrach, was lodged in the prison there, among the inmates of which there had already been several cases of cholera. Arriving at Ebrach on August 20th, he was placed in separate confinement with three other prisoners, and supplied with prison clothes. In a few days after his arrival he reported himself sick with diarrhœa, from which he had also suffered at the period of his departure from Munich, was admitted into hospital, and suffered an attack of cholera, from which he rapidly and completely recovered. On August 27th, the person who attended him during his illness, took cholera and died, and in a few days the epidemic extended throughout both the male and female divisions, between which there had been no intercommunication excepting through the officials and the guard, all of whom, as already related, continued healthy during the entire course of the epidemic. The first case among the female prisoners was that of a woman who had washed the linen of the male prisoner on the day after his arrival from Munich, before cholera had developed itself in him, and several days before he reported himself sick. This woman passed through the disease in the milder form of cholerine, and, like the male prisoner, rapidly recovered. None of the three prisoners with whom the man was confined prior to his illness were attacked. There were in the prison six hundred male prisoners, arranged in classes, between which there is little communication, yet the disease showed itself speedily throughout all parts of the prison, reached its climax in the men's division on September 11th, in the female division on September 13th, and then declined, having carried off about ten per cent. of the prisoners. From this history it is inferred that the disease had been introduced by the prisoner from Munich, and from him been disseminated throughout the establishment. Dr. Pettenkofer, however, says that the disease was not propagated by contagion, no clue to its spread by means of personal intercourse having been elicited by the most careful inquiry. The three prisoners with whom the first patient was originally confined could not aid in the propagation of the disease, both because none of them personally suffered from the epidemic, and because they were not liberated from their isolated confinement so as to mix with the other prisoners until after the disease had become general. The large cesspools in the garden, into which the stools of the already infected prisoner from Munich had passed, and the badly-arranged necessaries of the women's division, into which all their dejections were emptied, are considered by Dr. Pettenkofer as having formed the centres of infection, from which, by means of the fermenting process set up in the excreta brought to these points, the exciting poison of the disease was distributed throughout all classes of prisoners.* Elsewhere, Dr. Pettenkofer says that the most intimate communication between places may occur, without leading to the introduction of cholera; while, on the contrary, this disease has often

* Loc. cit., pp. 123 et seq.

broken out in places whose communication with the cholera sick could not be demonstrated. The former fact he explains on the supposition that the requisite soil-relations were wanting. In Munich, the outbreak of cholera was preceded for a full month by a general prevalence of diarrhœa and summer cholera of such a character that, had epidemic cholera been present, they would have been attributed to the epidemic cause, and been viewed as mild cases of cholera. Out of five hundred officials employed in the Industrial Exhibition, very few escaped this simple disease; and although it would seem that all recovered, since the first death recorded as occasioned by cholera took place on July 29th, some of them were so severely prostrated by the disorder as to be with difficulty removed home. In the three first cases of developed cholera in Munich, no intercourse with cholera patients, or with persons who came out of neighbourhoods in which the epidemic was raging, could be demonstrated, neither was there any ground for supposing this to have been the case. Do not these facts, like similar ones in this country formerly referred to,* point rather to the spontaneous production than the extrinsic origin of cholera?

Dr. Pettenkofer is well read in the writings of English authors on cholera, and refers to them on several occasions in his work, which forms a portion of the Report of a Commission appointed by the Minister of the Interior to conduct scientific investigations into the Indian cholera. We cannot help surmising that he has, perhaps unintentionally, borrowed his idea of the agency of the cholera dejections in the production of cholera from our fellow-countryman, Dr. Snow; but that, not finding the Doctor's views to accord with the history of cholera, he has, in common with Thiersch—who also attributes the propagation of cholera to the rice-water stools of cholera patients, in a state of fermentation—essentially modified the original suggestion. Although we disbelieve Dr. Snow's theory, we are firmly of opinion that to him of right belongs all the credit that may attach to the suggestion, that the evacuations of cholera patients are either directly or indirectly the means of spreading this disease.

The several opinions we have cited, however much they differ in other respects, agree in considering some local condition or other as necessary for the production or development of cholera, save only that Dr. Carpenter believes the predisposition to zymotic disease—and he considers cholera as a zymotic disease—may be induced by personal as well as by local causes. Dr. Pettenkofer's view of the nature of the local causes of cholera is sufficiently definite and simple, and to it we shall have no further necessity to refer. With these exceptions, nothing can well be more vague and unsatisfactory than the opinions that have been usually expressed as to the nature of the localizing causes of cholera. Unmindful of the proposition, that every effect must spring from some definite cause, it has been common with sanitary inquirers at once to refer the same effect to several causes, and several effects to the same cause, instead of endeavouring to trace each result to its proper origin. Thus it has frequently been said, that cholera and fever arise, or are localized, by the same causes, run in the same track, and haunt the same localities.† First, we believe,

* British and Foreign Medico-Chirurgical Review, vol. xvii. pp. 289, 291.

† Report of the General Board of Health on the Epidemic Cholera of 1848–9, pp. 20–22.

promulgated in this country, this assertion has been repeated by Pettenkofer and other foreign authorities. No doubt fever and cholera do very often visit the same localities, and prevail amongst the same classes of persons; but this arises from the co-existence in such cases of the conditions incidental to both diseases, and not from both being the normal consequence of the same conditions. There are many localities in which fever is rife, that cholera has passed over. There are others in which cholera has been prevalent, but fever a rare visitant. It would, indeed, be easy to point to others which have been severely visited by both; but it is almost unnecessary to observe, that this fact points to no necessary connexion in the ætiology of the two diseases. These assertions, the result of personal observation, are strikingly confirmed by Mr. Simon, who thus expresses the result of his very wide experience during the cholera epidemic of 1849, in his Second Annual Report:

"On the one hand, it is unquestionably true that many habitual seats of fever were visited by cholera; on the other hand, many of the worst fever nests in the whole metropolis were unaffected by it; and it struck with extreme severity in a class of houses habitually exempt from fever. See, for instance, how malignantly it prevailed along the line of Farringdon and New Bridge-streets, and in Fleet-street and Ludgate-hill, where their line intersects that just mentioned; and here, you will observe, not only in those obscure and ill-ventilated courts and by-ways where fever is the familiar visitant of a hungry and crowded population; but also, and very strikingly, in spacious and airy houses situate along the main thoroughfare of the City, and inhabited by opulent tradesmen, by members of the various professions, or by officers of assurance companies." (p. 94.)

Neither is it true, as has frequently been affirmed, that cholera has almost exclusively visited such places as are liable to frequently-recurring attacks of other epidemic or zymotic diseases, and the death-rate whereof is high. A very trustworthy and remarkable example of the contrary is afforded by the "cholera area" of St. James's, Westminster, the particulars of the outbreak in which are so admirably described in their Report by the committee nominated by the vestry to investigate the history of that sad visitation. This district, although one of the most densely peopled in London, has been found, on a careful inquiry into its mortuary statistics for the preceding seven years, exclusive of the few days of epidemic visitation in 1854, to have sustained an annual mortality of only twenty and a half in the thousand, of which less than one twenty-second part was occasioned by zymotic disease.* "It likewise deserves mention, that, of the 537 cholera deaths of the late epidemic, 323 occurred in houses which, during the past seven years, had suffered no deaths from other zymotic disease."

Amongst the local conditions that have been supposed, almost in an equal degree, to develope an outbreak of cholera, poverty; the over-crowding of houses; defective house ventilation; want of cleanliness; dampness; impure water; lowness of site; the effluvia from the decomposition of the various organic *débris* allowed to collect in poor and neglected localities; the emanations from human and animal excrement, whether accumulated into cesspools, or allowed to rot in foul drains or lay-stalls; the malaria from fetid ditches; the miasms from City grave-yards; and the stinks from knackers' yards, bone-crushing, catgut-spinning, and

* Report of Committee for Scientific Inquiries, p. 50.

other offensive establishments, have been insisted on by different authorities.* That each and several of these circumstances have been found associated with cholera is an unquestionable fact. That most of them are by no means its necessary adjuncts is not less true. Whilst cholera has not spared the thinly-occupied houses of wealthy persons, it has very often left harmless some of the most densely-crowded cottages of the poor; and whilst it has gathered victims from the main streets, it has occasionally left untouched the courts and back slums in which offensive accumulations are allowed to form, and offensive trades are wont to be carried on.† Witness, for example, the prevalence of cholera in 1849 amongst the families of the respectable and wealthy tradesmen, professional men, and others, mentioned by Mr. Simon as occupying houses in Ludgate-hill, Farringdon-street, and the neighbourhood. It would be easy, did our space allow, to cite many illustrations of the same fact that have fallen under our own observation, as well as of its opposite; of localities notorious for the co-existence of several of these insalubrious agencies, which have remained unvisited at times when the epidemic was raging in their neighbourhood. We may therefore set aside most of the presumed local causes as in no degree necessary to the development of cholera, and concentrate our attention upon impure water, lowness of site, and the emanations arising from the decomposition of animal refuse.

That impure water has a powerful influence over the intensity of cholera outbreaks is unquestionable. In his Report to the General Board of Health on the cholera epidemic of 1849, Dr. Sutherland says, that the injurious effect of unwholesome water had been manifest in nearly every affected place; and adds, that "a number of most severe and fatal outbursts of cholera were referrible to no other cause except the state of the water supply," and this especially where "the water was obtained from wells into which the contents of sewers, privies, or the drainage of graveyards had escaped."‡ Since that time much additional evidence of a confirmatory character has been collected. Two examples are recorded by Dr. Acland, in his valuable and interesting 'Memoir on the Cholera in Oxford;'§ the parish of St. Clement's, which suffered a large mortality in 1832, when the inhabitants had filthy water from a sewer-receiving stream; and an insignificant mortality in 1849 and 1854, when the water was derived from a purer source. The other case is that of the county gaol, in which cases have occurred in every epidemic; whilst the city gaol, which is not far from the other, has uniformly

* See Report on the Cause and Mode of Diffusion of Cholera. By W. Baly, M.D., F.R.S., &c. drawn up by desire of the Royal College of Physicians, pp. 16–20.

Report of the General Board of Health on the Epidemic Cholera of 1848–9, pp. 36–60; also Dr. Sutherland's and Mr. Grainger's Reports, forming Appendices A and B to the above.

Registrar-General's Report on Cholera in England in 1848–9, pp. 57–70.

Sanitary Condition of the City of London. By J. Simon, F.R.S., &c., p. 234.

† Green-street, Southwark, contained, at the time of the cholera epidemic of 1854, one knacker's yard, two bone-boiling and crushing establishments, besides a large catgut factory, the combined smell from the whole being most sickening, yet only one death from cholera occurred among its inhabitants, and no unusual number in the streets immediately around it. Compare this with Suffolk-street, also in Southwark, where deaths occurred in the houses of twenty respectable tradesmen.

‡ See Appendix A to the Board of Health Report on the Epidemic Cholera of 1848–49, pp. 14–16.

§ Dr. Acland's Memoir, pp. 51–52.

escaped. The only apparent difference between the two establishments in 1854, seems to have been that the supply of water for the use of the county gaol, and of which the soup and gruel were made, was pumped from a filthy mill-pool within ten feet of one of the prison drains. No sooner were the supply-pipes disconnected with this impure source, than cholera and diarrhœa ceased. Inquiries into the effect of the water-supply of the southern districts of the metropolis over the cholera epidemic of 1854, were instituted by Dr. Snow and the Registrar-General. These inquiries were, however, necessarily imperfect and inconclusive, for the local registrars were unable to return the source of the water supply in 803 out of 4059 fatal cases in houses supplied by the Lambeth and Southwark and Vauxhall Companies;* and Dr. Snow's laborious inquiry, which was limited to the early period of the epidemic, ceased before the disease had reached its height. A yet more elaborate and perfect inquiry was made by the General Board of Health at the close of the visitation, the results of which are given in the Report of the Medical Officer of the Board. These results are especially valuable, because they refer to two large sections of the population, residing in the same localities, "breathing the same atmosphere, comprehending the same classes, and averaging the same habits of life;" in short, placed in circumstances nearly identical, save that the one section, comprising a population of about 268,171 persons, drank impure water; whilst the other, numbering about 166,906 persons, used a clearer and comparatively pure water. The mortality from cholera among the drinkers of impure water—of water impregnated with the sewage of the metropolis, and containing in solution a large quantity of saline matter, derived from the intermixture of sea-water†—being at the rate of 130 to every 10,000, that of the drinkers of the purer water being only at the rate of 37 to every 10,000 persons living.‡ This evidence is greatly strengthened by being placed side by side with the mortuary statistics of the epidemic of 1848–9 in the same district, and by a comparison of the nature of the water supply on both occasions. The Lambeth Company, which in 1854 gave the pure water, supplied in 1848–9 even a worse water than the Southwark and Vauxhall Company. From the figures already quoted we learn that the population to which the Lambeth water was distributed in 1853–4, suffered a mortality less than one-third of that sustained by the drinkers of the water purveyed by the Southwark and Vauxhall Company. From a comparison of the mortality in the two epidemics, it appears that the tenantry using the purer water supplied by the Lambeth Company in 1853–4, suffered not a third as much as the same tenantry had done in 1848–9, when the water was impure. "On the other hand, the Southwark and Vauxhall Company, which pumped an impure water in 1848–9, pumped even a worse water in 1853–4;" and in consequence, notwithstanding "the general metropolitan pressure of the epidemic in 1853–4 was considerably lighter than in 1848–9, the houses supplied by the Southwark and Vauxhall Company in the late epidemic

* Weekly Return of Births and Deaths, vol. xv. p. 515.

† See Dr. Dundas Thompson's Report on the Chemical Composition of Metropolitan Waters during the Year 1854, in Appendix to Board of Health Report.

‡ Report on Impure Water, pp. 6–9.

suffered probably ten per cent. higher mortality than the same houses in 1848–9;" "the comparison of the two populations in the two epidemics stands thus:—In the one population (notwithstanding a generally lighter invasion of the disease) the cholera death-rate rose from 118 to 130; in the other it fell from 125 to 37.

But large as is its influence over cholera epidemics, impure water is not a necessary factor of cholera. The drinkers of the purer and comparatively uncontaminated water of the Lambeth Company in 1854 sustained a considerable mortality; and cholera has often prevailed with great malignancy in districts where the drinking water was perfectly free from fæcal contamination, as in the parish of Tynemouth, in 1849, and in several places named in Dr. Baly's Report.* It follows, therefore, that impure water is either only an accidental and occasional vehicle for conveying the poison of cholera into the system; or that, just as unwholesome food or the injudicious use of purgatives are determining or aggravating causes of cholera during an epidemic visitation, so is water impregnated with organic impurities. Probably, impure water acts chiefly, if not exclusively, by aggravating individual cases of the pestilence, causing such as might otherwise have been cases of simple diarrhœa, to pass rapidly into the state of "collapse." A careful consideration of the history of the sudden and severe outbreak in the Golden-square and Berwick-street districts of St. James's, Westminster, in 1854—which was apparently connected with the dietetic use of water from the Broad-street pump, found at a later period to have been vitiated by the leakage from a cesspool—appears to support this supposition. It should, however, be remembered when studying this visitation, that it occurred just when the pestilence was at its height. A similar, though less violent, outbreak occurred simultaneously at Rotherhithe; and several smaller districts which had previously escaped, also suffered at that period; thus showing that the epidemic influence was at that time most general—perhaps, also, most intense. Of the earlier cases in this memorable outbreak, it is reported "that premonitory diarrhœa was of short duration, or altogether absent." It is also said that no "certain information can be collected as to the relative amount of diarrhœa." Judging from the tables published in the Appendix to the Board of Health Report, the cases of diarrhœa in this district were fewer than those of confirmed cholera; whereas, according to the calculations made by the Medical Council of the General Board of Health, it appears that, in the metropolis at large, 1310 persons out of every 10,000 were, on the average, attacked by diarrhœa of some severity, whilst 99 only suffered from cholera. This is exclusive of milder diarrhœa, from which it is computed that 2064 in every 10,000 persons living suffered.† An examination of the mortuary statistics‡ gives additional probability to our supposition, for whilst the comparative mortality of cholera and diarrhœa in London, during this epidemic, is found to have been 46 by cholera to 25 by diarrhœa, the deaths in the Berwick-street and Golden-square districts were 477 from cholera to 37 from diarrhœa.§

* Dr Baly's Report, pp. 201–5. Liverpool and Edinburgh also afford good illustrations of severe outbreaks of cholera in towns where it seems impossible for the water to have had any influence.

† Report of the Committee for Scientific Inquiries, p. 10.

‡ Loc. cit., pp. 10, 98.

§ The disparity was even larger than is here represented, for if the deaths of persons who, This discrepancy becomes still more evident when the mortality occasioned by these diseases in the two already-named districts of St. James's, is compared with that in the bordering registration districts. Thus the mortality from cholera in the five sub-registration districts of Hanover-square, Charing-cross, Long-acre, All Souls, and St. Ann, amounted to 303, that from diarrhœa to 151—numbers which pretty nearly accord with the general metropolitan average, and are quite disproportionate to the comparative mortality from the same diseases within the "cholera area" of St. James's, which they surround on all sides.

It was announced by Dr. Farr, in his Report on the Cholera in England in 1848–49, that, as regarded London, "the elevation of the soil has a more constant relation with the mortality from cholera than any other known element; the mortality from cholera is in the inverse ratio of the elevation."* This statement was fully borne out, so far as London is concerned, by the history of that epidemic, and is in the main confirmed by the experience of the late visitation, although the mortality was then found not to be "so invariably in each district inversely as the elevation," as on the previous occasion. This general rule, "that the mortality of cholera is inversely as the elevation of the people assailed above the sea level," is frequently quoted without reference to the explanation of the circumstance offered by Dr. Farr, which does not differ essentially from the opinion of Pettenkofer, that cholera prevails more intensely in the low districts, because all the organic impurities of the higher ground, gravitating thitherward, there undergo chemical action. Although, then, the experience of London, and of many places in England, in 1849, was such as fully to justify the assertion, Dr. Farr elsewhere says, "that cholera will not only be fatal on low ground, but on high ground, if, from any concurrence of circumstances, the conditions exist there which are so constantly found in alluvial soils, lying on a level with, or below, the tidal waters."† Thus, cholera was more fatal in the village of Wrekenton, situated 500 feet above the river Tyne, in 1849, than in the narrow low-lying lanes and alleys of Gateshead which border the river's margin. Out of a population of 700, 100 died in the course of fifteen days.‡ With a single exception, the mortality in New York from cholera, in 1849-50, was greatest in the sixteenth ward, where it produced 778 deaths:§

"And when the reader is informed that this ward mostly occupies very high ground—that it is neither thickly covered with buildings, nor densely populated, he will at once begin to conclude, as others have before him, that it militates strongly against the idea that lowness and dampness favour the prevalence of the cholera. And perhaps no more striking illustration of the necessity of a full and minute knowledge of all the facts, and the danger of judging from a few, could be adduced than this."

taking the disease within the district, died beyond its limits be added to the above, the total deaths, as calculated by the parochial committee, amounted to nearly 700. See Report of the Cholera Inquiry Committee, p. 15.

* Report on Cholera in England, in 1848–9, p. lxi.

† Loc. cit., p. lxx.

‡ See Report to the General Board of Health, on the Sanitary Condition of Gateshead. By Robert Rawlinson, Esq., Superintending-Inspector, pp. 63–70. This Report contains very interesting accounts of the outbreak in Wrekenton, by Mr. Davis, surgeon, of that place, and Mr. Bennett, F.R.C.S., of Gateshead.

§ Dr. Wynne's Report, pp. 91–92.

Professor Davis, from whom Dr. Wynne is quoting, goes on to explain that the district, although reputed as the highest, driest, and most airy portion of the city, is undrained by sewers, and badly supplied with water; that the surface is uneven, and closely underlaid with micaceous slate, which frequently crops out; and that there are pools of stagnant water, which remain until evaporated by the sun. Hull excepted, Merthyr Tydfil, in Glamorganshire, including the outlying village of Dowlais, suffered more severely from cholera in 1849 than any district in the kingdom. The mortality from cholera and diarrhœa conjointly amounted to 1770 out of a population not exceeding 53,000. Merthyr is 500 feet, Dowlais 1000 feet above the level of the sea.* Abroad, cholera has been most fatal at much higher elevations, as at Bogota, 9000 feet above the sea level and several hundred miles from the coast; and at Mexico also, 7100 feet above the ocean. In Jamaica, it was fatal, in Newcastle, Manchester, Moneague, and other places elevated from 2000 to 3000 feet above the level of the sea; thus showing that elevation in itself only influences the disease so far as it affects the local conditions, and verifying the already-quoted opinion of Dr. Farr. The Report of the Registrar-General on the cholera epidemic of 1848 and 1849, is exclusively based upon the mortuary records. At that time, no reliable means existed whereby the number and allocation of cases could be computed. The committee of the Medical Council for Scientific Inquiries endeavoured to procure statistics of the number of cases of cholera and diarrhœa during the visitation of 1854; although, from the late period of the epidemic at which the inquiry commenced, the facts brought before them are incomplete, the very important conclusion has been deduced,—"that the *cholera-leaven*, be it what it may, was scarcely less diffused in the districts that suffered the lowest mortality, than it was in the districts where the disease was tenfold more fatal." We must refer to the Report of the committee for the very interesting facts and reasonings upon which this conclusion is grounded, as we have not space for a full quotation, and the facts are so tersely stated as to be incapable of condensation. It appears clearly, however, that if the mortality per-centage of cases of cholera and diarrhœa was the same in the higher terraces of the metropolis as in the lowest, the comparative number of persons attacked either by cholera or diarrhœa of some severity, would be about 1288 in the 10,000 in the higher regions, and 1741 in the lower regions of London. There are, however, grounds for supposing that whilst the proportional mortality from both diseases was higher than the average in the low-lying districts, it was much below the average in the higher districts, and that thus the "proportional number of persons that were attacked by diarrhœa or cholera in each must have been about 1490 in a myriad of the population."†

Having thus found that several of the presumed localizing causes of cholera are at least not necessary to its existence, since none of them has been found on all occasions to co-exist with cholera, which, on the other

* Registrar-General's Report on Cholera, p. xxxii. Report to the General Board of Health on the Sanitary State of Merthyr Tydfil. By T. W. Rammell, Esq., Superintending-Inspector, p 7. 1850.

† Report of the Committee for Scientific Inquiries, p. 15.

hand, has sometimes prevailed in their absence, it only remains for us to consider the influence of an atmosphere contaminated by the effluvia arising from decaying organic matter. It has long been known that the alimentary canal is very sensitive to such influence. Dr. Cullen long ago remarked,* that the effluvia from very putrid animal substances readily produce diarrhœa—an observation which has been confirmed by later observers. Most probably both the nature of the decomposing matter, and of the transformative process it is undergoing, are important elements in regard to the effect on the human constitution. Certain at least it is that districts in which the most powerful putrid odours tainted the air, have sometimes almost entirely escaped, whilst others contiguous to them have suffered severely.† We have personally taken much pains to investigate the precise conditions which from their more uniform co-existence with it, might be supposed to produce or to aggravate epidemics of cholera. The result of our observations has been that an atmosphere impregnated with the products of fermenting excrement is at once the most obvious and most constant concomitant of cholera. These exhalations were often found, even in a concentrated form, in houses where the existence of any palpable cause of insalubrity would scarcely be suspected, and thus the fact that the pestilence sometimes passing over slums and rookeries, knocked at the door of the comfortable annuitant or the wealthy tradesman, is readily explicable. During the epidemic of 1854, in a portion of the parishes of Chelsea, and of St. Saviour's, and St. George's, Southwark, exclusive of cases in which the notes made at the time of inspection are not explicit, we personally examined into the sanitary state of the houses occupied by 392 families, in whom deaths from cholera had occurred. Out of 2701 persons, 616 had cholera, besides 871 cases of diarrhœa. Four hundred and fifty of the cholera cases proved fatal. The inquiry extended alike to the dwellings of wealthy residents in good streets, as to those of lodgers in the most overcrowded and filthiest alley. Without devoting more space to the subject than we can afford, it would be impossible to convey the evidence in its fulness as it came before us, and tested as it was in every manner that we could think of, but an analysis of the numbers above given shows that the existence of the products of the peculiar decomposition alluded to were evident to the senses at the time of visit in 213 houses. In nineteen of these there were cesspools situated either below the house itself, or in such close proximity thereto that the soil had percolated through into the subsoil below the dwelling. In 220 cases, open privies were either erected against the main wall of the house, or so near to the back entrance as to allow of the emanations from the soil being observable within doors. In seventy-seven instances, branch drains of imperfect construction, having direct communication with the common-sewer, passed underneath houses; or a foul, open ditch; the main-sewer, or a principal branch, in a ruinous condition, was so near to the house as to influence its internal atmosphere.‡ In ninety-two of the houses were

* Cullen's First Lines of the Practice of Physic, by J. Rotheram, M.D., vol. iii. p. 118.

† As in the case of Green and Suffolk-streets, Southwark. See note, p. 70.

‡ For examples of these facts see Dr. Sutherland's Report on Epidemic Cholera in the Metropolis in 1854, pp. 30–34.

untrapped sinks or drains, connected directly with the street sewer, by which the foul sewer exhalations were conveyed to the internal atmosphere. Of the entire number of houses, in only thirty-three were no sources of this atmospheric contamination detected. Not to dilate further upon the precise manner in which the air breathed by the inmates became corrupted with this foul impurity, let it suffice that persons appeared to suffer in proportion to the contamination of the air they breathed by the "privy odour," and that immunity from this appeared to secure immunity from cholera. Strong confirmation of the opinion that the effluvia from decomposing cesspool soil are capable of inducing diarrhœa, is also afforded by a fact which came under our observation in the summer of 1855. Being requested by the General Board of Health to investigate certain alleged outbreaks of choleraic disease in the metropolis, we found that there had been a general outburst of diarrhœa in three or four contiguous streets of Bethnal-green, shortly after the opening of the ground for the construction of sewers, and the consequent disturbance of several cesspools. Whilst the upturned soil was exposed to the air, the atmosphere of the affected streets, never remarkable for its fragrancy, was excessively offensive, and to this cause the prevalence of diarrhœa was attributed, both by the residents and by several official visitors of the district; an opinion much strengthened by the disappearance of the disease as soon as the work was completed, and likewise by entire immunity from similar disease enjoyed by the inhabitants of surrounding streets precisely the same in character to those affected, but in which there had been no disturbance of the soil.

Let us now proceed to compare these observations with those of the several authors whose works are placed at the head of this article. It will be seen that they afford a strong confirmation of their correctness. Mr. Simon, in his Fifth Annual Report to the City Commissioners of Sewers, whilst he appears to consider the introduction of a ferment from without to be requisite for the production of cholera, says—

"The specific migrating power—whatever its nature, has the faculty of infecting districts in a manner detrimental to life, only when their atmosphere is fraught with certain products susceptible, under its influence, of undergoing poisonous transformation. . . . . Through the unpolluted atmosphere of cleanly districts it migrates silently, without a blow: that which it can kindle into poison, lies not there. To the foul, damp breath of low-lying cities, it comes like a spark to powder. Here is contained that which it can swiftly make destructive,—soaked into soil, stagnant in water, griming the pavement, tainting the air—the slow rottenness of unremoved excrement, to which the first contact of this foreign ferment brings the occasion of changing into new and more deadly combinations."*

The similarity of this view to Pettenkofer's, save in the supposition that the cholera dejections constitute the specific ferment, and that the lethal fermentation goes on in the soil, the atmosphere being only secondarily vitiated, cannot fail to occur to all our readers. But, in fact, Pettenkofer affords ample evidence in his Report, of the existence of abundant sources of this atmospheric contamination where cholera prevailed. Thus, after mentioning the inadequate surface drainage of Munich, he states that the

* Sanitary Condition of London, pp. 236, 234.

excrement of the population is collected into brick pits, or removed into cesspools from necessaries situated either within doors or near to the houses. These necessaries communicate with the cesspools by means of untrapped wooden spouts, through which the soil is conveyed, thus allowing a free passage for the gases of decomposition into the interior of houses. In other cases, the only conveniences are night-chairs, the contents of which are emptied into a cesspool, one of which often serves for several houses. The cesspools and pits are most commonly not water-tight, so that the loose earth below is completely penetrated by the liquid portions which there undergo slow decomposition. These cesspools and pits are periodically, but not frequently, cleansed. Pettenkofer estimates the daily production of excrement in Munich at 300,000 pounds, of which, as not more than a tenth part is removed, nine-tenths must putrify immediately around the dwellings of the inhabitants. The "necessary" accommodations of the prison in the old Convent of Ebrach are described as consisting chiefly of wooden night-chairs. Such privies as exist for the use of the prisoners, empty themselves into a stream, which, entering at the women's division, runs through the institution, and passes out of it at the men's side. The privies in the female division are thoroughly bad; the brickwork through all the floors is impregnated with excrement, which has even coloured the external surface brown. "The stink is a pestilential one." The privy-doors on all the stories are close to the entrances of working or sleeping-rooms, whilst the doors of rooms on the opposite side of the female division are similarly situated with regard to the wooden spouts by which the soil is conducted from the necessaries to the ditch. Dr. Pettenkofer elsewhere says, that the effluvia from the excreta entered the prisoners' sleeping-rooms; and, indeed, lays the greatest weight upon its doing so whilst the prisoners were asleep, this being, he says, the period when the organism is least able to resist the poison. At Gaimersheim, a village containing 974 inhabitants, and which enjoys a melancholy celebrity from having been nearly depopulated in times of plague, the ravages of cholera were confined to certain houses, whilst other groups of houses altogether escaped. The population of the houses attacked was 291, of whom 111 were seized by the pestilence, and 80 died. Water, it appears, is so scarce, that brown cesspool liquid is carefully collected, a pool filled with it being preserved so as to be at hand to extinguish fires. Before passing on to other reporters, we must quote another example of the kind of place in which Dr. Pettenkofer found cholera to prevail. It is Traunstein, where the epidemic confined its onslaught to the inmates of nineteen houses, all of which were carefully examined by the Doctor himself. Most of them were in a low-lying, damp portion of the town, and several of them were both damp and so placed that the moisture and impurity with which the subsoil was charged, must gravitate towards them. The necessaries and cesspools were within doors in thirteen of the houses, and although described as in good condition, were unfurnished with stink-traps. In five others, these conveniences were in walled courts close to the houses; in one only in the open air, and entirely separate from the dwelling. In one house only do the necessaries which are within-doors run into a canal, the fall of which is, however, so trifling, that the soil scarcely runs off, unless when the water is unusually high. The houses most severely visited by cholera were, without exception, those in which the cesspools are within-doors.

The "terrene cause," to which Dr. Barton attributes the pestilential epidemics of New Orleans, does not differ materially from the "soil relations" of Dr. Pettenkofer. Dr. Barton, indeed, does not limit the local cause to the single source of impurity, to which we, in common with Dr. Pettenkofer, are disposed to attach so much consequence; but his account of the state of the subsoil of New Orleans, whilst it shows the inhabitants of that city to be the victims of all the evils incidental to a residence in an imperfectly cleansed and undrained town, entirely accords with the other facts we have cited. After saying that the most fruitful cause of bad air is night-soil and town refuse, the Doctor points to the peculiarities of New Orleans, by which those evils common to it and all large cities are greatly aggravated. It is impossible to dig pits of two feet in depth, and in summer, in consequence of the rains, of even a less depth, without coming to water. Cesspools, or pits for the reception of night-soil, are therefore not applicable to the circumstances of New Orleans; and as manure is not required in the cultivation of the land, consisting as it does of fine rich alluvial soil, all the excreta of the population—the annual amount of which is estimated by Dr. Barton at five thousand six hundred tons of night-soil, and about fifty thousand tons of urine—are exposed to undergo decomposition within the city itself, exhaling, he says, "their noxious and poisonous gases to the atmosphere we breathe, absorbed by the water we drink, and contaminating our most private recesses, where, the air being mostly stagnant, it is apt to remain permanently."*

After reading Dr. Barton's account of the filthy state of New Orleans, it is easy to understand why the "upturnings" of so polluted a subsoil have always been precursors to an outbreak of pestilential disease. We cease to wonder, either at the frequent and terrible visitations of pestilence to which the inhabitants of this unhappy city have been exposed, or that the death-rates—which, on a sixty years' average, have exceeded 59 in the 1000—should, in sickly seasons, have attained to 89, 102, and even to 141 in each 1000 living inhabitants. It is not unworthy of note that upturnings of the subsoil during seasons of epidemic visitation have seemed to be attended with injurious results in this country. Cholera was peculiarly rife in several parts of this metropolis in 1854, where the ground was being excavated for sewers, and in several districts both the local authorities and the public blamed the works then in progress for severe local outbreaks.

Dr. Milroy's Report affords ample information on the nature of the localities in which cholera prevailed in Jamaica. The dwellings of the negroes, from which the fresh air was most carefully excluded, are described as wretched sheds, destitute of the most ordinary conveniences, and receptacles of the most disgusting filth. The medical men repeatedly speak in their reports of the virulence of the disease being mainly due to the condition of the patients' dwellings, and of the violence of the attacks being proportioned to the greater or less impurity of the

* Dr. Barton's Report, pp. 357-8.

atmosphere within the cabins. Dr. Wingate Johnson, Deputy-Inspector of Fleets, thus describes the parts of Port Royal in which cholera was most fatal, in the Report of the Jamaica Central Board of Health:

"Most of the houses have small courtyards attached to them, which are generally the abode of pigs and goats, and are also invariably the depositories of every species of disgusting filth, such as human ordure, as well as other excrementitious matters, stinking fish-guts, and putrid slops; in fact, everything is there to be found, excepting cleanliness or pure water. The stench perceivable in the vicinity of some of these localities is at times intolerable. The few inhabitants that do observe anything like decency (there being no public privies), generally resort to the beach facing the sea in front of the battery in the vicinity of the church. About this spot the night-soil is also generally deposited. When the sea breeze blows home, this place is directly to windward of the town;" . . . . (p. 35.)

That the disease really found the condition necessary for its development, if not for its actual production, in the impurity of the localities here described, is rendered more evident by the contrast between its ravages among the miserable occupants of such hovels, and the absolute immunity enjoyed by the inhabitants of more healthful dwellings.*

In Kingston, where one-eighth of the entire population was swept off by the pestilence, the amount of contamination upon the surface at all times is described as incredible. The back courts and privies—where there are any—are represented as universally foul, unventilated, and offensive. Speaking of Montego Bay, Dr. Milroy says:

"A large number of the dwellings of the lower classes have no privy accommodation at all. . . . . The offensiveness of the necessaries in many of the larger houses may be judged of from the circumstance that I had been advised not to put up at two of the chief lodging-houses in the town, in consequence of the notorious nuisances in their back yards. The landladies of both houses died from the epidemic." (p. 58.)

Even the barracks were not free from similar sources of atmospheric impurity, and there was accordingly a considerable mortality amongst the troops, although much less in proportion to their numbers than occurred among the lower classes of the civil population. The strength of the military force in the island in 1850 and 1851 was 1770, of whom 756 were white, and 1014 black troops. Cholera was fatal to 29 of the former, and 99 of the latter.

"The state of the privies in all the barracks which are not immediately close to the sea, is altogether most disgusting. It is difficult to exaggerate their abominable condition at Kingston, Spanish Town, and Up Park Camp. . . . . One of the principal thoroughfares in Spanish Town is purposely avoided, in consequence of the horrible pollution of the atmosphere from this cause. Equally bad is the state of things on the west side of Kingston Barracks; where, not to mention the abomination of the large open dung-pit within the walls, sending forth its foul effluvia all round, there are two or three huge vaulted cesspools immediately under the surface, and which, it is believed, contain the accumulated excretions of hundreds of men for a great number of years." (p. 121.)

"So great is the privy nuisance at Up Park Camp, that, in my opinion, it is one of the chief causes of the distressing amount of sickness which has so often, in former seasons, prevailed among the troops there, and of the persistent cleaving of the choleraic poison during last year to this station. The necessaries are three or four in number, situated a little in the rear of the barracks, between them and

* See Dr. Milroy's Report, p. 37.

the hospital buildings. The effluvia from them is at all times most offensive. In the evenings, the land wind brings the tainted atmosphere right upon the barrack-rooms, and the stench is then disgusting. . . . . Nothing can be worse than the construction of these places; they are literally shut boxes over huge pits of ordure, the fluid parts of which soak into the ground, while the solid matter goes on accumulating for one or two years, or longer. . . . . The privies of the officers are in front or to the south of the barracks, which are thus exposed to pollution in both directions." (p. 122.)

In his "general conclusions," Dr. Milroy says that the influence of local agencies over the development and type of the disease was unfailingly shown in every part of the island. "The mortality among the well-conditioned class was very limited; it might be counted by units; whereas that among the mass of the people was by thousands." The atmospheric contamination from filth within and around dwellings was the main exciting and intensifying cause of the disease.

Several of the Indian Reports afford ample evidence that cholera has there been equally partial to localities whose atmosphere was vitiated by the products of fæcal decomposition, as in this country. Jessore, where cholera broke out very malignantly in 1817, is described by Mr. Jameson* as "a crowded, dirty, ill-ventilated town." The pestilence was most prevalent at Calcutta, in districts that were intersected by pools, broad ditches, and channels, from which foul gases were continually evolved. The huts, constructed of straw or mud, "are generally from six to twelve feet square, placed so close to each other as to leave scarcely room to pass between." In these wretched hovels whole families, consisting of six or eight persons, usually reside; "and not unfrequently cows, pigs, and other domestic animals, add to the filth and foul atmosphere in which they abound." Even in the neighbourhood of Government House "there is a stagnant pool in which the whole neighbourhood deposit their filth, and whence a stench of the most noisome and injurious kind frequently proceeds." The same writer describes the cantonment at Meerut, occupied by her Majesty's 14th Regiment in 1819 or 1820, when it lost 41 persons out of 1200 by cholera, as having been very filthy: "The privies were too few in number, and could hardly be kept clean or wholesome. The effluvia proceeding from them was at times exceedingly offensive, even to a great distance; and many of the worst cases were those of men seized in them with violent spasms and vomiting."† Mr. Scot says that the epidemic first broke out at Madras, in Vipery—a situation "abounding with stagnant water, the receptacle for every species of filth"—among the natives residing in some huts about which much offensive and corrupted matter had been accumulated.‡ Nearly twenty years after the publication of Mr. Scot's Report, the medical officer in charge of the troops, in consequence of the repeated outbreaks of cholera in the regiment inhabiting the Vipery lines, advised the thorough purification of the neighbourhood, including the opening and cleansing of the obstructed main drain. These suggestions being adopted, Mr. Rogers, from whom we quote, says it was found, on subsequent inquiry, that the troops inhabiting these lines had escaped on several

* Report on the Epidemic Cholera Morbus in the Presidency of Bengal, in 1817, 1818, and 1819. By James Jameson, Assistant-Surgeon and Secretary to the Board, pp. 107, 110–115.

† Loc. cit., p. 312–15.

‡ Report on Epidemic Cholera in the Presidency of St. George, p. 49.

occasions when the disease was raging severely in the neighbourhood.* Mr. Rogers relates a still more apposite fact, with which we must close our illustrations from the Indian Reports:

"The Coom river nearly encircles the village of Chintandrepett. This river was made a privy of by hundreds of natives daily; and when the monsoon was heavy, and the bottom of this Augean stable thoroughly cleansed, no ill effect resulted; but if the monsoon failed, and the river remained uncleansed, when the hot weather returned, the water became low, and the filth at the bottom was exposed to the sun, the smell was most offensive, and an attack of cholera was the certain result, the only victims being those residing within a short distance of its banks."†

In Dr. Wynne's 'Report on Epidemic Cholera in the United States in 1849–50,' are several pertinent facts of a like kind. The history of the outbreak in the Baltimore almshouses, well placed in a healthy locality at a distance from the town, is one of the most remarkable. Out of 632 inmates, including 115 who either eloped or were discharged during the visitation, 99 died of cholera. The account is too long for quotation in full, but the outbreak was evidently dependent upon the unsuspected accumulation of cesspool soil, of the filth from a pigsty, and of the drainage from privies on a piece of waste land screened from observation by the north wall of the establishment. This piece of ground is described by the reporter, Dr. Buckler, as being "one putrid and pestilential mass, capable of generating, under the ardent rays of a midsummer sun, the most poisonous and deadly exhalations." The wind set pretty steadily from the north during the prevalence of the epidemic, and the inmates of the almshouses suffered in exact proportion to the exposure of their apartments to its influence.‡

Again, in Boston, U.S., it appears that wherever the air was impregnated with the miasmata arising from foul privies or other collections of night-soil, there cholera was rife. Dr. Clark describes one place in which 12 deaths occurred in two days, out of a population of fifty; the cause being, in his opinion, the accumulation of all the excrementitious and other refuse matter in its centre. In another locality, equally exposed to the effluvia from fæcal matter in a state of putrefaction, 200 cases occurred within a circle having a radius of a few rods.§

The Cholera Commissioners, in their 'Report on the Outbreak in Newcastle and Gateshead,' refer that painful tragedy to the same class of local circumstances as were found in conjunction with the heaviest mortality from cholera in the metropolis in 1854. They speak of the absence of water-closets and the deficiency of privies, as well as of the existence of dunghills; of privies overcharged from neglect of scavenging, or constructed against the walls of houses, "so as to allow of the liquid filth oozing directly through the walls into living and sleeping-rooms," or "so as habitually to bring these 'poison-pits' close to the windows or doors;" of foul, offensive, obstructed, or ill-constructed sewers; defective inside trapping, untrapped gully-grates outside, as having been evils so common, that even the houses of the upper and wealthier classes of Newcastle and Gateshead were not exempt from their influence.||

* Reports on Asiatic Cholera in Regiments in the Madras Army, from 1828 to 1844, p. 25.
† Loc. cit., p. 4. ‡ Dr. Wynne's Report, pp. 68–75. § Loc. cit., pp. 60–62.
|| See the Report, paragraphs 34, 59, 62, 63, 64, 66, 68, 75, 77, 125, and 127.

The same local sources of atmospheric impurity that thus seem to have been so influential in the case of cholera epidemics elsewhere, are blamed by Dr. Lucas for the outbreak in Brecon, in 1854. The entire town is represented to have been in the most unwholesome condition; but the force of the epidemic was chiefly expended in one district, through which flows the Mandrel, a filthy brook, which, receiving the sewage and drainage of the neighbourhood through which it passes, is little better than a common sewer. The mortality from cholera in the entire town was at the rate of 30·7 in the thousand; but in Bailyglas and Black Boy, the two districts most severely visited, at the rate of 83·0 and 79·7 in the thousand respectively. The Mandrel runs more sluggishly past Bailyglas than elsewhere, and here receives the soil direct from such privies as exist in the district. These evils were aggravated by special circumstances about the time of the epidemic visitation.*

A limited outbreak which took place in the North City Dispensary district at Grangegorman, Dublin, on Sunday, November 5th, 1853, strongly favours the idea that cholera is at least occasionally of local origin, and that the circumstance which we have found so frequently associated with it, is probably its efficient cause. Out of 6 cases, 5 were fatal; four of them in periods of ten, eleven, thirty, and thirty-one hours respectively.

> "All the patients lived close to one another; and though the locality where they resided is open, the hygienic conditions of their habitations were extremely bad. In their immediate vicinity, there was a very large collection, in a yard, of street manure and night-soil, collected by carts from the lanes and alleys of the neighbouring parts of the city. No other case of the disease occurred at this time. It did not become epidemic in Dublin for more than ten months after."†

Very striking as is the evidence of the local conditions usually associated with cholera epidemics on shore, that we have thus collected from so many sources, its occasional appearance in an epidemic form among the crews of ships at sea would seem to indicate that the presumed local condition is not required for the evolution of the pestilence; and if not required, that therefore it cannot be a cause of the disease, however much it may aggravate it. Upon this head we possess little positive information; but from the 'Report on the Cholera in the Black Sea fleet,' it appears that some of the vessels were, after the appearance of the cholera on board, less free from such atmospheric contamination than might be supposed. Mr. Rees, surgeon of the *Britannia*, attributes the outbreak on board that vessel, in a great measure, to defective ventilation; and adds, that when a return to port was decided on, the continued violence of the scourge, the crowded state of the middle deck, the discharges from the bowels and stomachs of the sick, and the want of adequate ventilation, had contributed to render the ship a laboratory of pest poison. In the *Albion*, 419 cases of diarrhœa and cholera occurred among a crew of 800, of which 69 proved fatal. The pestilence reached its climax, both on board the *Albion* and *Britannia*, on the 14th and 15th of August, 22 out of 25 persons attacked on board the *Albion*, on the last of these days, having died. The surgeon accounts for this fearful mortality, on the sup-

* See Dr. Lucas's Report, p. 15.

† Third Annual Report of the Commissioners of Irish Poor-Law, p. xxii.

position that the "evacuations from those previously infected had accumulated, and acquired a much more deadly influence from imperfect ventilation, in consequence of the state of the weather." The crews of all the vessels had been more or less on shore, where cholera was at the time prevailing in an epidemic form. Supposing that the earlier cases had received the disease on shore, and that their discharges accumulated between decks in the manner named by Mr. Rees and the surgeon of the *Albion*, there is no longer any difficulty in understanding how, in the hot, close, confined, still atmosphere of a ship's lower deck, atmospheric contamination would arise and become concentrated, so as possibly to induce the subsequent disastrous outbreak.

Cholera having thus prevailed almost exclusively in localities the atmosphere of which has been vitiated by the products of the decomposition of excrement, may we not almost venture to believe this to be the true localizing cause of the pestilence—the "terrene cause" of Dr. Barton, —the other factor to which we referred as a necessary contingent for the causation of pestilential epidemics? The isolated outbreaks, such as that in Tynemouth, already referred to;* that in Dublin, mentioned in the 'Report of the Irish Commissioners of Health;' and others which might readily be adduced; together with the fact related by Dr. Acland,† and other unimpeachable authorities, that cholera may arise without communication with any infected source, are, at the same time, readily explicable by, and afford support to, the supposition. In fact, such occurrences, and the sporadic cases presenting all the essential characters of the epidemic form of cholera which present themselves to our notice in ordinary seasons, are only explicable on the supposition that the poison is capable of production in this country under favourable circumstances. Such an opinion does not necessarily negative the transference of the poison, which, whilst it may thus, on the one hand, arise independently, may, on the other, be the means, under suitable conditions, of exciting a similar form of decomposition when accidentally removed to a fresh locality possessed of the requisite local conditions. Chemical analogy is rather in favour than not of the portability of the leaven, and many of the facts which have been adduced in proof of the presumed communicability of cholera, harmonize well with such a view. Again, the meteorological conditions which have been so frequently found to precede or accompany epidemic visitations of cholera, are perhaps favourable to some particular form of decomposition giving rise to poisonous exhalations; whilst the still, heavy atmosphere, so often prevalent throughout cholera epidemics, will tend to retard the rapid diffusion of such products of decomposition into space, which, thus detained in the immediate vicinage of their origin, will strike with aggravated violence. The strictly local nature of many outbreaks which are often confined to a single house, or a street, or to a small portion of a town, so that most large epidemics are, as it were, composed of a succession of small outbreaks, is also favourable to this view, which, again, harmonizes with Dr. Pettenkofer's observation, that the cholera miasm soon begins to lose its force, and this at very short distances from the place

* British and Foreign Medico-Chirurgical Review, vol. xvii p. 291.
† Dr. Acland's Memoir on the Cholera in Oxford, pp. 40, 73.

of its development, in consequence of its dilution with air. It is indeed true that the chemist has hitherto failed to detect any unusual element in the atmosphere during cholera epidemics, or to discover any peculiar product of decomposition which may be presumed to be the exciting cause of cholera. He has, however, been equally unable to detect the paludal poison, or to discover the nature of the peculiar decomposition of which it is the product.

Strong as is the evidence we have adduced in favour of the influence of season, and of a certain source of local atmospheric impurity in the causation of epidemics of pestilential cholera, we are far from asserting the case as proven. We set it forth merely as that which accords best with the known history of the disease, with the view of directing attention to the probability of cholera being, at least sometimes, of indigenous origin, and to its very constant co-existence in the malignant form with a definite cause of vitiated air. Had we desired it, the case might have been strengthened by additional evidence, and particularly by facts which would negatively tend to show that the removal of the supposed cause has both appeared to check and to avert epidemic visitations. It is certainly remarkable that so many independent observers have referred cholera to fæcal matter in one form or another; and this fact alone seems to show that there is some truth in the opinion. Putting aside the theory, that the recent cholera evacuations are the materies morbi—for no proof of their poisonous character has yet been adduced—our view accords with that of Dr. Pettenkofer and the other German authors, save that we incline to believe cholera may be produced by fæcal decomposition independently of the presence of the evacuations of cholera patients. It accords still more closely with that of Dr. Acland, who, although he in one place says—not apparently as a result of his own observation, but on the authority of Drs. Budd and Alison—"it can scarcely be any longer doubted that the evacuations of cholera patients are capable of communicating cholera,"* elsewhere propounds an hypothesis of the ætiology of cholera which agrees with that set forth in this article; excepting that, whilst we have not attributed the generation of the poison which causes the pestilential form of this disease, to the decomposition of any single form of fæcal matter, the Doctor limits it to that of the evacuations of diarrhœal patients.

"No one doubts," says Dr. Acland,* "that in a cholera period—1st, *persons die of diarrhœa and of choleraic diarrhœa, without passing into cholera;* and, 2ndly, *such cases do oftentimes pass into cholera.* . . . . Now the hypothesis is, that the first group are produced by 'atmospheric influence' (let the general cosmical conditions be so named), without any specific poison; and that the second group are produced by the same atmospheric influence as the first group, operating on discharges from the bowels, and producing a specific poison; the poison capable of acting on the individual who produced the discharges which can be so altered, or on other persons; the discharges innoxious, or incapable of communicating the disease *until so altered;* but when so altered, either within or without the body, capable of distribution through the atmosphere, probably either in a dry or in a gaseous state, and of absorption by the lungs; or capable of solution in water, and of absorption by the digestive organs. Or, more briefly, one cause (the atmosphere) produces the first group of disease, and along with the disease an organic product

* Memoir, p. 73. † Loc. cit., p. 76.

(alvine discharge), which is innocuous until altered by the very cause which produced it, and then it becomes the cause of the second group; so that it might be theoretically, and perhaps truly, said, that if the cause which produced the diarrhœa ceased before the discharges could be acted upon, then they would remain for ever innocuous."

This hypothesis of Dr. Acland's, for he only offers it in that light, tallies with the well-recognised fact that dysentery is frequently produced by inhaling the odour of dysenteric evacuations.* Whilst, however, we are gratified at learning that Dr. Acland has independently arrived at conclusions which approximate in several respects to our own, we are not disposed to limit the generation of the poison, but rather to believe, until more accurate observation shall have determined otherwise, that the "atmospheric cause" acts almost, if not quite, as much on ordinary as on diarrhœal alvine discharges.

We venture to hope that, should unhappily another visitation of cholera occur in this country, the entire subject of fæcal decomposition will be systematically investigated, and that the several views which have been put forth on the subject, and to which we have referred, will, as far as circumstances admit, be tested in a logical and scientific manner.†

Review VI.

1. *Anatomisch Physiologisch Onderzoek over het fijnere zamenstel en de werking van het Ruggemerg.* Door J. L. C. Schroeder Van der Kolk.—*Amsterdam,* 1854. 4to, pp. 90. Met drie Platen.

Anatomical and Physiological Inquiry into the Minute Structure and the Functions of the Spinal Cord. By J. L. C. Schroeder Van der Kolk.—*Amsterdam,* 1854. With three Plates.

2. *Neue Untersuchungen über den feineren Bau des Centralen Nervensystems des Menschen.* I. *Medulla Spinalis und deren Bulbus Rhachiticus.* Von Joseph v. Lenhossék, Doctor der Medicin, &c., Professor der Anatomie und der gerichtlichen Medicin an dem Kais. Kön. Lyceum in Klausenburg.—*Wien,* 1855. 4to, pp. 70. Mit iv. Tafeln.

New Researches into the Minute Structure of the Central Nervous System in Man. I. *The Medulla Spinalis and Bulbus Rhachiticus.* By Joseph v. Lenhossék, M.D., &c., Professor of Anatomy and Legal Medicine at the Imperial and Royal Lyceum in Klausenburg.—*Vienna,* 1855. With four Plates.

* Pringle's Observations on Diseases of the Army, fourth edition, pp. 22, 23, 86, 346. Lind on Fevers and Infection, p. 241, London, 1779. Copland's Medical Dictionary, pp. 699, 704.

† The investigation of fæcal fermentation during healthy periods is, however, quite as important as that during times of epidemic visitation. Without an acquaintance with the ordinary products of such decomposition, it is clearly impossible to determine the existence of any specially poisonous product during epidemic periods. Just as the last proof of this article was corrected, we received the Seventeenth Annual Report of the Registrar-General, containing a lucid summary, by Dr. Farr, of the facts deducible from the death statistics of the cholera epidemic of 1854. These facts afford support to the opinion that cholera is, now at least, indigenous to this country, and but an aggravated form of a disease continually present amongst us. "Cholera itself," says Dr. Farr, "has probably always existed in England." Its intensity apparently depends "chiefly on local and meteorological circumstances."

3. *Disquisitiones Microscopicæ de Medullæ Spinalis Textura, imprimis in Piscibus factitatæ.* Conscripsit PHILIPPUS OWSJANNIKOW.—*Dorpati Livonorum*, MDCCCLIV. 8vo, pp. 51. Accedunt tabulæ tres cupro expressæ.

Microscopic Researches into the Structure of the Spinal Cord, chiefly in Fishes. By PHILIP OWSJANNIKOW.—*Dorpat*, 1854. With three Plates.

IN all recent investigations into the structure and functions of the nervous system, the anatomists and physiologists of this country have constantly borne a distinguished, and sometimes a pre-eminent part. Since Sir Charles Bell, in 1811, first surprised the scientific world by the indication of his interesting discoveries, we have had the ingenious and important inquiries of Dr. Marshall Hall, who, if not strictly unchallengeable in all his claims to originality, and if in certain of his views he must yield the palm of priority to Unzer and Prochaska, has at least so largely developed what others had merely foreshadowed, has so enriched the aggregate by additional facts and explanations, and has so enhanced its value by new practical applications, that we must assign to him merit of the very highest order, even if later investigations, dependent chiefly upon more improved modes of examination, should ultimately require modifications of his views as extensive as those which he has enforced upon his predecessors. The names of Reid, Grainger, Swan, Solly, Todd, and Bowman stand also honourably illustrious in this department of inquiry; while, among those who have especially brought the aid of the microscope to bear upon their researches, Mr. Lockhart Clarke has justly attracted considerable attention, as well among our own as among foreign observers. Thus the mysterious and difficult topic has a double interest for us, and we watch its growth with one glance towards the progress of the science, and another to the conspicuous share in its advancement which belongs to our able countrymen.

Looking at the subject as practical physicians, it is pleasing to observe how much of this progress has been due to those who belong to our laborious class. While we admire the energy which finds opportunity in the midst of so many anxious distractions, for investigations so abstruse and so delicate, we cannot doubt that the very habitudes which bring the practitioner into his manifold relations with the manifestations of life, whether in their normal or anormal aspects, are precisely those which are fitted to communicate a just direction to his inquiries, as well as to supply the appropriate checks, by suggesting a qualification here or interposing a negation there, so as to give circumspection to his steps and weight to his deductions. In this way, while the phases of health supply illustrations which the phenomena of disease limit and define, we look back again to the facts and principles eliminated, either thus only, or with the aid of whatever other means of inquiry, and seek a reflected light, to be thrown in its turn upon other morbid phenomena, so as to facilitate that surety of diagnosis of a variety of diseases, without which their proper rules of treatment are rarely obvious, and never irrefragable. It is, therefore, with the aims of the practical physician that we now turn our attention to certain recent researches into the minute anatomy and the functions of the spinal cord, confining ourselves on the present occasion to the labours of two or three of our continental brethren. If the natural growth of scientific doctrine, with the truly scientific ever slow and hesitating, does not yet entitle us to anticipate any signal portion of that rich guerdon which is still in store for discoverers in this interesting department, the name which heads our list may at least assure us that we shall gather nothing that does not proceed from the careful research of a maturely experienced and penetrating intelligence.

I. Professor Schroeder Van der Kolk does not commence his observations without an acknowledgment of those difficulties which invest his subject, and which have hitherto conduced to so many diversities of result. Having shortly discussed the previous researches and speculations of Ehrenberg, Valentin, Remak, Hannover, Stilling, Volkmann, Wagner, and of Todd and Bowman, he passes to the narrative of his own inquiries. Induced by his success, in the year 1847, in demonstrating a close relation between the peripheral distribution of the sensory and motor nerves, through his discovery of the law that everywhere throughout the body the sensory ramifications of a mixed nerve pass to the surface of the part which is moved by the muscles receiving their motor fibres from the same nerve, so that, while the latter become the instruments of motion, the former supply sensation to the part moved,* he was led to infer, farther, that there must exist an intimate central union between the motor and sensory nerves of any individual trunk, and to seek for the proofs of this union in a more scrutinizing examination of the structure of the spinal cord. Although the result failed to correspond fully with his requirements, still it appeared to him that his investigations threw important light on most of the questions relating to the structure and functions of the cord; and he justly offered them as material contributions to so uncertain a field of research, into which a multitude of vivisections had hitherto introduced more confusion and inconsistency than substantial truth. They were first announced in the Proceedings of the Section for Natural and Medical Science of the Utrecht Provincial Society, towards the close of June, 1848, and reported more fully in the autumn of the same year, before the Royal Institute of the Netherlands.†

We shall not pause to specify the views thus originally promulgated, and now reproduced by the author; or to contrast them with those advanced in the interval by other inquirers. In the latter, Schroeder Van der Kolk discovers many discrepancies, but he regards them as generally confirmatory of his own prior inferences, though framed without these having reached the cognizance of the several investigators. He thus passes in review the researches of Clarke, Engel, Schilling (of Dorpat),

* Tijdschrift der Wijs-en Natuurk. Wetenschap. van de Eerste Klasse van het Kon. Ned. Inst. 1847, p. 44 sqq.

† We have not seen the original Dutch notice (Aanteekeningen van de Sectie-vergadering van het Prov. Utr. Gen., Junij, 1848), or its translation into Swedish by Liedholm (Hygiea, medicinsk och Pharm. Monads-skrift, B. XI., 1849, p. 553 sqq.), referred to by the author; but an abstract, also by Liedholm (Om ryggmärgens function och histologiska byggnad, &c : Hygiea, B. XVII., 1855, p. 593 sqq.), of the later publication, now before us, has been kindly recommended to our attention by Professor A. Retzius, of Stockholm. This we have examined, and with such satisfaction as to its general clearness and accuracy, that we might have adopted it at once, as a lighter labour than forming an abstract of our own, were it not that we prefer, as a duty to our readers, to turn to original sources wherever these are accessible.

and Gratiolet, and adverts to the opposing opinions of Kölliker. Meanwhile, having himself renewed his investigations with improved methods, and having also widened their field, still with the uniform effect of strengthening his deductions, he now proceeds to present to us the whole, as the ripe result of prolonged and reiterated experience; and assuredly as such, and as proceeding from a source so enlightened, they have every claim upon our attentive consideration. It is at this their matured stage that we lay them before our readers.

A primary object was to ascertain the best method of securing such sections of the spinal cord as presented the fittest conditions for a successful scrutiny. After testing the processes of other investigators, and resorting to a diversity of expedients of his own devising, he arrived at the conclusion, that by the following plan he attained the maximum of advantages, with the minimum of contravening defects. The cord, cut into moderate-sized portions, was first hardened in alcohol, and so soon as the requisite degree of consistency was reached, all beyond this injuring the distinctness of the preparations, the fine sections were made by means of a broad, keen-edged razor. Such a section was now laid upon a glass slide, with a little distilled water, and a covering-glass placed over it; the edges of the latter being then alternately pressed down very gently, so as to force the water between the fibres, without breaking up the texture. A milkiness is thus speedily caused, which is to be washed away by a continuous dropping of water at the margin of the covering-glass; and this, with the alternating pressure, is repeated till the turbidity ceases to appear. In this way, by a cautious manipulation are removed the fat globules and the detached molecules which obstruct the transparency. By now holding the glass obliquely, and bringing a current of water against the edge of the covering glass, the latter glides from the object without injuring it. The surrounding water is next wiped away, and a few drops of a concentrated solution of chloride of calcium are applied to the section by means of a glass rod; the covering glass is replaced, pressed down gently, and allowed to remain; and, in the course of half an hour, or even earlier, a degree of transparency begins to be obvious, which gradually increases, so that in eight or ten days all the fibres are defined with distinct outlines, and can be readily discriminated from the minute capillary vessels. The edges of the glasses are finally luted together with asphaltum. In this way a number of preparations are made, compared with each other, and the best and most distinct retained. In objects hardened with chromic acid, the author was not so successful in displaying the ganglion-cells and their fibres, as with alcohol. In the use of the microscope, he has derived generally the best results from the employment of a power of from eighty to one hundred diameters.

The author does not enter into any complete description of the spinal cord, but confines his details to the principal facts which have been noted by himself, and which he illustrates by reference to a series of lithographic figures. The reciprocal communication of the multipolar ganglion-cells, by means of their connecting filaments, he has examined in a variety of longitudinal and transverse sections; and he holds that, by means of his preparations, he has demonstrated the nature of this in the most conclusive manner, though not always with equal facility. Sometimes two ganglion-cells are found adjacent to each other, and connected by a filament of considerable thickness; usually, the connexion is apparent between neighbouring cells, or it takes place between those which are more remote from each other, so that not rarely a connecting thread passes over the nearest, without communicating, that it may unite with another more distant; sometimes the cells are conjoined by more than one individual filament. These ganglion-cells are found most abundant in the anterior horns of the grey substance, as indeed has been remarked by most writers, and chiefly near the points of entrance of the nerves; their most considerable aggregations being in the cervical and lumbar enlargements of the cord, where the grey matter expands into wider dimensions, and the emerging nervous trunks are the most numerous. Besides these ganglion-cells in the anterior horns, others occur in the posterior horns also, but of smaller size, and fewer in number. In so far, most of the author's observations here agree with those of Clarke, Schilling, Gratiolet, and Kölliker: but he has met with another unvarying group of ganglion-cells, which appear to him of essential importance, and which have not been adverted to by others; unless, as he considers probable, they have been noted by Kölliker, without, however, his assigning to them their just relations. These cells lie together in a small and compact group, among the radiations of the posterior grey commissure, into which their filaments evidently pass. They differ from those in the anterior horns, through the smaller number of their threads; many of them are oblong, triangular, and, where least in size, they are commonly very closely arranged. There are, farther, isolated ganglion-cells between the white substance, or longitudinal fibres, of the cord. These cells are in small number, lie in the lateral expansions of the grey substance, and chiefly in the vicinity of its mass, and have been remarked by Clarke.

From all this, the author coincides with Clarke in deducing that several distinct vertical columns of multipolar ganglion-cells exist in the cord, extending throughout its whole length: that of these cells the most considerable are in the anterior horns; that next are those at the side of the posterior commissure; then those in the grey substance between the anterior and posterior horns; and lastly, those in the posterior horns themselves, which rank as the smallest. But these columns of cells must not be regarded as subsisting independently of each other: on the contrary, they are all of them more or less closely connected. Neither are they, viewed in their longitudinal direction, of uniform expansion. Not only are they larger and richer in cells at the cervical and lumbar enlargements, but the proportion of cells increases at all points where the roots of nerves penetrate into the cord and its grey substance; so that they constitute thus a series of more or less dependent clusters, placed longitudinally above each other. Their connexion with the roots of the nerves, like their reciprocal communication, is demonstrated by means of both transverse and longitudinal sections; the latter as near as possible to the entrance of the anterior roots, in the direction of the anterior horn of the grey substance, or rather parallel with the course of their nervous fibres. By a transverse section he has succeeded repeatedly in tracing the nervous threads distinctly and uninterruptedly from without into the horn; dividing themselves into thicker or thinner fasciculi, some of which pass along the outer margin of the horn, while others distribute themselves through its substance. At the entrance into the grey substance lie usually a few multipolar ganglion-cells, from which it is sometimes possible to follow excentric filaments passing into the nerve-roots, or lateral radiations, as has been very clearly represented by Clarke, although the importance of the fact escaped his cognizance. But the connexion of the nerves with the ganglion-cells, or rather their origin from them, on the anterior or motor side, he has best demonstrated by means of longitudinal sections. Upon the whole, he judges that there can remain no doubt that the roots of the motor nerves spring from the cord, and specially out of the ganglion-cells of the anterior horn, which are mutually conjoined into a plexus, and frequently separate themselves into more or less distinctly segregated groups.

The leading question remained—in what way are these motor nerve-roots connected with the brain, through the medium of the mesh of ganglions into which they pass? That the anterior medullary fibres are the channels for the operation of the will on the motor nerves, the author receives as beyond dispute. But the connexion between the medullary fibres and the grey substance is not so obvious. To demonstrate this, it is necessary to examine the disposition of those transverse fibres, which are seen among the longitudinal fibres on all sides, as radiations, more or less divaricated, from the grey matter. From the divergent and contorted course of these fibres, it is rarely possible to trace them: but the author considers that he has shown that they form a curve, and pass in the direction of the longitudinal fibres; having especially substantiated this by means of a very fortunate longitudinal section from the antero-lateral column, in which the innermost longitudinal fibres were seen moreover to bend towards the grey substance, and pass into the ganglion-cells. As the general result of these and of other observations, he thinks it manifest, that while, as all writers have remarked, the longitudinal white cords maintain for the most part an uninterrupted parallel course, still that transverse fasciculi, issuing from the grey matter, separate and spread themselves among the white substance, and unite with a portion of its fibres; so that the longitudinal fibres, as channels of the will, communicating with these transverse fibres, convey the influence of the will to the ganglion plexus out of which the motor nerves take their origin.

The investigation of the structure of the posterior horn, and of its intrant nerve-roots, presents still greater difficulties. After again briefly narrating the views of other observers, Professor Schroeder Van der Kolk proceeds with the result of his own researches. A thin longitudinal section, at the entrance of the posterior roots, shows that a portion of these penetrates the cord, but immediately afterwards curves upwards in the posterior longitudinal column. The fibres run parallel with the white fibres, with which they afterwards unite; and are then covered by those of the sensory nerves having a higher origin, so that they may be said to lie imbricated under each other. Besides these fibres, passing thus longitudinally in the posterior column, other transverse fibres, issuing from separate fasciculi, dip towards the centre, or the posterior horns. These are best seen in a transverse section, taken at the level of a nerve-root passing into the cord, and are especially fine and delicate where traversing the gelatinous matter of the posterior horn. The author has traced them to groups of ganglion-cells, without, however, being able to show any actual communication. In addition to these nerve-roots, there occurs here another description of fibres, which appear to the author not to have attracted sufficiently the attention of other writers. They pass round the whole posterior horn like a band or girdle, and appear to proceed chiefly from the transverse radiations which here, as in the anterior horns, intersect the longitudinal column, and spread out with many branches at those parts of the cord where no nerve is seen to enter. These encircling fibres possess numerous small, generally oblong, ganglion-cells, and communicate with fibres issuing from the posterior grey commissure. It thus appears, that at the posterior part of the cord there are two descriptions of nerve-roots, of which the one ascends immediately in the white substance, and appears to proceed directly into the brain, constituting undoubtedly the channel of sensation; while the other roots traverse the white substance towards the posterior horn, through which they penetrate, and, mingling in part with the encircling fibres, lose themselves apparently in the ganglion-cells of the centre of the grey matter between the anterior and posterior horns. The latter form thus, in all likelihood, the apparatus of reflex action, and direct the stimulus through the group of ganglion-cells, with which they appear to be connected, into the anterior cell-groups from which the motor nerves derive their origin.

If a longitudinal section be now made through the posterior horn, it is seen obviously that the so-called gelatinous substance is composed of fine, translucent, longitudinal fibres, running parallel with each other, and much more slender than the white, ascending medullary, or sensitive fibres. These delicate fibres exist in the greatest abundance in the cervical and lumbar enlargements of the cord, and do not appear to pass uninterruptedly upwards as sensory fibres, otherwise the posterior horn could not present smaller dimensions in its dorsal than in its lumbar portion. But if we advert to the origin of the motor nerves from groups of ganglion-cells, and consider that these groups must be reciprocally connected in order to bring the different muscles into co-ordination of action; and that farther, during certain conditions of irritation of the spine, a stimulus may excite many, or even the whole, of the nerves of the cord simultaneously into convulsive energy, and thus extend the reflex motions to parts remote from each other,—it becomes more than probable that these longitudinal translucent fibres are connecting fibres, that is, that they serve to unite together the different cell-groups throughout the cord, and are thus the peculiar agents for the co-ordination of the movements.

In his views of the texture of the commissures of the cord, the Utrecht Professor chiefly coincides with those of Schilling. The anterior commissure is essentially distinguished from the posterior by the decussation of its fibres. After their intersection, these fibres are deflected, and run in part along the margin of the anterior fissure, interlacing themselves within the white substance; and in part enter the inner edge of the anterior grey horn, where they mingle with the encircling fibres already described, which spread themselves thence in the medullary columns, and join the longitudinal fibres. They are not seen to pass directly over into the roots of the anterior nerves. Their function is probably to maintain a connexion between the motions of the right and left sides of the body, through the intervention of the middle group of ganglion-cells, acting upon that in the anterior horn. The posterior commissure varies greatly in dimensions, being very broad in the lower part of the lumbar portion of the cord, much smaller in the dorsal, and again broader in the upper cervical portion. Its fibres have a parallel course, without intersection; those nearest the central canal passing into a group of ganglion-cells, and the others traversing from the middle of one side to the middle of the other side, where they appear to terminate in the central cluster. In this, as in a more general focus, are united the reflex fibres, the encircling fibres of the posterior horn, and the posterior commissure. Between the two commissures is the central canal, which the author maintains to be an open channel, a continuation of the fourth ventricle, and lined with epithelial cells.

Such is a condensed recapitulation of the researches of Schroeder Van der Kolk, to which it has been our endeavour to give all the distinctness attainable without the advantages of the accompanying illustrations, or of those minor explanatory details by which he himself elucidates the steps of his investigation. From a general review, he considers himself entitled to deduce the following as the sum of his conclusions:—

1. The ganglion-cells, especially in the anterior horn, are joined reciprocally by more or less divaricated connecting filaments, and thus unite into more or less distinct groups.

2. From the ganglion-cells, especially in the middle and anterior portions of the anterior horn, arise the motor fibres, which unite at the margin of the grey substance into one, or several, contiguous nervous bundles, quitting the cord in a transverse direction, to compose the roots of the motor nerves.

3. Along the outer margin of the anterior horn, run encircling or marginal fibres, which have their origin from the radiations expanded in the longitudinal columns, and are connected with the ganglion-cells situated in considerable numbers on the outer edge of the horn. These cells communicate with others placed more profoundly; and thus ultimately with the group of ganglion-cells from which the motor nerve derives its origin.

4. The anterior longitudinal columns are composed of white medullary fibres, for the most part parallel, which pass into the already mentioned transverse radiations, and through them convey the influence of the will to the ganglion-cells in the grey substance. The longitudinal fibres, placed the nearest adjacent to the grey horn, bend immediately round, in order to attach themselves to ganglion-cells.

5. The posterior nerve-roots include two descriptions of nervous fibres: those for sensation proper, and those for reflex. Hence the greater thickness of the posterior roots compared with the anterior.

6. The sensory nerve-roots, immediately after their entrance into the cord, proceed upwards along the posterior columns, to reach the brain as the seat of perception. They do not penetrate into the grey matter.

7. The fibres for reflex action pass transversely towards the posterior horn, and make a number of interlacements between the longitudinal fibres; while a portion of them proceeds through the so-called gelatinous

matter of the posterior horn into the middle of the grey substance, where they appear to connect themselves with the ganglion-cells. Possibly they send communications also to the encircling fibres, which everywhere surround the grey posterior horn like a band.

8. These encircling fibres proceed in great part from the radiations, which spread themselves from the posterior horn into the medullary matter; they surround the horn, and at its basis bend themselves from either side towards the middle, to terminate in the group of ganglion-cells which receives also the reflex fibres. Among these encircling fibres are scattered a number of generally oblong ganglion-cells; while a few ganglion-cells are also found in the gelatinous substance, especially near its centre.

9. The posterior horns of the grey substance consist principally of very slender longitudinal fibres. Now, as these horns are at least five or six times thicker in the cervical and lumbar enlargements than in the dorsal portion of the cord, it follows that these delicate fibres must exist in far greater proportion in the one situation than in the other; and therefore do not pass unbroken throughout the whole length of the cord, but terminate for the most part in the cervical and lumbar enlargements, where reflex actions and motions are most abundantly excited and combined. They appear thus, by their longitudinal direction, to connect more or less closely several cell-groups placed above each other, and constitute therefore longitudinal communication-fibres (*communicatie-draden*).

10. The posterior commissure, composed of grey fibres, passes partly into contiguous ganglion-cells, partly into cells placed in the middle of the grey substance, and partly becomes connected with the encircling fibres of the posterior horn.

11. The anterior commissure forms a decussation: its fibres take a direction forwards, in part to terminate directly as radiations betwixt the innermost, anterior, longitudinal cords; in part to pass over to the inner edge of the anterior horn, where they unite with the encircling fibres, which derive their origin from the radiations, as already described.

12. The fibres, as well of the anterior as of the posterior commissures, have no connexion with the roots of the nerves directly, but probably are associated with the anterior through the medium of connecting filaments between the different groups of cells; and both commissures consist of grey fibres.

13. A canal exists persistently within the cord, having its interior lined with epithelial cells, and appearing occasionally to contain an albuminous fluid. It is of smaller calibre in man than in most animals.

Having thus detailed his views with regard to the minute structure of the cord, Schroeder Van der Kolk proceeds to discuss the physiological inferences to which they appear to lead. He considers that he has thoroughly established, that the motor nerves have their origin within the cord, and especially from the multipolar cells in the anterior grey horn; and argues against the notion of their arising directly from the brain, as surrounded by many impossibilities. The influence of the will is merely conveyed to them along the anterior and lateral columns; and it thus follows, that the number of longitudinal medullary fibres, which serve as conductors from the brain, may be relatively small, and are only required to tally with the varying number of groups of ganglion-cells in the anterior horn. These, again, are necessarily in relation with the individual muscle, or part of a muscle, or group of muscles, which enters into any simple or combined movement. Hence, if we compare, by means of transverse sections, the thickness of the antero-lateral columns at different heights of the cord, we find that these only enlarge slightly in their course upwards. In the posterior columns, on the other hand, it has been seen that the proper sensory fibres bend immediately upwards on their entrance into the cord, and that thus these columns must contain as many fibres of this description as the sensory nerves supply. Accordingly, an ascending series of transverse sections shows that the posterior and postero-lateral parts of the cord become, by the successive increments of the sensory nerves, much thicker in their passage upwards than the anterior columns. This point is well illustrated by the very exact representations in the plates of Arnold. The necessarily greater abundance of groups of ganglion-cells, in situations where there are the greatest requirements for simple and combined movements, explains also the varying degrees of thickness of the anterior horn of the cord, and especially the existence of the maximum at the cervical and lumbar enlargements.

But, besides the proper sensory fibres, which proceed upwards, it has been seen that the roots of the sensory nerves supply other transverse fibres, which, splitting into different fasciculi, pass to the posterior horn of the grey substance, and appear to lose themselves in the group of ganglion-cells situated in the midst of the grey substance betwixt its horns. These Schroeder Van der Kolk recognises as reflex nerves. He does not, however, concur with Marshall Hall in admitting a special system of excito-motor nerves: that is, of particular nerves which produce also the motion of which the reflex sensation is the stimulus. This, he considers, is a hypothesis which stands upon no assured basis. It is enough that the roots of the motor nerves receive the excitement to action from the group of ganglion-cells, whether that be originally communicated through the will anteriorly, or by reflex posteriorly. We may thus imagine the group of ganglion-cells as a battery with two poles, or rather as a battery capable of being charged in two directions: the one pole is in connexion with the channels for the influence of our will; the other, through the medium of different combinations of ganglion-cells, is in relation with the reflex fibres; so that an individual group becomes susceptible as well of the stimuli of psychical as of physical agencies. As we must admit, under this idea, two descriptions of nerves in the posterior roots, those for sensation and those for reflex, we find an explanation of their comparatively greater thickness, which is known to be fully double that of the anterior roots. According to this view, all reflex action takes place by a definite course, which guides its operation. This is further regulated through the instrumentality of the longitudinal slender fibres of the posterior horn, which the author conceives to be communication-fibres, bringing the different groups of ganglion-cells, as well as the reflex nerves, into reciprocal connexion, so as to account for the occasional diffusion of the action over remote organs, or over combinations of movements, especially in states of great irritation of the cord, as in the attacks of the epileptic, or under poisoning from strychnine. In animals destroyed by strychnine he has found considerable congestion, with small extravasations of blood, in the grey substance; a pathological fact which he regards as confirmatory of his views.

It has been shown that the groups of motor cells, as they have been designated for the sake of distinction, out of which spring the motor nerves, possess, as it were, two poles; that is, that they are connected on the one side with the conductors of our will (anterior columns), and posteriorly with the reflex nerves, through the medium of other ganglion-cells. If, now, these reflex nerves be connected with a number of groups of motor cells by means of the communicating fibres, so that, by the instrumentality of reflex, a co-ordinate movement, as a leap, may be effected, we are entitled also to consider that, through the medium of the anterior fibres, conducting the will, those groups out of which a combined or determinate movement arises may with equal facility be brought into action. The cause of the co-ordination of the muscular action is thus, as Volkmann rightly judged, in the spinal cord, and not in the cerebellum; otherwise it would have been impossible to witness a determinate co-ordination of reflex movements in the frog, after decapitation. The commissures, connecting together the right and left divisions of the cord, he considers also to be more or less closely related with the function of reflex. The fibres of the posterior commissure appear to be implanted, in part, in the same ganglion-groups in which the reflex fibres terminate, and in part in the small groups of ganglion-cells at the side of the central canal. It is probable, therefore, that these fibres serve for the lateral reflexion, by transmitting the impression received by a group of ganglion-cells to those on the opposite division; while the fibres of the anterior commissure serve rather to maintain the harmony and balance of our voluntary movements on either side of the body.

Much has been disputed with regard to the question of the sensibility or insensibility of the grey matter. According to the supposition of the author, the grey matter of the cord avails solely for motion, the posterior portion being subservient to the reflex function and to the co-ordination of motion: while sensation is transmitted upwards exclusively by the posterior and lateral medullary columns, and has probably its proper centre in the medulla oblongata. In the medulla oblongata also is probably localized the centre from which the more universal reflex movements and convulsions take their origin; and it is to its condition, therefore, that experience has convinced him the physician should chiefly direct his attention in cases of epilepsy, a recourse by which he has frequently succeeded, where the disease has not been of too long duration, in procuring a recovery, through the means of derivative applications to the nape of the neck. The pathological change which results from protracted epilepsy, he has reasons for believing to be an induration of the medulla; but this subject he hopes to have further opportunities of pursuing.

As a summary of the chief points in his physiological deductions, Schroeder Van der Kolk finally offers the following propositions:

1. The different primitive filaments, which distribute themselves as motor nerves in a muscle, or muscular apparatus, appear to arise from a group of reciprocally associated ganglion-cells; they receive the influence

of the will along the anterior white columns, and the transverse or radiating fibres connected with these, which pass into a similar group ; and this influence, by diffusing its stimulus equally over all the cells of the group, produces a simultaneous and equivalent action in all the motor threads emerging to constitute the nerve.

2. The number of these anterior fibres, the conductors of our will, must thus be in correspondence with the number of cell-groups, and the different combinations of which these are capable, and consequently is much smaller than the number of medullary fibres for the sensory nerves, in the posterior column ; so that, by the continual accession of new sensory nerves, the white medullary matter at the back part of the cord increases more in thickness in its course upwards than the anterior portion, a fact fully demonstrated by the appearance of the cord at different transverse sections.

3. Where a larger number of nerves, to be distributed to muscles, issues from the cord, as for the extremities, there must necessarily exist also a larger number of the cell-groups from which they arise ; and hence it is that the anterior grey horns in the cervical and lumbar enlargements are so much thicker than in the upper part of the neck or in the back.

4. In animals exercising only the simpler muscular movements, and in fishes, we have a more slender cord ; and the grey substance, as well as the ganglion-cells, is less abundant where the requirements for combinations of movement are also less.

5. The reflex movements do not take place by transilience or transverse conduction, but the reflex nerves appear to terminate, partly in a central group of ganglion-cells more or less closely connected with the various groups of motor cells, and partly in the fine longitudinal fibres of the posterior horns.

6. The posterior horns of the grey substance, through which probably the different groups of ganglion-cells are mutually connected, appear to serve chiefly for the co-ordination of the movements produced by reflex. These movements are more or less general, in proportion to the more or less irritated condition of the grey substance, or of the ganglion-cells.

7. Through their connecting filaments, the groups of motor cells appear to be so conjoined, that, just as merely a stimulus to a single toe suffices in a frog to produce, through reflex, a co-ordinate movement or a leap, so merely an impression is possibly requisite to produce also a strictly determinate movement, such as a step, which can then be modified, according to circumstances, by means of separate impressions on each of the cell-groups. The source of the co-ordination of the movements is seated in the cord, and not in the cerebellum.

8. The transverse commissures seem subservient to the preservation of the harmony of the movements between the two sides : the anterior, apparently more in connexion with the channels of the will, maintains the harmony of the voluntary motions ; while the posterior preserves that which is involuntary and by reflex, the equilibrium of the body, &c.

9. Both the horns of the grey substance appear to stand in the most absolute relation with the function of motion : the anterior as its direct

source; the posterior as rather for reflex and co-ordination. Neither shows evidence of being endowed with sensibility.

10. The medulla oblongata is apparently the general middle point where the reflex influence is transferred to either side, and upon the irritated condition of which depend all widely-diffused spasms, as convulsions, epilepsy, &c.

In an appendix, Schroeder Van der Kolk enters into some details regarding certain more recent views of Rudolph Wagner, and others, which, though independently made, he considers confirmatory and illustrative of his own observations. These we forbear noticing more particularly for the present; contenting ourselves with concurring in his recommendation, that the highly-interesting observations of Schiff and Clarke, on the anterior and lateral portions of the cord, should receive the attentive consideration of other inquirers.

II. The division of inquiry which has been occupied, and not unworthily, by Lenhossék, differs materially from that which has been so ably cultivated by Schroeder Van der Kolk. Laboriously comprehensive in his details, these, however, are, with the former, wholly of an anatomical description; and we have therefore neither scope for that unity of design, nor for that originality of conception, which sustain our interest in the discussions of the other, and leave us, in the apparently logical development of his hypothesis, a sense of the gain of a solid acquisition to science, or at least of a vantage-ground from which the physiologist may proceed more confidently towards ulterior researches. Besides, let us confess, the style of the author is too much that labyrinth of inverted periods and intercalated clauses which seems to linger in Austria, even with some of its most eminent writers, after it has begun to disappear from the other great fields of Teutonic literature; and which, from its involvement and perplexity, is intricate enough in itself to be peculiarly unfitted for the exposition of intricate textures. It is already difficult, where the minute structure of the central nervous system is considered as a series of detached parts, to trace the order of these, and unite them into the clear conception of a connected whole: but the difficulty is greatly increased where the complexities of language are added to the complexity of subject, and a sense of confusion arises, from which the mind is apt to turn aside, and pause, with a feeling of fatigue and dissatisfaction. A little effort, however, will conduct us through these perplexities, and will enable us to extract briefly such portion of our author's observations as relate more immediately to the structure of the cord, and therefore connect themselves more strictly with our present topic. Meanwhile, we have to thank him for the intelligent labour of his researches, and leave their success to be judged by the abstract which we subjoin.

Dr. Lenhossék's paper was read before the Imperial Academy of Sciences of Vienna, in 1854. His investigations were made by means of sections prepared according to the second method of Clarke, in employing which he congratulates himself upon his remarkable success. Finding advantages in chiefly using the lower magnifying powers of the microscope, he points out the danger of error in passing at once the chasm between an observation by the naked eye and another by means of those higher powers of the microscope so generally selected, and suggests the propriety, at least, of adopting progressive advances. The grey substance of the cord he describes as constituted by an amorphous, transparent basis, in which are imbedded the ganglion-cells. These are of two descriptions: of which one is universally diffused, while the other, differing essentially from this, is found in certain situations only, and admits of being again divided into two kinds, according as its groups occur in clusters, or scattered. The former, along with the hyaline basis, composes the proper substance of the grey matter. The latter includes a description of cells which are for the most part of a fusiform shape, which present regularly their long diameter in a line parallel with the axis of the grey matter, and which are often placed behind and above each other, so that they are brought into contact both by their surfaces and their extremities. They lie principally in the anterior horns: but they are placed more laterally, and between the anterior and posterior horns, in the lumbar enlargement; above which, and for a short distance only, they predominate in the posterior horns. Hence the representation, by Clarke, of an individual vesicular column, he considers unfounded. The most voluminous of the clustered groups occur in the cervical and lumbar portions, and produce the enlargements in these situations. These cells are conspicuous by their deep-yellow pigment, their great size, and the distinctness of their processes, and constitute what have been regarded by Müller as the proper ganglion-cells of the cord. Both descriptions of ganglion-cells, in as far as has been ascertained by the aid of the best instruments, are multipolar; and both present the most manifold intercommunications, whether amongst themselves separately, or between the two varieties reciprocally, so as to be connected in a continuous chain from the extreme point of the *conus medullaris* to the brain. The scattered or isolated groups of ganglion-cells occur first in the medulla oblongata.

Longitudinal fibres, extending upwards into the brain, cannot be distinctly demonstrated within the grey substance; and, if this have been sometimes imagined, it has been chiefly through a misconception, owing to the surprising distance to which a filament often proceeds, passing in its course other intermediate ganglion-cells, before uniting itself to a process of that with which it becomes ultimately connected. The primitive nerve-fibres, on the other hand, speedily quit the ganglion-substance, proceeding uniformly downwards at an angle of more than thirty degrees to the spinal axis, and unite to form the roots of the nerves beyond the periphery of the cord. In his description of the course of the fibres of the white substance, he sustains generally the views of Clarke; and he takes occasion to point out, that, as the white substance on either side of the cord is completely separated by the fissures, there can be no opportunity for the decussations, recently generally admitted, of either the anterior or posterior medullary columns. The histological elements of the white substance, up to the medulla oblongata, are longitudinal fibres; which form slight undulations where they are pressed aside by the central tracts of the primitive nerve-fibres, in the passage of these outwards to form the roots of the nerves. He maintains the view, that the primitive fibres of the nerve-roots, whether motor or sensory, simply traverse the white substance; and that no portion of them bends itself upwards to proceed to the brain along with the longitudinal fibres. The primitive fibres of the medullary substance are more slender than those of the roots of the nerves. They appear to issue at very acute angles from the grey matter, and probably proceed from the free cell-nuclei of Kölliker. The central canal of the cord he considers as permanently open, and describes it as lined with cylinder-epithelium.

The primitive fibres of the roots of the nerves proceed, as we have already noted, from the grey, or ganglion-substance, and their course is direct through the medullary matter, without giving or receiving communication. Their immediate origin from individual processes of the ganglion-cells is often, but not in the majority of instances, very distinct, and they arise from both descriptions of cells; the purely motor fibres springing from the anterior horns, and the purely sensitive from the posterior horns, of the grey substance. By the central relations of the general nerve-roots are constituted four distinct anatomical systems: 1st, the anterior, or purely motor system; 2nd, the posterior, or purely sensory system; 3rd, the radiated system, or central portion of the roots of the plexus of the pia mater; and 4th, the lateral, or mixed system, which includes the two upper roots of the accessory nerve of Willis, the par vagum, &c., and has its chief connexions with the medulla oblongata and mesocephalon. The fibres of the radiated system distinguish themselves essentially from all the others, by their passing from the extremities of the *processus reticulares*, or from thread-like processes emanating from the grey substance; by their diverging outwards on all sides; and by their forming an angle with the spinal axis similar to that of the sensory and motor tracts, but opening upwards, or in a reversed direction. The anterior spinal nerve-roots derive their origin from the proper ganglion substance, but especially from the great ganglion-groups in the anterior horns. A portion of the elementary fibres passes uniformly, on either side, from the more internally situated of these groups, to the anterior horn of the opposite side, traversing thus the commissure placed in front of the central canal, and crossing each other at very acute angles. The posterior, or sensory nerve-roots, derive their primitive fibres from the posterior horns of the grey matter; chiefly from the proper ganglion substance, and only partially from the great ganglion groups. They have no direct communication with the anterior spinal roots; but are brought into close relation with them by means of the middle layer of the ganglion groups, as these are seen to dispense filaments in both directions. The fibres also decussate with those of the opposite side, by means of the commissure passing behind the central canal; but this at an angle so acute that their course appears to be parallel, and the intersection frequently escapes observation. These, like all the other elementary fibres, stand as little as those of the anterior spinal roots in connexion with certain separate divisions of the medullary substance of the cord.

The fibres of the radiated system, emerging from the surface of the cord at innumerable points, throughout its whole length, and passing into the pia mater, form the nervous plexus of the pia mater described by Purkinje. Their distribution afterwards is uncertain, but is probably chiefly into the arachnoid membrane of the cord. The appearance of the ganglion-cells in this plexus, partly interposed between the primitive fibres of the fascicules, and partly attached to their exterior, is a peculiarity to which the author directs attention, as suggesting a probable relation between the function of these nerves and that of the sympathetic. On the other hand, their origin from both the anterior and posterior horns of the grey substance, as the alleged centres of both the motor and sensory functions, appears to denote their twofold capacity of action, and to explain the convulsions, and indications of suffering, remarked in living animals on irritation of the pia mater. The author, however, takes occasion to point out here the difficulty of isolating the necessary conditions, in all experiments made by means of vivisections, with a view to determining the special functions of the different portions of the cord; and accounts thus obviously enough for the heterogeneous and contradictory results arrived at by different inquirers. In his views of the origin of the spinal accessory nerve, he agrees in the main with Clarke. With the exception of the two upper roots, which appear to stand more immediately in relation with the par vagum, he regards the primary origin and central course of its root-fibres as completely identical with those of the radiated system. In this nerve also we have interposed and attached ganglion-cells similar to those of the plexus of Purkinje, but of larger dimensions. As the grey substance does not present any arrangement of a proper system of fibres, its horns cannot justly be designated as cords: neither can we, unless very partially with reference to the portions involved within the *processus reticulares*, admit a division of the white substance into subordinate cords, instead of retaining merely the more general division into columns; its fibres pursuing a longitudinal and parallel course uninterruptedly to the brain, as it were in a mass, and without being further separated by any distinct grooves or fissures.

III. The Memoir of Owsjannikow appears in the form of an Inaugural Dissertation, presented to the Faculty of the University of Dorpat. While he gracefully acknowledges the aid and encouragement in his researches conferred upon him by Professor Bidder, there remains enough, in the details of his investigations, and in the clearness of the exposition of their results, to manifest the great extent of his individual merit and success; and we trust that this able treatment of his topic may secure for him a reception well fitted to undeceive him in the modest expression of his belief, that he has selected a theme likely to attract few readers.

Preparing his objects by hardening them in a solution of one part of pure chromic acid in two hundred parts of water, and afterwards slicing them longitudinally or transversely into the necessary thin sections, he points out the advantages he has derived from selecting the class of fishes as the subjects of his investigations, owing to the singular transparency and distinctness under the microscope of the segments thus procured. The fishes whose spinal cord he selected for examination, were the Petromyzon fluviatilis, the Ammocœtes branchialis, the Lucioperca sandra, Esox lucius, Salmo salar and trutta, Accipenser sturio and ruthenus, Thymallais velifer, Abramis brama, Leuciscus jeses, Silurus glanis, Gadus lota, &c. In the two first of these, where the spinal cord is flat and riband-like, a longitudinal section presents only longitudinal fibres, nearly parallel to each other, running in the direction of the axis of the cord, occupying the whole of its periphery, and corresponding to the white substance in the higher animals. Within these longitudinal fibres there is a broader column of substance, deeper in tint, which is found to be replete with cells. These are in shape somewhat fusiform, or approaching to triangular and are placed with their long axis in the direction from within outwards. Still nearer the central canal, are observed other longitudinal fibres, of considerable comparative breadth, which have been described by Müller; and among these are found large and round ganglion-cells, each sending out two filaments, which appear to divide multifariously among the fibres in which they are immediately imbedded. The nuclei of these circular cells are of greater magnitude than those of the fusiform cells, but the nucleoli are similar in both. From each of the fusiform cells, a filament is seen to proceed, which traverses the outer longitudinal fibres, and emerges to form part of the posterior root of the spinal nerves. A filament, which proceeds to join the anterior root, is better observed in a transverse section. A third branch, also best seen in transverse sections, passes to the other side, to be attached to a cell there, and to form a commissure; and a fourth ascends upwards towards the brain. Sometimes also a fifth branch is observed to issue from a cell placed transversely, the precise course of which the author has been unable to determine, although once he succeeded in tracing an apparent junction with one of the round cells; and he indicates the reality of this conjunction as an important topic for further inquiry.

Proceeding in his examination of the other fishes, whose spinal cord is more funicular in form, he points out the position of the ganglion-cells as constantly in the grey substance; and as it were imbedded in a stratum of cellular tissue, from which that substance derives its colour. In transverse sections, he meets with a corroboration of his previous observations: filaments are seen to emerge from each cell in three directions; one towards the anterior nerve-roots, another to the posterior nerve-roots, and a third inclining inwards, to pass before the central canal, and unite with a cell on the other side of the cord. In longitudinal sections, and in tracing the root of the nerve inwards, its elementary fibres are seen to bend upwards among the ascending longitudinal fibres, and then to connect themselves with the cells. From each of these another filament now passes, taking first a reversed direction upwards, or in a line gradually receding farther and farther from the central canal; and then, coming finally into mutual contact, they form together the white substance of the cord, the fibres of which pursue a parallel course till they reach the nerve-cells of the brain. Hence it becomes easy to explain the progressively greater abundance of these longitudinal fibres, and the consequently greater thickness of the white substance, in the upward course of the cord. Grouping together the results obtained from both sections, he forms the conception of a cell, or association of cells, seated in the grey substance, giving origin to four filaments, pursuing each the course which has been described: but whether the cells communicate reciprocally with each other in the separate divisions of the cord, by means of a fifth filament, he leaves to be considered only as a physiological probability, if not as a necessity; and points to the shape of the cells, which sometimes gave indications of more than four emergent branches, as lending strength to his surmise, that such an intercommunication actually takes place, although he has been unable to substantiate it by visual proof.

From the sum of the investigations which we have thus briefly, and but in part, indicated, he deduces the following conclusions:

1. All the fibres of the spinal nerves which enter the cord become connected with ganglion-cells.

2. To each ganglion-cell extends one filament from the anterior spinal nerve-root, and another from the posterior; while a third serves as a commissure between the separate sides of the cord.

3. From each cell a filament ascends to the brain, the aggregate of these constituting the white substance.

4. The fundamental mass of the cord, containing the cells and filaments, is composed of areolar tissue, which, placed in the greatest abundance around the central canal, and freely pervaded by bloodvessels, imparts its peculiar colour to the grey substance.

5. The gelatinous substance, or substance of Rolando, is constituted by areolar tissue.

6. The alleged cells interspersed in the posterior horns and in the gelatinous substance are merely the corpuscles of the areolar tissue.

7. The axis-cylinders are of a round figure, and consist of the same substance as the ganglion-cells.

8. The axis-cylinders in the grey substance are provided with a peculiar membrane, which encircles also the ganglion-cells, and is distinct from the fundamental mass of areolar tissue.

9. In the spinal cord of the Petromyzon and the Ammocœtes the axis-cylinders are naked, and receive no proper covering from the areolar tissue in which they are placed.

10. In these fishes, round ganglion-cells are found close to the broad fibres, and send out branches which split into multifarious divisions.

11. The spinal nerves possess anterior and posterior roots in both these kinds of fishes.

The author has added a few notices of the results of his observations on the spinal cord of man. The central canal he describes as patent and empty, and beautifully lined with epithelial cells. He denies the existence of ganglion-cells in the posterior horns, and especially towards their apices; as well as that of cells around the central canal, alleged to exist by Kölliker, who, he thinks, has been deceived by the presence of the epithelial cells and the areolar corpuscles. Like Schilling and others, he has observed the fibres from the ganglion-cells passing into the longitudinal fibres of the white substance; and he maintains that none of the cells are apolar. The principal substance, or the axis-cylinder of the nerves, is identical with that of the ganglion-cells. The quadripolar cells, or quinquepolar, as he is prone to regard them, he considers to be subservient to the reflex movements; while the multipolar transmit the influence of the will, and are chiefly seated in the brain. We need not dwell upon one or two pathological inferences advanced by the author, because they seem to us to depend upon that vague kind of hypothetical reasoning, of which any quantity may be employed in medicine, but unfortunately in any direction, to prove or disprove at pleasure.

We should have brought into collation here the results of the researches of M. Brown-Séquard, had they not been already so recently noticed in the pages of this Review. It will suffice for our present object to observe, that we do not yet see sufficient grounds to admit them as so wholly subversive of all previous doctrines of neurology, as the French commentators allege regarding them. Indeed, upon the whole, on reviewing the doctrines laid down in the several able treatises of which we have just concluded an abstract, we are glad to discover tokens, not, certainly, of an overthrow of all our previous conceptions on the subject, but rather of a steady advance, and a reciprocal confirmation of those principles which have been gradually developed as the results of the more recent investigations of our micrographers and physiologists. Though there are undoubtedly, as might have been anticipated in so difficult a subject, discrepancies in the views of the authors before us, still their points of agreement are more numerous than their points of difference, while the former may even be said to be more essential in their character: and it is especially of the solidity of our progress in the knowledge of the minute anatomy of the cord that this may be truly asserted, however manifest it be that there is still vast scope for our inquiries, and that no views hitherto promulgated are so distinctly based as to be entitled to command an universal assent. As to the growth of physiological doctrine, it is, as usual, rash generalization which leads to error in all its departments, and in none more than in this. We can never thoroughly master the wonderful mysteries of that theme which brings us to the confines between an animated and a brute existence, and which would seek to unfold to us the union of both. Yet it is possible that we may still make approaches to it of a nearer description than any which have been hitherto effected, and in directions which have as yet remained unopened, as by lights which it would be presumptuous to foreshadow.

Who shall tell, for example, that it may not yet be shown that the memory is a material garner, in which are stored, as an actual presence, the images it recals? What can be more of a truism than to state, that it is not the eye that sees, or the microscope, or any other optical instrument? These merely transmit the representation which is to be received elsewhere. The eye may be perfect in its structure: but some pressure behind, on the optic nerve or on the sensorium, prevents the conveyance or the perception of the image, and there is no vision; for it is clear that, to produce this, the picture must be carried in its integrity to the point at which it becomes cognisable. The skill of the photographer has rendered us familiar with pictures, in which considerable groups of figures, with their adjuncts, are so inconceivably minute, that they are only visible, yet then distinctly visible, when under a powerful microscope. Who, then, shall say, that sees these triumphs of art, and knows the greater wonders of nature, that memory does not work through the impression of an actual photograph, inscribed and retained within the brain; as if it were but a part of the very limit of our faculties, that we discover nothing in art which has not previously existed in nature? We possess no analogues for similar impressions from other sources; but there is nothing contradictory in the idea that sound, more turbulent in its movements than light, may also, in its own way, impress its phonograph or phonotype on the brain, and so of the other senses. To recal a scene, then, or a set of features, or a landscape, or a strain of music, may be merely to direct the faculty which first perceived, on that point where the impression was first perceptible, and where it has since remained: and if age bring back the recollections of youth better than those of yesterday, it is but because the undeteriorated apparatus sufficed better, at the one period than at the other, to transmit and preserve the necessary impressions which the mind is otherwise still sound enough to appreciate.

But such speculations, in the present state of our knowledge, may be received almost at pleasure either in jest or in earnest. Even if they could be admitted as ingenious and subtle while they continue in the abstract, they fail ridiculously when we seek their application: and least of all, even could they be carried to a demonstration, are they fitted to be received as furnishing an ultimate explanation, which some have attempted, of the union of mind and body. Between the material and the immaterial world there is, to the human cognizance, an eternity of distance: the fiat of Omnipotence, which has created both, binds them to each other. Nowhere is it easier than here to pass words for realities, and make a new turn of expression assume the dignity of a new truth or a new discovery. But prudent judgments, like those whose labours we have introduced to our readers, will content themselves with the strict field of investigation; and will teach us that the nearer we approach the final difficulty, it is only to perceive it the more distinctly, and consequently to regard it the more humbly. When we stand in wonder before that Ineffable Power which has joined matter to intelligence, and which has made it conscious, not only of self-existence, but of other existences, so as to enable it to act upon these through observation and reason, we shall be ready to own with Tillotson, that a perfect knowledge of nature is nowhere to be found but in the Author of it; and that no less wisdom and understanding than that which made the world, and contrived this vast and regular frame of existence, can thoroughly understand the philosophy of it, and comprehend so vast a design.

Review VII.

On the Nature and Treatment of Club-Foot and analogous Distortions involving the Tibio-Tarsal Articulation. By Bernard E. Brodhurst, Assistant-Surgeon to the Royal Orthopædic Hospital, &c.—*London*, 1856. 8vo. pp. 134.

The activity of the orthopædic surgeons continues unabated; and limited though the sphere of their operations be, there seems as yet no lack of cases to employ them, or of interest attaching to their studies and practice. The curing of club-feet, which was familiar to Hippocrates, but forgotten by his successors, was revived in the last century, and in 1806 had attained the degree of excellence described in the following operation by Sartorius. After he had divided the tendo-Achillis, he says,—(as we are told by Mr. Brodhurst)

"Having fixed the knee, I grasped the foot with both hands, the thumbs being applied to the sole and the fingers to the dorsum of the foot. First, I extended the ankle; then, gradually increasing the force, flexed the foot as much as possible. Now my assistants held the foot firmly on the table, and I, with all my strength, applied myself suddenly to move the limb forward, which I accomplished, but with such cracking and noise as though all the bones were broken. The patient (a boy of thirteen years of age) screamed terribly: the great pain, however, was soon allayed. On examining the foot, no fracture was found." (p. 10.)

Anchylosis and an useful limb were the result to the patient, and to the profession a stimulus in the right direction, which other surgeons soon followed. Renewed observation and thought developed milder plans of treatment. Delpech of Montpellier conceived the rules for the sub-cutaneous division of tendons, which are substantially those acknowledged in the practice of the present day; Stromeyer proved and made them popular by extensively using them; and he was the occasion of their introduction into England.

"Dieffenbach's account of Dr. Little's return to Berlin, as 'Apostel der Tenotomie,' after having been operated on by Stromeyer, in Hanover, is worthy of its author.

"'A month had elapsed,' writes Dieffenbach, 'since Dr. Little had taken a letter from me to Dr. Stromeyer, in Hanover, when suddenly my door was opened, and the individual who had left me a cripple, entered with a vigorous, rapid step. I cannot tell which was greatest, my astonishment or my joy, but I think the latter. Without delay I examined his foot, and found the shape normal, the sole in contact with the ground, the arch of the foot less; the calf of the foot had begun to be developed, and the entire lower extremity had gained its normal length. A miracle could not have struck me more forcibly; and I must confess that I was never in my life so taken by surprise at the successful result of a surgical operation as by this; and I esteem Stromeyer, who had done it, even luckier than Little, who had been benefited by it.'" (p. 22.)

Since February 26th, 1837, when Dr. Little first divided the tendo-Achillis in England, orthopædy has made great strides. Whilst Velpeau still pursues the violent treatment of Sartorius, Tamplin, Lonsdale, Adams, Coates, Bishop, and Lizars, with other foreign surgeons, have prosecuted the subject; and now we find ourselves occupied with a volume on club-foot from the pen of Mr. Brodhurst, an author already favourably known by his 'Treatise on Lateral Curvature of the Spine.' The present work is only to some extent a new one; for the most part it is composed of papers published by the author in the *Medical Times and Gazette.* After an introduction on the History of the Surgery of Club-foot, the subject-matter of which we have already noticed, and an imposing list of "works referred to," extending over sundry pages, we come to the immediate subject of the book. It is divided into five chapters, of which the first two are devoted to Congenital Talipes, and the third to the distortions commencing after birth. Before the final chapter on the Treatment of the Disease, the subjects of Structural Pathology and Physiology in relation to Structural Shortening of Muscles, and Reunion of Tendons, are separately considered.

The first of these chapters is a compact account of the Physiology, Diagnosis, and Anatomical Pathology of Congenital Talipes; and the descriptions of the author differ in no important respects from those which previous writers have given of the same things. There is no new variety of these deformities to describe, and accordingly the author briefly details the ordinary characters and several degrees of talipes varus or inverted foot, talipes valgus or eversion, talipes calcaneus, in which the foot is unnaturally flexed, and talipes equinus, in which it is extended, together with the various distortions compounded out of more than one of them. It appears to us important to observe, that all these forms of distortion occur primarily in the muscles and joints of the foot proper, and, until a later stage of the retraction, are entirely independent of the great muscles of the calf. Varus and valgus are affections of the tibiales and peronei muscles respectively, and occur only at the joint which is capable of lateral extension—viz., that between the os calcis and astragalus on the one part, and the cuboid and scaphoid on the other. The former bones do not partake in the deviation; they are as incapable of doing so as they are of eversion and inversion in the normal movements of the foot. The tibio-tarsal joint, or true ankle, on the other hand, admits only of flexion and extension; but here again the excess of muscular action which occasions talipes equinus and calcaneus, appears at first sight confined to the proper muscles of the foot, and the great superficial muscles of the calf, which are inserted into the heel, are only subsequently affected. It is in this later stage that the ligaments are elongated, and the tarsal bones rotated on their axis, while in a still later stage they are compressed into unnatural shapes.

"Rotation of the tarsal bones is in rare instances so great that their replacement becomes exceedingly difficult; yet dislocation does not occur, but rotation on their axis only. An instance of dislocation is, however, recorded, and the specimen is shown in the Strasburg Museum. The astragalus is dislocated inwards and forwards, and displaced transversely, with its posterior surface in contact with the malleolus externus." (p. 33.)

The author's description of the whole of this subject is exceedingly clear, and supplies what one yet feels to be a want in this part of the book—viz., drawings of the skeletons of the deformities which he describes.

The interesting question as to the cause of congenital club-foot, has developed a variety of opinions, which may be classed as follows:—

1. Malformations and displacements of the tarsal bones.
2. Affections of the muscular system.
3. Malposition in utero.
4. Disordered nervous influence.

There is no need to discuss the first of these opinions, for it is plain that bones are not spontaneously misplaced. No better illustration of the adaptation of parts to altered circumstances can be given than in the instance of the bones. It needs but to contrast their overgrown condition in hydrocephalus and the arrest of their growth in hemicephalus, to recognise this rule; but never, except from the absence of a centre of ossification, do we find them the occasion of deformity. No such defect is pretended to exist in club-foot. As to the third opinion, it is sufficient to observe, that deformities are occasionally found in fœtuses at the third or fourth month of gestation, while they are still lying in an abundance of liquor amnii. That the real cause lies in the nervous system, as the author avers, and that the muscles effect the displacements and deformities in question, upon the instance of a faulty nervous influence, cannot be doubted, since those systems have been detected in the act of producing them very soon after birth, at a period of life not materially differing from that in which the congenital deviations occur. Thus convulsions issuing in slight club-foot, have been produced in a sucking child by emotion in the mother: much more may a similar disease be expected to follow the like cause before the close union of mother and child is broken by parturition. The concurrence of club-foot with various cerebral and spinal diseases is abundantly common, and children who are born with these distortions are frequently subject to convulsive disorders. Moreover, while a fresh cerebral excitement has been known to reproduce distortion after its cure by surgical means, it is interesting to notice that sometimes congenitally-distorted limbs are relaxed, and regain their normal position during the abeyance of nervous influence which occurs in sleep, and under the influence of chloroform. An additional argument might, we think, be drawn from the greater liability of boys than of girls both to cerebral and to spasmodic affections. The statistics of club-foot, in respect to sex, are wanting in Mr. Brodhurst's volume, but he mentions an interesting fact, which is worth much as an illustration of this argument:—

"Congenital distortions are sometimes hereditary. In 1853, I operated on a child for talipes varus of both feet, who had three brothers, all of whom were born with double varus; and in 1855, a fifth boy was born in this family, also with varus of both feet, who also was under my care, and on whom I operated. There were three girls in this family born intermediately with the boys. None of them, however, were in any degree distorted. The father of these children and his brother were both of them born with double varus, and also their grandfather. In each generation there were females in this family, but amongst them no instance of distortion." (p. 55.)

The whole subject of non-congenital talipes is handled in Chapter IV. After birth many causes of distortion come into operation, besides those which appear to act during uterine life. To the morbid influence of cerebral affections are added various local injuries of the foot itself, the limb, or its nerves; primary, or, as it is called, essential disease of muscle; inflammation of muscle, of integuments, of joints; forced or voluntary mal-position, and debility. Most of these causes of distortion are of course incident to the circumstances of extra-uterine life, and the deformities vary with their cause. We will confine our notice to but one or two of them.

Non-congenital talipes occurs almost always during infancy; and one is inclined to ask, wherein, then, is the practical distinction between the distortions which occur before, and those which come on so soon after, birth? The forms assumed by the foot do not materially differ in the two cases, though a practised eye and hand easily distinguish one from the other. As, however, paralysis is by far the most common cause of acquired talipes, whilst that deformity which exists at birth bears indications of previous spasm, there is necessarily some difference in the mode of treating the two affections. When spasmodic distortions occur in the child (and they are most common in the first three years of life), they form a most suggestive illustration of the mode in which club-foot is produced *in utero*. Some cerebral disturbance usually accompanies the spasm, but often it is transient, and in many instances is not observable. If some irritation in the system can be detected as originating the affection of the brain, the removal of the cause not unfrequently at once relieves both brain and foot. Occasionally a permanent talipes appears after a slight convulsion, or even without any cerebral symptom having ever been traced or observed. Such a case can only be remedied by surgical treatment.

The paralyses to which the majority of club-feet are due, are distinguishable into three kinds:

1. That arising from organic change in the nervous centre is common to infancy and to adult age. The following case exhibits the occasional connexion of paralysis and spasm,—

"A child, three years of age, met with a severe contusion of the head and fracture of the skull from a blow. In three days the symptoms of concussion had passed away, but those of inflammation ensued. The muscles of the face were spasmodically affected, and the thumb was flexed into the palm. Some few hours later, the hand was clenched, and prone; and, on the following day, the forearm was flexed, the leg was flexed on the thigh, and the thigh upon the trunk, and the extensors of the foot were in a state of clonic spasm. Hemiplegia succeeded. Finally, the spinal column became curved, the concavity being of course towards the paralysed side, and the flexor muscles of the leg and the extensors of the foot became permanently retracted. The fingers also were folded into the palm, the flexor muscles being retracted." (p. 61.)

2. Paralysis may arise from traumatic lesion of nerve-trunks. It is necessarily a rare occasion of distortion, though at the same time it is suggestive of structural change in nerves being a possible cause of deformity in some obscure cases.

3. The third cause of paralysis is disease of the muscles themselves, *myogenic* paralysis, the essential paralysis of infants. In the author's opinion, this obscure affection is probably rheumatic, and is occasioned by too rapid cooling of the body, especially during the recovery from debilitating diseases. Yet he finds the disease limited to one period of early life—from the ninth to the eighteenth month,—preceded by acute sensitiveness of the affected muscles, and sometimes involving single muscles, sometimes associated muscles, sometimes both extremities of one side. When we consider these facts, and the large proportion of male children shown in Dr. West's table to be liable to the disease, we are by no means prepared to acquit the cerebral system of being in some sort the cause. We should draw no argument from Cruveilhier's observation of the atrophy of the anterior roots of the nerves in these cases. Emaciation of the nerves is found in the nerves of the leg in old cases of congenital clubfoot,* but equally with emaciation of the muscles to which they lead. The wasting of the nerves has not been proved to be antecedent to that of the muscles, and the occasional suddenness of the paralytic attack forbids us to expect that it should be so.

The condition of the muscles under the various circumstances in which they are placed in club-foot, constitutes an important study in reference to the treatment of the deformity. Muscles which have for a short time been affected with spasm, exhibit no change of structure; indeed it is long before they present an amount of structural alteration inconsistent with the restoration of their functions. Accordingly, in

* Anatomie Pathologique, tome i. livraison 2.

all recent cases of club-foot arising from spasm, the muscles may, after treatment, resume their action. But as the muscles belonging to an anchylosed joint will waste, shorten, and degenerate, so those which have occasioned permanent displacement of the tarsal bones, and have become inactive, are subsequently altered in structure. Atrophy soon follows inaction, and the structure then undergoes a fatty degeneration; the whole muscle, thus become a slender inextensile cord, forms an insuperable obstacle to the restoration of the foot to its natural position.

"But although the contracted muscles are shortened, their extremities being approximated, they have not at birth undergone structural shortening; nor, indeed, does structural shortening occur until inaction, or some other abnormal condition, has destroyed the power of the muscle: or, if structural shortening is ever found at birth, it is so rare as to prove the rule. That structural shortening has not taken place at birth, is proved by the restoration of the shape of the foot through mechanical means alone. And further, it is proved by the unaided restoration of the limb on cessation of spasmodic action, when the bones do not interfere to prevent the antagonistic action of the muscles, as is witnessed in talipes calcaneus, and in club-hand. In these congenital affections, as spasm ceases to affect one set of muscles, their opponents resume their power, and restore the normal shape of the limb." (p. 92.)

The presence or absence of this so-called structural shortening, is accordingly a question of serious import as to the requisite treatment of the case. If the distorted foot can be moved towards, or replaced in, its natural position by the temporary application of pressure with the hand, or if during sleep, or the anæsthetic action of chloroform, the foot spontaneously resumes its normal shape, it is plain that the offending muscles are neither permanently retracted nor destitute of contractile power; whilst structural shortening must exist, if the shortened muscle prove incapable under any circumstances, of elongation and further shortening. The division of the tendon of the muscle is undertaken with a different purpose in the two cases.

The division of the tendon of a muscle capable of contraction, whether healthy or affected with spasm, is followed by the instant withdrawal of the proximal from the distal end of the tendon, and by the complete cessation of spasm in the muscle so affected. If the limb be kept at rest, the tendon will reunite so perfectly as to leave little trace of its previous division, and the muscle will be found capable of gradual and complete extension to its original length. In this case, therefore, the tenotomy is resorted to for the purpose of removing spasm. If, on the contrary, the tendon be suffered to reunite at its original length in the case of a muscle permanently and structurally shortened, nothing is gained by the operation, the muscle remains inextensile as before. It is indispensable to obtain an elongation of the whole structure at the expense of the new uniting medium; and there seems scarcely any limit to the elongation of which that medium is capable in the early period of its organization. Accordingly, tenotomy is used for a muscle structurally shortened, that it may be lengthened without being stretched. The author supports these views by the results of many experiments which he has made on animals, as well as by the quotation of authorities; and he comments thus in the following passage, upon some observations on the divisions of tendons, which have recently been published in the *Medical Times and Gazette*:—

"Mr. Adams differs from former experimenters, and states that the space between the divided extremities of the tendon increases from one to two and a half inches. Now this is entirely at variance with what is known to occur, both in man and animals, when the limb is kept at rest, and in a position to favour reunion. But in the experiments undertaken by Mr. Adams, and for which rabbits were chosen, after division of the tendon the animal was allowed to move about, without any protection to prevent motion of the limb. Union was effected, but the uniting medium was stretched and rendered weak. Also in man the same occurs, when extension of the soft material is effected too rapidly. Not unfrequently, in animals, reunion does not take place, if the ends of the tendon are not in some measure approximated; but the ends of the tendon are gradually more widely separated, until the intervening space may be several inches in length, the lower portion of the limb being drawn downwards by the action of the antagonistic muscles. I have known this to occur in a dog: the intervening space became half a foot in length.

"That the uniting medium may be drawn out even to a greater extent than two and a half inches, is well understood; and in orthopædic surgery this is a most important circumstance, allowing, as it does, of the restoration of a limb to its normal position after structural change in the muscles has taken place, and after the formation of adhesions." (p. 103.)

The last chapter of the work is devoted to the subject of treatment, and principally to the surgical and mechanical management of the various deformities. A few observations are added on the constitutional treatment which is requisite in certain forms of talipes, but they are not intended to be systematic or complete.

Not every case of talipes requires tenotomy, or even mechanical treatment. If dentition, worms in the alimentary canal, and similar sources of irritation, occasion the distortion, that effect vanishes with the removal of its cause. Congenital calcaneus, again, has a tendency to spontaneous cure, and often is cured merely by the naturally greater muscular power at the back than at the front of the leg When the distortion is permanent, much discrimination is needed to appropriate the remedy to the deformity. By a judicious employment of surgical treatment to replace the feet, and the adaptation of suitable instruments along the lower limbs, Mr. Adams has recently restored some power of walking to a patient who was absolutely paralysed below the muscles of the hip. So also in partial paralysis of the muscles of the feet, which appear to constitute the majority of the non-congenital cases of talipes, tenotomy on one side of the foot, and support on the other, will do much to make up for the otherwise irreparable lesion. It should be observed that acquired distortions, whatever their cause, need never be allowed to increase. Mechanical means should be employed to prevent that tendency, even though the cause which gives rise to the distortion be still in action; tenotomy, however, should never be resorted to until the cause has ceased to act.

We need not follow the author through his description of the operations for dividing the several tendons, or of the after-treatment of the various cases, in which we observe nothing new. We are interested to observe, as confirmatory of our opinion, that the muscles of the calf are only secondarily affected, that the author always divides the tendo-Achillis last, sometimes not until the distortion of the foot itself has first been rectified by treatment, and sometimes not at all. To the suggestion of Dr. Little, which was carried out by Mr. Solly, that in extreme cases of congenital varus the cuboid bone should be removed, Mr. Brodhurst somewhat demurs, not deeming it to be necessary under forty years of age, and only then if other means had failed.

The author makes some judicious observations on the subject of unnecessary tenotomy :—

"In the treatment of distortions, it has been laid down as a law by an Edinburgh authority, that whatever structures are tense must be divided. This statement demands considerable qualification; its implicit observance would lead to fatal mistakes. For instance, in division of the hamstrings, if the knife be not limited to section of the tendons, but is permitted to divide all the structures that are tense, the peroneal nerve will necessarily be incised, together with the fascia and tendons. And it is both unnecessary and hazardous to follow the precepts of Phillips, to divide the retracted muscles in the sole of the foot, as well as the plantar fascia. In cases of old varus, it is of much importance to distinguish between the structures which it is necessary to divide, and those which may be extended mechanically; for if, following the advice above referred to, all the tense and shortened structures were to be divided, nothing would be left in the sole of the foot, and on the inner side of the foot and leg, undivided, but the bones. It is therefore important to determine which are the structures which it is imperative to divide, and to recognise the extensibility of others." (p. 112.)

The author differs from both Mr. Lizars and Dr. Little as to the age for operating on cases of congenital talipes. Mr. Lizars thinks two or three years of age the earliest time at which the division should be attempted. Dr. Little prefers the operation about the age of six or eight months. Mr. Brodhurst, however, considers four or six weeks after birth not too early for operation, if the infant be robust. The earlier the deformity is removed, the easier is the treatment, and the more perfect is the eventual development of the foot. He finds no difficulty in applying the instrument, and no evil consequence from its pressure :—

"The operation, when performed at this time, and the after-treatment, are so simple, that I hold it to be unjustifiable in the surgeon to seek delay, except on other grounds than age alone. The health of the child may require delay; but I know no other reason for postponement, if it be not the convenience of all parties concerned." (p. 114.)

For our own part, we should venture to add one qualification of the author's rule—viz., that the operation should not be practised early in hereditary cases. It is impossible to predicate of any spasmodic talipes that it will not recur; but when there is a known hereditary tendency, it would, we think, be premature, before the period of early childhood is past, to operate for a deformity which is so likely to recur.

The observations in this article apply almost entirely to those cases of distorted feet which by common consent are referred to the care of the orthopædic surgeon. On the subject of distortions arising from disease of joints, we find a passage of which we are not sure that we gather the right meaning. Speaking of articular inflammation as a cause of distortion, the author deprecates the employment of force to effect a reduction of the deformity, so long as any inflammation continues. "The muscles," he says, "become rigid to prevent motion and pain, and as the joint is restored, they likewise are restored to their normal condition. But when articular inflammation is the cause of muscular retraction, force should never be used to overcome retraction." All this is recognised practice, but when the author adds, "It is preferable to divide, when necessary, every tendon around a joint which interferes with motion, than to risk re-exciting inflammation," we are inclined to say, it would be preferable to let such a case alone. We should be glad to read the author's views on this part of the subject more at large in another edition.

On the whole, we have read the work with satisfaction and profit. That it is well got up, is due to the publisher; that it is well illustrated, must be mainly attributed to the artist; its literary merits belong to Mr. Brodhurst alone.

Review VIII.

1. *Die in und an dem Körper des Lebenden Menschen vorkommenden Parasiten. Ein Lehr- und Handbuch der Diagnose und Behandlung der Thierischen und Pflanzlichen Parasiten des Menschen.* Von Dr. Friedrich Kuechenmeister, Prakt. Arzt in Zittau. Mitglied der k. k. Gesellchaft der Aerzte zu Wien, &c. &c. — *Leipzig*, 1855. pp. 486.

The Parasites occurring in and upon the Living Human Body. A Manual of the Diagnosis and Treatment of the Animal and Vegetable Parasites of Man. By Dr. Friedrich Kuechenmeister.

2. *Fragmentarische Notizen und Abbildungen zur Helminthologie und Parasitenlehre.* Von. Dr. Med. A. Kraemer, Professor Extraord. und Praktischem Arzte zu Goettingen. ('Illustrirte Medizinische Zeitung,' Band iii. Heft 6, mit ii Tafeln.)

Fragmentary Notices of Helminthology and Parasites. By Dr. A. Kraemer. With Plates.

3. *On Cystic Entozoa in the Human Kidney, with an illustrative Case.* By T. H. Barker, M.D., Fellow of the Medical Society of London. 8vo. pp. 18.

4. *On the Trichina Spiralis.* By Dr. Bristowe and Mr. Rainey. ('Transactions of the Pathological Society of London,' vol. v. Session 1853-4.)

5. *Note on Dracunculus in the Island of Bombay.* By H. J. Carter, Esq., Assistant-Surgeon Bombay Medical Establishment. (With a Plate.) ('Transactions of the Medical and Physical Society of Bombay,' No. 2. New Series. Years 1853-4.)

6. *Observations on the Structure and Nature of the Filaria Medinensis, or Guinea Worm.* By George Busk, F.R.S., President of the Miscroscopical Society. ('Transactions of the Microscopical Society,' vol. ii. p. 65 et seq.)

Of all that great harvest of scientific information which is daily being reaped by the disciples of Bacon, there can hardly be a more welcome sheaf to the physiologist and the physician, than the mass of knowledge the last few years have afforded us respecting the various forms of parasitic life.

To sum up what has been accomplished in this department of zoology, would be impossible in the limits of an introductory paragraph. Even in respect to their influence upon practical medicine, the results of late researches are too diverse, as well as too important, to be included in a single passing allusion, without risking the charge of flippancy. Something of this kind, however, we must attempt, if only to give the reader the key-note of the following article; the clue by which those who rightly appreciate the dignity of the art of healing, will trace out the details of the various observations and dissections recorded in the works we shall allude to.

But a short time since, and medicine scarcely recognised any save the human entozoa. And although it did not claim to understand much about even these, still, by a curious kind of fatality, the little it did venture to affirm respecting them was almost uniformly wrong. It supposed them to arise by spontaneous generation; a process in which a peculiar cachexia of the person they inhabited formed an essential (if not the chief) part. The innumerable horde of symptoms they were alleged to produce, included many of the vaguest and most trivial description. Their whole function of nutrition was a mystery: its structures were generally misinterpreted; the substances it acted upon often doubtful; its details always completely unknown. Their reproductive organs prepared what were obviously the germs of an offspring; and what therefore, as such, almost disproved the theory of their own spontaneous origin. But not even the boldest and most skilful conjectures could follow these germs to their maturity. Lastly, the medical treatment that was directed toward the removal of these entozoa paralleled the obscurity of what was alleged about their life and habits. The constitution of the patient was to be strengthened, so as to shake off the cachexia to which these creatures owed their origin. Or conversely, it was to be poisoned, so as to induce the wretched entozoon to emigrate in sickness or disgust. Or, by a judicious selection of the poisonous drug, or a dexterous adjustment of its dose, the parasite was to be slain, while its living domicile remained unhurt;—a kind of perilous reversal of the juggler's feat of breaking an iron anvil on a person's stomach. And finally, even where the parasite inhabited the intestinal canal, and was therefore really amenable to agents introduced into this cavity, the physician and the quack often occupied much the same footing. Not only did the knowledge of the former scarcely surpass that of the latter, but even the treatment of both was alike empirical. They did but vie with each other in the quantity and virulence of the purgatives they prescribed. Indeed, on the whole, the worm-doctor often had the advantage. In rare instances, it is only fair to suppose that his longer experience, or his better choice of remedies, might afford him a real superiority over his orthodox competitor. But, in the vast majority of cases, his inferiority itself placed him on a vantage ground. For inasmuch as he knew less, he ventured more; and would scarcely think any risk or suffering too great for his patient to go through, provided only he himself could add another specimen to the scores of bottled victims by which he displayed his success on his well-known stall in some country fair or market.

Now, however, we seem to be rapidly approaching an era in our knowledge of these creatures, when every one of the above propositions respecting them must be exchanged for its logical contradictory. The theory of their spontaneous generation has long died out, though retained (we may almost say, *embalmed*) in some medical treatises of a much later period. The symptoms of their presence have been weeded of many of the trivial details formerly included amongst them. Their treatment is probably so far simplified, that no practitioner of respectable skill would now think of salivating a patient for cysts of the liver, or dosing him with camphor to poison a Guinea-worm in his leg. The cachexia alleged to be necessary for their origin, is known to be often wanting; and even when it exists, to be the effect, rather than the cause, of their presence. Last, but not least, their development has in many instances been traced with sufficient success to add a new and interesting chapter to the physiology of the generative function; to explain the way in which the parasite gains access to those structures of its victim which it is destined to inhabit; and, in many cases, to afford a prophylaxis that will ultimately go far to compensate for the limited efficacy of the still empirical (though now intelligent) treatment which is directed specially to their removal from the body.

The work which figures at the head of our list, does indeed denote something like an epoch in the history of the anatomy of these creatures. Since the time of Bremser, no treatise that we know of has ever laid before the public so large a mass of information as that it furnishes. And it is but justice to the author to add, that he is no mere compiler. On the contrary, much as has been undoubtedly contributed by other inquirers—much as we owe to Steenstrup, Eschricht, van Beneden, Siebold, Kölliker, Dujardin, Diesing, Owen, Busk, Rainey, and many others—Küchenmeister has himself added so large a number of observations and experiments, and these form, in some respects, so fitting a climax to the labours that have preceded them, that we may congratulate ourselves on having (slightly to alter a cant phrase of the day) the right man for the right book.

Almost the first paragraph of the work contains a statement which, were it not that Peter Pindar's laughable doggrel is too untranslateable to have ever experienced the honours of a German edition, seems expressly levelled at that immortal couplet which records the existing belief of the naturalists of his time, and states that

> "These fleas have other fleas to bite 'em,
> And these fleas, fleas, *ad infinitum*."

"So far as is hitherto known," says our author, "the parasites that infest the human species are *not* troubled with parasites themselves." Hence, sad as it is to lose so good a metaphor, we trust the British orator will give up all further citation of these lines; and concede to this stock quotation the repose so much needed by it, the Phœnix, the Upas-tree, the gardens of the Hesperides, and the foot-notes of the Eton Latin Grammar in general.

The *Helminthoid* class includes the three chief groups of Cestoid, Nematoid, and Trematoid worms: the affinity of which is evinced by the close analogy of their structure. In all three, the special senses are absent;

the integument is a softish chitinous envelope, more or less minutely wrinkled in squares; the (unstriped) muscular tissue is arranged in longitudinal and transverse bands, and consists of a delicate pulpy sarcode that sweats through the integument on the continued application of water, and collects in drops on its exterior; the canals that enclose and convey their nutritious fluids are generally multiple, often elaborately branched; and lastly, their development generally includes a stage of active or passive migration, during which the animal occupies a different habitation, and possesses a simpler and lower grade of structure, than those it respectively assumes in order to reach its maturity.

The *Cestoid* group is the first to claim our notice. Let us trace the history of an ordinary tape-worm or Tænia, *ab ovo.* The joints of the adult creature seem, in some species, to undergo a disintegration within the intestine of the animal they inhabit. Thus Küchenmeister (p. 10) on one occasion found the wall of the large intestine of a dog occupied by a white, sandy powder, the particles of which, on examination under the microscope, turned out to be innumerable ova of a *tænia serrata* higher up in the bowel, and were accompanied by separated joints of the animal. Hence it would appear not impossible that such liberated ova may sometimes experience the next stage of their development within the body of the animal to which the parent is attached. And in one or two instances the connexion of the *Cysticercus cellulosæ* in the muscles, with the previous presence of *Tænia solium* in the intestinal canal, has been verified in the human subject. A systematic inquiry will perhaps reveal numerous cases of this kind; and show that this contingency constitutes one of the most serious dangers which the mature parasite inflicts on the animal it inhabits, and one of the strongest indications for its removal.

But in a majority of instances it would seem that the joints (*proglottides**) of the worm are discharged from the body either singly or in numbers, still retaining an active vitality during a short period. At least it is difficult to conceive this expression as exaggerated, when we notice (what some of our readers have perhaps done) the violent contractions they offer shortly after their expulsion. The long single joint thus expelled often exhibits what seems to be an alternate contraction of the longitudinal and transverse fibres, which further engages the two sides of the segment in varying degree at different times. The result of this sequence of contractions is to produce movements which, supposing the disengaged joint to be lying on the ground, almost simulate† those of progression in a vermiform animal; and are, at any rate, capable of moving it to some little distance from the spot on which it may have

* It is really necessary to protest against some of the Latin and Greek terms which are being gradually imported into the language of science, as research multiplies objects of observation, and demands new names for them. The term *proglottis* is now made use of to denote the severed joint of a mature tape-worm filled with eggs or pupæ, or the analogous structures of some other entozoa. If this kind of nomenclature goes on, no student will, by and bye, be qualified to attend medical lectures or read medical books, without a year or two's preliminary study of glossaries.

† Küchenmeister describes such joints as "discharging eggs during their march along the ground"—an expression which is physiologically incorrect, and certainly exaggerates anything the Reviewer has ever seen of such movements under the most favourable circumstances (for example, in vivisections of animals).

fallen. The same contraction has been seen by Dujardin to expel some of the ova from the interior of the segment.

The structure of these ova (as they are called) exhibits far too marked a differentiation to entitle them strictly to such a term. It is only in the earlier stages of their development, if even then, that they are really the analogues of ordinary ova. As regards their contents, the possession of the hooklets renders these contents an embryo, rather than an ovum. As to their shell, it has seemed to the writer of this article, that even in the blind extremities of the branched canal which constitutes the oviduct, this is a distinctly calcareous substance, composed chiefly of carbonate of lime. And by the time they reach the central segments of this tube, their transparent shell is not only much thickened, but is converted into a dark yellow, or rather brown mass, by the interstitial deposit of a chitinous substance. At the same period, if not earlier, the hooklets of the enclosed embryo may readily be detected, without any preparation, by a moderately good microscope, especially with the aid of an achromatic condenser. The admixture of these organic elements with the calcareous shell, imparts to the latter that extraordinary power of resistance to chemical, and even mechanical violence, which it certainly possesses. The striæ which seem to pass perpendicularly to the surface of the shell throughout its whole mass, appear on careful examination to correspond with radii from the centre of the ovoid embryo; and to be, therefore, not quite parallel to each other. From the effects of carefully shifting the light that falls upon them, there is some little doubt whether they may not be due to circumstances of refraction, rather than to any really fibroid or tubular arrangement of definite structure. The capacity of resistance just alluded to, is an important point for consideration in connexion with the possibilities of the transmission of the embryo in time and space. The dilute acids and alkalies have little immediate effect on this leathery husk: and even after having been applied to it for hours, scarcely effect more than a slight swelling and transparency.

How far they may affect the life of the embryo within this dense and horny case, it is difficult to say: though from analogy we can hardly doubt that many chemical reagents can destroy its capacity of development, just as even an external deposit might perhaps have the same effect by preventing all interchange of gases, or any entry of oxygen from without. But what between their mechanical toughness and their chemical resistance, these pupa-sacs (as Mr. Busk, we believe, proposes to call them) defy, to all appearance, most sources of injury or decomposition. After months of exposure to warmth and moisture, the pulpy and putrid *débris* of segments of the *Tænia solium* yield ova which show no sign of any approach to degeneration or decay. And the writer of this Review has been struck by the remarkable way in which the size and structure of these ova sometimes allow them to elude all precautions that may be taken against their mechanical dispersion. In spite of every attempt to ensure their destruction, by steeping the specimen glasses he may have used in strong acids, and by afterwards bathing them in the flame of a spirit-lamp, he has once or twice found the characteristic ova appear most unaccountably in healthy and diseased tissues or secretions of the human body which he has subsequently examined with these glasses.

The speedy death of the expelled joints is followed by their putrefaction; a process which is, of course, hastened by warmth and moisture. And the dissolution of the parent tissues ultimately sets free the eggs contained in their interior, to be carried by the winds or waves wherever accident may determine. How vast a number of them miscarry, is evident when we attempt to take the census of a single tape-worm; or imagine the millions of eggs such a parent foists upon society during the years it may inhabit a given animal. What becomes of these abortive germs, how long they retain any vitality, and what are the circumstances that may rob them of it,—are questions we cannot answer, save by the conjecture that their albuminous and fatty materials are either applied to the soil in a decomposed form, or are consumed as food by various of the minute invertebrata that throng the surface of the earth and the waters. But the more fortunate minority of these eggs, the destiny of which is to eat instead of being eaten, after many and long wanderings of this passive nature, are at length engulfed by some unconscious animal in company with its food; and, through its alimentary canal, attain the locality of their second form of existence.

During this passive migration the ovum has retained its previous size ($\frac{1}{700}$th in.) and shape. But its thick wall now bursts, and sets free the enclosed embryo, which is an ovoid body, of nearly equal size, armed with six hooklets at one extremity. Impelled by instinct to begin its active migration, the embryo pierces the first portion of its path by bringing together the anterior pair of hooks so as to form with them a kind of wedge-shaped stiletto; and now drags itself forwards in the same direction by means of the two succeeding pairs of hooks, which it uses (to adopt the simile of our author) like a person who, in attempting to get out of a bow window, thrusts himself forwards by his elbows. In this way the minute embryo penetrates the body it inhabits, and only ceases its efforts on reaching the place its instinct recognises as suitable for its abode prior to the next series of changes it has to undergo. Streaks of reactive inflammation and exsudation generally indicate the minute channel by which the embryo thus traverses the wall of the digestive canal in its course to the liver, or other organs.

The migration by which the hooked embryos of the *Tænia solium* or the *Tænia cœnurus* traverse the body of the animal they inhabit, is thus suggested to be an active one:—a true locomotion, effected under the impulse of an instinct, and by means of certain special organs. But we must not hastily prejudge this important question by assuming that their migration is exclusively of this kind. On the contrary, in the present stage of our knowledge we may preferably bear in mind that there are fair grounds for conjecturing that the process is partly a passive one, at least in some of these parasites. Such a view is indeed suggested by the comparative rarity of the streaks of reactive inflammation alluded to —which ought, on the theory of an active migration, to be as numerous as the *Cysticerci* or *Cœnuri*, and as long as the interval between their site and their starting-point in the intestinal canal. And the channel of this passive migration is equally obvious. Presuming that the hooked embryo had once penetrated a vessel, it could hardly fail to be swept away by the rapid current of blood, and thus carried onwards, until arrested by its own attachment to the walls of the vascular system, or by its impaction in the narrowing calibre of a particular set of vessels. The numerous modifications which might be impressed on such a process by the size of the embryo, its choice of nutriment, its vigour of movement, we dare not attempt to indicate. It may suffice to add that the numerous (and therefore authentic) cases in which entozoa have been found in the blood, are completed by a communication from Leuckart to Küchenmeister, in which he states that he has found the embryos of Cestoid worms in the blood; and in such numbers, that he inclines to regard this as the ordinary channel of their migration, and the clue to the wide diffusion of their *scolices* throughout the body.

According to Küchenmeister, this intermediate stage of life seems to vary greatly in different species of Tænia. The embryo of one species, for example, penetrates the liver, where, by a true alternation of generation, it is converted into a cyst, from the interior of which are developed the heads (*scolices*) of the future tænia (*Echinococcus veterinorum seu scolicipariens*). An equally definite alternation probably occurs in another species, in which the similar cyst developes secondary and tertiary cysts, prior to the formation of scolices within their interior (*Echinococcus hominis seu altriciparien*s). In both, the cysts may fail to execute the last act of development, and remain barren of scolices.

The *Cysticercus* and *Cœnurus* also appear to represent a stage of development that necessarily implies the growth of new embryos within and from the original one. In the animal inhabited by the *Cysticercus cellulosæ*, the muscles, the areolar tissue, the brain, or the eye, are occupied by a variable number of larvæ thus produced from the embryos of the *tænia solium*. The *Cœnurus cerebralis* that so frequently inhabits the brain of the sheep, is similarly produced from the embryo of a *tænia cœnurus;* with the difference, that instead of the embryo enlarging after the shedding of its hooks, and developing a single scolex from a granular thickening of its interior, it produces a number of scolices (800 or more).

The larvæ thus formed only complete their development into their corresponding species of Tænia on being introduced into the alimentary canal of another animal. And as some of them—such as the cysticercus—occupy situations in which they are not necessarily fatal, a vast majority never experience this development at all, but degenerate and decay in the animal they inhabit, on reaching the term of their existence. Others—as the Cœnurus—almost ensure their liberation from the tissues they temporarily inhabit, by causing the death of their host. But even of these, a large proportion necessarily miscarries: indeed, putrefaction of the surrounding flesh appears always to kill them in a very short time. The experimental proof of these propositions it is one of Küchenmeister's chief merits to have established. These experiments, some of which date as far back as 1851, have been since repeated and confirmed by many other observers: especially by Von Siebold.

The conversion of the six-hooked embryo contained in the ovum of the tænia solium into a *Cysticercus cellulosa*, may be effected by feeding swine with the joints of the tape-worm mixed in its food. In seven weeks from the time of first feeding three swine with these eggs, he found one of them to contain Cysticerci, the vesicles of which, the size of a hempseed, began to exhibit a central cloudiness. A fortnight later, of the Cysticerci contained in a second swine, the largest individuals had attained the size of a pea; and their heads were beginning to be distinct. A fortnight later still, a third pig was occupied throughout its whole body by Cysticerci of different size and maturity.

The conversion of the Cysticercus into the Tænia, or of the scolex into the mature worm, has been similarly effected in this animal. But our author adduces a much more striking example of the same fact, having succeeded in feeding a condemned criminal with the Cysticercus, and verifying the presence of the tænia in his intestine after execution. The criminal (condemned for murder) was fed with Cysticerci in numbers of 12, 18, 15, 12, and 18, at five corresponding meals, 72, 60, 36, 24, and 12 hours before death. They appear to have been partly disguised by their resemblance to the grains of rice in warm rice-soup, partly by their likeness to the small bits of paste in a kind of vermicelli soup; and partly foisted on the unhappy wretch by being substituted for the small lumps of fat in blood-puddings.* The Cysticerci had all lain 72 hours in a cellar before being thus devoured by him; and hence some of them had been 130 hours outside a living organism. The necropsy, made 48 hours after death, revealed ten young tæniæ attached to the intestine by their hooks and suckers. Their length was $\frac{2}{3}$—$\frac{1}{3}$ inch; and they had an appendix of half this length, depressed or inverted, like that seen in the Tænia of the dog's intestine three days after being fed with the Cysticercus of the rabbit.

The production of the cœnurus from the eggs of a corresponding Tænia (*Tænia cœnurus*), and again of this tape-worm from the Cœnurus, precisely repeats the more material points of the preceding statements. The course of the author's experiments, however, reversed the above order.

He first fed a dog with the *Cœnurus cerebralis* obtained from a sheep; and on killing him two months after, detected the above tape-worm. The very same day, the joints of this *Tænia cœnurus* were administered to a sheep. Fifteen days after, the latter animal began to show signs of the rotatory disease; and in three days more was so "stupid," that it had to be killed. The necropsy showed the surface and the third ventricle of the brain occupied by fifteen vesicles of about the size of a hempseed: the cerebral surface and substance in their vicinity being traversed by yellow streaks of exsudation, like the burrow of an itch-mite.

Our space obliges us to sum up a host of negative experiments performed by Küchenmeister and others, as leading chiefly to the following conclusions. Each species of tænia has its own definite cysticercus:—the ova of the *Tænia solium* produce none but the *Cysticercus cellulosæ;* those of the *Tænia serrata*, the *Cysticercus pisiformis;* those of another tænia, the *Cysticercus tenuicollis;* those of the *Tænia cœnurus*, none but the *Cœnurus.* And these experiments rarely succeed in more than one or

* What does our English reader think of the moral side of this experiment? The Reviewer is aware that much may be said in favour of using these and similar opportunities for the promotion of science. But he protests against a living fellow-creature being regarded in the light of a mere subject of experiments of this kind, even though he be a murderer whose hours are numbered. And he ventures to think that few would controvert the conclusion of one of the most eminent physiologists of the day, who indignantly alluded to this experiment as being "debasing to our common nature."

two species of the domestic animals:—the pig, for example, is the only animal (beside man) in whom the *Cysticercus cellulosa* has been thus produced: the *Cysticercus tenuicollis* is limited to the goat and sheep; the *C. pisiformis* to the rabbit; the *cœnurus* to the sheep. In other words, it would seem that most, if not all, of these parasites are so far limited to one or two species of mammalia, that all others enjoy an absolute immunity from their attacks. It even appears likely that the animal infested by a given parasite at one stage of its development, may be altogether free from the presence of the same species at a later stage of its evolution; may be infested, for example, by a given Cysticercus, but not obnoxious to its subsequent Tænia.

The importance of such facts it is not easy to overrate. If hereafter confirmed (and many and careful must be the experiments that alone can establish them), they will add another and remarkable illustration of the way in which Nature antagonizes an extreme fecundity of the ova of these entozoa, by the parsimony with which it supplies the conditions of all further development. From the very mode of their introduction into the body, eggs and larvæ must alike be scattered in millions, before one of them can light upon its future dwelling-place. And even this numerical disproportion is increased as we trace it into further details. The larvæ are sometimes imprisoned during their whole life in the animal they infest. They are often destroyed by its death and putrefaction. Thy are probably the prey of many small animals, whose alimentary juices kill and dissolve them. But one or two species of higher animals are qualified to serve as their habitations, or are fitted to nourish their organism with the delicate chylous fluid which they appear to demand as their food. And lastly, even in the particular species they affect, a casual diarrhœa may prevent their attachment to the alimentary canal; just as an artificial one can remove them from this tube, after many years of uninterrupted fixation to its walls.

Of the two species of *Echinococcus* that infest the human subject, the *scoliciparíens* has been traced by Von Siebold and Küchenmeister into a tænia that inhabits the intestine of the dog. The *altriciparíens*, in which a secondary cell-growth is interposed between the enlarged embryonic cell and the larvæ or scolices, has not yet been successfully traced to maturity: but it may be conjectured to produce a Tænia that infests man as well as some of the domestic mammalia.

The presence of this Echinococcus is so frequent in Iceland as to constitute a dangerous endemic disease. Schleipner and Thorstensen fully confirm each other as to its extraordinary frequency. The former saw 57 persons suffering from this malady during his stay in the island; he regards it as far more common inland than on the coast; and estimates that in some of the worst districts it affected two or three individuals of every family. Further, his estimate, that it formed one-eighth of the total cases of disease, tolerably corresponds with that of Thorstensen, who calculated that it affected one in seven of the whole population.

Our author devotes a few pages to the discussion of this endemic; not, however, with much success. He notices the prevalence of vertigo among the cattle (suggestive of Cœnurus); tells us the size of the sheep, the amount of their wool, the species of the dogs used to assist in the pastoral care of these animals, as well as of the equally numerous oxen. He alludes (with more significance, we think) to the remarkably even temperature of the seasons; to the warmth and moisture of the air caused by the hot-springs; and to the comparative deficiency of vegetables. Finally, he points out the carelessness of the butchers, the rustic simplicity with which the population pay the rites of Cloacina—or rather altogether ignore her fane; and the extreme uncleanliness of their dung-heaps. The suggestions he offers mingle the zeal of the discoverer and the art of the physician just as irregularly and confusedly as the above details. Indeed, if we may say anything so invidious, this chapter offers us that exquisite disorder in order which seems to characterize, not so much the German philosophy, as the logical forms and the etymological structure of the German language itself.

Deferring all consideration of the prophylaxis that belongs equally to these and other entozoa, we would just point out that many of these circumstances may be at once eliminated from any inquiry into the causation of the epidemic. Dung-heaps are dirty, butchers careless, and water-closets scarce, in many other parts of the world besides Iceland. The influence of warmth and moisture would probably favour the production of more entozoa than one single species. And even granting that the comparative rarity of the swine in this island explains the comparative rarity of the Tape-worm, it does not seem at all made out that the dozen other species of Tænia elsewhere inhabiting the domestic mammalia, are unusually frequent here. The Cœnurus, even if often present, would not explain the Echinococcus, or more than partially answer this causative requirement. Indeed, the first and most specific part of the inquiry is evidently that of determining what is the species of Tænia amongst these mammals that corresponds to the human Echinococcus: a question that if it cannot be answered directly by feeding any of these animals with Echinococci, might perhaps be indirectly replied to by detecting some species with disproportionate frequency in their alimentary canal. That such Tænia, whatever its species, is not often developed in the intestine of the person infested with the Echinococcus, is a conclusion that is almost forced upon us by the presumable infrequency of all Tape-worms amongst these islanders. It would therefore almost appear as if human beings, though amenable to the ravages of the scolex, did not afford the suitable habitation for the mature worm into which the Echinococcus is developed.

Our author devotes a few pages of his Appendix to a consideration of some observations by Zeller and Virchow which would claim for one or two cases of alleged "alveolar colloid" of the liver, the real import of Echinococci in this organ. The grounds for this diagnosis are so simple that it is not necessary to lay much stress upon them; the detection of the parasite being one which of course negatives every other view. Indeed, Küchenmeister states that he had been so long aware of the chief peculiarities of this state in the lower animals, that he had passed over it in a few words; though he fully recognises the importance of Virchow's remarks in relation to human pathology.

The chief peculiarities of this state of liver refer to the size and arrangement of the cysts. They are mostly small, of a size varying from a millet-seed to a pea. They occupy the interstices or alveoli of a striated and apparently fibrous tissue; the cavities of these neighbouring alveoli communicating with each other by orifices of variable size. In respect to their contents they differ, not only in the fact that some (especially those on the surface of the liver) are barren of all traces of the cast-off hooks; but also in the degree of degeneration of the parasite and the hepatic fluids which their gelatinous mass exhibits. Lastly, as regards their arrangement, they take a course in the direction of the biliary vessels towards the intestine, in the neighbourhood of which they seem to be younger, more fertile, and less degenerated. Hence he regards Virchow's suggestion—that they are in some way the common offspring of a single immigrant, and that they extend mainly along the lymphatics of the organ—as probably correct. Indeed, he suggests that they are in point of fact processes of a single cyst, radiating from it as a centre along these vessels, to be then constricted and shut off from the central parent sac;—processes which are therefore fertile or otherwise, according as that part of this sac from which they are constructed had or had not begun to develop scolices at the time of their isolation. The theory is ingenious; and though open to some serious objections, evidently combines more of the facts than any other yet offered.

How far the tribe of Helminthia, so sparingly represented in man by the *Bothriocephalus*, offers any analogy to the above Tænia in effecting an active migration, remains at present very doubtful. Küchenmeister inclines to decide this question in the negative, in all those species which are apparently devoid of the hooklets that evidently form the organs of this locomotion in the embryo Tænia. Where the embryo Bothriocephalus possesses these hooklets, he thinks such a migration may possibly obtain. But in those species in which the embryo and the mature worm resemble each other in the absence of these hooks, he suggests that the migration is never more than a passive transference from the intestine of one animal to that of another. Indeed, in some cases it would seem that there may be no migration at all; that the ovum, extruded from the parent worm in the intestine of the animal it inhabits, then and there allows the escape of an embryo; which, even at this early date of existence, possesses the bothridia and the central bulb that distinguish the adult. But in the absence of such a resemblance, this mode of development is at most an unlikely one.

The passive migration, however, seems to be more frequent in these Bothriocephali. This migration seems to be generally from the intestine of a lower animal to that of a higher one; the carnivorous habits of the latter being made instrumental to its own reception of the worm. Thus, Creplin's observations, according to which the asexual and immature worm contained in one fish, becomes converted into a mature Bothriocephalus in the intestine of another fish, or of some bird of prey that has devoured and digested the body of its former habitation, represent a frequent occurrence in the development of this group of the cestoid entozoa. Even here, however, we are left in doubt as to whether there may not have been a previous stage, in the body of some other animal, in which the ova underwent a development to the degree of scolices, prior to being devoured by the fish that conducted them to their immature state. Hence, applying all this to the *Bothriocephalus latus* that infests the human subject, it is evidently impossible to say how this worm is first developed, where its scolex or larva is formed, and by what means (or even in what form) it is introduced into the intestinal canal. As regards its geographical distribution, Küchenmeister inclines to connect it with the migrations of the Tartar tribes, and to the moist character of the countries in which this worm occurs; in both cases, we need scarcely say, without any attempt at a definite conclusion.

The remarkable prevalence of the *Bothriocephalus latus* on certain of the northern coasts of Europe, seems to place its development in some definite relation to the fish-eating habits of the people who inhabit those districts. And though this entozoon certainly occurs in inland countries —such as Switzerland—to which any conjecture of this kind will hardly apply, still the liability seems never to approach the maximum stated for some sea-coasts. At least it is difficult to avoid deducing this conclusion from statements like that of Huss and others, as to the distribution of an endemic disease of this kind in the above localities. Huss (Krankheiten der Schweden) describes the Tænia lata (presumably the *Bothriocephalus latus*) as extremely common on part of the Lapland frontier (Norbattin) in Finland, and on the shores of the Gulf of Bothnia.* On the coast itself, there is scarcely a family altogether free from it—old and young, rich and poor, natives and immigrants, alike suffer from the worm. Indeed, even in one or two large towns on the mouths of rivers, at least two per cent. of the whole population experience its attacks. On passing inland, the frequency of the disease diminishes, until, eight or nine leagues from the coast, it almost ceases to be found. It is very interesting to notice that, in curiously direct contradiction to these striking facts, the natives themselves believe it to be hereditary. Huss himself seems to incline towards the more reasonable theory that attributes it chiefly to the diet, to the milk and fish—especially the salmon—that form so large a staple of the food in these districts. While he is careful to point out that the mountaineers inland are almost devoid of it, in spite of a diet almost exclusively carnivorous.

The section devoted to the *Trematoid* worms does not afford so much that is new to the pathologist. The admirable researches of Steenstrup, Von Siebold, and others, on the alternation of generations that prevails in many of the *Distomata*, have been so many years before the English public in the shape of Mr. Busk's 'Translation of Steenstrup's Memoir' (Ray Society, 1845), that we are spared the necessity of further allusion to their details. Our author ventures to conjecture that the flukes which infest the sheep, are derived from the *Cercariæ* contained in the various snails of the low marshy pastures in which the affected flocks are fed. But the stages by which the egg of the *Distoma* undergoes its development into the *Cercaria* remain still unknown; or rather, though we have some analogical grounds for conjecturing the nature of these changes, we are quite ignorant of the animals in which the embryo resides while going through them, and the degree in which each change of residence furthers their advance to maturity.

* The comparative absence of saline content in these waters is interesting in connexion with the prevalence of the worm in inland lake districts of Europe. It perhaps points to a fresh-water fish as the source of its introduction to the human body, or at least to a fresh-water marine animal, as its dwelling during one of the previous stages of evolution.

The comparative rarity of the *Distoma hepaticum* and *lanceolatum* in the human subject renders them less interesting to the physician than many other entozoa. The separation of the two species is rightly insisted on by our author.

Three other species are added—the *D. heterophyes, hæmatobium,* and *ophthalmobium*—on the authority of Bilharz, Griesinger, and Diesing. Of these three *Distomata,* the first has been noticed twice, in large numbers, in the small intestine of an Egyptian; the last once in the eye of a child five months old, at death. The remaining species, *D. hæmatobium,* is especially interesting, alike from its frequency in some parts of Egypt (according to Griesinger, 117 times in 363 necropsies, equal to 33 per cent.), and from the grave and characteristic symptoms and appearances to which it often gives rise.

The difficulty that Bilharz and Griesinger have both found in determining the nature (ovum or larva) of the embryo within the body of the animal inhabited by the parents (the *Distoma* being bisexual, the female often contained in a canal of the male), is one that it seems impossible at present to clear up. The *Distomata* inhabit the vena portæ and its branches, the intestinal canal, the walls of the urinary bladder, the ureters, or even the pelvis of the kidney. In the liver they seem to do less mischief than in the urinary apparatus: though the choking-up of the portal trunk with adult *Distomata,* and the deposit of eggs in the substance of the liver itself, which they bring about, must necessarily derange the function of this important organ, may starve it of blood, or perhaps irritate it to abscess. In the intestine they are often associated with appearances resembling those of dysentery:—with congestion, extravasation of blood, deposit upon and beneath the mucous membrane, fungoid excrescences, and croupy exsudations that occupy ulcerated patches of the bowel. In many of these cases, the eggs of the creature may be found wedged in long rows within the intestinal vessels,* or in and beneath such exsudations, or on the free surface of the mucous membrane. Hence Bilharz had suspected whether the dysentery endemic to Egypt might not have to the presence of these distomæ the same relation as the itch has to the *acarus.* But both he and Griesinger (says our author) convinced themselves that the coincidence was accidental; that there was no causal relation between the dysenteric appearances and the *Distomata,* since in many such diarrhœæ the parasite could not be detected.

Is not this decision rather too impartial? Or rather, is not this an impartiality somewhat akin to carelessness? No one can suppose that *all* dysentery is due to the *Distoma,* either in Egypt or any other country; just as no one has any right to presume that *all* cutaneous irritation, even in the Highlands, is of that kind for which sulphur used to be thought the only specific. But surely, when we consider what must be the physical effects of the presence of the *Distoma,* and what lesions it must inevitably produce, we can hardly doubt that such damage to one of the most vascular and inflammable tissues of the body, must often be followed by changes that (from their nature and locality) will almost necessarily simulate dysentery.

Such a conclusion receives a strong confirmation when we turn to the

* A further confirmation of the passive migration of entozoa by means of such channels, alluded to at p. 118.

lesions produced in the urinary apparatus. Here the mucous membrane appears swollen in places which are covered with a soft, sandy, rotten mass, that is firmly fixed to the subjacent tissue. The microscope shows this mass to consist of the full and empty shells of the parasitic ova, imbedded in a mixture of blood, exsudation, modified epithelium, and crystals of uric acid. The thickening of the submucous tissue often produces stricture of the ureter, which is followed by retention of urine, and all its dangerous consequences :—degeneration of the kidneys, pyelitis, dilatation of the pelvis, or atrophy of the renal substance : or the masses themselves become the nuclei of calculous deposits, and thus aid in the chlorotic exhaustion these creatures produce in the person they inhabit, by the consumption of blood they imply. Lastly, it seems not unlikely that the dislodgment of clots into the general circulation sometimes brings about pneumonia, in the way described by Virchow, and illustrated by the clinical researches of Kirkes.

Amongst the *Nematoid* worms, Küchenmeister first describes the *Trichocephalus dispar*, that haunts the large intestine and end of the ileum of the human subject. According to Rudolphi, this entozoon is extremely common : and the fact that it is rarely detected, must be ascribed chiefly to its colour and contents rendering it so like the fæcal mass with which it is mixed, as to allow it to escape notice, unless the latter be carefully washed and sifted so as to collect the contained entozoa. Such a mode of examination also shows that they are generally present in very large numbers.

The well-known details of their anatomy receive little addition from Küchenmeister's researches. Their mode of attachment to the intestine—some kind of fixation being, one would think, indispensable to their sojourn in the canal—also remains still unknown.

It is chiefly with respect to their development that Küchenmeister comes forward as a discoverer. He regards it as very probable that the *Trichina spiralis* and the *Trichocephalus dispar* have to each other the same relation as that which his observations would deduce for the *Tænia solium* and the *Cysticercus cellulosæ*. To speak more exactly, he suggests that just as the myriads of Cysticerci disseminated through the muscular system of a pig, represent the devoured larvæ of tape-worms, so the innumerable Trichinæ found in the muscles of man and certain mammalia are the larvæ of the Trichocephalus that inhabit the intestine.

Such a theory can of course only be based upon facts as definite as those which Küchenmeister brings forward in the case of the Cestoid worms. And in the absence of these, but little stress can be laid upon analogies to a different class of entozoa :—a class in whom many of the chief conditions of development appear to vary so remarkably, even in kindred genera.

The weak point in our author's theory therefore is, that he has hitherto fed animals with flesh containing Trichinæ, without producing any such results. And Leuckart seems to have been scarcely more successful ; save in a single experiment, in which Trichinæ were found in the intestines of mice who had been fed thus two days before.* But it is obvious that

* Unhappily, the remaining mice of the group had devoured each other (imitating the vices of their tyrants, as exemplified by the cats of Kilkenny). The zeal of these converts to carnivorous food (like that of their human antitheses, the vegetarians) appears to lead them into indiscretions.

such an experiment will not support any conclusion whatever. While the apparently conclusive results of Herbst* (in which the trichinous flesh of a badger had disseminated the same parasite through the flesh of three puppies in three months after) are not only opposed by the failures of the above observers to reproduce his results, but seem quite at variance with any phenomena of migration hitherto known to us. For the entire cyst can hardly be capable of any transit from the alimentary canal to the muscles, whether active or passive. And the Trichina it encloses not being a parent, or even a nursing individual, can scarcely be supposed to reach the muscles for a mere re-imprisonment—an imprisonment which, unlike that it has already completed, would neither multiply the numbers of the parasite, nor yet advance its development toward maturity.

We are therefore left to comment on that close similarity of structure in the *Trichina* and *trichocephalus* which forms at present the only support for the author's inference. In no organ is this so well marked as in the intestinal canal. In both, this tube exhibits precisely the same appearance and subdivision; while in both, the segment that occupies the anterior half of the body is dilated into a series of small sacs that give it quite a moniliform appearance. It will be interesting should future researches confirm the existing views as to the definite number and arrangement of these pharyngeal pouches; and show (what it will really be necessary to prove) that their form is neither produced nor seriously modified by any mere contraction of their sarcode lying externally to the tube beneath their chitinous envelope.

In respect to the anatomy of the *Trichina*, Luschka's researches appear to constitute one of Küchenmeister's chief sources of information. It is, however, strange that his citations of many observers do not include the really admirable descriptions and drawings of Dr. Bristowe and Mr. Rainey in the Pathological Society's Transactions (Session 1853—4); though the fact is probably due to his not having met with this publication.† In the absence of any verdict from eminent helminthologists, we will merely record our own individual conviction, that the view taken by those excellent observers as to the import of the appearances they describe, seems an untenable one: that even their own observations seem by far more easily reconciled with the decay, than with the formation, of the Trichinæ in the fatty cysts they describe. Indeed we have little doubt that, novel and interesting as it may be to find adipose tissue developed within a closed membranous cavity, the polar, as well as the intra-cellular fat they describe is a mere indirect result, or local accident, of the decay of these worms. Whether the membrane that sometimes surrounds and isolates the adipose tissue is exclusively the product of the human muscular mass, or of the parasite, it seems impossible at present to decide. But in any case it is difficult to avoid the impression that they have misinterpreted their own excellent observations;—that they have, so to speak, spelled them backwards; and have allotted the series of pheno-

* Annales des Sciences Naturelles, vol. xvii. p. 63.

† If the Reviewer (as a stranger to this Society) might take such a liberty, he would suggest a more liberal distribution of its Transactions to Foreign societies. At present they are printed, but scarcely published as their great value demands.

mena they so ably describe and illustrate to the wrong end of the life of the *Trichina spiralis.*

The *Ancyclostomum duodenale* is too important a member of the Nematoid class to be left without notice, from its extreme frequency and danger of its presence in Egypt and other tropical climates. Its length is about one-third to half an inch, its width about one-twentieth its length. Its head has a rounded apex; and its extremity, which is bevelled at the expense of its posterior surface, is provided with hooklets that occupy converging papillæ. The mouth contracts to open into a thick, muscular pharynx, which, widening as it passes downwards, ends, after occupying about one-seventh of the body, in the intestine. The sexual differences of the male and female are very interesting. Its pathological significance is chiefly due to the hæmorrhage caused by these parasites: which are often present in thousands between the valvulæ conniventes of the duodenum, jejunum, and ileum; and not infrequently in the submucous areolar tissue. In short, the physician practising in Egypt must never forget that the chlorosis of this climate is often the result of repeated and small hæmorrhages from the intestine, caused by these parasites. Turpentine, as Griesinger points out, promises to be the best remedy, both as a styptic and as a vermifuge.

The *Filaria medinensis* receives a consideration which will rather astonish those of our readers who are unacquainted with the peculiar manner in which the modern German sometimes studies the sacred writings. For out of about twenty pages devoted to this interesting parasite, about ten are set aside to its historical mention in various ancient authors. And no small share of the latter may be regarded as an attempt to prove that the "fiery flying serpent" which the Bible describes as sent among the Israelites in the course of their desert journey, was nothing more or less than the filaria! This amazing specimen of *exegesis* we shall not attempt to analyse. Whether our author's Hebrew be good or bad, it is hardly necessary to inquire; though from the remarkable care and distinctness with which it is brought forward, one is inclined to suspect that it is about on a par with the classical knowledge of Latin the display of which sometimes throws country gentlemen into ecstasies at agricultural meetings. In sober earnestness, such disquisitions are, to our mind, no legitimate object of criticism. The most placid of microscopists or chemists could scarcely bring himself to bestow a minute examination on the saliva that had just been wilfully spat into his face. And the man who attacks and insults the creed in which many (if not all) of us affect to live, and hope to die, commits a crime against all social truth and morality which goes up to a higher court than any we can imagine ourselves to preside in.* We will only hope that any one who may see fit to translate this otherwise excellent work, will (if only out of deference

* A single sentence is all that we shall adduce to justify these remarks:—"It may be supposed, either that Moses desired by the image of the (brazen) serpent to warn them against the danger of breaking off the worm, and to indicate that only he could recover who extracted, or had extracted from him, a creature like the uninjured serpent; or it is an indication, that they could only be assisted by a brazen instrument, probably a kind of cutting knife (of brass, because the Egyptians used knives of flint-stone), or a red-hot iron, such as is still made use of by the Abyssinians to open the abscess containing the filaria; and that Moses, by the brazen serpent, wished to make his countrymen more patient under the operation."

to our English feelings) remove this very silly and superfluous part of it: treat it, in fact, as a *Filaria* that has unfortunately crept into the body of the work, but which can be easily turned round one's finger, and so gently extracted.

By a not uninstructive fatality, the section on the *Filaria* is, in all other respects, one of the most meagre in the book. Our author considers that "the origin of this worm in the human body is still veiled in obscurity." We can venture to assure him that his own remark, a little further on, "it is an important circumstance that the English officers, who never went about with naked arms or feet, and never slept on the earth, remained free from the worm"—that this remark, we say, contains all (and more than all) that he seems to suspect. The observations of our zealous and able Indian colleagues* seem pretty conclusively to have established the most important facts in reference to its history and development, though they leave many details to be discovered by further researches.

The first stage in which we find the *Filaria medinensis* in the outer world is that of a minute worm. In this latter condition it has a length of about $\frac{1}{60}$th of an inch; and a breadth which, nowhere more than $\frac{1}{25}$th of its length, dwindles in the posterior fourth of its body to an invisibly fine point. Its usual haunts are the soft muddy shores or bottoms of tanks. But after the heavy rains in the regions where it is endemic, it may be found almost anywhere in the position these have left it by evaporation; a fact which obviously accounts for much that we know respecting its endemic distribution in various parts of Sennaar and Arabia, and especially explains its frequency in the marshy or dry beds of pools or wells, as well as rivers and torrents. It is therefore to the naked flesh of the person brought into contact with them that the parasites attach themselves, probably penetrating the sweat ducts, the calibre of many of which ($\frac{1}{1200}$th of an inch) would readily admit of their entry. And hence Macgregor's table of 172 cases—out of which 72 per cent. affected the feet, 20 per cent. the legs, 7 per cent. the thigh, and scarcely more than 1 per cent. the scrotum or hands—is easily explained by the circumstances of this inoculation: as is also its prevalence on the backs and shoulders of water-carriers. The period of incubation generally ranges between twelve weeks and twelve months; the longer duration probably representing the minimum of that diffuse suppuration or abscess in the areolar tissue, which first calls attention to the malady. A single bath in a tank has been often known to affect three or four persons at this distance of time, even though they have travelled hundreds of miles apart from each other shortly after bathing in company.†

This tank-worm, which is generally smaller and more slender than the young Guinea worm, has in all other respects the closest resemblance to it: is, indeed,

* Amongst these we may specially allude to a brief but pithy note "On Dracunculus in the Island of Bombay," by H. J. Carter (with a plate), in the Transactions of the Medical and Physical Society of Bombay, for the years 1853 and 1854: and to the descriptions of Forbes (Calcutta Medical and Physical Journal, vol. i. p. 215) and Duncan. (Id. vol. vii. p. 273.)

† The writer of this article lately had an instance of this kind brought under his notice by one of the sufferers, an officer of the Indian army. Here the period of incubation was twelve or thirteen weeks. In some officers, subsequently affected from the same tank, the period was seven months.

"Identical in form, colour, and general appearance. Like the young Guinea-worm, when fresh, it is very active; twisting and twirling about, seeking shelter or concealment in small pieces of silty *confervæ*, never quiet till it gets embedded in them; and frequently holding on by its tail, as if the latter were prehensile, or entangled in the mass by its temporary curvature. It swims after its head, but can fix its slender extremity to an opaque substance, and work the body into it. As the water evaporates, the tank-worm loses its energy; and perishes altogether, as it dries up or becomes putrescent: a few minutes in either state being sufficient to arrest its vitality irretrievably. Under similar circumstances the young Guinea-worm appears to be scarcely more hardy:* in short, both have extraordinarily little tenacity to life."†

The nutrition of the tank-worm and Guinea-worm can scarcely be followed into those details in which alone any contrast would be very important. But it can hardly be doubted that the parasite, bathed in the rich nutritional fluid of the human areolar tissue, must derive much of its nourishment from this fluid by endosmose through its own integuments. The size of the Guinea-worm might on such a view constitute a true hypertrophy — a direct result of the advantageous circumstances in which those Filariæ are placed which find their way into a human body. No doubt these circumstances are in some degree analogous to those which regulate the development of the Cestoid immigrant in the human intestinal canal. But the parallel may perhaps differ in one very important respect. The final habitation of the Cestoid worm, whether it raise the parasite an additional step in its general development (as in the Tænia), or convert the sexless, barren neuter, into a teeming parent (as in the Bothriocephalus)—in either case, however accidental as respects the individual, it seems to be quite essential to the species. In other words, without the intervention of that phase of life which implies this habitation, there is no reason to suppose that ova could ever be produced, or the species kept up. But it seems not impossible that the filaria may perhaps possess a very different relation to the human race. The Guinea-worm is probably (as Mr. Busk‡ has ably pointed out) the nursing or proligerous individual — the impersonated uterus — of the filaria: the analogue of the *Cercaria* in the *Distoma:* the middle of an alternate generation, or rather of a series of metamorphoses, both extremes of which remain unknown to us. Indeed, the differences between the tank-worm and the Guinea-worm scarcely amount to more than such a phase of development as this view would imply. But the tank-worm itself is found in such vast numbers§ on the soil of the regions where it is endemic, after the heavy periodical rains, that, considering the rarity of the disease, it is almost impossible to suppose all these individuals can have been even indirectly derived from the human body. The same remark will still more forcibly apply to many of the desert solitudes in which the Guinea-worm is often acquired. The fact that, in so many ages, the latter has not been propagated by infection beyond the limits of particular districts or countries, evidently points to the alternative,— either that the earlier (probably Infusorial) stage of the parasite's life is only possible under the conditions here present, or that a similar law restricts

* Mr. Busk's observations, however (Transactions of the Microscopical Society, vol. ii. p. 65), indicate a greater tenacity of life in this climate.

† H. J. Carter, *loc. cit.*

‡ Transactions of the Microscopical Society, vol. ii. p. 65.

§ H. J. Carter, *loc. cit.*

that unknown stage of existence which succeeds the extrusion of the young Guinea-worm, and raises it to the grade of development implied in the production of sexes, and finally of ova. In any case, there is at present no reason to suppose that a further infection can take place; or that a young Guinea-worm, extruded from one human being, can penetrate the tissues of another, and reproduce a nursing individual like that in which itself was once enclosed. The act of infection probably only takes place through the intervention of a whole cycle of metamorphoses, including at least one act of true generation. And if the above numerical statements be valid, attempts at prophylaxis may be almost reduced to covering (oiling ?) the exposed limbs of the healthy in these regions.

The space at our disposal will not allow any notice of the careful description Küchenmeister accords to the second group of parasites, distinguished by the possession of transversely striped muscles. The various Itch-insects of man and the domestic mammalia are very well delineated; and even the lively and amusing Flea, as well as the common (alas! too common) Bug, receive all the attention they so perseveringly claim. We hoped to find something about the prophylaxis and treatment of the former of these two insects. But our author is right. No man can altogether avoid its attacks: and no amount of instruction would teach many people (otherwise extremely able and dexterous) to catch fleas. Of this useful and agreeable art we may truly say, *nascitur non fit*:—caution, dissimulation, promptness, daring, coolness, and a host of other valuable qualities, can alone enable any one to become a successful hunter of these small deer. But though few in our profession can practise among the poorer classes without making the intimate acquaintance of these curious creatures, we are aware that dignity forbids our inquiring further into their history and habits. Were it not for such a *veto*, into what queer by-ways of natural history might we not seduce our readers. If mere ganglia sufficed for authorcraft, how interesting an autobiography even a Louse might write! The days of solemn meditation, attached by one claw (the finger locked *behind* the thumb) to the base of a long and strong hair: the grave domestic life: the sebaceous or epithelial materials that serve the economical family *pour tout potage*: the large egg-sac, carefully slung round a gigantic tree in the shape of a hair, hanging defenceless, but dangerous; *gare à qui la touche!* The martyrdom the attached sinecurist will undergo rather than leave its post, clinging to any hair it may chance to grasp long after the cruel physiologist has retrenched limbs, body, and each successive joint of the particular claw! Lastly, the delicacy of its respiratory tastes: the stern dictation of the residence of some species by the thinness or thickness of the hair of particular regions; and the ease with which all are amenable to the deadly (though flattering) process of inunction! But we refrain from entering into any subject so suspiciously amusing.

The *Vegetable parasites* form a second (and, of course, smaller) section of Küchenmeister's work. Of much less interest on the whole than the Animal parasites, we can yet recommend the descriptions given by our author as careful and explicit enough to form a good text-book on the subject. Of course, there are many details in respect to which it is impossible to expect that any one person can be acquainted with all that has been observed. Thus, on turning to the description of the *Cryptococcus cerevisiæ* (better known as the *Torula cerevisiæ*, or yeast plant), we find no mention of the fact (discovered, we believe, by Mr. Hoffmann, of Margate) that this vegetable parasite is really identical with the *Penicillum glaucum;* and, under favourable circumstances of exposure to air, developes the spores that prove this identity.*

In like manner it would not be difficult to add one or two species to the list of *Fungi* described in the work. But on the whole, the volume bears evidence of a praiseworthy industry, which has been so far successful, that it would not be easy to show any omission of importance. Indeed, whatever future additions may be made to our knowledge of these vegetable parasites, it is probable that many of them will be rather connected with phytology than with practical medicine. The distinction between a parasite which does destroy structure, and one which does not, though quite arbitrary and unscientific, will probably, in the long run, turn out to be intimately connected with those disturbances of function that chiefly attract the attention of the physician. The *Achorion*, or the *Trichophyton*, which bring about a definite lesion in the tissues of the hair and scalp, claim, *ipso facto*, an amount of attention few would be likely to devote to the fur of a feverous tongue, which may be cleaned off by the first mutton-chop that is masticated during convalescence. In like manner, just as the male† *Sarcoptes scabiei* is not only a smaller and more short-lived animal, but one which does not burrow in the human skin like the female, so this important distinction which is here connected with the sex, is, in some of the Acari that infest the lower animals, related to the species. The horse, for example, is troubled with two or three species of Acarus; some burrowing in the skin like the *Sarcoptes* of the human subject, some merely infesting the hair of the animal. The latter, when transplanted to man are comparatively harmless; while the former repeat the well-known symptoms of itch. This difference is, indeed, strictly analogous to one which may be traced in the *Pulex irritans* and the *Pulex penetrans.*

It is true that we cannot be said at present to know precisely which *Fungi* possess such a direct pathological significance. But this part of the inquiry can hardly be settled by the microscope alone. While supposing the medical evidence decides (as it seems, for instance, to have done in the case of the *Sarcina ventriculi*) that the parasite neither implies nor produces any specific derangement of the part of the body in which it is found, it will scarcely interest us whether the genus includes but one species or fifty. In fact, to speak candidly, the path of the phytologist will here diverge from that of the physician, towards a region which any one who rightly appreciates the true responsibilities of our profession, has no call to explore.

Such an allusion to the motto "*ὅβιός βραχύς ἡ δε τέχνη μακρή,*" may well remind us that we have hitherto said nothing about the therapeutic part of the work we have thus briefly noticed. Its only fault is that of too

* From an observation of Dr. Hassall's, detecting this plant in the stomach, we incline to conjecture that such a development may exceptionally take place in the fermenting contents of this organ.

† Kraemer, in the Illustrirte Medicinische Zeitung. Heft 2. München. 1855.

great a copiousness, or rather too small a discrimination, of remedies. The herring cure for tape-worms, the use of a pitch plaster as a depilatory, and other venerable modes of treatment of like doubtful efficacy, distract the reader's attention from the extremely valuable methods of treatment with which they are intercalated. Many of these remedies well illustrate that physic has its fashions as well as dress; and that the undue popularity a drug often acquires on its first introduction into the Pharmacopœia, is in many instances compensated by an equally unjust oblivion when the gloss of novelty has once worn off. Perhaps there are few better examples of such a contrast than may be found in the remedies for tape-worm. When Madame Nouffers first sold her secret remedy of the male fern to Louis XV., the price she received is sufficient evidence of the value attached to it by the ministry of this unhappy monarch. By and by, finding that it was less certain against the Tænia than against the Bothriocephalus (a statement which applies to the comparative efficacy of all the ordinary anthelmintics against the two), there arose a prejudice that its efficacy was entirely confined to the worm which was the more amenable to its influence. And ever since that time, this statement, first brought forward by the great authority of Bremser, has been repeated by writers on therapeutics, with the not unnatural result of almost checking all use of the drug in a country where the Bothriocephalus is little known. It is especially interesting to notice the comparative neglect into which this most useful remedy has gradually lapsed, because scarcely a month passes by without some sensible practitioner in the country coming forward to vouch for its efficacy in one or other of the Medical Journals. And we have reason to know that there are two or three physicians to large London hospitals who have for years scarcely ever had occasion to adopt any other remedy than this time-honoured anthelmintic, recommended by Theophrastus 2150 years ago.

Review IX.

1. *Der Harnsäure-Infarkt in den Nieren neugeborener Kinder in seiner physiologischen, pathologischen, und forensischen Bedeutung.* Von Julius Hodann, Wundarzt am Hospital zu Allerheiligen in Breslau. Nebst einer kolorirten Tafel.—*Breslau*, 1855. 4to. pp. 33.

The Physiological, Pathological, and Forensic Significations of the Uric-Acid Infarctus of the Kidneys of New-born Children. By Julius Hodann, Surgeon to the Hospital of All Saints at Breslau.

2. *Recherches sur les Maladies des Enfants Nouveau-nés.* Par M. Seux, Médecin-en-chef de l'Hospice de la Charité de Marseilles. ('Des différents états qui ont été désignés sous le nom vague d'ictère des Nouveau-nés,' chap iv.)—*Paris*, 1855.

Researches on the Diseases of New-born Children. By M. Seux, Chief Physician of the Charity Hospital at Marseilles. (Chap. iv. On the Different States which have been included under the vague Title of the 'Icterus of New-born Children.')

3. *Beiträge zur Pathologischen Anatomie der Neugebornen.* Von D. F. Weber, a. o. Professor der Pathologischen Anatomie in Kiel.

Dritte Lieferung. (Art., Icterus der Neugebornen.) *Kiel*, 1854. pp. 78.

Contributions to the Pathological Anatomy of New-born Children. By D. F. Weber. (Art., Icterus of New-born Children.)

It is extemely probable that the first contractions of the uterus after the rupture of the membranes and consequent discharge of the fluid of the amnios, so protecting to the child, exert an influence over the circulation between mother and offspring, and prepare for important changes in the course, direction, and even nature of the blood within the system of the latter. The internal organs and skin, as the placental ebb and flow becomes impeded, begin to experience the first of a more or less intense and long-continued state of vascular congestion; the vessels of the cranial contents swell, those of the thorax often become so gorged that the capillaries of the pleura and of the pericardium form extravasations, and, where the scalp is relieved from the pressure, cephalhæmatoma frequently arises. With this engorgement of the internal viscera and injection of the vessels of the skin, there are reasons for believing that changes already ensue in the qualitative constitution of the blood, changes only, of course, to be perfectly worked out by the self-dependent organism assuming its new offices of respiration, digestion, and the production of caloric. The somewhat sudden alterations in the direction and nature of the blood, here alluded to, are inducted without much turmoil, so to speak, in most children; whilst in others they bring about conditions scarcely to be regarded on the one hand as *pathological*, and yet not fairly to be considered as coming within the range of the normal conditions of the new-born child. One of these results, a recoil, as it were, before the equilibrium is attained, is seen in the occurrence of the well-known *icteroid colouration* of infants, and another in the presence of *renal-infarctus* by uric acid, a far less frequently recognised condition of the new-born child. The *former*, as we have stated, has long been known, but we must add, not always well discriminated; for under the terms *jaunisse des néophytes* of Sauvages, and *icteroides corporis infantum* of Juncher, things very different in their nature and relations have been mixed up together. The *latter* is a subject of modern inquiry altogether, and a *résumé* of our present information concerning which we shall now proceed to lay before our readers, reserving a few remarks on *icterus neonatorum* for the second place.

If the kidney of a new-born child is divided from its convexity towards the pelvis, a little short of complete separation, so that the two halves may be kept together at the base when folded back, the pyramidal masses of the tubuli cut through from the cortical layer down to the papillæ, are frequently observed to be filled with a bright chrome-yellow coloured substance. As numerous tubuli are opened into by the section, the pulverulent deposit appears to be as if free, and which, either by a gentle scrape of the scalpel, or the flow of a little water, may be easily removed. The latter being effected, the remaining deposit is then seen to be retained in the unopened, engorged, infarcted tubuli. Under the microscope, the removed deposit appears composed of brownish-yellow irregularly-rounded scabrous masses, which break up under pressure into amorphous granules, like those of the urate of ammonia. The larger masses are also more or less mixed with epithelial cells and membrane of the tubuli. In two cases, pure forms (cylindrical and rhombic) of uric-acid crystals have been seen by Hodann; and Virchow has referred to the occasional occurrence of a violet colouring matter, due to the presence of some of the constituents of blood. Hesling has further described spindle-shaped cells, having a vesicle at their termini filled—as the epithelial cells are said to be by Hodann—with the amorphous granules. The chrome-like deposit does not appear to be soluble in alcohol or in cold water, but hot water seems to partially dissolve it. This, according to Schlossberger, is proof that some of the uric acid is in combination with ammonia. The caustic alkalies also dissolve it. If a small portion be placed on a porcelain dish, and heated, and nitric acid then added, a beautiful red colour is developed, as carbonic acid and nitrogen are given off with effervescence. This latter or *murexid* test, indicates the presence of uric acid. In some cases, however, the chemical demonstration of uric acid is by no means easy, or at most only a negative result is arrived at. Both Hodann and Müller (of Breslau) have experienced this. The former advises, as the more satisfactory method, the following procedure:—*

"The kidneys being divided, and the presence of the infarctus determined, they should be dried either in the sun or by the heat of the stove. They soon shrivel up to a dark-brown skin, especially when placed on a disc of glass. The pyramids become only barely discernible as roundish elevations, but the *infarctus* is as evident as at the time of section; the striæ indicate the position of the dried-up corrugated uriniferous tubuli: the former are no longer of a yellow hue, but rise up from the dark-brown ground as blood-red yellowish stripes. Thus prepared, the object will last for years. A few strokes are now to be made, in a shaving manner with a fine knife, obliquely through the pyramids. The shaved-off particles—perhaps amounting to $\frac{1}{20}$th or $\frac{1}{10}$th of a grain—are to be allowed to fall on a porcelain capsule, then moistened with a few drops of distilled water, and are to be boiled over the flame of a small spirit-lamp. With the help of a fine knife or needle, the boiled-out parings are to be removed, a small quantity of nitric acid mixed with the residual fluid, and the effervescence having ceased, a drop of solution of ammonia being added, the purple or carmine red murexid-colour immediately appears." (p. 10.)

Such are the general characters of the *uric infarctus* of the kidneys of the new-born child. In a few instances it is of brown, or brownish-red, or straw colour; according to Hodann, a chrome-yellow is always its original hue, the darker and less frequently occurring shades being due to commencing decomposition.

"The commencing and disappearing infarctus are easily to be distinguished, particularly by means of a lens, though the naked eye will suffice. . . . .

"At the commencement, the termini of the efferent tubuli are filled, the calyces and pelvis are empty, showing no trace of colour in the fluid they contain. When the infarctus is at its height, the tubuli are filled not only so long as they run in a parallel direction, but also where their undulatory course indicates the boundary between cortical and medullary substances. In the calyces and pelvis, single pollen-like chrome-yellow granules are, as Cless correctly observes, already to be seen. As the infarctus disappears, much the same phenomena are witnessed as at its commencement, with the exception that the calyces, pelvis, ureter, and even bladder—nay, further, the anterior circumference of the prepuce in boys—

* We have prepared a kidney for permanent demonstration, and applied the murexid test satisfactorily as thus indicated.

contain the excreted matter in greater or less quantity. At the commencement, the fluid in the tubuli, behind the *infarctus*-material placed at the outlets of the papillæ, holds suspended the insoluble particles; towards the disappearance, the fluid between the papillæ and cortical matter is generally void of them. On pressure of the papillæ, the powder escapes, and fills the calyces." (p. 8.)

Both Virchow and Hodann comment on the lengthened period during which the deposit resists decomposition. The latter found that after a divided kidney had been exposed for forty-five days, and the whole had become "pappy," the infarctus injection, though much deeper in colour, was plainly visible. At a later period the granules were recognisable by the unhelped sight, even after decomposition had proceeded for three months. In the latter case the granules appeared like a fine powder upon the *débris* of decomposition, which had subsided to the bottom. The period and frequency of occurrence, and some other relations of the *infarctus*, are next proper to be considered. It is due to the following writers to state that the profession is indebted mainly to their labours for its more exact information upon these matters: viz., Cless, Engel, Schlossberger, Virchow, Hesling, Martin of Jena, Hoogeweg, and particularly Hodann of Breslau, whose researches we have just passed under notice.

The number of cases microscopically examined with the above purposes in view, and their details, so far recorded, are 427 in number; of these—

(a)	113	were dead born	and showed	no trace of infarctus.
(b)	1	died during birth	,,	traces of infarctus.*
(c)	31	,, soon after birth . . .	,,	in 1, traces of infarctus.
(d)	51	,, during the first day of life	,,	in 9, infarctus.
(e)	157	,, between the 2nd and 14th day	,,	in 81, infarctus.
(f)	74	,, between the 14th and 60th day	,,	in 27, infarctus.
	427			118

Now if we take alone those children (d, e, f,) who lived a few hours and upwards, we find that forty-two per cent. ($\frac{282}{117}$) exhibited renal infarctus, and that a broad line of demarcation must be drawn between infants who may fairly be said to have lived, and those who have not survived the birth. Two examples in the above table, however, viz.,—b, c, appear to militate against the doctrine that the presence of the infarctus would prove the independent vitality of the child; but this certainly must be said, that no case has yet been recorded in which the infarctus has been observed in the child before the act of labour has commenced, and that out of 427 children, not one born dead showed its presence. Further analysis of the histories of the cases above quoted shows that it is not until three-fourths, or eighteen hours, of the first day of extra-uterine life has been attained, that the infarctus will probably be found, and that traces of it may continue even to the end of the second month. We have not been influenced by the very general observations of Weber, Bednar, and others, upon this subject; the latter, however, it may be proper to observe, states† that, in every fourth examination of the bodies of new-born children and infants at the breast, taking place between the fourth and seventy-sixth day, he has found the deposit. We shall return to this point again. Relative to the diseases from which children pre-

* Since this article was written we have ourselves observed an analogous case.

† Die Krankheiten der Neugebornen und Säuglinge, &c. Dritter Theil, s. 189. Wien, 1853.

senting the infarctus have died, and the production of the latter, it does not appear any special connexion can be shown. Bednar is also of this opinion, at least he says he found the deposit in union with "the most different diseases" (op. cit.). General debility and atrophy, atelectasis, trismus, and convulsive maladies, apoplexy, pneumonia, icterus, &c., have been found in connexion with it. The only one of the above and other affections which at all inclines to an intimate relation, is *trismus.* The whole of Schlossberger's cases who died from this affection (five in number) had uric infarctus, as did likewise those (three in number) of Hodann. The examples of the latter, recorded by Charcelay (though reduced by him to a form of albuminous nephritis), were all in union with some convulsive disorder. A close connexion between *icterus* and the infarctus has been affirmed by some; but in none of Hodann's cases of the latter was any form of icterus present, whilst in a high degree of icterus the infarctus was not to be found. Hodann observes that:—

"Its connexion alone with one diseased condition—viz., *urolithiasis*—we cannot deny; because when the latter makes its appearance in early infancy, it is undoubtedly related to the infarctus, now become of pathologic import, and giving rise to the condition in question." (p. 18.)

The structural changes found after death coincided with the diseases above mentioned. Beside these, however, were found congestion of the contents of the cranium, even in many cases slight extravasations, and these in children the least to be suspected of such from their marked external atrophy. Both Schlossberger and Hodann agree in this, as also in the occurrence of occasional ecchymosis within the thorax. In the gall-bladder, according to Hodann, a reddish-coloured bile was generally present, in which the broken-up blood-corpuscles were demonstrable. Schlossberger found the reverse of this. Hodann observed the kidneys themselves to be—as the rule—of normal structure, always full of blood, and the cortical substance constantly darker coloured than the tubular matter. Frequently the two were separated by a narrow, dark, blood-red line. On the other hand, Schlossberger found the renal tissue generally anæmic, and but very rarely hyperæmiated, it appearing to him as if congestion of the organ excluded the infarctus. The interesting question may be now asked—is this uric infarctus of the kidney to be regarded as of physiologic or of pathologic moment? Before replying to it, it is necessary to inquire into the more exact seat of the origin and locality of the infarctus. As ordinarily seen, it is no doubt situated in the *tubuli recti* of the kidney. But is the deposit first formed there? If we adopt that view of the function of the renal organ which regards the cells lining the uriniferous tubules convoluted and straight as the instruments by which the solid matter of the urine is elaborated* (Bowman, Carpenter, &c.), and the *corpora Malpighiana* as the parts undertaking "the transudation of the superfluous fluid through the thin-walled and naked capillaries of which they are composed," we must regard the infarctus as arising in the place where it is usually observed. But this is not the theory adopted by Ludwig and Valentin, nor is it exactly that of Kölliker. In reference to this question, Hodann remarks:

* The reader is referred for some remarks upon this subject to the second volume of Dr. Morehead's Clinical Researches on Disease in India, p. 214 *et seq.*

"It thus appears that a diluted urine is formed in the Malpighian bodies; and that in the so-called convoluted canals (where greater vascularity secures to them a higher signification than that of the straight tubuli,) reciprocal actions ensue between the blood and the fluid coming from the Malpighian bodies, and that here the urine is perfected. It is probable that in the convoluted canals very important constituents of the urine first appear, *e.g.*, the greater portion of the urea; and in them also (considering the frequent degeneration of their epithelium), it seems likely that certain substances of the urine, *e.g.*, the colouring principle of it, are prepared." (p. 19.)

"As a result of investigations hitherto made, and from my own dissections (microscopic), I can but believe that the infarctus is formed in the convoluted uriniferous canals, and is first retained in the *tubuli recti*, in order to be afterward thence discharged. Whether it originates in a deposit from already-formed urine, or whether, being secreted from the vessels, it is only mixed with the diluted urine of the Malpighian bodies, is a point which shall be afterwards considered." (p. 21.)

In regard to the deposit being viewed as the result of a *physiological* or of a *pathological* process, we would observe that Schlossberger in his first essay (1842) was rather disposed to consider it as one of the latter; but in his second tract (1850) he appeared more doubtful about it, though still rather inclined to his first opinion. Virchow is decided as to its physiological character, and so are Hesling, and Martin of Jena. We shall allow Hodann to speak for himself:—

"After years' study of the matter before us, I am obliged to declare for the physiologic character of this excretion. Had it a pathologic import, it should (as before observed) be constantly found in connexion with certain diseases, and not as present with this and that affection, and then again absent from them. The reason of its being at first regarded as a pathologic product, clearly arose from the circumstance that no healthy child dying, diseased ones only could be examined. Engel declared, almost at the first, that it belonged to the normal condition, and also appeared in children who had died a violent death. It is much to be regretted that he did not express himself more fully on this point, since his so definite remark would seem to result from extensive experience. If the excretion appeared as a physiologic act at a determined time after birth, and vanished at another equally determinate, the solution of the question would be easier; the infarctus would then only occasionally be found when death happened to ensue during the period of its existence. But since we find it commencing, persisting, and disappearing; and since (though with considerable trouble) we can follow the discharge of the urates in living healthy children, we must view it in a physiologic light until its opposite relation be distinctly proved." (p. 25.)

Is it possible—it may be asked—for the infarctus to be a cadaveric product, or to be formed in the "agony," like certain cardiac clots and effusions into serous envelopes? Hodann replies—

"Supposing it to be so, it ought to be found in the bodies of all new-born children, or of those who die very young, or else we must admit that it is only in individual cases that such a physiologic or pathologic condition of the urine is present which necessitates its formation during the agony—an admission which would still more closely limit us to an inquiry as to what such condition may be. But I am decidedly of opinion that even now this question is satisfactorily answered in this—that the infarctus cannot be a cadaveric product, since it has been observed both at its onset and at its termination; and a series of investigations is before us which proves to a certainty its excretion during life." (p. 23.)

The explanation of the process (viewed physiologically) leading to the infarctus, is, according to Virchow, to be sought in that early and important change in the blood-plasma, which the sudden assumption by the child of the functions of respiration, of digestion, and the generation of caloric, immediately necessitates. So soon as the first steps in these great offices are made, the constitution of the circulating fluid becomes altered, important mechanico-chemical reciprocal changes taking place between its qualitative elements. Albumen, fibrin, &c., are destroyed, and urea, hippuric acid, and urate of ammonia remain as *dêbris*, the latter salt being, according to Virchow, a kind of abortion of urea, which is thrown out from the uriniferous tubes, as is also the case in adult life after great turmoil and important revolutions in the system, *e. g.*, in intermittent fever, general catarrh, rheumatic and gouty seizures. Hesling, Martin of Jena, and Piper agree to a great extent to the views of Virchow. After a description of the structure and functions of the kidney, and of the characters of the fœtal urine, Hodann details the process of labour, and observes that by this act—

"Every secerning organ awakes to a different and independent activity. Already even the blood perhaps commences to develope another chemical function in the Malpighian corpuscles and convoluted canals; it begins (forced by the unusual impulse, and debarred from returning to the maternal system) to produce the more solid constituents of the urine; and first among them—at the expense of the albumen—the uric-acid salts, as the forerunners of urea. This activity may proceed to such a height that the normal saturation is exceeded, and thus the urine can exceptionally let fall the urates so soon as it enters the *tubuli recti*, even during the act of labour, particularly when the latter is a severe one to the child. . . . . My opinion, that the commencement of the infarctus and the chief lever of its formation lie in the centric flow of blood, in the first hyperæmia of the kidneys produced by the act of labour, could only be overthrown if hereafter the infarctus should be found in children coming under investigation after removal by the Cæsarian section from the dying mother; or who have been removed from the maternal body before parturition had begun. Amongst my own dissections two such cases have occurred, where, after the sudden death of the mother, this juridical act was obliged to be performed; both children were nearly ripe, and afforded no trace of infarctus." (p. 26.)

Though according to the majority of the Continental pathologists who have investigated the subject before us, the uric-acid infarctus is to be regarded rather in a physiologic than in a pathologic aspect, several of them have stated their belief that its retention in the kidney may occur, giving rise ultimately to *calculus renalis aut vesicæ*. The relation between the two states may be thought to be further established by the fact, that while the half of all cases of calculus occurs before fourteen years of age are completed, it is only before four years that such calculi are formed exclusively of urate of ammonia; and that after ten years of age are attained, such calculi are comparatively rare. (Martin, De Lithogenesi, &c.; Prout.) Fortunately, the deposit is not ordinarily retained in the kidney, but is sooner or later mechanically washed along the urinary passages, discharged *per urethram*, and deposited on the "diapers." The occurrence of the latter, we find by reference to Graetzer,* to have been remarked some years since. "Feiler," says this

* Die Krankheiten der Fœtus, p. 110. Breslau, 1837.

writer—"speaks of children that have brought with them urinary calculi and sand into the world. The sand is then found on the napkins, which are not unfrequently, under these circumstances, coloured by blood." The true nature of the "spots on the napkins," well known to nurses as of occasional occurrence, and of the blood-red and carmine-coloured stains alluded to by several writers, is clearly resolvable into the infarctus deposit. Hodann's experiments, carefully conducted, are such as to leave no doubt, we think, upon this matter. Napkins of fine white linen were employed, and so arranged as to be a kind of filter for the more fluid portion of the urine; great care being taken in their arrangement, that no fæcal matter should fall on them. The coloured spots and stains on all these napkins were microscopically examined :—

"Blood-red and carmine-red spots I have never observed, but rather a reddish, or more frequently a brownish, or still more often a straw-yellow colouration. In most shades of it I could generally detect the urates by the microscope, after having allowed the coloured margin to moisten for some time with distilled water in a large watch-glass. In two cases, where the colouration was very deep, the residuum thus obtained (necessarily a very slight one) decidedly, though faintly, answered to the murexid test." (p. 23.)

Thirty-three cases were investigated; and in 14 the discharged infarctus was found, and most surely about the ninth day after birth. The transit of the deposit was likewise traced by the author step by step through the various urinary passages, to its final reception by the napkins :—

"Microscopic observation had satisfied me that the infarctus was forwarded from the calyces to the renal pelves, from the latter by the ureters to the bladder, and was thence ejected. . . . In one instance, of a boy fourteen days old, dying from trismus, the chrome-coloured matter lay like an encrusted ring around the prepuce, and permitted (as it was rather abundant) of microscopic and chemical demonstration." (p. 22.)

One of the most important questions connected with this infarctus of the kidney in the child, is that of its forensic value and import. Can it be accepted as a support to, or in any case in lieu of, what is usually called the "lung test" in medical jurisprudence? We have already alluded to some points which bear upon this question. It has not yet been discovered in children who have perished before labour has commenced. Not one of 113 dead-born infants exhibited the infarctus; and were it not for the two cases recorded by Martin of Jena,* and Hoogeweg, it might be said that the presence of the renal engorgement was proof of several hours' independent existence of the infant. In Martin's example, the child died after having breathed a few times; the meconium entered the air-passages, and suffocated it: whilst in that of Hoogeweg, the labour had been of more than thirty-three hours' duration, the child's heart ceasing to beat three-quarters of an hour before its completion. All that we yet know, certainly goes to prove that the infant *must have breathed*, for the infarctus to be found; but certainly not to show that the child was capable of living after birth, in such a sense of the term as would be of avail in a case of doubtful infanticide. On the other hand, also, the absence of the infarctus could not be

* And our own case, the kidney of which was lately exhibited at the Medical Society of London.

accepted as showing that the infant had not had an independent vitality. Elsässer, in a work* we cursorily noticed in our fourteenth volume (p. 377), thus writes, in connexion with the question before us:—

"In the numerous examinations of dead-born children conducted in the Catherine Hospital, which has now existed for twenty-five years, the yellow injection of the kidney has never once occurred. In children who have breathed, it has been found abundantly, even from the first day. Consequently, where in otherwise normal kidneys this injection is found, it may be laid down with almost absolute certainty that the child has lived; the converse proposition, however, cannot be maintained." (p. 76, op. cit.)

Weber, in the work described at the head of this article, expresses himself somewhat differently, but evidently speaks from no more precise knowledge of the matter than is to be gleaned from the original investigations previously mentioned by us:—

"In conclusion (says Weber), I have yet to observe, in reference to forensic medicine, that the 'sand' so much discussed as occurring in the uriniferous tubules of new-born children, cannot be regarded as a sign of established respiration. Although it is very often met with there in children who have lived a short time, yet, on the one hand, there are exceptions to this; and, on the other—what is still more important—it is found in individual cases where the children have died during birth." (Op. cit., Dritter Theil, p. 78.)

The following are Hodann's conclusions:—

"In forensic cases, its *absence* is no proof that the child has not breathed; its *presence* no surety that the child died at a definite time shortly after birth, but rendering it highly *probable* that death took place between the first and sixtieth day afterwards.

"If exceptionally found in cases where the 'lung test' went to show that respiration had not been established, it must yield in significance to the 'lung test;' if met with where the 'lung test' would indicate that respiration had ensued, it would support such test.

"If the lungs are putrid, or not to be obtained, or if a judgment must be arrived at through the kidneys, the presence of the infarctus would support the opinion that the infant had breathed, and render it at least probable that it had lived during the process of labour." (p. 31.)

We need scarcely say how much it is to be desired that the true value of the "infarctus test" be satisfactorily settled, as it is a sign of great durability. The renal engorgement has been found persisting for months after the occurrence of decomposition, and even for years after the kidneys have become dry and mummified. Observation will require to be especially directed to children known to have died before the commencement of labour, and to those not born, but removed by the Cæsarean section, on account of the sudden death of the mother. In concluding this subject, we feel called upon to thank H. Hodann for his very complete and interesting monograph. There are several matters of much value in relation to the topic we have been discussing in the work before us, but upon which we have not space to dilate. The occurrence of the renal infarctus in the lower animals, the characters of the urine at different ages, the fallacy to be guarded against in mistaking certain fibroid and pigment formations for the infarctus, and which appear to have misled Billard, nearly thirty years ago, who, no doubt, had then

* Untersuchungen über die Veränderungen im Körper der Neugeborenen durch Athmen, Lufteinblasen, &c., &c. Stuttgart, 1853.

the deposit before him; these and other questions are discussed at greater or less length by the surgeon of the Breslau Hospital.

We shall now pass to another condition apparently having its origin in the transitional steps made from *intra* to extra-*uterine* existence. The skin of the ripened fœtus and new-born infant in some respects approaches to the character of mucous membrane, in so far as it is delicate and highly injected. At birth the vessels of the tegumentary surface are so flooded with blood as to bestow a deep-red colour upon the child, a colour varying, however, very much in intensity, as well as in respect to the time it continues. Between the third and fifth day it disappears in many children, gradually and simply being supplanted by the natural hue of the skin. In others, during this change, when the red colour has been of medium depth, on pressure being made with the finger, a faint orange tint is produced, but which, on the pressure being removed, is replaced by the fading red. In some of the latter cases, towards the decline of the redness, a yellowish or orange tint appears as if suffused throughout it. In those instances where the redness is intense, as the colour passes away the whole skin of the infant frequently presents a distinct yellowish-orange or jaundice-like hue before the natural white colour of the integument is attained. This "icteroid colouration," however, is rarely of a true yellow-saffron hue, but rather of a "raw sienna" tone, with a slight dash of green in it; or in some cases, where deep in intensity, it is of a dirty orange tint. According to most observers the conjunctiva is free from colouration, the urine does not contain any of the elements of the bile; none of these are to be found in the serum of the blood, nor in any other fluid of the economy. Such examples we shall consider—following M. Seux—as forming a class (I.) distinct from the next series (Class 2), but between which we may at once confess *we* are not *always* able to discriminate. Class 2 is composed of cases which the following extract from M. Seux will fairly illustrate:—

"At a period varying from one to ten days, but more especially during the first four after birth, the skin of the infant exhibited a yellow tinge, varying in depth in different individuals. Generally of a light yellow the first day, it gradually increased in depth until it sometimes had a saffron-like appearance; the conjunctiva evinced similar shades, and the mucous membrane of the mouth was also slightly coloured yellow. The child sucked as usual, the digestive organs were not in the least disturbed, and the motions remained yellow and of the natural consistency. The pulse was normal, the skin of the proper temperature, the urine stained the napkins yellow, the face did not express pain; in fine, except for the yellow colour of the skin and of the conjunctivæ, it could not have been said that the infant was unwell. Sometimes a few days after the appearance of the jaundice, ophthalmia, with free secretion of pus, came on; the colour of the latter was like that of the skin; indeed, it was so similar that the nurses designated the affection as 'jaundice fallen upon the eyes.'"

In these cases, forming the second class, the icteroid colouration generally soon disappears, or demands but the very simplest therapeia.

Another and fewer number of examples we must group together as forming a third division. In these at any time between the second and eighth day after birth, particularly after exposure to cold, a yellow colour of the skin, and generally of the conjunctivæ, begins to appear. Soon more or less constitutional disturbance ensues, there is constipation, and dislike to take nourishment, the pulse varies (96 to 102, Seux), some emaciation occurs, the motions, such as they are, are pale, the urine is frequently coloured, and the skin scarcely maintains its proper temperature. In these cases, however, it will be found the rule, that under the employment of a proper therapeia a return to health will ensue in a week's time. In a few instances, it must be owned, such will not happen to be the case, for as the jaundice progresses the region of the liver becomes tumefied and painful to the touch, matters go on from bad to worse, and the child dies during the second or third week of its existence. After death, to quote Case III. of M. Seux:—

"The liver measures transversely fifteen centimètres, and ten from before to behind; it is of a reddish-grey colour. The gall-bladder is distended by very thick green bile; its internal surface presents no redness, on the contrary, it is stained green. When cut into, the liver emits a great quantity of black blood with which it was gorged. The substance of the organ breaks up everywhere with the greatest facility, and if scraped by the scalpel, becomes reduced to a pulp: it is a softened liver." (p. 276.)

A fourth, and a most important class of cases may be thus cursorily characterized: at any time within the first week of life, but mostly within the first half of it, symptoms of *mal-aise* appear in the child, with marked disturbance (varying in character) of the digestive organs. Soon a jaundiced hue pervades the skin, and is accompanied from the beginning by diarrhœa, abdominal tension, or even signs of acute and intense enteritis; or the jaundice is associated with hæmorrhage from the umbilical cord or its seat, or with phlebitis of the umbilical vessels, or with pyæmia, and which carries off the little patient. In other cases, again, what has been termed, rather absurdly, "malignant hepatitis," co-exists with the icterus, or the latter complicates *sclerema*,* or is in union with severe *muguet*, erysipelas, erythriasis, pleurisy, or *atelectasis*. The child, as may be readily believed, too frequently dies, rarely, indeed, surviving the third week. After death the anatomic changes then found vary of course with the complications above alluded to. In 45 cases recorded by M. Hervieux, the *icterus* was in union with sclerema in 31 instances; with enteritis in 15; with muguet in 5; and with pneumonia in 2. In M. Seux's worst forms:—

"The jaundice has always been coincident with enteritic inflammation, and only in two cases of twenty-six did the icteroid colouration *precede* the intestinal disorder. . . . Five children died on the fourth day after the appearance of the jaundice; one on the sixth, carried off by 'muguet,' combined with intense enteritis; the other three died, one on the fourth, one on the fifth, and one on the eighteenth day; the first two from simple enteritis, and the third under the influence of a gangrenous affection." (p. 268.)

In some of these cases the fat, bones, ligaments, cartilages, &c., (Billard, Hervieux), occasionally the sclerotic, the serous and mucous membranes, with the fluids they contain, have been found coloured yellow.

In a fifth and last class of instances may be placed those of congenital jaundice, in which either the mother being jaundiced has brought forth a jaundiced child,† or where the jaundice is produced by certain malfor-

* See on this and correlated points a valuable paper by Bierbaum, lately published in the 27th vol. of the Journal für Kinderkrankheiten.

† We were informed by the midwife at the Marylebone Infirmary, that such had occurred at that Institution (a short time before a visit we lately made to the wards, through the favour of Mr. Mushett); and that the "poor mother was quite alarmed at her yellow child."

mations, such as non-closure of the *ductus venosus*, absence of the hepatic or other ducts, as also those forms of fœtal jaundice described by Lobstein under the head of *kirrhonosis*. For examples of the latter varieties we may refer our readers to the work of Graetzer, which we have previously quoted. Now to all these varieties of jaundice, the general titles of *icterus* or *cholœmia neonatorum* have been applied most indiscriminately, and futile attempts have constantly been made to discover some single pathologic element as their more frequent or general cause. Hence we have had given as such cause, obstruction by viscidities, &c., of the gall-ducts (Baumes, Underwood, Frank, Dunglisson, Bamberger); narrowing of the biliary ducts, with suppurative inflammation of the liver (Heinke, Richard, de Nancy); accumulation in the gall-bladder of bile too viscid to pass away (Porchet); acute idiopathic hepatitis (Bouchut); passive congestion, or hyperæmia of the liver (Billard); the spread of a reflected irritation of peritonitic or enteritic inflammation to the liver (Valleix, Seux); sudden ligature of the cord (Morgagni); pressure of the liver and head during labour* (Chambers); disordered function of the hepatic organ in its transition from an office of hæmatosis to one of biliary secretion (Hervieux); to a vicarious function of the liver for the lungs, and to physiologic changes of the blood itself, &c. &c. (Virchow, Martin of Jena, and others).

From what has been already stated, it is clear that the jaundice of the new-born child may be produced by, or intimately associated with, many very different morbid conditions, but concerning which, as we are not discussing the subject in detail, it would be here out of place further to dilate upon. It is sufficient for our purpose to draw attention to the fact that forms of icteroid colouration coming under Class I., must certainly be distinguished from those of Classes 3, 4, 5, as essentially distinct in nature, and as not being cases of true jaundice at all. With respect to Class 2, we are in this difficulty,—viz., we doubt as to the propriety of maintaining it, believing that its extreme cases run respectively into I. and 3, or, at any rate, we are not always able to say whether a particular instance should be referred rather to one of these classes than to Class 2. Much of the difficulty of distinguishing between certain forms of the two types, arises from the fact that, according to our own observation,† the conjunctiva is quite as frequently as not tinged in examples of Class I., a circumstance denied by many others, and the occurrence of which is hence deemed by them as at once diagnosticating Class 2. We should, therefore, admit that we are as yet unprovided with a sure means of differentiating between *all* instances of true *icterus* and the *physiological* icteroid colouration of the new-born child. The propriety of the general distinction being made, however, has long been admitted, being first

* In a child we lately saw at the Marylebone Infirmary, the labour had been tedious, the pressure great, and cephalhæmatoma had arisen; nevertheless, the cutaneous injection was comparatively very slight, and was said to be quickly disappearing: here no icteroid colouration was manifesting itself. From further inquiries which we have made, and from what has come under our observation, we have been unable to trace a relation between pressure during labour and the intensity of the yellow colour of the integuments.

† The writer of this article would here beg to acknowledge the debt he is under to Dr. Robert Lee, for procuring him the *entrée* of the British Lying-in Hospital, in order to observe children from the birth; an advantage not commanded, of course, at the Royal Infirmary for Children.

recognised (so far as we are aware) by Seutin in 1797.* He remarked that—

"new-born children often have an apparent jaundice only, perhaps, as a consequence of a tedious labour, since he had very frequently observed children with such an appearance, but wanting all other signs of jaundice except the yellow colour." (Graetzer, op. cit. p. 120.)

What, then, it may be asked, is the nature of this icteroid colouration of new-born children? If it is not jaundice—if it does not depend upon the presence of some of the elements of bile in the blood—what is its interpretation? Is it, like the renal uric-infarctus, rather of a physiologic than of a pathologic import? According to some pathologists, a state of *polycholia* is present, an excess of bile is formed too great for the demands of the system, and which the skin is called upon to assist in disposing of for a few days. The origin of this polycholia is variously explained; it has been assumed that during the act of labour, and from the subsequent changes in the direction, &c., of the circulation, the liver becomes overflowed, as it were, by blood, and its functional activity called rapidly into action, yet with an embargo upon it all the time. Again, the liver has been viewed as an organ that can act vicariously for the lungs, kidneys, and skin, and hence that it is called upon to purify the blood and eliminate the carbon, when the duty of the pulmonary organs, &c., is imperfectly effected. A combination of these causes has been adopted by some. According to Hennig:†—

"Shortly after birth the liver is exposed to the danger of being overfilled with blood, and has besides occasionally to assume the deranged functions of the skin, and even of the kidneys. . . . By this polycholia, prematurely-born children are particularly liable to be attacked; it appears about the third or fourth day after birth, continues from four to fourteen days—longest in unripe new-born infants—and gives rise to no mischief beyond the arrestation of the oxygenised supplies to the blood accompanying the disturbed functions of the skin."

The same writer believes that besides this polycholia there is also some change in the colouring matter of the blood; but it does not appear to us that he can, more readily than ourselves, always discriminate between such cases as—following the views of some—we have placed in Class 2, and those of Class I.

In opposition to such theories as the above, it has been maintained that in the cases under consideration there do not exist signs either of hyperæmia of the liver or of deficient respiration; and secondly, it has been asserted (Lehmann) that there are no pathologico-anatomic facts which favour the view that the liver can act vicariously for the lungs; and that the separation of the carbon by the liver, as compared with that by the lungs, is so trifling, that the hepatic organ can hardly be regarded as essentially a blood-purifying apparatus, in so far as the elimination of carbon is concerned. In reference to the question of a compensatory power of some kind possessed by the liver in respect of the lungs, we may, *en passant*, refer the reader to a note in Dr Stokes' work 'On Diseases of the Heart,' &c. p. 259, and to some observations at p. 9 of the Introduction to 'Dr. Morehead's Clinical Researches on Disease in

* Beiträge zur ausübenden Arzneiwissenschaft.

† Lehrbuch der Krankheiten des Kindes, &c., p. 112 *et seq.* Leipzig, 1855.

India." In the opinion of some recent pathologists, the more satisfactory explanation of the icteroid hue of new-born children is that which, leaving the liver and bile entirely out of the question, regards it in the same light as the renal infarctus, *i.e.*, from a purely physiologic and *blood* point of view. But even here there is much obscurity. Levret, Grisolle, and others, consider the colouration more as a kind of ecchymosis, in the progressive changes of the hæmatin of the blood of which, it has its origin. To quote the words of M. Seux :—

"There truly exists a jaundice limited to the skin and related to the changes which the latter is subjected to after birth; the blood which at the moment of birth floods the integument, giving it a decided red colour, gradually retires; in doing so it assumes different colours, varying from a light yellow to a greenish yellow; these tints are evidently the result of a portion of the materials of the blood slowly resorbed, just as we observe in ecchymosis." (p. 250.)

So Bamberger,* speaking of *icterus neonatorum*, observes, that in certain cases—

"It arises from the progressive colour-changes of the hæmatin which, in consequence of the extreme cutaneous hyperæmia accompanying the first moments of life, is deposited in the skin, and remains there. It is analogous to the pigment-stains which continue after sinapisms and other cutaneous irritants."

In further elucidation of the cutaneous injection, M. Prosper Despine remarks (Seux), that it is mainly seen, or at least is chiefly followed by, the yellow colouration when the umbilical cord has been suddenly tied without the precaution of allowing it to bleed to a slight extent, which relieves the congestion of the skin. The following observations of Weber appear to us as worthy of extract—

"That it is connected with disturbance of the circulation, *at least in the skin*, produced probably by the abnormal prodromata of the act of labour, is further apparent to me from the fact that children having very red integuments, exhibit spots *still more intensely red* than the rest of the surface of the body. Such, for instance, are observed at those places on the brow which correspond to the frontal suture, and where, from the forcible undersliding of one frontal bone beneath the other, a fold of the skin arises during the passage through the lower pelvis. In this fold the circulation must be more or less hindered until the birth of the child. When we now observe, after a few days, that not only is the whole surface of the body (which was before very red) coloured yellow, but that these spots are especially so in which the circulation felt the pressure—when we further observe at these spots small extravasations of blood—and, finally, when we remark that this variety of *icterus* belongs to those transient forms not in the least obnoxious to life, I believe that we are justified in considering that such icterus has not its origin in pathologic changes of the liver, but that it depends on a direct pigment-metamorphosis, in part of true though small extravasations of blood in the skin, particularly at the before-mentioned spots, and in part of the colouring matter of such blood as has stagnated for some time in the hyperæmiated capillaries of the skin. The misfortune is, demonstrative evidence of this cannot be obtained; such cases never come to a post-mortem examination, so that an hyperæmia of the liver cannot be absolutely denied." (p. 44.)

Virchow takes a very different view from the above. We have already seen that he leans to the early occurrence of important qualitative changes

* Handbuch der Speciellen Pathologie und Therapie, &c. Redigirt Von Rud. Virchow. Band 6. Abtheil. i. Seite 590.

in the blood, mainly evincing themselves as a general breaking up, as it were, of this fluid. As one consequence of these changes in the blood at birth, we have (according to Virchow) the deposit of uric-acid salts (whose excretion is as rich as it is sudden) coming on during the first or second day, accompanied by considerable congestion of the kidney (Virchow, Hodann), and even occasionally by extravasation of blood, or the exudation of serum within the tissue of the renal organ. It is assumed, however, that it is the metamorphoses of the *blood-plasma* which are the bottom of the uric infarctus; but the *blood-corpuscules* likewise undergo change(!) They do so; and the visible expression of such change is seen in the *cutaneous icteroid colouration* of the infant. Martin of Jena agrees with the opinions of Virchow. We may, in conclusion, observe that, more than ten years back, M. Decaisne alluded* to an idiopathic icterus occurring in the adult, in which the discolouration of the skin was produced by some change in the constitution of the blood, and was altogether unconnected with a redundancy of bile. M. Decaisne was replied to, however,† by the assertion that such discolouration was merely *sallowness*, the result of a cachectic condition of the body in general, in which less blood, or blood of an altered character (other than connected with jaundice), circulates in the cutaneous vessels. It may not be altogether out of place for us here to remark also, that Messrs. Calvert and Moffat have lately called attention‡ to the icteroid colouration of the skin produced by the internal employment of the carbo-azotates of iron and ammonia. The patients (they inform us) became yellow as if they had a severe attack of jaundice, and not only the skin but also the conjunctivæ became coloured. The time necessary for this colouration to ensue seemed to vary, according to the individual, from two to sixteen days; the mean being seven days. The quantity of carbo-azotate generally producing it has been about a scruple, and it disappears in two or three days after the agent has ceased to be administered. Braconnot, who employed the carbo-azotate of potash, did not obtain the result in question. The writers above referred to think this was probably due to the insolubility of the salt employed, or that he (Braconnot) did not use true carbo-azotate of potash.

Review X.

Mémoires de la Société de Chirurgie de Paris. Tome Troisième. Fascicule 4. Tome Quatrième. Fascicules 1–4. 4to.—*Paris,* 1853–5.
Memoirs of the Society of Surgery of Paris.

After a longer interval than desirable, we resume our analysis of the Transactions of the Paris Surgical Society, commencing with the portion of the volume we left unfinished in our last notice.§

I. *On the Reduction of Dislocations of the Shoulder when Complicated with Fracture.* By M. Richet.

This paper is founded upon a highly interesting case which occurred

* Gazette Médicale, Mai, 1845. † Ranking's Abstract, vol. ii. p. 224.
‡ Pharmaceutical Journal, September, 1856.
§ Vide Brit. and For. Med.-Chir. Rev., vol. xiii. p. 465.

to the author, in the person of a man sixty-eight years of age, in whom the sub-coracoidean luxation of the left humerus was complicated by a fracture of the anatomical neck of this bone. He came to the hospital on the 8th of September; but replacement of the bone was not attempted until the 11th. The patient being then placed completely under the influence of chloroform, M. Richet surrounded the shoulder firmly with both his hands, so that the two thumbs rested on the acromion, and the four fingers of each hand were carried up to the summit of the axilla. By thus operating upon the head so as to move it from within outwards, the reduction was almost insensibly effected with the exertion of very little force. The fracture was then carefully set, and the bandages were retained until the end of October. The patient was seen accidentally nine months after, and was found able to execute all the movements of the limb without difficulty, while there was no perceptible shortening.

M. Richet points out how unsatisfactory the treatment of this description of accident has hitherto been; and believes that no improvement could have taken place until the discovery of anæsthetics, which, by annihilating muscular resistance, allow of the above manipulation being made with success. In luxation of the shoulder, this is indeed the only resistance to be encountered; for M. Richet believes that that attributed to the fibrous structures of the joint, or the insufficiency of the aperture in the capsule, has been much exaggerated. Numerous opportunities of examining such cases after death have always shown the opening in the capsule to be large, irregular, and jagged; while when dislocation is produced experimentally in the subject, very moderate direct pressure made by the hand on the axilla, suffices to effect reduction. Even in the case of dislocation of the femur, when, from the shape of the parts concerned, entanglement of the head in the edges of the capsule may occur, this is best remedied, not by extension, but by rotation and direct pressure. This last alone sufficed in some of the author's experiments, in which the head of the femur was dislocated, and its shaft sawn through below the trochanters—so as to simulate dislocation complicated with fracture. The general conclusion at which M. Richet arrives is, that while extension must continue as the general method of treating luxations unaccompanied with fracture, even here direct pressure (or as he terms it, *refoulement*, or pushing back) will always prove a powerful auxiliary; and is, in the case of dislocations complicated with fracture, the only procedure practicable, to the exclusion of extension.

M. Gosselin, reporting to the Society upon this paper, observes that although somewhat analogous procedures have been employed occasionally, and that the use of such direct pressure has been formally recommended by Chassaignac and Morel-Lavaillée; yet that the merits of converting what were little other than suggestions into clinical facts, and supporting them by experimental researches, is M. Richet's. He does not think that the employment of anæsthetics is so essential a feature as it is deemed by M. Richet; but that *refoulement* should be tried even when these are contra-indicated,—as, for example, during the state of nervous shock immediately after the accident. The procedure, the reporter observes, will not be applicable to all cases; for the diagnosis is often, owing to the great tumefaction, long obscure, so that the nature of the accident sometimes has not been detected for ten or twenty days, when the adhesions and changes consequent on inflammatory action would much impede *refoulement*. In other cases, when the head of the humerus is very high up or deeply placed, it may be impossible to reach the upper parts by the hands. M. Gosselin lays down the following as the principles of action when there is luxation with fracture of the upper extremity of the humerus or femur :—

"As soon as the double lesion is recognised, and whatever may be the epoch, always attempt reduction by the process of *refoulement*, having recourse to chloroform if nothing contra-indicates it. Repeat the attempt several times when the first does not succeed. If reduction is not accomplished, and there is sufficient of the upper fragment, try extension in combination with *refoulement*, the immovable apparatus being now advantageous. If success is still wanting, first treat the fracture, and when it is consolidated try reduction, employing if necessary the pulleys and dynamometer. Finally, if reduction is still impossible, favour, by passive motion, the formation of a false joint between the dislocated head and the neighbouring parts." (tome iii. p. 478.)

II. *On Incomplete Luxation of the Tibia Forwards.* By M. Désormeaux.

In this paper, M. Désormeaux describes a well-marked case of incomplete luxation of the tibia forwards, with more exactitude than any prior writer. He also enters at considerable length into the etiology and symptoms of the accident; but we must content ourselves here with his summary:

"1. Luxation of the tibia forwards is the result of the exaggerated extension, or more properly speaking, of the reversal forwards, of the leg upon the thigh. In this position, the articular surface of the tibia presents a surface inclined backwards, upon which the femur slides towards the ham. 2. In incomplete luxation, the displacement is limited by the tension of the portion of the tendon of the semimembranosus which is inserted above the external condyle of the femur. 3. The injury to the ligamentous parts is less than is generally supposed, and may be confined to partial laceration of the anterior crucial ligament. 4. The principal symptoms of incomplete luxation of the tibia forwards, are : (1) The projection of the tibia forwards, allowing the anterior parts of its glenoid cavities to be felt ; (2) The projection of the condyles into the ham, and, consequently, the increase of the antero-posterior diameter of the articulation ; (3) The absence of actual shortening ; (4) The rotation of the foot inwards, and the facility of the movements of rotation around the axis of the leg, as long as the foot does not go beyond its median position—the movements taking place in the hip-joint, when the point of the foot is carried outwards ; (5) The very great mobility of the leg upon the thigh in every direction, due to the new relations of the osseous surfaces ; (6) The angular separation which takes place between the articular surfaces of the tibia and femur during flexion ; (7) The position of the patella, the anterior surface of which looks forwards and upwards ; and the depressions observed at its sides. These two symptoms disappear when effusion takes place into the joint. 5. Among these symptoms, those which are of most service in distinguishing incomplete luxations are, the absence of shortening, the extent of the projection of the tibia, and the direction of the patella, with the depressions at its sides, when effusion is not present. 6. The best mode of reduction consists in the flexion of the limb, combined with slight traction." (tome iii. p. 554.)

III. *On Sub-astragalian Luxations.* By Paul Broca.

This is a valuable memoir, in which the author seeks to introduce a more methodical nomenclature of some of the displacements of the ankle, which, under the names of dislocations of the astragalus, and dislocation of the instep, have been confounded with each other, however different in nature they may have been. M. Broca states that no one who has not made an analytical examination of the cases that have been published, can have the least idea of the amount of contradiction and error that are due to this adoption of incorrect denominations. He has carefully examined more than 130 recorded cases of so-called dislocation of the astragalus; and we know of no more useful labour that can be performed by an inquirer endowed with the requisite critical sagacity and honesty of purpose, than this marshalling together of facts and supposed facts, estimating them at their true value, acknowledging some as the worthy servants of science, degrading others to a position to which their own worthlessness, or the incompetence or carelessness of their describers justly assign them, and conferring upon them all appellations that are expressive and distinctive.

The following is the division of the luxations of the instep proposed by M. Broca:—

"We may study in the region of the instep, several groups of luxations quite distinct from each other. 1. *Tibio-tarsal* luxations; 2. *Sub-astragalian* luxations, in which the astragalus maintains its relations with the bones of the leg, while the rest of the foot is carried in a variable direction; 3. *Medio-tarsan* luxations, in which the posterior row of the tarsus preserves its relations with the bones of the leg, while the front row is entirely or in part displaced; 4. Luxations of the *astragalus, properly so called*, in which this bone, as a result of a complex movement and great violence, is expelled from its position, without the other bones of the foot losing their reciprocal relations. In other words, a luxation of the astragalus is nothing else than the union of a tibio-tarsal and a sub-astragalian luxation." (tome iii. p. 571.)

M. Broca defers the consideration of this last form of dislocation until a future opportunity; and in the present memoir he only occupies himself with the second and third of the above groups, which have never as yet been made the subject of a distinct monograph.

1. Sub-astragalian Luxations: of these, three species have been admitted,—sub-astragalian luxation, properly so-called, in which the other parts of the foot are carried from beneath the astragalus, which retains its normal position; displacement of the calcaneum alone; and separation of the scaphoid alone. Deferring the consideration of this last for awhile, M. Broca first proceeds to examine whether there are sufficient grounds for admitting the possibility of the *calcaneum* being luxated alone, contrary to the inferences that would be drawn from its anatomical examination. The imperfect and erroneous accounts given by ancient writers, and the incomplete histories of the only three cases furnished in modern times, by Chelius, Hancock, and Rognetta, prohibit any such admission being made.

He therefore confines his attention now to *sub-astragalian luxation, properly so called*, which accident, though isolated examples had been recorded, especially by Arnott, Macdonnel, and Hancock, was only formally described by Nélaton in 1847, and then under the title of partial dislocation of the astragalus. This luxation may take place laterally or *backwards*, the lower surface of the astragalus lying, in this last case, upon the dorsal surface of the second row of the tarsus. Of this there is, however, only one example on record, occurring in the person of Mr. Carmichael, and related by Macdonnel in the fourteenth volume of the Dublin Journal (1835).

Lateral displacements are much less rare, although they have been described by most narrators without a distinct appreciation of their nature. Leaving out cases the accounts of which are too imperfect, M. Broca has collected 19 cases, several of these being verified by autopsies. Of these, the displacement occurred thirteen times externally and six times internally. In 1 case the nature of the cause was not stated, in 6 it was due to the direct action of a heavy body upon the astragalus, and in all the other cases it arose from a fall on the foot, that is, from an indirect cause. M. Broca finds the explanation of the operation of this latter in the fact of the sub-astragular articulation being the principal seat of the motions of adduction and abduction of the foot. In dislocation inwards there has been forced adduction, and in dislocation outwards forced abduction: and although in the cases published there is scarcely any account given of the position of the foot at the time of the accident, yet in 10 of these cases the details of the symptoms after its occurrence are sufficiently precise to enable us to ascertain that in 4 luxations inwards the axis of the foot was in a state of permanent adduction, and in 6 luxations outwards it was in a state of abduction.

Lesions of surrounding parts.—The sub-astragalian ligament is the first torn, its external fibres, however, sometimes resisting, which may explain the irreducibility of the luxation in certain cases. The two sub-astragalian synovial capsules are also partially or entirely torn, and the astragalo-scaphoidean ligament is always found ruptured to a great extent, allowing the passage of the head of the astragalus. One or both of the fibrous bundles, forming parts of the lateral ligaments uniting the astragalus to the calcaneum, are also ruptured. The rupture of the integument by the head of the astragalus is one of the most frequent complications; but although all the cases collected by M. Broca have presented it with the exception of four, it must not be considered an almost necessary occurrence. The existence of sub-astragalian luxation has usually hitherto been confounded with luxation of the astragalus, properly so-called; and when the integument has not been broken, the relative position of the astragalus has been often too imperfectly indicated to allow of more than a strong suspicion being entertained that some of these cases were examples of simple sub-astragalian dislocation. The numerous tendons in the region of the head of the astragalus often undergo rupture or displacement, and the tibial arteries may become ruptured or stretched. Fractures may also complicate these luxations. The articular surfaces of the calcaneum or astragalus may become fractured or detached, but these are of no great importance. In one case the tibia was fractured as well as the fibula, while in 6 the fibula alone was broken, all these (6 in 13) occurring in cases of external luxation.

Symptoms.—In luxation *outwards* there is usually more or less abduction; the external edge of the foot is almost always raised, the inner resting on the ground; when a wound exists, it allows of the issue of the head of the astragalus below and in front of the *malleolus internus;* the tendon of the *tibialis posticus* is either displaced or torn; and the posterior tibial artery is either ruptured or stretched. These signs are present when the luxation exists in its first degree, the astragalus still lying in part upon the calcaneum: but, in the second degree, when the two bones are quite separated, and the calcaneum becomes placed on the side of the fibula, there is shortening of the limb, and the tibia, fibula, and astragalus, together, make a projection of two or three inches externally. In the luxation *inwards*, the positions of the foot and of the wound are the opposites of those stated above, the astragalus usually lies on the dorsal surface of the cuboido-scaphoidean articulation, and it is the anterior tibial artery that may become torn or stretched.

There are two essential signs common to both luxations:—1. Whenever the head of the astragalus retains its normal relations with the bones of the leg (which may be easily ascertained, as it always strongly distends or passes through the skin), we may be certain we have to do with a sub-astragalian luxation. 2. The movements of flexion and extension of the instep, which are lost in tibio-tarsal luxation, and in total luxations of the astragalus, are preserved in sub-astragalian dislocations.

Diagnosis.—From what has been said of the signs of this accident, it is evident that its diagnosis is easy, and yet the greater number of cases have been confounded with luxation of the astragalus, properly so-called. The means of distinguishing the two are stated above. It might also be mistaken for lateral luxations of the foot, inasmuch as the tibio-tarsal luxation outwards gives to the foot a form and position somewhat resembling internal sub-astragalian dislocation, and internal luxation of the foot resembles external sub-astragalian. Here, again, if we find the head of the astragalus projecting, the luxation is sub-astragalian; if not, it is a luxation of the foot. Again, certain fractures, accompanied by displacement, may strongly simulate these luxations. Such are what M. Broca terms *sub-trochlean fractures of the astragalus*, in which, the bone becoming divided horizontally into two segments, the lower of these retaining its attachments to the calcaneum, may become displaced with it laterally, if the force has ruptured the ligaments uniting the calcaneum with the bones of the leg. The diagnosis is again to be derived from an examination of the head of the astragalus. If this is found projecting on the back or the internal edge of the foot, we are certain that it is a case of sub-astragalian luxation, and not a case of sub-trochlean fracture.

Prognosis.—This is much influenced by whether the luxation be complicated by a wound or not. In the latter case, the displacement is usually less considerable and reduction more easy. Among the 5 cases collected, the luxation was only irreducible in one: but of 17 cases complicated with wound, reduction only took place in 5 of them, and in 2 of these the patients died. Of the 12 irreducible luxations, immediate amputation was performed in 3 cases; in 1 the astragalus became necrosed, and was detached; and in 8 cases this bone was extracted, 6 patients recovering and 2 dying.

Treatment.—The rules for this are thus summed up by M. Broca:—

"1. In luxations unaccompanied by a wound we must attempt reduction, and especially by means of the knee. If our attempt fail, we must wait. In the event of an abscess afterwards occurring we must open it, and subsequently extract the astragalus—an operation which is *then* attended with remarkably little danger. 2. In luxations, when there is a wound, we must also attempt reduction; having recourse to *débridement* and tenotomy, if necessary. When reduction is not possible, the astragalus should be at once removed, as by this operation three-fourths of the patients are saved, and it is of less gravity than amputation of the leg, and has the advantage of preserving the functions of the limb. (tome iii. p. 616.)

2. Medio-tarsan Luxations.—This is the name proposed by M. Broca for displacements which may occur between the first and second rows of the tarsus, the luxation being termed total when the scaphoid and cuboid are simultaneously displaced, and partial should either bone be displaced separately. This division of the Memoir need not occupy us long, as the author states his belief that these dislocations, which the anatomical conditions of the parts render highly improbable, have, in point of fact, never yet occurred, the conditions which have been described as such having been faulty interpretations. First, as to *total medio-tarsan luxations*, passing by two cases so vaguely indicated by Petit as to amount to little more than assertions, M. Broca knows only of two others which have been published as examples of medio-tarsan luxation. Both are found in Sir A. Cooper's work, under the appellation of Dislocation of the Os Calcis and Astragalus. The first of these was only observed by some students, and so vaguely related to Sir A. Cooper as to be quite worthless. The other, observed by South under Cline, Cooper, M. Broca is convinced, inserted in his work after merely reading the appellation given it by Mr. South—viz., a luxation of the astragalus and os calcis; by which he signified a luxation between the two bones—the case being, in fact, a fine example of sub-astragular luxation inwards. Cooper, however, in his nomenclature, applied this term to medio-tarsan dislocation, and accordingly the case was so regarded by him, which it could never have been had he perused the details. But this same case (that of Gilmore) had also been observed under Cline, by Green, and was likewise communicated by him to Cooper, as an example of dislocation of the astragalus outwards, so that Sir Astley has twice inserted the same case—once as an example of luxation of the astragalus outwards, and again as a medio-tarsan luxation!*

M. Broca passes under critical review all the observations purporting to be examples of separate luxation of the *scaphoid*, and comes to the conclusion that its occurrence remains entirely unproved, all the cases being capable of explanation in admitting a sub-astragalian luxation, and some being explicable in no other way. In denying that these luxations have ever been met with, he does not assert the impossibility of their occurrence, however strongly such assertion may seem to be supported by anatomical theory. All he now maintains is, that the history of these pretended luxations has been founded upon erroneous bases, that not a single fact demonstrates their existence, that all documents hitherto published witness against them, and that if one or other of these lesions should come hereafter to be observed, it will doubtless present different characters than those hitherto attributed to it.

* See Cooper on Dislocations, Fifth Edit., pp. 342 and 349.

Besides those we have now noticed, this livraison, completing the third volume, contains the following Memoirs, of which we content ourselves with merely transcribing the titles: Follin, On the Ophthalmoscope; Rigal, On a New Mode of Ligaturing Erectile Tumours; Giraldès, On Mucous Cysts of the Maxillary Sinus; Jarjavy, On Dilatation of the Parotid and other Ducts; and Sédillot, On Cheiloplasty in an old Case of Cancroid. Turning to the fourth volume, and passing over an interesting biographical sketch of Augustus Bérard, and the Compte Rendu of the highly valuable labours of the Society of Surgery, during the ten years 1843–53, we come to a paper entitled

IV. *Researches on the Cysts of the Wolffian Bodies.* By M. Verneuil.

M. Verneuil thus expresses himself in regard to the origin of cysts in general:—

"My researches upon cysts in general have led me long since to the adoption of a division somewhat resembling that of M. Cruveilhier and the authors of the *Compendium de Chirurgie.* We recognise cystiform productions which originate in a circumscribed accumulation of liquid in accidental cavities formed at the expense of the cellular tissue of any part of the body, by a mechanism analogous to that which gives rise to subcutaneous serous bursæ: but while we thus provisionally admit this variety, known as *serous cysts,* and which seems to us to be artificially arranged with cysts properly so called, we strongly protest against the strange abuse that is made of this word. In our day, in fact, we find it applied to any tumour without distinction, provided only that it be more or less spherical, and that without scarcely any account being taken of the nature of its walls or of its contents.

"We attribute to cysts properly so called but one single origin, the *glandular element:* and we thus form a very natural pathological family, embracing a great number of diseases, the superficial differences of which disappear before examination. Hence, we do not limit ourselves to ranging here the dermoid cysts of authors, but guided by a much wider generalization, we lay down the following propositions:—1. All organs which contain compound glands, or even simple follicles, and, *à fortiori,* the glands themselves, are susceptible of forming cysts. 2. We never meet with true cysts (hydatids, being accidental products, are here excluded) in any organs or regions in which glands are not present. 3. If a region contains several species of glands, it may also present various species of cysts. 4. The richer an apparatus is in glands, the more frequently will cysts be observed. Certain anatomical conditions have, however, to be taken into account, which it is not our object here to exhibit." (tome iv. p. 79.)

The following are M. Verneuil's conclusions respecting the special cysts it is his object to illustrate:—

"1. The Wolffian bodies, whose functions relate to the early phases of intrauterine life, become atrophied in the human species towards the fourth month of gestation, but leave *débris* during the whole of life, which are known in the female as Rosenmüller's bodies, and are situated within the substance of the broad ligament. In man, they form a mass of canals towards the head of the epididymis, constituting the *vas aberrans* and hydatid of Morgagni. The researches of MM. Kobelt, Follin, and Gosselin, have established these facts. 2. These glandular *culs-de-sac* are liable to distension by fluid accumulating in their interior, and then give rise to cysts known as cysts of the broad ligament, and small cysts of the testis. 3. These productions have been seen by a good number of observers,—as Velpeau, Delpech, Bright, Huguier, Gosselin, Follin, &c.: but, with the exception of the last, these observers have misunderstood their origin, and have explained their formation by hypotheses that should be abandoned. 4. In the female, they acquire a variable size, from that of a pea to that of an orange, and they may be sessile or interstitial, pediculated or free. Inflammation of the peritoneum and of the genital passages asserts an influence on their production. 5. In man they ordinarily remain very small. M. Gosselin has given a complete description of them. 6. The symptoms are absent or obscure, and in the present state of our knowledge no precise diagnosis can be made. 7. The prognosis is of little gravity, and treatment cannot be put into force. 8. Their origin is explained by the general laws which preside over the formation of cysts, and is moreover proved by direct observation. Their identity in the two sexes is beyond all doubt." (p. 84.)

V. *On the Seton.* By M. Bouvier.

In this paper, M. Bouvier advocates the use of small, or what he terms *filiform*, setons. A subsequent memoir of his upon the same subject has given rise to a very prolonged discussion in the Académie de Médecine, during which the doctrines of revulsion and irritation as held by ancients and moderns, were propounded with most wearisome prolixity.* The present paper is of a purely practical kind, the author urging the more frequent recourse to the use of the small seton as a means of counter-irritation. A peculiarity of its action is the small amount of irritation it determines in the skin, and the suppurative inflammation it induces in the subcutaneous cellular tissue. Its immediate, primary effect is of little intensity as compared with the cautery or the moxa, while its consecutive effects are not less considerable. In no other mode can suppuration be so easily maintained for an almost unlimited period. Except in cases in which it is desired to excite immediate energetic action of the skin, the seton procures a not less effective derivation than that which is obtained from other excretories which induce a destruction of the skin, while it is superior to them in its prolonged and continuous action.

M. Bouvier believes a principal reason why so valuable a means is so much neglected, to be the formidable mode in which it has hitherto been employed, and that much smaller setons and needles may be used than it has been customary to employ. Thus, an ordinary suture-needle, either straight or curved, providing its extremity be flattened and sharp at the sides, very well suffices. The size should vary, according to whether one or more threads have to be carried, always remaining much less than the ordinary seton-needle, and a spring at the larger end should receive the thread in place of an eye. The seton may in a few days, if desired, be enlarged by the addition of other threads. The passage of such a needle causes but little pain. M. Bouvier has made many trials of the various substances used as setons. Those which are most permeable to the pus favour best its discharge, such as slips of linen, or threads in juxtaposition. Of such substances, silk is least irritating, cotton somewhat more so, and wool most of all. Catgut also produces an abundant suppuration. All things being equal, the irritation is proportionate to the amount of distension of the tissues induced by the seton. Impermeable setons, which need not be removed in order to clean them, are of such simple and easy employment, that they should always be preferred, except in special cases indicating the use of the others. Among these M. Bouvier mentions with approbation threads of vulcanized caoutchouc, ordinary thread enveloped in caoutchouc, and gummed silk. Metallic substances, owing to their

* See Bulletin de l'Académie de Méd., tome xxi.

polish, excite but little irritation, and the author has employed small gold, silver, or gilt chains, fastened at their extremities, as precautionary setons, to be excited into activity if required.

In regard to the *direction* given to a seton, this should always be such as to bring one of the orifices in the skin into a state of declivity. In the usual mode of applying a seton to the neck this rule is violated, and the seton being placed horizontally, the pus tends by its weight to fuse below the level of the two apertures, and to form collections there that are emptied with difficulty. Were the seton made longitudinally, as it used to be by the old practitioners, this inconvenience would be avoided. By throwing the head back and employing curved needles, setons are as easily passed longitudinally as horizontally; and both the author and M. Mélier have adopted this mode of passing them with advantage. In cases requiring it, two longitudinal setons may be passed side by side, or multiple filiform setons may pass in different directions, the cruciform being a favourite with M. Bouvier.

VI. *On Stricture of the Urethra.* By M. Alphonse Guérin.

This is a memoir of some importance, inasmuch as it is based upon the examination of the urethra of 100 subjects brought to Clamart, having signs of gonorrhœa upon them when they died. In one-half of these cases stricture was met with, and it is to the mode of formation of this, and the rules of practice flowing from its consideration, that the author directs our attention. He was much surprised to find the mucous membrane quite devoid of all trace of cicatrix and false membrane except in the few instances in which were false passage and the like. The following are his conclusions:

"1. Fibrous strictures of the urethra scarcely ever proceed from inodular (cicatricial) tissue. 2. False membranes are never found on the mucous surface of the canal. 3. Fungosities to which blennorrhagia has been attributed, can only be exceptional occurrences. 4. The mucous membrane of the urethra is never exclusively the seat of stricture, and in all the cases I have seen, stricture of this membrane was the consequence of a lesion situated externally to it. 5. In the great majority of cases, stricture is due to the retraction of the indurated fibres of the reticular tissue subjacent to the mucous membrane. The point of departure is often a deposit of plastic lymph. 6. In strictures exhibiting projecting knobs beneath the skin, incision from without inwards is the only means of obtaining a certain cure. 7. It is because stricture is situated beyond the mucous membrane, and involves the subjacent fibrous tissue, that surgeons who scarify the canal obtain in fibrous strictures less deeply placed than those mentioned above, cures which would be in vain sought from dilatation. 8. In gonorrhœa, the glandules of the urethra which extend obliquely within the substance of the mucous membrane, to a length of more than a *centimètre*, being filled with muco-pus, it is very improbable that a single injection would act throughout their whole extent. On the other hand, throwing in several caustic injections, one after the other, endangers the increase of the urethritis to a degree of intensity in which the inflammation becomes propagated to the subjacent reticular tissue. 9. The most certain means of preventing stricture consists in treating gonorrhœas that have become old, and no longer cause pain, by compression of the glandules, made by the successive passage of several bougies." (tome iv. p. 141.)

We subjoin an interesting extract concerning some of the consequences of the deposit of plastic lymph in the spongy tissue of the urethra:—

"If the existence of this plastic deposit is admitted, we are furnished with the explanation of a fact hitherto unexplained—I mean the frequency of stricture opposite the bulb. In fact, the meshes of the reticular tissue being so large at no part of the canal as at this point, it is here the inflammation should most readily extend from the mucous membrane, and here, consequently, ought the deposits of coagulable lymph be most frequently met with.

"When once the lymph is deposited in the meshes of the reticular tissue, it impedes the passage of the blood which goes and comes from the bulb to the glans, and this obstacle to the circulation, conjoined to the induration of the fibrous membrane, explains why persons who have suffered from severe gonorrhœa, find so much difficulty in excreting the last drops of urine. I have shown, in a memoir I presented to the Académie de Médecine, that the excretion of the semen and urine, in all that part of the urethra which is placed anteriorly to the bulb, is due to the pressure exerted upon the contents of the canal by a column of blood propelled from the bulb towards the glans by the contraction of the bulbo-cavernous muscle.

"This obstacle to the circulation also explains how, in old and well-marked stricture, the glans, as compared with the rest of the penis, acquires so greatly an increased development, that I have been able from this alone to form my diagnosis in a very large number of cases. When a deposit of lymph exists in the spongy portion, the blood propelled by the bulbo-cavernous muscle may still reach the glans, though with difficulty; but its return being obstructed, this portion of the penis becomes tumefied by the stagnation of blood, a portion of which may also become coagulated in the absence of the movement which maintains its fluidity." (p. 133.)

We pass over a memoir upon *Obstetrical Anæsthesia*, by M. Houzelot, and a Report upon the same by M. Laborie, both lamenting the little way the obstetrical employment of anæsthetic agents has made in France. Neither communicate anything upon the subject that is new to the English reader.

VII. *On the Hereditariness of Syphilis.* By M. Cullerier.

In this paper, the surgeon of the Lourcine attacks the generally received opinion, that constitutional syphilis can be communicated to the fœtus by the male as well as by the female parent. He long held the opinion that it could, and gave the usual caution to men about to marry, though exhibiting symptoms of constitutional syphilis, of the danger they ran in infecting their offspring. The numerous instances, however, in which perfectly healthy children have resulted from such marriages, have induced him to alter his opinions; and he appeals to the experience of all practitioners, in confirmation of his assertion that men, the subjects of secondary or tertiary symptoms years after their marriage, the consequence of early primary affections, yet procreate entirely healthy children, in whom no trace of syphilitic disease can be detected.

On the mother's side, syphilis may become hereditary under all circumstances; and when it has once entered her system, she may produce syphilitic children at all epochs of the evolution of the disease; and that whether she exhibits actual symptoms, or seems to enjoy good health in the intervals of the appearance of these. Interrogation of the mothers in special hospitals, in which syphilitic infants are born, proves that either they were ill at the time of delivery, that they had been so during their pregnancy, or that they had exhibited primary or constitutional symptoms a longer or shorter time prior to becoming pregnant; and could the mothers in private practice be questioned in a similar way, there can be no doubt the same results would be arrived at.

So, too, were the cases of reputed communication of syphilis by the male parent subjected to a rigorous analysis, their number would undergo a singular reduction, and there would scarcely remain others than those in which, at the time of connection, a contagious affection, and consequently a possibility of infecting the mother, existed. It is upon the condition of the mother being so infected, that the possibility of the infant becoming so, entirely rests. Again, in cases in which repeated abortion has been supposed due to syphilitic taint, treatment of the mother is of avail, while directed to the father it is useless. One thing that renders tracing the history of these cases difficult, is that the fathers, while they willingly enough admit venereal accidents that may be referred to their youth, stoutly deny the existence of any contagious symptom that may have existed at the time of marriage.

M. Cullerier submits M. Ricord's views to a criticism they have long stood in need of, and shows upon what slight grounds this sarcastic critic of other men's views builds up his own theories. He appeals to other practitioners also for the results of their observations, pointing out, if his own views become confirmed, how consolatory they must prove as compared with those of Ricord; according to which, a man who has once had constitutional syphilis, is never certain that he is rid of it, and always continues in danger of procreating syphilitic children. As the number of syphilitic women is infinitely less than that of syphilitic men, especially in private practice, where such cases are quite exceptional, the presumption of hereditariness becomes restricted within very narrow limits; and the daily experience of every practitioner proves the great rarity of syphilitic births as compared with the prodigious number of individuals who have suffered from syphilis prior to becoming fathers.

After expressing his disbelief that several pathological appearances, such as suppuration of the thymus, hepatisation of the lung, pemphigus, &c., are really signs of hereditary syphilis, M. Cullerier makes the following practical observations:—

"It only requires some acquaintance with a hospital in which are lying-in women and new-born infants, to become convinced of the inexactitude of those authors who give as signs of hereditary syphilis the slight corporeal development, the miserable appearance, and wrinkled skin of these infants, giving them, according to Doublet, a resemblance to an old man, or causing them to appear shrivelled up and half boiled, *semi cocti*, in the words of Fallopius.

"In the immense majority of cases, the infant who has derived a syphilitic taint from the mother, when it has continued to live through the whole intra-uterine period, is born well-formed and in good health, and undergoes its normal development during two or three months. Toward this period, sometimes earlier, rarely later, general symptoms are manifested, the precursors of the venereal affection about to exhibit itself. The child sucks with less avidity, it sleeps badly, the bowels are disordered, the skin of the face assumes a bistre colour, and very soon unequivocal signs of lues appear. In most cases, mucous tubercles are observed, about the arms first, then on the genital organs, next in the folds of the skin, and on any parts of the body that become soiled with urine or fæces. At the same time that the tubercles become developed, spots appear on the skin of the chest and abdomen, which are nothing else than roseola, but in general so fugacious a roseola, that one does not always see the child soon enough to perceive it, while also it is often taken for simple erythema, so frequently met with in sickly and ill-cared-for children. As the consequence of hereditariness, the other forms of syphilis are more rare. Thus, the papular form is quite exceptional. The pustular form is somewhat less so, and a variety of it, impetigo, is often met with upon the face and head of children who exhibit mucous tubercles or roseola. It is rather in the relapses of hereditary syphilis than in its primary manifestation, that those forms, lichen and ecthyma, are met with. Lesions of the osseous, fibrous, and cellular tissues are very rarely seen as early occurrences, although they are sometimes met with. But when syphilis has begun by the skin and mucous membranes, as is the usual case, it may afterwards invade these tissues, if its progress has not been arrested by treatment. . . . .

"All I wish to call to mind is, what every one knows very well, first, that it is extremely rare to find infants born with symptoms of syphilis; and next, that those in whom these will be exhibited at a later period, may offer, until this epoch, all the signs of good health and a good constitution; and especially if the mothers during their pregnancy have not fallen into a state of too advanced cachexia, if they have not become too much exhausted by moral emotions or physical privations, or by the treatment they have been submitted to." (tome iv. p. 257.)

After adverting to the remote period assigned by some authors at which hereditary syphilis may still appear, M. Cullerier observes—

"These questions are not always easy of determination, and they must remain very obscure when we have only the accounts of mothers and nurses to go by. But when we are able to observe these infants from the moment of their birth, we find that it is towards the age of six weeks, or of two or three months, that the first symptoms are seen. Sometimes, but rarely, it is not until the fifth or sixth month, and hardly ever so late as towards the end of the first year. Thus, if I were required to lay down a general rule as to the epoch of the appearance of hereditary syphilis, I should not hesitate to say that it is during the first twelve months of life that it manifests itself; and that when an infant, about whom we are uneasy, has completed a year without any manifestation whatever, we may regard it as indemnified from the disease it might have derived hereditarily. This precision is very different to the vagueness of the statements of most authors, and to that uncertainty which threatens the child during its whole life with the consequences of the faults or the misfortunes of its parents. Future experience will decide whether it is an exaggerated pretension." (p. 261.)

VIII. *On a New Mode of performing Lithotomy in the Female.* By M. Vallet.

In this paper M. Vallet, Senior-Surgeon of the Hôtel Dieu at Orleans, recommends a new mode of performing vesico-vaginal lithotomy, by which fistula, the ordinary sequel of the operation, may be avoided. It consists in making the incision into the bladder in a transverse direction, and proceeding at once, after the extraction of the calculus, to the application of sutures. Two cases are given in which he so operated. The patient is placed as in ordinary lithotomy, and the operator, seated opposite her upon rather a high chair, passes in a univalve speculum, which he confides to an assistant placed on his left, and who forcibly depresses the lower wall of the vagina; two other assistants keeping the labia stretched by the aid of bent levers or of their fingers. A catheter is introduced, and its extremity is directed towards the *bas-fond* of the bladder, so as to project the portion of the organ that is to be cut into. The incision extends over a space of three *centimètres,* commencing at the middle part of, and external to a line drawn from the urethra to the ureter (representing one of the sides of the *trigone*), and reaching to the same point on the opposite side. Immediately after the calculus has been removed, some injections are thrown into the vagina, in case of hæmorrhage, which is rarely considerable, and then three or four points of suture are introduced, leaving rather less than a *centimètre* between each. The threads are tightened sufficiently to completely approximate the edges of the wound, and their ends cut off. A gum elastic, or gutta percha catheter is left in the bladder, care being taken that it does not come in contact with the sutures. Great care also is taken to keep the catheter clear for the passage of the urine. On the seventh day the patient is examined by aid of the speculum, and if the union appears firm the sutures are removed, the catheter being kept in somewhat longer.

In the author's first case, three points of suture were employed. They were removed on the seventh day, the catheter being left in until the twelfth. Although the patient at first discharged her urine involuntarily, as before the operation, in a few weeks she recovered the power over the bladder, and was discharged quite well. In the other case, one of the three points of suture was found to have cut through the edges of the wound, and a minute fistula remained, to remedy which the edges were pared, and another suture employed several weeks after, with complete success. Both these cases were reported to the Society two years after their occurrence.

For future operations, the author recommends an instrument he has devised, and tried with success on the dead subject, to facilitate the projection of the portion of the bladder to be divided, in order to render its incision more easy and more exact. It consists of a kind of grooved director, somewhat longer than the ordinary female catheter, which is flattened over the last fifth of its extent, where also a moveable branch, four *centimètres* long, is attached, and which, by a rotatory movement, can be brought into a transverse position, giving the director a cruciform appearance. Along this transverse branch the knife is to be carried when making the incision.

We must reserve the notice of the remaining two fasciculi of this volume for another opportunity.

Review XI.

Lectures on the Principles and Methods of Medical Observation and Research, for the use of Advanced Students and Junior Practitioners. By Thomas Laycock, M.D., F.R.S.E., F.R.C.P., Professor of the Practice of Medicine, and of Clinical Medicine, in the University of Edinburgh, &c. &c.—*Edinburgh*, 1856. Post 8vo, pp. 218.

The objects which Professor Laycock has proposed to himself in the preparation and publication of this volume, will be best set forth by the following extract from his Preface:—

"When about to enter for the first time upon his duties as Professor of Clinical Medicine, and to deliver the summer course of Clinical Lectures for 1856, in the

University of Edinburgh, the author looked about for some elementary work on the inductive philosophy which he could recommend to his class, for their instruction and guidance in clinical observation and research. He found several sufficiently able manuals of *physical diagnosis* adapted to students; and good elementary works on the uses of the microscope and on the *routine* of the clinical wards, with systematic instructions 'how and what to observe.' But he found none which instruct the medical student in a simple and easy form how to use his reason; none which explains to him in especial the nature of the mental processes by which knowledge is acquired in his particular sphere of labour; none which teaches him the applications to practical medicine of those aids to the intellectual powers which modern inductive philosophy uses so commonly and so efficiently. The student would inquire in vain for a short and practical exposition of the numerical method of research, in its special applications to practical medicine, or of that still more effective and philosophical method, the *analogical;* a method which, when once understood, is singularly easy of application, and equal (the writer is deeply convinced) to the solution of all the problems of life and organization that it is possible for the intellect of man to conceive, however profound they may be. A method, in short, of unlimited powers, and specially adapted to the needs of medical science."

After pointing out that the peculiarities of medical reasoning require a special direction to be given to the ordinary systems of logic, to adapt them to the needs of the medical inquirer, Professor Laycock continues:—

"Seeing this defect in medical literature, the writer determined to deliver to his class of Clinical Medicine a few lectures in which elementary principles and processes of observation and inquiry should be presented to it in as simple and attractive a form as possible, and as devoid of metaphysical phrases as they well might be made. He was not regardless, however, of the necessity that they should also be as practical as possible; that is, that they should be adapted to the actual position and wants of the student. It is with this object that simple illustrations are introduced; it is with this object, too, that the attention is directed rather to the observations of the processes of disease than of the products or results of these processes. . . . It further seemed to the author of some importance that the student's attention should be directed especially to the natural history of disease, with a view to a more philosophical, more really practical, and more truly natural system of medicine than has hitherto been given to the world. Hence the introduction of a subject not often noticed, and when noticed, always imperfectly, in systematic works,—the order of succession of vital phenomena." (Preface, pp. vii.—xiii.)

Fully agreeing with Dr. Laycock as to the fundamental importance of a well-trained reasoning faculty for real success in practising the art of medicine, still more in extending the boundaries of the science, we must express a general doubt whether such training can be effectually given by the study of books. To us it seems essential that the student should enter upon his medical course with a mind which already knows how to exercise itself aright; and this practical knowledge is acquired in early years (as we have had large opportunity of observing) far more certainly and effectually under the guidance of a judicious teacher, who sets his pupils to reason about "common things," and then makes them comprehend the *rationale* of the processes they have empirically gone through, than by the study of systematic treatises on the science of reasoning. In fact, however valuable the study of such treatises may be as an exercise to the mind, like that of classics or mathematics, we have a strong conviction that such study does not in itself engender any special capability of dealing with the ordinary affairs of life, still more for resolving the intricate problems of physiology or medicine. For the forms of logic deal only with certain propositions, whose certainty is assumed (like that of the axioms of geometry) as essential to the reasoning process; whilst in reasoning about "common things," still more in medical reasoning, the degree of probability which attaches to the data is one of the most difficult parts of the inquiry, which pure logic gives no assistance in solving. And thus we have lately had to witness the humiliating spectacle of one of the greatest mathematicians and logicians of the age entirely "possessed" by the delusions of table-turning, spirit-rapping, and the like, just because he was not capable of applying his formulæ to the detection of the fallacies which lie at the root of the whole system.

To a mind which has been already trained in the art of reasoning, and from the art has been led upwards to know something of the science, it must be most useful, in the prosecution of his medical studies, to be made to perceive in what the special peculiarities of the living system consist, and what modifications of his pre-formed habits of thoughts are required in his new vocation. But this, again, cannot, in our opinion, be taught in abstract propositions, by any means so effectually as by a course of practical instruction. It is the proper vocation of the physiological teacher to unfold to his pupils step by step the mysteries of organization, to show them how to apply the great doctrines of causation to the actions of the living body, and to lead them to work in their own minds such simple problems, as may serve for an introduction to the more complicated. And by the time the student enters on his clinical course, he ought to be well prepared to reason upon the phenomena of disease, to bring to bear upon the consideration of them not only the knowledge of healthy action, but an acquaintance with its conditions, and thus to search out the latent source of morbid phenomena, and to devise means for its rectification. In guiding his pupils in such a course, the clinical professor, like those who are engaged in the antecedent training of the student, should trust more to example than to precept. A *clinical* lecture ought surely not to be a disquisition upon abstract principles, but upon the interpretation of phenomena; and in the various processes which this interpretation involves, there is such a wide scope for the exercise of *all* the intellectual processes, that it must be the fault of the teacher if he do not find abundant opportunities of pointing out the application of the fundamental principles of reasoning to the science and art of medicine. But if the student have not had some such preliminary training as we have described, he will scarcely be likely to profit by even the very best clinical instruction to more than a limited extent; and the clinical professor must feel that he wastes his time in endeavouring to teach men how to reason upon one of the most intricate of all subjects, who do not know how to reason upon the most simple.

In all this we dare say that Professor Laycock would fully concur with us; and we shall presume that he intends his present publication merely to supply, so far as it may, the deficiency which he has experienced, without supposing that it will serve as a royal road to the art of medical reasoning, for those who have never been trained to reason well about common things. We have endeavoured carefully to estimate its value from this point of view; and with every wish to do the author full justice, we are compelled to say that his treatise is far from coming up to our idea of what might reasonably be expected from the successor of the Cullens, the Gregories, and the Alisons. We believe that Professor Laycock is capable of much better things, if he will only give himself time, and will thoroughly digest and systematize his abstract views, giving due heed to other men's knowledge and experience as well as to his own. The table of contents at once strikes us as rather a catalogue of hastily-concocted fragments, than as a bill of fare of a well-arranged banquet; and this impression is not dissipated but strengthened, as we advance through the pages to which it introduces us. But we should be doing injustice to its able author if we did not, at the same time, give him credit for the many acute suggestions which are scattered through the volume, and for his clear-sighted exposure of many prevalent fallacies. In fact, he seems to us to have succeeded much better in showing what course is to be avoided, than what is to be followed; and his book is, consequently, more likely to do good by teaching its readers what *not to do*, than what *to do*. In the present state of medical art and science, however, this is a lesson which every one ought early to learn. For if ever there be a case in which the system of the circumlocution office—"how not to do it,"—may be rightly carried into action, it is in the avoidance of the temptation to the mischievous routine of empirical practice, which satisfies both doctor and patient that "something active" is being done, the said "something" being too often about the last thing in the world that ought to be done.

To these general remarks, we shall now subjoin a few extracts, which may serve to give an idea of the merits and demerits of Dr. Laycock's treatise.

One of the most common of all errors in medical reasoning is the substitution of a theory for a fact—a probability for an actuality:—

"This is so common an error, that you can hardly open any medical essay without meeting with it; and it is a very insidious error. There are two or three principal modes in which it occurs. One is the use of collective words or general terms, as facts, which are essentially theoretical—as 'tonic,' 'diaphoretic,' 'fever,' 'inflammation,' and the like. In the search after accuracy, a definition is given, and then it is thought that the word has a definite meaning. This is not so, however. Naturalists have not even defined what a species is, although they have only objective phenomena to deal with, and can place the objects before them. In defining processes and states of living bodies, we must remember that we do not know the entirety of any one process, for we have never observed it—sometimes, indeed, only a small portion of a process; as to the remainder, we draw conclusions only, that is, establish theories or probabilities. Nothing is so difficult to handle in this way as the phenomena of life, because all vital phenomena are continuous, or run into each other. It is this continuity that renders it impossible to define a species with absolute strictness, or even what is animal and what vegetable. All words and terms in medicine, then, are sources of fallacy." (p. 23.)

A few pages further on, we meet with the following apposite illustration:—

"It is asked, is cholera an infectious disease? can cholera be communicated? in the most perfect good faith, and without the slightest suspicion that in the use of the collective term itself, there is a fundamental fallacy. Cholera is but a word by which a group of symptoms is indicated; it is not a thing. The questioner meant, therefore, to ask this question—Can the *materies morbi*, the cause of the symptoms, be generated in the bodies of the sick, and communicated to the bodies of the healthy, so that in them a similar disease or group of symptoms will result? Now it has in fact been assumed, without due inquiry, that the group of symptoms designated by cholera are the only phenomena which resulted from the *materies morbi;* so that during the whole of the first epidemic at least, it was hardly guessed that, etiologically, the immensely greater number of cases of diarrhœa were cases of slight cholera, that is, due to the same cause. I need only add, that there was still less suspicion, that in such slight cases the *materies morbi* might be generated and given off equally, as in the more intense forms." (p. 41.)

Now although we are ourselves inclined to accept this view of the case, we cannot adopt it with the unhesitating confidence that the author himself expresses in its truth, as if it were a point now universally conceded. Has he not been guilty of the very fallacy which he exposes in others; and is not his assumption of a *materies morbi*, transmitted from one individual to another, just as hypothetical as the limitation of the designation cholera to the ostensible cases of that disease? Our next extract will show how strong is the trust placed by Dr. Laycock in his own theory of contagion, in a case to which the profession generally would think it utterly inapplicable:—

"Another illustration of the fallacious substitution of a theory for a fact in causation, is afforded in the current theory of the rapid spread of the epidemic poison which excites the group of phenomena termed influenza. It is highly characteristic of a strictly infectious agent; and but for the circumstance that an insidious unperceived theory takes the place of fact in the consideration of the question, would suffice to establish its infectious character. So soon as the mind addresses itself to the question of causation, it instinctively compares the spread of influenza with its knowledge of the mode of the spread of other epidemic diseases that are known to be infectious, as the exanthemata. But it finds that the identical characteristic of influenza, which to an unbiassed judgment would most strikingly indicate its infectious character—namely, its rapidity of spread—is wholly different from these. They require many months to infect an entire population; influenza never requires as many weeks. This dissimilarity being noted, and no other facts as to influenza being brought into comparison, the inference is drawn, that the diseases are really dissimilar as to the contagious element, and that the influenza spreads too rapidly to be caused, like them, by an infectious agent. Then, as the mind cannot rest satisfied without a cause being assigned for the rapid spread, the agent nearest the apprehension—namely, the atmosphere—is selected, and so it is concluded that influenza spreads in consequence of some change or unusual 'influence' in the atmosphere. This false theory of epidemic causation is not peculiar to influenza; it is the most common, as well as the most mischievous of the epidemiological fallacies. Since the days of Sydenham, who in modern times gave it extended currency under the term 'epidemic constitution,' it has obstructed our progress in ascertaining the true nature of epidemic diseases, and has been erroneously applied equally to cholera, plague, yellow-fever, as to influenza." (p. 43.)

There is an old proverb, that "those who live in glass houses should not throw stones;" and Dr. Laycock, in thus attacking the doctrine which derives its sanction from the accumulation of a large mass of carefully-observed phenomena, and which has been regarded by men not inferior to himself as the hypothesis best fitted to account for the facts of the case, must not be surprised if he finds his own doctrine—which, while fully as hypothetical, is based on a far more limited induction—scouted as absurd by the united voice of the profession. To our own minds nothing is more clear than that the phenomena of the spread of influenza are totally inexplicable by any form of the doctrine that the reproduction of the poison in the living human body, and its transmission by contact, are necessary conditions of its epidemic diffusion; and though we would not exclude contagion as a possible means by which it has been occasionally conveyed whither it might not otherwise have reached, yet of all diseases of the zymotic class, we believe that influenza is one of the least contagious. And the striking modification in the type of *other* diseases, which has been observed by intelligent practitioners in the metropolis and elsewhere, during the prevalence of influenza, has always appeared to us one of the most striking illustrations of the truth of Sydenham's doctrine of the influence of "epidemic constitution." We admit that the term is hypothetical; but so is the term "contagion" in every instance in which we cannot prove the direct transmission of a *materies morbi;* and the question is, which hypothesis is best founded on induction from facts?

We give the following paragraphs from the fifth lecture, "On the Due Estimate of Treatment, and on the Management of the Case," as examples of the practical wisdom with which this part of the work abounds:—

"*Estimate of Probabilities.*—This, perhaps, is the department of medicine in which the most profound sagacity may be shown. Practical medicine is confessedly a conjectural art; to conjecture wisely is therefore the essence of the art—the whole sum and pith. The numerical method affords us a numerical estimate of the probabilities in a given number of cases; but this is not of much help to the practitioner at the bedside, who has to determine the probabilities in the individual case before him, and which may or may not be more or less similar to the cases estimated numerically. What, in fact, is essential to this sagacity in especial, is essential to form the whole man as a practitioner. He must have the habit of minute and accurate observation, so as to be able quickly to detect all possible circumstances that can throw light upon the case; he must have sound experience, so that he can compare what he now sees with the results of that experience; and he must have knowledge, that he may correct and extend his observations, and correct and simplify his conclusions. Nothing but constant and painstaking exercise of the faculties necessary to these mental processes, can give the requisite quickness of perception, comparison, and deduction. But in proportion as these faculties are possessed by the practitioner, as well as in proportion as they are exercised, will he be sagacious in his estimate of probabilities. Some men are wholly unfit, naturally, for the exercise of the art, simply because they want the requisite faculties of mind; some because they want the requisite industry." (pp. 144—146.)

"*Error of Impatient Expectation of the Results of Treatment.*—When a practitioner has clearly laid down his plan of treatment, he must carry it out steadily, and patiently await the result. Nothing is so detrimental to success in treatment, as an indefinite conception of the end to be attained, or of the means to be used. The practitioner so situate is constantly vacillating; being swayed by every change of symptom in the patient, by every expression of opinion he may hear. He is, therefore, constantly changing his remedies or method of treatment. Thereby he renders the symptomatology more confused by superadding the varied phenomena induced by drugs to those of the original affection, and thus at last his diagnosis is utterly bad. In consultation-cases in which treatment has been long continued, or, if not long continued, has been actively pursued, the first step in the examination is a careful separation of the results of this kind of treatment from the results of disease. Thus you may be called into a case in which the starvation-system of certain homœopathists [and of many allopathists also] has been rigidly carried out. You may be told it is gastric fever; but in reality the patient is perishing for want of food and drink. In cases of intestinal obstruction, it is not an unfrequent circumstance to find the patient suffering more from the effects of drastic purgatives incautiously taken or wildly administered, than from the primary disease. It is a great point in your art to know when to do nothing, and to be able firmly to resist all solicitations to be very actively doing something. A clear conception of the case in all its bearings can only [alone?] give you this admirable quality. And for this, therefore, you must incessantly labour, never being content until you have exhausted every available source of the knowledge that can help you in your diagnosis." (pp. 146, 147.)

Per contra, when we turn to the last lecture, on the Analogical, Philosophical, or purely Inductive Method of Research, we experience little save disappointment at the very imperfect view of medical philosophy which it contains. After the grandiloquent panegyric on the "analogical method," in the preface, as "equal to the solution of all the problems of life and organization that it is possible for the intellect of man to conceive," we were quite unprepared to find our author dismissing it after so summary a fashion, as if he was tired of his subject and wanted to have done with it. He informs us that "the primary or fundamental principle of life is the unity of structure and function of organisms both in time and space" (p. 181); and he tells us (p. 189) that it is by the application of this principle that we are to distinguish true analogies from false ones. The greater part of the lecture is taken up with the illustrative application of this analogical method, in the investigation of the pathology of bronzed skin, and of gout and rheumatism; in which Professor Laycock doubtless shows great acumen, but at the same time lays himself widely open to criticism, both as to the soundness of his data and the justice of his conclusions. Our space, however, does not allow us to follow him through these inquiries; and we must conclude by expressing the opinion that, with much that is sound and good, the book contains much that is questionable (to say the least), and would have probably attained a far more perfect development, had its period of incubation been two or three years instead of two or three months.

Review XII.

The Australian Medical Journal. Edited under the Superintendence of the Medical Society of Victoria.—*Melbourne.* No. 1, January, 1856. pp. 180. No. 2, April, 1856. pp. 160.

The appearance of a quarterly Medical Journal of considerable merit, in a district which but few years back was unknown and uninhabited, is no unimportant sign of the times. The editors observe in their opening number, that—

"The temporary disorganization which succeeded the discovery of a new source of material wealth, has gradually subsided, and the colony of Victoria now occupies, in its social and domestic characteristics, a position unsurpassed by any dependency of the British Crown. Every element of future greatness and national progression has received a powerful acceleration during the past three years, and the self-adjusting principles upon which civilized society is constructed has operated insensibly, but with unerring certainty, in restoring the equilibrium briefly disturbed by a sudden interruption of the tranquil and industrious avocations of the people. With an immense influx of population, higher tastes, superior talents, and elevated desires have accompanied other acquisitions; and the press, at once the representative and the auxiliary of public sentiment, has fully sustained, by its progress and improvement, the high estimate which free countries entertain of its power and utility. Politics, literature, and law, have their representatives; agriculture, horticulture, science, and religion, possess their recognised exponents; and even the humourist has his special vehicle of thought. The medical profession alone seems to have been left destitute of an accredited organ to maintain and fortify its proper position in the social fabric, of which it constitutes an important support. We need not, therefore, enlarge upon the advantages to be derived from the establishment of a medical journal."

These advantages, manifest as they are under all circumstances, where hitherto no medium of intercommunication has existed for the members of a liberal profession, are peculiarly striking under the relations that present themselves in Australia. We there have a rapidly growing and wealthy population, an amount of intellect in every way commensurate with the vigour that marks all the branches of the great Anglo-Saxon tree, and a soil in which there have as yet been no cultivators, where all the experience of the past may be brought to bear without any of the drawbacks incident to the slower growth and development of the older countries. The editors of the *Australian Medical Journal* have indeed a glorious field before them! That they may be fully conscious of the great responsibilities that rest upon them; that, as the guardians of the ethics of the profession, they may keep the loftiest goal before their eyes and pursue it; that they may rise superior to the temptations that beset the journalist, and scorn petty jealousies, seeking only the advancement of science and the good of the common profession,—such are the wishes we offer to our friends in the young but stalwart colony. They have entered into their new path with a vigour which justifies fair hopes; and none will rejoice more than their brethren on this side of the globe to see those hopes realized.

The information contained in the Journal is distributed under the following heads:—Original Contributions; Hospital Reports; Reports of Medical and Scientific Societies; Editorial Articles; Reviews; Extracts from Medical and Scientific Literature; General Correspondence, Medical News, and Answers to Correspondents.

We may find an opportunity of giving an analysis of one or more of the papers. We subjoin the titles of the communications contained in the first number, which cannot fail to interest our readers, — viz., 1. On the Principles of Pathology, by W. B. Wilmot, M.D. 2. Sunstroke, its Causes, Consequences, and Pathology, by C. Travers Mackin. 3. On Barbiers, by Richard Eades, M.B. 4. Case of Autumnal Fever, with Remarks, by R. T. Tracy, M.D. 5. Epilepsy produced by Pressure on the Brain, by John Maund, M.D. 6. A Cure of Recto-labial Fistula, by Edward Barker, Senior Surgeon to the Melbourne Hospital. 7. On Lloyd's New Operation for Stone, by W. Gillbee, Honorary Surgeon to the Melbourne Hospital. 8. Case of Supposed Rupture of the Uterus, with Spontaneous Evolution of the Fœtus and Placenta Prævia, by G. S. D. Butler, M.D.

The mere enumeration of the papers certainly does not convey much to our readers, but it serves to indicate the path which our Australian colleagues have entered upon. Their own prowess will secure their future career; but we trust they will not refuse to accept from us the cordial assurances of the sincere sympathy and good fellowship, with which their first essays will be hailed in the mother country.

PART SECOND.

Bibliographical Record.

Art. I.—*A Practical Treatise on the Disorders of the Stomach with Fermentation.* By James Turnbull, M.D., Physician to the Liverpool Royal Infirmary. *London*, 1856. pp. 160.

In the first chapter, Dr. Turnbull examines very briefly the process of natural digestion, stating the arguments used to support the idea that the conversion of food into chyme is a fermentation of a peculiar kind. In the second chapter, he discusses the chemistry of the changes involved in the alcoholic, the saccharine, the viscous, the lactic, the acetic, the gallic, the pectic, the benzoilic, the sinapic, the ammoniacal, the putrid, and the fatty fermentations. It must be confessed, however, that science is in the rear of what practical observation requires on these heads, and consequently only the most important of them are discussed in detail. In some, if not all fermentations, the growth of organic forms of an elementary nature, such as the yeast-plant, is looked upon by the author as an important part of the process. The action of certain chemical agents in arresting fermentation is pointed out; such as bichloride of mercury, sulphate of copper, carbonate of soda, mustard, wood spirit, creasote; others, such as turpentine, chloride of lime, weakened but did not destroy it; tannin precipitated the ferment, but without arresting fermentation; and gallic acid, on the other hand, caused an abundant dark head. Experiments were likewise made by the author upon lactic fermentation, which, however, none of the reagents mentioned, except wood spirit, had the power of completely checking. It may be remarked that the arrest of fermentation is not exactly the same thing as arrest of putrefaction; for arsenious acid, so decided an antiseptic, has no great power in checking vinous fermentation. The third chapter commences the medical part of the work, and is devoted to the analysis of 33 already published cases of *sarcina ventriculi*, to which Dr. Turnbull adds 3 of his own. As might have been anticipated from the introductory chapters, he considers this organic growth as having more causal influence upon the morbid phenomena, than is usual among pathologists. He thinks it is produced by, and also propagates, a morbid fermentation. The palliative effect of hyposulphite of soda, and of alkalies, he attributes to the partial arrest of fermentation which they exerted in his experiments; and seeing the complete arrest effected by wood spirit, he advises a trial of that as a remedy for *sarcina*. The next chapter is occupied by "Dyspeptic disorder" with morbid fermentation but without *sarcina*, and in both stomach and bowels. In this Dr. Turnbull expands upon the narrow view of the process of digestion which his title would have seemed to imply, and carries on the arguments which had been applied to the stomach into the further regions of the duodenum, ileum, and colon. He attributes English autumnal cholera to the acid fermentation of fruit in the bowels. Bichloride of mercury having been shown in the second chapter to be an anti-fermentive, "not only explains," he says, "how mercurials act in these disorders of the bowels, but may also throw some light on the manner in which mercury acts as the most energetic remedy in eradicating syphilis." The fifth chapter examines the action of other remedies on similar principles. The sixth enumerates the various tendencies of different articles of diet to ferment, and explains their value as aliments. In order to feel the force of the author's reasoning, it is necessary to agree with him that a vast number of organic acts are explained by referring them to "fermentation" —that not only cookery and digestion, but that indigestion also, diarrhœa, colic, flatulence, syphilis, the poisoning by bitter almonds and by German sausages, the acidification and the alkalinisation of urine, the growth of sarcina, and the pain in catarrh of the stomach, have their nature elucidated by ascribing them to this word.

ART. II.—*Nomos: an Attempt to Demonstrate a Central Physical Law in Nature.—London*, 1856. 12mo, pp. 198.

IN the year 1842, Mr. W. R. Grove, in a course of lectures delivered at the London Institution, propounded a theory of the correlation of physical forces, which subsequently assumed this expression, "that the various affections of matter which constitute the main objects of experimental physics—viz., heat, light, electricity, magnetism, chemical affinity, and motion, are all correlative, or have a reciprocal dependence. That neither, taken abstractedly, can be said to be the essential or the proximate cause of the others, but that either may, as a force, produce the others . . . each merging itself as the force it produces becomes developed; and that the same must hold good of other forces, it being an irresistible inference that a force cannot originate otherwise than by generation from some antecedent force or forces."

In investigating the relations of the different forces, Mr. Grove commenced with each in its turn, and indicated the manner in which it could mediately or immediately produce the others. The clearness with which this correlation was shown, led "many to regard all the different natural agencies as reducible to unity, and as resulting from one force which is the efficient cause of all the others."* But Mr. Grove adhered to the simple expression of correlation, urging that "in tracing any force back to its antecedents, we are merged in an infinity of changing forms of force;" and in reply to the question, "Can we suggest a proposition, definitely conceivable by the mind, of force without antecedent force?" states, "*I* cannot, without calling for the interposition of creative power."

* The Correlation of Physical Force. Second Edition. 1850.

The conclusion at which the author of 'Nomos' arrives, is, "in short, that the inorganic world is ruled by one single law, of whose operation the phenomena of electricity, magnetism, light, heat, chemical action, and motion are only so many signs—the law, that is to say, of the laboratory; and that no secret in the world of inorganic nature can be fully understood except upon this assumption."

But even with this "assumption," we fear there are many "secrets in the world of inorganic nature" which cannot be "fully understood." But further, what this "law of the laboratory" is, we are at some loss to comprehend. The term is used "to express that central law to which the philosophy of the laboratory appears to point;" which, gathered from the whole of the work before us, is nothing more than the correlation of forces as stated by Mr. Grove; but which receives some definition at the conclusion of the survey of electricity—viz., "an action of duality, out of which arise, under peculiar circumstances, certain marked movements—an action which depends not upon incomprehensible imponderables, but upon certain definite and comprehensible properties of matter."

The author proceeds then to show that this law "dominates in nature," and that it "may be a cosmical law." The first application of it to the movements of the earth requires two assumptions—*e.g.*, "Let us assume that currents of electricity surround the earth in a direction which is parallel to the plane of the ecliptic; let us assume that similar currents proceed from the sun to the earth, and emerge upon the part which is nearest to the sun; and we may soon see that the earth must move around the sun, and that she must rotate upon her axis as well as move onwards in her orbit."

The old "cookery books" usually commenced their directions in regard to culinary ichthyology with the most important advice, "first catch your fish;" but the author of 'Nomos' assumes that his fish are caught, and proceeds to garnish his empty dishes in an extremely pleasing, but scarcely satisfactory manner.

The "law of the laboratory" comes to the rescue of distressed comets; but here, again, there are "assumptions," honestly admitted to be such, too numerous for our patience to count.

The "law of the laboratory" is brought to explain "some of the phenomena of natural heat;" which is represented as nothing more than a current of any kind under difficulties. Here we have the tides explained by expansion of the solid substance of the earth; an expansion which does not appear to affect the water, and which expansion is due to the heat of the moon (!) being focussed within the globe itself. The conclusion with regard to heat is, that "natural heat" (*i.e.*, heat outside the laboratory) is identical with "artificial heat" (*i.e.*, heat produced in the laboratory, but which we should have supposed to be as natural as the other). And what this has to do with the "central law," we leave our readers to divine; and also how "the law provisionally named the law of the laboratory," renders these secrets "fully understood."

Then, after two delightfully short chapters, each occupying about one quarter of a page, we pass to the conclusion of the whole matter as already stated. The author starts by defining "imponderable agents" thus—"Agents, that is to say, which are quite beyond the scope of physical inquiry;" and he concludes with an "assumption." The reader, however, although he will probably have no more definite idea of a "central law" when he has finished the book than he had when he commenced, will find much to interest him in the perusal; and will find abundant and apt illustrations of the law of correlation of forces, which is at present the most general expression that can be scientifically employed with regard to them.

ART. III.—*The Life of Henry Cornelius Agrippa von Nettesheim, Doctor and Knight, commonly known as a Magician.* By HENRY MORLEY, Author of 'Palissy the Potter,' 'Jerome Cardan,' &c.—*London,* 1856. Two vols., pp. 304, pp. 332.

IN the fermentation preceding and accompanying the great events of the first half of the sixteenth century, Cornelius Agrippa occupied a position which fully justifies the choice of Mr. Morley in selecting his biography as illustrative of the times he lived in. In a purely medical point of view we learn but little from the work, but all who are interested in what bears upon the development of the human mind, will read the account of Agrippa's struggles and the analysis of his works with sympathy. In the middle ages, when natural science was mystified by the alchemist and by an admixture with a cabalistic theology, it is not surprising that a man who "began his life by mastering nearly the whole circle of the sciences and arts," should describe "Physic as another art of homicide, mechanical, though claiming the name of a philosophy."

The biographer offers no temptations to the present generation to follow in the footsteps of Agrippa, so that it is needless to warn the medical reader against the influence of the scholar's scepticism—the very crudity of which is a sufficient antidote against the poison he might have infused in his own day.

It is out of our province to inquire more fully into the features that characterize the doings or the writings of the great Magician, but those who desire an analysis of them, and who wish to know how they were received by his contemporaries, will find Mr. Morley's volumes an acceptable addition to their libraries.

ART IV.—*A Comparative Inquiry as to the Preventive and Curative Influence of the Climate of Pau, and of Montpellier, Hyères, Nice, Rome, Pisa, Florence, Naples, Biarritz, &c., on Health and Disease; with a description of the Watering-places of the Pyrenees, and of the Virtues of their respective Mineral Sources.* By ALEX. TAYLOR, M.D., F.R.S.E., Cor. Member of the Hist. Institute of France, &c. &c. A new edition, considerably altered and enlarged.—*London,* 1856. pp. 355.

THE plan followed by Dr. Taylor in the well-written book before us, is one that offers a better prospect of enabling us to determine the curative influence of climate, than the results which we may obtain by having regard to meteorological data only. He considers, justly, that we obtain a safe guide as to the effects of a given climate, by ascertaining the influence it exerts upon the native population.

"Thus, if in any climate we find that its agency is decidedly of a sedative kind, and that it proximately acts by modifying the tone of organs, we would, *à priori*, infer that such a climate would be unsuitable to that kind of diseased action depending upon general want of tone and a low state of functional energy. But, again, if in any climate we find that acute inflammatory affections—for instance, of the mucous membranes of the stomach or air passages—are a common disease with the natives, it would not seem to be a wise or logical proceeding on the part of a physician, to send to such a climate a stranger who was liable to be easily affected by these very maladies."

Regarded in their influence upon the invalid, Dr. Taylor classes climates as exciting, sedative, and relaxing: Nice, Naples, Montpellier, and Florence belonging to the first; Rome and, *par excellence*, Pau to the second category; Pisa and Madeira being instances of the third. Pau and Montpellier may be said to differ as much as, in our country, Torquay and Margate; no one would think of sending patients indiscriminately to these two places, and it is equally important in recommending residence abroad, that we should carefully distinguish between the localities, as we should between a stimulant or a narcotic medicine. The climate of Pau, to which Dr. Taylor devotes his chief attention, is characterized by the stillness of the atmosphere, the absence of free communicable humidity and of sudden transitions from heat to cold. Such conditions enable the invalid, without risk of sudden chills or the disturbance of the function of any organ, to enjoy out-door exercise throughout the winter, and amply account for the remarkable immunity from disease, and the longevity, of the inhabitants of Pau. The sedative action of the climate of Pau is shown by its physiological influences, and by the influence it exerts upon the *morale.* The inhabitants show a slower circulation, and are more phlegmatic than their countrymen generally; and visitors equally exhibit, after a time, a permanent reduction of their pulse and a modification of temperament. We are unable to give as full an analysis of Dr. Taylor's work as we might wish, but our readers will anticipate, even from what we have said, that the remedial action of Pau "may be summed up in one general principle: viz., wherever disease depends upon increased nervous and arterial action permanently produced, either by temperament or by some causes leading to more active disease," there we may expect a beneficial influence from this climate.

We are tempted to add a few words concerning a place which has been known for some years to the sojourners at Pau, but has of late acquired an extensive reputation, owing to the Emperor and Empress of the French having resorted to it—we mean Biarritz. It is a fishing hamlet about five miles from Bayonne, first brought into notice by the English residents at Pau, who went there during the summer to enjoy the invigorating sea breezes.

"It is most irregularly built, some parts of it being situated on a succession of cliffs, others in a species of ravine. The houses, being mostly intended for summer use, have all, more or less, a north-western aspect. And this is a matter of great importance, for in a southern climate, when a house has a southern exposure, its inhabitants are condemned during the day to darkness, for the admission of the sun's rays into the house would be intolerable; whereas, with a north-western aspect, the houses receive through their open windows the refreshing sea-breeze from the west, which most generally sets in every morning about ten o'clock, and the snuffing in of which is a real luxury, and goes far to neutralise the effects of a southern sun.

"The health of the native population at Biarritz is most satisfactory, and the advanced ages which they attain, as evidenced by the registers and the inscriptions on the tombstones in their romantic churchyard, prove how favourable the climate is, even with the drawback of hard work, the accidents of the sea, and of food not of the most fortifying and nutritive description."

Biarritz may be regarded as auxiliary to Pau; the warmness of the atmosphere and of the sea enable delicate individuals to bathe with benefit, who, in a northern climate, could do so only with prejudice to their health; at the same time it offers an agreeable retreat in summer from the sultry heats of Pau.

In concluding this brief notice of Dr. Taylor's book we would add that, as a scientific guide and a pleasant companion to the South of France and the Pyrenees, it will prove of great value to the physician at home, or to the invalid in quest of health.

ART. V.—*Addresses to Medical Students. Delivered at the instance of the Edinburgh Missionary Society.—Edinburgh*, 1856. pp. 266.

AMONG the dangers that beset the student of medicine when he first enters upon his career, none is probably more serious, or fraught with more pernicious results, than the temptation to employ the scraps of knowledge that he picks up, as arguments against the truths of Christianity. The ardour of youth, the fascinations of a new field of knowledge, the pleasure excited by a sense of emancipation from all intellectual control, conspire to lead away many a young man into the maze of infidelity, which is closely allied to the slough of immorality. Such is not a necessary result of the admission of the acolyte to the portals of science; but the danger is great, and the temptation is one too often yielded to. It is more necessary, perhaps, to the student of medicine than to the student of other professions, that his steps should be guided by men whose larger experience and more intimate knowledge of the relations that science and religion bear to one another enable them to assist him; and we therefore hail with satisfaction any attempt made with the view to showing the young student how to pass through the difficulties that environ him, how to reconcile facts which, to his crude mode of reasoning, appear subversive of religion. The addresses before us, like the addresses of the Christian Medical Association of London, are intended not only to promote religious sentiment among the students, but to show how their calling is hallowed, and their labours become more beneficial to themselves and to their patients, if they mirror themselves by the light of Christianity. We sincerely trust that the good which is intended by such associations may be fully realized; that they may tend to advance the best interests of the community, and to promote that charitable feeling which may be said to be a characteristic feature of the members of our profession, whatever the faults otherwise laid to our charge.

Of the addresses individually, we would speak generally in terms of praise; but to him who would hear the voice of the wise and good physician, the man of large heart and profound knowledge, on the momentous question of the reality of Christianity, and of its influence upon humanity, to him we would especially recommend the prefatory essay by Dr. Alison. The University of Edinburgh, and other schools of medicine, would do well to secure the distribution of such papers as the one written by the Nestor of medical science, among those who are entering upon their studentship.

Art. VI.—*Military Sanatoria. Letter addressed to H.R.H. the Duke of Cambridge, Commander-in-Chief, &c., on the Introduction of Mineral-Water Establishments for the Use of the Army.* By P. Pincoffs, M.D., M.R.C.P., &c., late Civil Physician to the Scutari Hospital. —*London,* 1856. pp. 37.

In the October number of this Review we adverted to the fact that the establishment of military sanatoria had been suggested by Dr. Pincoffs, and was under the consideration of Government. In the pamphlet before us, Dr. Pincoffs explains in a lucid and succinct manner the grounds upon which the recommendation has been made, and the advantages to be derived from its adoption. In France and Germany, lengthened experience has shown that many soldiers whose constitutions are undermined by the wear and tear inseparable from military duty, are often, within a very short period, restored to health, by being sent to the sanatoria established in thermal and other watering-places, after having been treated for months to little or no purpose in the excellent hospitals of their various stations. Numerous arguments of a philanthropic character may be adduced for following the example set by continental nations, but none is so likely to be of weight with those to whom the soldier's welfare is immediately entrusted, than the fact that France, with a standing army nearly three times as large as that of England, should have fewer out-pensioners. On the 1st of August, 1856, there were 51,530 pensioners in the French army (including about 3000 in the Hôtel des Invalides), whilst our own pensioners amounted to 59,987.

"With all due allowances for the differences of the two services, owing to the conditions under which enlistment takes place, the length of service required, the vicissitudes of climate to which our soldiers are especially exposed, I do not hesitate to say that the comparatively small number of pensioners in the French army may in a great measure be accounted for, by the timely use and decided benefit derived from their thermal establishments; whilst the larger proportion of our pensioners is chiefly owing to the system hitherto pursued, of retaining a man in foreign service until his constitution is entirely broken, and then discharging and pensioning him; whereas he would have a fair chance of recovering his health and returning to duty were he sent home in time to such sanatoria as I propose."

We cordially recommend the perusal of Dr. Pincoffs' pamphlet to all who have the welfare of the soldier at heart; as it appears that the movement which he has initiated may prove successful (for we learn that a small experiment is already being carried out upon his principle at Bath), we trust it may react upon the civil sanatoria, infirmaries and convalescent institutions already existing, but often languishing for want of funds because the public are not sufficiently acquainted with the purport and importance of such establishments.

Art. VII.—*On the Composition of Food, and how it is Adulterated; with Practical Directions for its Analysis.* By W. Marcet, M.D., F.C.S., Licentiate of the Royal College of Physicians, Assistant-Physician and Lecturer on Physiological and Pathological Chemistry to the Westminster Hospital; formerly President of the Edinburgh Medical Society; Corresponding Member of the Société de Biologie of Paris, and Société de Médecine of Geneva, &c.—*London,* 1856. pp. 178.

When we introduced Dr. Hassall's important work, 'On Food, and its Adulterations,' to the notice of our readers, we suggested that an abridged work of the kind would prove very acceptable to a large class of persons. The book which Dr. Marcet now offers is a substitute for the one we anticipated seeing from Dr. Hassall's pen. It is in a great measure, and confessedly, based upon the researches of the Analytical Commission of the 'Lancet,' and of other inquirers in the same field of science, while the author gives plain and simple directions for the methods most suitable for the detection of the various adulterations that are commonly practised.

We are glad to perceive that Dr. Marcet discourages the prevailing tendency to see "death in every pot," while he tells us how we may readily discover the fraud that tends to increase our bills and to diminish the rational enjoyment of our meals. Now that our tradespeople must know how easily they are found out, we trust that, for their own sakes as well as ours, they will universally appreciate the maxim of honesty being the soundest policy.

Dr. Marcet divides the whole subject into seven chapters, which successively treat: 1. On farinaceous and saccharine food. 2. On Spices. 3. On infusion of vegetable substances, other fluid vegetable food, tea, coffee, sauces, olive oil. 4. On fermented beverages. 5. On animal food, meat, fish, milk. 6. On mineral or inorganic food, salt, and water. And 7. On preserved food.

The descriptions of the substances are clear, the details of the microscopical and chemical analyses are succinctly and intelligibly given, and the illustrations are executed with that care which characterizes the productions of Mr. Lens Aldous and Mr. Hart.

We have no doubt that Dr. Marcet's volume will be extensively consulted, both in and out of the profession. We may add the suggestion, that a future edition would be rendered more complete by being provided with an index, without which no book should appear which is intended to serve for reference.

Art. VIII.—*Sull' Uso della Gomma-Resina d'Asa Fœtida adopterata per prevenire la Morte del Feto nelle Gravidanze morbose cagionate da inerzia dell' Utero.* Memoria del Dottore Gaetano la Ferla, Vice-Presidente della Societa Medica di Malta.—*Malta,* 1855. pp. 29.

On the Use of the Gum-resin of Assafœtida in Preventing the Death of the Fœtus in Morbid Pregnancies caused by Inertia of the Uterus. By Dr. Gaetano La Ferla, Vice-President of the Medical Society of Malta.

The author takes credit to himself for discovering a power he believes to exist in assafœtida of preventing the death of the fœtus and abortion when these phenomena depend, as he supposes, upon an asthenic condition of the uterus. Among the causes of this state he enumerates depressing mental emotions, terror, venereal affections, the abuse of mercurial inunctions, a relaxed constitution of the body, amenorrhœa, repeated menorrhagia, leucorrhœa, hysteria, and neglected miscarriage. His cases do not appear to us to be by any means conclusive; thus in the first, in which the death of the fœtus in three pregnancies depended upon syphilis, the venereal taint probably wore itself out, the fourth child—being the first born after the assafœtida treatment—living five days, while the children subsequently born continued to live (*rimasero in vita*). In the author's fifth case, his method succeeded after the patient and her husband had been subjected to anti-venereal treatment! His plan may be briefly stated to consist in the daily administration of assafœtida in such doses that from ten to fifteen drachms shall have been taken before the period at which the death of the fœtus occurred in the preceding pregnancy. All we should be inclined to say in favour of the efficacy of the system as reported, is, that it did not prevent the good results of the "strong soups, the decoction of chamomile, and the goat's milk, which he wisely administered to the cachectic patients with whom he had to deal; and that it did not counteract his efforts to remove from them every source of annoyance and disquiet, to keep them agreeably occupied, and to inspire them with the hope of an auspicious delivery."

ART. IX.—*The Complete Handbook of Obstetric Surgery; or, Short Rules of Practice in every Emergency, from the Simplest to the most Formidable Operations connected with the Science of Obstetricy.* By CHARLES CLAY, M.D.—*London*, 1856. 16mo, pp. 290.

IN the preface to this work, the author informs us that he was originally led to prepare it for publication from the belief that no similar work existed in medical literature, and that one on such a plan was really required.

Without going quite so far as to admit that no such work exists in medical literature, we are quite willing to concede that we are not acquainted with one of a similar kind in the English language; and that, as far as we can judge, the author has succeeded in making it one of great practical utility. The work describes upwards of one hundred and eighty operations, many of which are not adverted to in the more ordinary treatises on midwifery. In several instances, the descriptions are illustrated by original and instructive woodcuts; and some of the articles, such as those on Embryotomy, Spontaneous Evolution or Expulsion, Hæmorrhage, Ovariotomy, and Version, are so elaborate and complete as to be worthy of taking rank with the best essays extant on these subjects.

The introductory chapter on Chloroform, although concisely written, contains some good general rules for its employment. We extract the following:—

"*First.*—IN CASES OF LABOUR. It may be used in severe, short, but ineffectual pains, which restrain bearing-down efforts. In these, chloroform renders uterine contractions longer, stronger, and more efficacious; and thus it accelerates the accomplishment of the process.

"*Second.*—Where the parts are rigid and unyielding, it assists in dilating the parts, relaxes the muscular fibre, and relieves the severity of pain arising from rigidity.

"*Third.*—In long-protracted cases, worn down and suffering from nervous debility, and also irritability, it restores the physical powers, relieving both pain and anxiety.

"*Fourth.*—In some forms of convulsions it has been found useful.

"*It is not to be used.*—In convulsions of apoplectic or epileptic type:

"Or when the patient is strongly opposed to it.

"And even when the aversion to it is only moderate, it should not be urged.

"*The time for exhibiting it.*—It should not generally be given until the second stage of labour is established; unless some unusual severity of pains harasses the patient unnecessarily, when it may be used somewhat earlier." (p. 5.)

We agree also with the following :—

"*Necessary cautions.*—The pulse to be constantly felt, and if any untoward effects arise, the handkerchief to be removed.

"At first admit a free mixture of atmospheric air.

"Temperature of the apartment must be moderate.

"Patient not to be placed in deep insensibility (or snoring).

"Never commence chloroform in large doses.

"Preserve insensibility. Watch narrowly its effects.

"Never give chloroform immediately after a full meal, nor yet after long fasting. If a choice can be made as to time, select about two hours from the last food taken." (pp. 6–7.)

From the chapter on Embryotomy, we give the following quotation as illustrating the manner in which the subject is treated :—

"*Object.*—To reduce the child, to enable it to pass where the pelvic diameters are too small to allow a living child to pass from deformity or tumours; or where the head, from disease, is too large, though pelvis natural; and thus to save the mother at the expense of the child.

"*Necessary Conditions.*—When the head, though compressed, will not pass; when there is only just room for a mutilated fœtus to pass; when the forceps cannot effect delivery; when there is hydrocephalus.

"*Instruments.*—There are more in use than necessary; perforator, crochet, bone-forceps, craniotomy-forceps, cephalotribe, kephalepsalis, osteotomist. There is very seldom any necessity for more than the two first.

"*Dimensions of Pelvis requiring them.*—Antero-posterior diameter, according to Osborne, $2\frac{3}{4}$ inches; Clarke, $3\frac{1}{2}$; Burns, $3\frac{1}{4}$; Le Bois, 3; Aitkin, $2\frac{1}{2}$ to 3; Busch, 3.

"*Smallest Diameter allowing it.*—According to Dewees, 2 inches; Baudelocque, $1\frac{2}{3}$; Hull, $1\frac{3}{4}$; Burns, $1\frac{3}{4}$; Gardien, $1\frac{1}{2}$; Hamilton, $1\frac{1}{2}$; Davis, 1.

"*Mortality.*—To the child always; to the mother 1 death in 5." (p. 68.)

The subject of spontaneous evolution or expulsion is put very clearly before the reader, and illustrated by several diagrams copied from the work of Chailly. The explanation of the process given by Dr. Douglas in contradistinction to that offered by Dr. Denman, is, as we conceive, very properly insisted upon; the body of the child in these cases being doubled upon itself and expelled as in breach presentations, rather than rotated, so that the head emerges first from the pelvis—a mode of expulsion, it is almost unnecessary to add, which can only take place exceptionally, when the child is small and the pelvis capacious.

The chapter on Uterine Hæmorrhage is extremely comprehensive, and treats of the subject under the two heads of its general characters and varieties. The former comprising more particularly its statistics, dangers, symptoms, modes, and general treatment; and the latter its several varieties as occurring during pregnancy, during labour, and after delivery.

The next subject discussed by our author is Ovariotomy; the general history, symptoms, diagnosis, and treatment, remedial and operative, of ovarian tumours, are very fully but concisely set forward. Few men would appear to have had more experience in regard to extirpation of the ovaries than Dr. Clay, and few therefore have a right to be heard more authoritatively upon this question:—

"I have now operated," he observes, "seventy-one times (for extirpation of the ovary), and the general results are as follows—71 cases: 49 recoveries, 22 deaths. Taking these cases in groups as they occurred—

"The first 20 cases, 8 died, 12 recovered.
"The second 20 cases, 6 died, 14 recovered.
"The last 31 cases, 8 died, 22 recovered.

"Thus, the mortality has been gradually lessened.

"Of the first 20, deaths 1 in $2\frac{1}{2}$.
"Of the second 20, deaths 1 in $3\frac{1}{3}$.
"Of the last 31, deaths 1 in 4.

"Here, as it might naturally be expected, experience has gradually lessened the mortality in my own practice from 1 in $2\frac{1}{2}$ to 1 in 4. And I have a confident hope that it will be reduced still further, from improved diagnosis, experience in operating, and lastly, in the mode (which practice only can command) of after-treatment being better understood." (pp. 158–159.)

We fear, however, that the experience of others will not enforce the very favourable opinion thus expressed of the results to be expected from the operation. The facts collected by Dr. Robert Lee* are far from encouraging; and apart from the risks and dangers of the operation itself, which in many cases can neither be anticipated nor foreseen, we would submit that the difficulty of always making an accurate diagnosis must ever constitute an insuperable barrier to its frequent performance. Nevertheless, we admit that, under certain circumstances, it may be rendered justifiable; and in such cases the experience and practice of our author, as laid down in the present chapter, may be usefully appealed to.

ART. X.—*Researches on Pathological Anatomy and Clinical Surgery.* By JOSEPH SAMSON GAMGEE.—*London*, 1856. 8vo, pp. 216.

THESE memoirs, though exhibiting industry and powers of observation on the part of their author, would have found their proper place in one of the journals, whence, if thought desirable, they might have afterwards been collected, with the advantages of a more matured consideration and a less hurried composition than they now exhibit.

The papers are eight in number:—1. On Rupture of the Heart from External Violence. Mr. Gamgee relates an interesting case of this occurrence, and, from a survey of several recorded cases, comes to the conclusion that rupture of the heart, without penetrating wounds, takes

* Medico-Chirurgical Transactions, vol. xxxiv. p. 13.

place more frequently than is generally supposed. 2. On Dry Gangrene. Two cases are given. 3. On Cystic Sarcoma and Cancer of the Breast. The difficulties of diagnosis in tumours of the breast, when complicated with cysts, are dwelt upon; and the author suggests that some of the confusion that prevails might be obviated by substituting for the terms "cystic sarcoma," and "cystic carcinoma," the appellations "glandular, or benign tumour with cysts," "scirrhus, or encephaloid with cysts." 4. Treatment of Cancer by Landolfi's Paste. Mr. Gamgee is rather severe in his criticism upon the mode in which M. Landolfi's pretensions as a curer of cancer have been handled by the German medical press; but we should be glad to know how any man can be characterized other than as a charlatan who declares that "in 3000 out of 4000 cases which I have treated by this method, the patients had no recurrence of cancer," without furnishing any clinical proof of the accuracy of such a statement. A most adverse report was made by the Paris Commission. 5. On Syphilisation. The author here gives us an account of a visit he paid to Sperino of Turin, who seems to have thoroughly inoculated him with his doctrines. The most satisfactory part for us is to learn that the Professor has suspended his unjustifiable experiments at the *Syphilicome* upon the unfortunate beings committed to his charge. 6. On the Neapolitan Mode of performing Lithotomy. Mr. Gamgee describes the mode of performing the lateral operation as he witnessed it at Naples, and which he finds to be identical with that advocated by Moreau. The maximum mortality, he says, varies from 15 to 10 per cent. From a comparison of some of the results obtained from lithotomy and lithotrity, he gives a decided preference to the former operation; but the figures he adduces stand in need of a far more rigorous examination than he has bestowed upon them, before such conclusion can be accepted. 7. Treatment of Fractures of the Lower Extremity. The treatment by suspension is here described, and that by the starch-bandage recommended. 8. On Calcification and Ossification of the Testis. References are given to the various cases and specimens on record.

ART. XI.—*Index Morborum Internorum Systematicus. Præmissis de Nosogenesi Aphorismis.* Auctore O. BANG, M.D. et Professore in Universitate Hauniensi; Regi a consiliis Conferentiarum; Collegii regii cui summa rerum medicinalium in Dania cura incumbit, sodali; commendatore ord. Danebrogici et Wasa, cruce argentea ord. Danebrog. ornato; membro societ. med. Hauniensis, Dresdensis, et Hamburgensis honorario, Suecicæ, Norwegicæ, Bonnensis, Heidelbergicæ ordinario, Academiæ Imperialis Medicæ Gallorum correspondente.—*Hauniæ*, 1855. pp. 36.

A Systematic Classification of Internal Diseases; preceded by Aphoristic Remarks on the Origin of Disease. By O. BANG, M.D., Professor in the University of Copenhagen, &c.

So long as the candid physician is constrained to acknowledge that the intimate nature of disease, the actual derangements accompanying morbid conditions in the majority of instances, are incapable of positive demonstration, so long will the attempt to establish a sound and comprehensive system of nosology fail to meet all the difficulties that invest such attempts. The foundation upon which any structure of the kind can as yet be built up, must necessarily be in a great measure hypothetical, and that hypothetical foundation as necessarily varies according to the individual mind that constructs it. This should not be the case with any department of inductive science. It is high time that the experience of the past should restrain men's minds from yielding to the fascination of dreamy speculations. Most of all does it behove men who occupy the responsible position of instructors, to beware how they hold up a Will-o'-the-Wisp to their auditors in the place of a steady and guiding light.

These remarks have been specially suggested to us by the perusal of Professor Bang's aphorisms, which form the basis upon which his nosological system rests. He commences by attributing disease to three different kinds of effects produced by the action of injurious influences (*potentiæ nocentes*) upon the human frame; according to his views, disease will manifest itself either in reaction to the influences, or by vicarious action in an organ for the one first exposed to injury, or by localization in an organ in which the influence may produce less harm than in the part first attacked. With all the respect due to a man in the position of the author, we cannot but think that, in arguing about vicarious action and localization as two distinct primary elements of disease, he establishes a distinction without a difference. But if we apply this mode of elucidating the nature of disease to any given case, how are we to determine into which category it fits? No indications are given by which we are to make our selection. Were this only a plaything of the learned Professor, which he takes up to beguile an hour, we should not be disposed to treat it seriously; but these three elements of disease are made the elements of his practice, for the treatment of disease is to be based upon them, and the aid they afford in its elimination. In paragraph 20, we read—

"Because in the chief symptoms of the majority of diseases we find nature exercising a curative influence, and the powers of nature are as it were the source of the diseases. The physician ought always to be the minister and interpreter of nature, and never disturb her efforts, so long as he thinks he may attain his end by his three remedies, reaction, vicariation, and localization." (p. 14.)

The classification itself recognises two main divisions: the first of these is the diseases of the cardinal systems, or universal diseases: these are subdivided into dynamic and material diseases; the former include angiopathia and neuropathia; the latter, hæmopathia, with all those affections ordinarily set down to the presence of a poison in the blood; eccrinopathia, or disorders of se- and excretion, and trophopathia, or diseases of nutrition. The second great division includes the diseases of organs or topical affections.

ART. XII.—*Memorie della Società Medico-Chirurgica di Bologna—Del Tetano Traumatico Trattato.* Del Dottore LEONZO CAPPARELLI.—*Bologna*, 1855. 4to, pp. 210.

Memoirs of the Medico-Chirurgical Society of Bologna—A Treatise on Traumatic Tetanus. By Dr. L. CAPPARELLI.

THE Medico-Chirurgical Society of Bologna having, in January, 1853, announced a prize of 500 francs for the best monograph on Traumatic

Tetanus; but one, the treatise named at the head of this article, was sent in: and although it was found not to have fulfilled all the requirements of the proposed subject, it was, in consideration of its many and special merits, awarded the prize. In thus deciding, the Council of Adjudication acted, we think, wisely; the work before us is a full, elaborate, and interesting essay on a most important subject, the consideration of which is brought down completely to the present time.

The author divides his treatise into three parts, the first containing a description of traumatic tetanus, with the functional disturbances to which it gives rise; its diagnosis and its course. In the second part, he considers the causes, the pathological anatomy, and the pathogeny of the disease; and in the third, he discusses its treatment and prognosis. We can merely glance at a few points in one or two of the subdivisions of these heads.

The direct causes of traumatic tetanus may, as the name of the disease itself suggests, be classed under the single categorical head of lesion of the tissues, and they are studied by the author in three subdivisions: viz., violent lesions, artificial lesions, and sores. An examination of 125 well-established cases of the first of these varieties, shows that tetanus proceeded from punctured wounds, in 28 instances; from lacerated wounds, in 22; from gun-shot wounds, in 17; from contusions and contused wounds, in 15; from cutting wounds, in 11; from fractures, in 19; from burns, in 6; from dislocations, in 3; from bites, in 2; from pulling, in 2. With respect to the susceptibility of the several parts of the body, the following gradation was observed: the hand was the part injured, in 31 cases; the foot, in 30; the lower extremities, in 30; the upper extremities, in 14; the head and neck, in 11; the shoulder and back, in 6; the abdomen, in 3; the thorax, in none. The preponderance of punctured and lacerated wounds as causes of traumatic tetanus, and the greater liability of the disease to follow injuries of the extremities than those of the trunk, head, or neck, are well known; but reliable statistics are so important, that we quote the numbers given by the author. Artificial lesions are considered under the subdivisions of major and minor surgery (*alta e bassa chirurgia*); and of sores, not usually classed among the causes of tetanus, one produced by the application of caustic potash to a suppurated inguinal bubo, and a few other instances quoted from different authors, are brought forward as examples. From an examination of 116 cases of traumatic tetanus, furnished by his own experience and that of others, the author deduced the following results as to the ages at which the disease is most likely to occur: of these 116 cases, 41 took place at between 10 and 20 years; 37 between 20 and 30; 15 between 40 and 50; 14 between 30 and 40; 4 between 50 and 60; 4 between 1 and 10; and 1 between 60 and 70.

In reference to treatment, the author does not, we fear, add to our means of combating this formidable malady.

The seventh chapter of the third part treats of spontaneous recovery from traumatic tetanus; and the eighth and last, of the prognosis of the disease.

Art. XIII.—*Summary of New Publications.*

Among the books recently received, we would first advert to the 'Army Meteorological Register, for Twelve Years, of the United States,' compiled under the direction of Brevet-Brigadier-General Thomas Lawson, Surgeon-General to the U.S. Army, a work evincing great labour and research. We mention Dr. Conolly's important work on the 'Treatment of Insanity' merely to state that the review which we have prepared of it, unavoidably stands over to our next number. From India we have received a translation of a Treatise by Vogel, 'On the Disorders of the Blood,' carefully executed by Chundar Coomar Dey.

In Medicine we have to mention the appearance of a third and enlarged edition of Dr. Williams' admirable 'Principles of Medicine;' and a second edition of the very illustrative and practical Lectures by Dr. Todd, on 'Diseases of the Nervous System.' Dr. Bower Harrison has issued 'A Few Remarks on the Perforating Ulcer of the Stomach and Bowels;' and we have also before us a reprint of Dr. Laycock's paper 'On the Pathology and Treatment of Contagious Furunculoid.' A very useful exposition of the Varieties of Continued Fever, by Dr. Peacock, we warmly recommend to the student; and to the attention of the profession generally, Dr. Ballard's little book 'On Artificial Digestion.' From Germany we have received: the collected Essays of Professor Virchow, of which we hope to give an analysis in our next; a work by Lebert, 'Die Cholera in der Schweiz;' and the first two parts of a large work by Dr. Spiess, of Francfort, entitled, 'Pathologische Physiologie, Grundzüge der gesammten Krankheitslehre.'

Among recent Surgical Works we have to name a fourth edition of Professor Syme's 'Principles of Surgery,' and the completion of the second edition of Maclise's 'Surgical Anatomy.' An interesting Address by Mr. Langston Parker, 'On the Treatment of Cancerous Diseases by Caustics,' places this subject in a more hopeful light. Mr. Zachariah Laurence has issued a reprint of the papers 'On the Pathology of Cancer,' which have recently appeared in the 'Journal of the British Medical Association.'

In Midwifery we announce a new work by Dr. Rigby, 'The Constitutional Treatment of Female Diseases;' a useful little book by Dr. Swayne, entitled 'Obstetric Aphorisms for the use of Students commencing Midwifery Practice.' A new edition of the 'Dublin Practice of Midwifery,' by Dr. Maunsell, is before us; and Dr. Bedford's work on 'Obstetrics' has already, within little more than a year, attained the honour of a fourth edition. To Dr. Bozeman's 'Remarks on Vesico-vaginal Fistula,' we shall revert at a future time; while of Dr. Gardner's work 'On Sterility,' we would rather say nothing, but that it is our duty to protest against the breach of good taste manifested in some of the illustrations.

In Sanitary Science we have to add to the Essays of Drs. Barclay and Tripe—to the subjects of which we hope to advert more fully in our next—the Parliamentary Blue-books 'On the Census of Ireland,' a work of great labour, in which Mr. W. R. Wilde, of Dublin, has borne an important part. They contain, among other valuable information, an elaborate abstract of the Epidemiology of Ireland, which we shall take an early opportunity of introducing more fully to our readers. In conjunction with this Report, we may mention that the Registrar-General has issued his 'Seventeenth Annual Report of Births, Deaths, and Marriages.'

Dr. Royle, with the able assistance of Dr. Headland, has brought out a third edition of his well-known 'Manual of Materia Medica;' Dr. Wood, of Philadelphia, has forwarded to us an important work, in two volumes, on the same subjects; while a seventh edition of Dr. Dunglison's work, 'New Remedies, with Formulæ,' together with an eighth edition of the same author's 'Human Physiology,' have just come to hand. Third editions of two well-known English Manuals 'On Physiology,' by Dr. Carpenter and Dr. Kirkes, have to be mentioned; and a sixth edition of Fownes' 'Elementary Chemistry,' under the joint supervision of Dr. Bence Jones and Dr. Hofmann.

A Monograph, by Mr. Swan, 'On the Origin of the Visual Powers of the Optic Nerve,' deserves special mention; a valuable Monograph has also reached us from France, by M. Godard, 'On Monorchidism and Cryptorchidism.' 'The Mechanism of the Gubernaculum Testis,' forms the subject of an Edinburgh Prize Essay, by Dr. Cleland. Dr. Beale has published in a collected form his 'Researches on the Liver;' and the first two parts of a large work 'On Human Anatomy,' by Professor Henle, have come to hand, of which we will at present only say that they present a new feature, in form of illustrations by coloured woodcuts.

Mr. M'Cosh's 'Advice to Officers in India,' which may be recommended to young hands, has obtained the honour of a second edition; a work of an analogous character, 'Hygienic, Medical, and Surgical Hints for Young Officers of the Royal and of the Merchant Navy,' has been issued by Dr. Saunders, R.N.

The new volume of the Third Series of the 'Guy's Hospital Reports' contains numerous articles of great value, among which we would specially mention Dr. Taylor's article on Poisoning by Strychnia, since republished in a separate form. The thirty-ninth volume of the 'Medico-Chirurgical Transactions,' of which we hope to give an abstract in our next, is of the high character which has usually distinguished its predecessors; nor can less be said of the seventh volume of the Pathological Society. A series of excellent Addresses are on our table, which handle the Science and Polity of Medicine in a variety of aspects, and have been published 'by request,' the names of the respective authors are Drs. Parkes, Chambers, Richardson, Ramsbotham, Miller; and Messrs. M'Whinnie, De Morgan, and Inman.

We may not conclude this summary without bestowing a word of praise upon a little book by Mr. Rhind, dwelling upon the advantages to the invalid of travel on the Nile; and we would also introduce to our readers the new French bi-monthly periodical, with a name bearing a familiar sound, the 'Revue Étrangère Médico-Chirurgicale,' the first number of which appeared on the 16th October, 1856.

PART THIRD.

Original Communications.

Art. I.

An Essay on the Cause of the Coagulation of the Blood.*
By E. Brücke, M.D.

§ 1. The fact that blood, when removed from the human body, coagulates in a few minutes, is universally known. Three things happen as consequences of this removal—

1. The blood comes into a lower temperature.
2. Its motion is lost, and it is reduced to a state of rest.
3. It is brought into contact with air.

Formerly, therefore, coagulation was referred to these three causes, or to one or two of them.

On a little examination, it would hardly appear that a *decrease of temperature* would be sufficient to cause the phenomena of coagulation; for the blood of fishes, turtles, frogs, and other cold-blooded animals, is fluid at a still lower temperature; and yet if removed from the body, it coagulates, without undergoing any change in that particular. Hewson, the immortal inquirer into the properties of the circulating fluid, demonstrated that it is possible to *freeze* the blood while yet fluid, and that after being thawed again, it will coagulate in the ordinary way. He has pointed out that a high temperature hastens coagulation, and that a low one retards it. In common with many other physiologists, I have myself found this to be the case; but I cannot agree with what has been asserted by some, namely, that at a temperature near the freezing point, coagulation is entirely prevented; for I have seen blood coagulate at every temperature above 32° F., and even below that point, provided the blood itself was not frozen. Having caught the blood of a horse in a small, wide-mouthed glass vessel, I corked it, and placed it in a jar containing a mixture of chloride of calcium and ice, in which it was cooled, and kept thoroughly fluid during 1h. 30m. The whole was then taken into an ice-cellar, and buried in the snow. Twenty-four hours after, the freezing mixture had a temperature of 31° F.; nevertheless, the blood had coagulated on the surface and on the walls of the vessel, so that about six parts were fluid, and one part coagulated. Another portion of the same blood placed under similar circumstances, was found to be entirely coagulated on the

* This Essay was submitted to the physicians and surgeons of Guy's Hospital, on the 15th Dec., 1855, in competition for the Fifth Triennial Prize, founded by Sir Astley Cooper.

fourth day. In this case the temperature was 32° F. Similar effects resulted when the blood of frogs and of turtles was treated in the same manner. In some cases it had thoroughly coagulated in twenty-four hours, in others it was only covered with a thin film. Sometimes, however, I have found the blood of frogs remain fluid for eight days while kept in the snow, and that of turtles for four days; and yet this blood has afterwards coagulated when raised to a temperature of 54°—58° F. All these experiments were conducted in precisely the same manner, and the blood refrigerated as quickly as possible, so that I can only look to the quality of the blood for an explanation of the difference of the results.

The period of fluidity decreases slowly with the increasing temperature until it reaches about 50° F., when the coagulation takes place more rapidly. It is from this cause, I believe, that some writers have been led into the error of stating that blood, at a temperature lower than 50°, does not coagulate. Undoubtedly they did not watch long enough to observe the coagulation. Turner Thackrah, in the second edition of his 'Inquiry into the Nature and Properties of the Blood' (London, 1834), says, p. 67: "A temperature of 120°—130° considerably accelerates the concretion of the blood, and one of 100°—110° generally does so, but in a less marked manner. A temperature of 40°—50° retards coagulation." In these remarks he is fully correct.

§ 2. Hewson also showed that it is not possible to keep the blood fluid by agitation, even at the temperature of the living body. He knew that the blood, even in the living body, commonly coagulates in those parts in which it ceases to circulate; but he found that in the tied jugular vein of a dog, above two-thirds of the blood continued fluid, after having been at rest three hours and a quarter;* and in one case, a part of the blood in a dog's heart was found uncoagulated thirteen hours after death.† Therefore *rest alone* cannot be the cause of coagulation. The blood of a terrier, which was suffocated by fastening a hog's bladder over his mouth, I found entirely fluid seven hours and thirty-five minutes after death. Also the blood of a large spotted or Dalmatian dog, killed in the same manner, was quite fluid six hours and thirty-two minutes afterwards. Nevertheless, when removed from the vessels, coagulation in both cases took place in from seven to ten minutes. I have also preserved thoroughly fluid the blood in the hearts of turtles (Emys Europæa), while it has been at rest, six, seven, and even eight days, in a room having a temperature of between 30° and 34° F., three days in a temperature of 50°, and twenty-four hours in a temperature of 75°.

These experiments were conducted in the following manner. Having tied the great arteries half an inch from the heart, and also the vena cardio-pericardiaca,‡ when, after some time, the heart was swollen with venous blood, I tied the great veins also where they enter the auricles. The heart was then cut out, and suspended in oil previously heated with plaster of Paris, and washed with distilled water. Some time after the heart ceases to beat, the red particles subside, leaving the clear plasma

* An Experimental Inquiry into the Properties of the Blood. First Ed., p. 18. London. 1772.

† Ibid. p. 71.

‡ I give this name to the little vein represented in Bojanus, Anatome Testudinis Europææ. Vilnæ, 1819–1821, tab. xxix. fig. 160, æ 4.

above; but the fibrin does not coagulate until the last trace of life has disappeared. This experiment I repeated frequently with the same result; the blood always remained fluid in the heart, but when taken out it invariably coagulated, even when covered with oil.

§ 3. Hewson believed that atmospheric air was a powerful cause of coagulation. The chief experiment which induced this belief, he relates at page 20, as follows:—

> "Having laid bare the jugular vein of a living rabbit, I tied it up in three places, and then opened it between two of the ligatures, and emptied that part of its blood. I next blew warm air into the empty vein, and put another ligature upon it; and letting it rest till I thought the air had acquired the same degree of heat as the blood, I then removed the intermediate ligature and mixed the air with the blood. The air immediately made the blood florid where it was in contact with it, as could be seen through the coats of the vein. In a quarter of an hour I opened the vein, and found the blood entirely coagulated; and as the blood could not in this time have been completely congealed by rest alone, the air was probably the cause of its coagulation."

It is very true, and I have often proved it, that air blown in, in the manner he has described, hastens coagulation; but it is not always so. Having laid bare the right jugular vein of a shepherd's dog, which had been suffocated, I operated on it precisely as Hewson describes; and afterwards on the left jugular in the same manner, but without blowing in air. This experiment was performed at 10 o'clock a.m., and at 2·30 p.m. the blood in both jugulars was but incompletely coagulated, the fluid part coagulating after it had run out; and it was easy to perceive that the blood which was mixed with air had coagulated slowly, the red corpuscles having subsided so as to render the clot sizy. That this was not owing to a general want of coagulability in the blood of this animal, is proved by the fact that the blood in the other parts of the body coagulated not more slowly than in many other cases, so that by 4·45 p.m. the whole was solidified. The temperature of the room was 68° F.

I also tied the great arteries of an Emys Europæa, blew a quantity of air into the left subclavian vein, in order that it might mix with the blood in the heart, and then, having tied the veins, I cut the heart out, and laid it in oil purified as above mentioned. Twenty-four hours after, the blood was quite fluid, but coagulated when taken out. This experiment, repeated frequently and at a variety of temperatures, gave always the same result.

In June, 1854, I blew a quantity of air into the left subclavian vein of a living Emys Europæa; and the air having advanced into the arteries, I then tied the vein, covered it again with the sternum, and placed the animal in a cellar of 50° F. After it had been there five days, I found nearly the whole bulk of the plasma sanguinis in the great cisterna lymphatica; so that I could scoop it out with a watch-glass, in which it coagulated in from ten to twenty minutes. The bloodvessels had retained the corpuscles with very little plasma, and nowhere was there the slightest trace of a coagulum. This experiment, repeated three days after, gave the same result.

I also repeated it in a higher temperature. After having introduced the air, I brought the animal into a room of 75° F.; and after a lapse of twenty-four hours, I let the blood out of the heart. It was foamy, but thoroughly fluid, and coagulated in the usual way.

I tied the two aortæ of an Emys Europæa, with a view to confine the blood entirely to the circulation of the lungs; and then established artificial respiration for the space of one hour. The blood became very fluid, but no clots were formed in the vessels; and when taken out, the process of coagulation was by no means uncommonly rapid. The first distinct film was formed in fifteen minutes; but even after a lapse of one hour and eight minutes, on cutting through the clot I found some drops of a coagulable fluid.

Turner Thackrah* cites cases in which the arterial blood coagulated sooner than the venous blood of the same individual; and it has often been disputed which of the two kinds usually coagulates the earliest. The truth is, that arterial blood may coagulate either slowly or quickly, and venous blood may do so too. There are *other* conditions which overpower the influence of gases so much, that that influence cannot be distinguished; but I observed that in numerous experiments in which the blood coagulated uncommonly slowly, it was extremely venous.

It is well known that bright blood, excluded from the air, becomes dark by changing its oxygen into carbonic acid; but it becomes even darker by its natural contact with the living heart and vessels. If, while a rabbit is being suffocated, a vein is suddenly opened, the blood flows out as dark as ink; and frogs' hearts, which, being distended with blood, are then tied and inserted into an inverted receiver full of mercury, become, under these circumstances, so dark, that every trace of red vanishes; whereas the blood placed in the same receiver apart from the heart, becomes only of a dark red. Blood which is thus deprived of its oxygen, coagulates more slowly than that drawn from the living animal; this I observed generally with respect to the blood of frogs, toads, and turtles retained in the heart, and thus kept in oil or in quicksilver. The slowest case of coagulation was shown by the blood in a turtle's heart which was kept for twenty-four hours in hydrogen gas. After having been exposed for an hour to atmospheric air, the first thin film was visible, which I removed; a new film was then slowly formed, which I also removed; a third film then appeared, but so slow was the operation, that the coagulation was not complete until about four hours after its commencement.

In warm-blooded animals, the blood abstracted an hour or two after death coagulates also slowly. Having suffocated a rabbit by drowning it, I took blood about an hour afterwards from the right jugular vein; and, in a temperature of 63° F., coagulation began in twenty-five minutes, and finished in forty minutes. Three hours afterwards I took some blood from the left jugular, in which there was already a slight coagulum; but the part which remained fluid concreted so slowly, that it did not begin until thirty minutes after; and even in the space of an hour it was not completed, the clot still containing some fluid drops, which, however, coagulated when removed and placed in a watch-glass; still, these striking cases are exceptional. I frequently took blood from suffocated dogs, several hours after death, which clotted in seven or ten minutes. The blood of

* Op. cit., p. 42. Lond. 1819.

living dogs coagulates in the air in from two to five minutes, and even sooner; but in warm-blooded animals there are two causes which retard the coagulation of blood taken after death—viz., first, the chemical alteration which the blood has undergone, and secondly, the reduction of its temperature.

It appears, then, that air assists coagulation, but not always powerfully. The contact of other foreign bodies with the blood in the vessels also hastens its coagulation in as great, or as it would appear, in an even greater, degree. Thus, having inserted a rolled platina wire into the jugular vein of a suffocated dog, I drew it out after fifteen minutes, covered with a clot. Further, having inserted, by means of a glass tube, some mercury into the heart of a turtle, after having tied the great arteries, I then tied the veins, cut out the heart, and kept it for eight days in oil at a temperature of 36° F. On examination after that time, the mercury was found to have dispersed itself in numerous globules in the arterial trunks, and in the right auricle. The globules were covered with a clot, but the rest of the blood was fluid, and coagulated as usual afterwards. Virchow,* De Castelnau, and Ducrest also found coagula round the globules of mercury which they had conveyed into the right side of the heart of dogs and rabbits; and Magendie, Carswell, John Davy, and Schroeder Van der Kolk, each observed the coagulating power of other foreign bodies, when conveyed into the stream of the blood.

It has been often observed, that when blood is placed in a cup, coagulation generally begins on the surface, and that if it takes place slowly, a film can be removed from the surface, the blood below being fluid. But it must be remarked, that it takes place nearly as soon on the walls and on the bottom of the cup, leaving the middle of its bulk still fluid. Once, as has been already mentioned, some of the blood of a turtle remained fluid for four hours, exposed to the air in a watch-glass; and Hewson† relates some striking instances of the same kind: in fact, it is a general rule. The difference in the time between the coagulation of the outer and of the inner part of the blood, is, however, not always constant, and is of course the greater when the coagulation is slower.

§ 4. On the other hand, although many writers‡ derive coagulation from the influence of air; yet every precaution may be taken to exclude air, and nevertheless the blood, in its normal condition, cannot be kept fluid.

In the years 1853 and 1854, I instituted a series of experiments which enabled me to confirm the statements of those who assert that in general blood needs no air for its coagulation. By means of a glass tube united with a well-washed and dried caoutchouc tube, I took from the jugular vein of a living dog seven cubic inches of blood, and immediately passed it into an inverted graduated tube filled with mercury. This blood coagulated entirely, and without a buffy coat; which proves that it had not been so long fluid as to allow the red corpuscles to subside. This sign must be carefully looked for when the conditions of the experiment forbid

* Archiv für Path. Anatomie, von R. Virchow, u. B. Reinhardt, vol. i. p. 315.

† Op. cit., pp. 69, 70, 71.

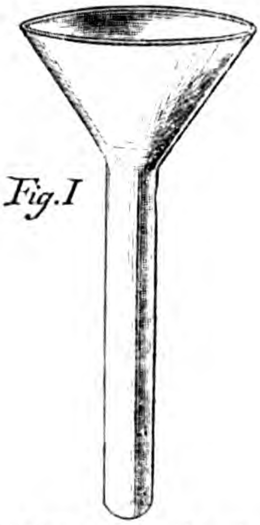
‡ The literature of the long-combated question, Whether or no air is the cause of the coagulation of the blood, is collected in Hamburger:—"Experimentorum circa sanguinus coagulationem, specimen primum." Diss. Inaugur. Berolini, 1839.

direct explanation. It must be remembered that the corpuscles of different specimens of blood subside in different times. This subsidence is greatly retarded by the motion which arises from the blood being warm while the surface is cooling by exposure to the air. This is one reason why the blood of the amphibia is more frequently buffed than that of mammalia and birds. Another reason is to be found in the larger size and smaller number of the corpuscles, as well as in the fact that coagulation does not so soon commence. Thus, the blood of some mammalia (for example, of horses) which coagulates comparatively slowly, has more tendency to produce a buffy coat than that of others. *Cæteris paribus*, the corpuscles subside sooner when fewer in number, and when they have a tendency to adhere to each other. Therefore, when blood whose corpuscles have once subsided, is again agitated, they fall more rapidly, because they are united into little clusters or rouleaux, which are not separated by the agitation.

In this experiment, the pressure upon the blood was 4·33 in. of quicksilver less than that of the atmosphere, but I did not discover the smallest air-bubble. Therefore I cannot agree with Sir Charles Scudamore, who thinks that the loss of carbonic acid is the cause of coagulation. In numerous other experiments in which blood was conducted into mercury without contact with air, it was completely coagulated without any loss of carbonic acid.

The blood of a turtle conveyed in the same manner into mercury, also coagulated without contact with air; but thinking it possible that the free oxygen circulating with the blood has some influence on coagulation, I instituted an experiment in which time was given it to combine with the organic substance before the coagulation could commence. I tied all the arteries coming from the heart of a turtle, and cut them below the ligature. I then tied the vena cardio-pericardiaca, and cut that also. Finally, I tied all the veins, and cut them above the ligature, in order to free the heart, now swelled with blood, from every connexion with the body. The heart, thus treated, was inserted below the glass vessel (fig. 1.), previously filled with mercury, and inverted over a quantity of the same metal. It was still regularly pulsating, and its contained blood became very dark. Twenty-four hours afterwards, finding the heart in a state of rest, I squeezed it by means of a curved crucible-forceps. The blood rose quite fluid in the mercury, but coagulated there without any contact with the atmosphere.

Fig. I

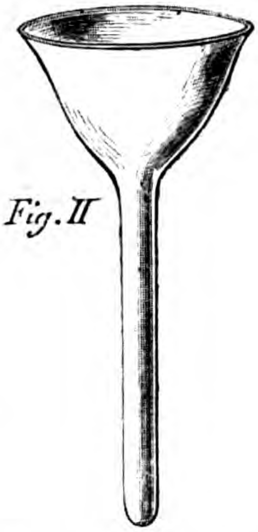


The glass vessel used in the experiments on the hearts of turtles (reduced one-half).

It is known that air adheres to the surface of glass, and exists there in a state of condensation. In order to discover whether this were a source of fallacy, I expelled this air by boiling the quicksilver in the glass vessel, and then repeated the experiment, but the result was the same; and in many trials, I never succeeded in keeping the blood of the turtle fluid in quicksilver.

§ 5. I was more successful in my experiments with the hearts of frogs. On May 27th, 1853, I tied the great arterial trunks of a frog; and when the heart was distended with blood, I also tied the veins, cut out the heart, and placed it under an inverted glass vessel (fig. 2) filled with quicksilver. It ceased to pulsate after one hour, and after five hours I squeezed it with the curved forceps, in order to liberate the blood, which then rose in the tube. Next morning the red corpuscles had subsided, a very small clot having been formed. The fluid residue was then exposed to the air, and coagulated thoroughly; and in half an hour had already thrown out some serum. The temperature was 68° F. I have repeated this experiment very often, and at different seasons of the year, but with different results. Sometimes the blood was not coagulated at all, even after having been for twenty-four hours free in the tube; sometimes it was in part coagulated, and sometimes completely so.

Fig. II

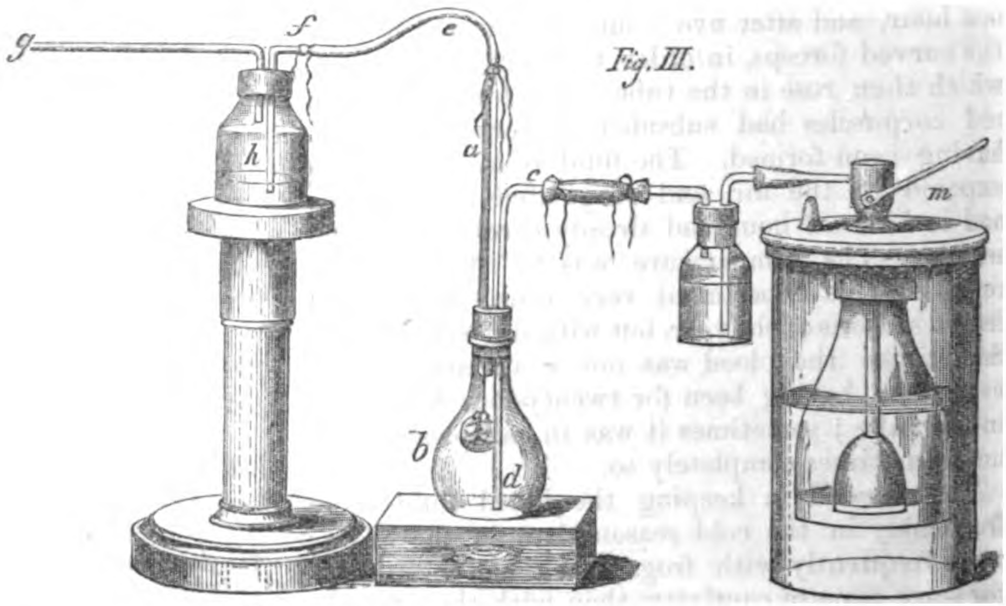


The glass vessel used in the experiments on the hearts of frogs (reduced one-half).

I succeeded in keeping the blood fluid more frequently in the cold season than in the warm; more frequently with frogs which had been kept for some time in captivity, than with those freshly procured; more frequently, if I liberated the blood after the heart had been twelve or twenty-four hours in the quicksilver, than if it had only remained there two or three hours. In some instances the blood did not coagulate, even when freed from the quicksilver, and exposed to the air; but this happened in the spring, when the frogs had been kept in captivity all the winter, and the blood was therefore deteriorated in a high degree, although it was not altogether deficient in coagulability, for that portion of it which flowed when the heart was cut out was found to have clotted.

I made similar experiments in a somewhat different manner, and with varying results. I placed the frogs' hearts, tied as before, in the same glass vessels filled with purified oil: and after six, twelve, or twenty-four hours, I squeezed them, so that the blood trickled down in the oil. In this condition I kept it even more frequently fluid than in the quicksilver; but I attempted in vain, under any circumstances, to retain in the fluid state the blood of recently-caught toads (Bufo cinereus) or of turtles. (Emys Europæa.)

§ 6. Further, I tied the great arteries and the vena cardio-pericardiaca of a turtle, and then the great veins, with a long thread of double silk, inserted the silk through a glass tube (fig. 3, *a.*), the upper opening of which was contracted, and having fixed the end of the silk to the outside of the tube with wax, I then cut the heart out. This glass tube (*a*) was already passed through a cork which fitted the mouth of the bottle (*b*), and the cork was also perforated by another tube (*c, d*), through which pure hydrogen was pressed into the bottle (*b*), in order gradually to expel the atmospheric air through the glass tube (*a*), the caoutchouc tube (*e*), the glass tube (*f*), the bottle (*h*), and the glass tube (*g*). I commenced this experiment in the morning; in the evening, when all pulsation had ceased, I began rapidly to drive in the hydrogen, which was developed in an apparatus similar to Döbereiner's platina fire-machine (*m*). Then, untying the caoutchouc tube from the glass tube (*a*), I rapidly withdrew the



The apparatus described (one-fifth the natural size).

silk and replaced the caoutchouc tube. The heart fell into the bottle, and shed a quantity of dark and dichromatic* blood, which next morning, when I disunited the whole apparatus, I found to be coagulated, whilst the blood which still remained *in* the heart, was thoroughly fluid.

This blood being preserved in a watch-glass, was, after one hour only, covered with a thin film; and even in the afternoon, by cutting through the clot, I obtained some drops of fluid blood, which afterwards coagulated.

I varied the experiment by tying, not the veins, but the arteries, with the long thread of silk. This was done on the 1st of October, 1853, at 11 a.m. On the 2nd, at 8½ a.m., I found the heart without any movement. Having withdrawn the silk, as before, the heart fell, and dark dichromatic blood ran out, which in an atmosphere of hydrogen gas, and without any contact with air, completely coagulated in two hours. I repeated this experiment at different times, but I never succeeded in keeping the blood fluid.

I also repeated those experiments which Professor Brücke describes in the Monthly Reports of the Academy of Vienna, vol. x. p. 1070. He does not there speak at all about coagulation; but asserts that blood becomes dichromatic in hydrogen, nitrogen, and carbonic-acid gases; but not in oxygen, nor in common air. From experiments made on frogs, toads, turtles, and dogs, I am able to assert that dichromatic blood coagulates equally well with that which is not dichromatic.

The conclusions which I draw from all my experiments are as follows:

1. Air usually hastens the coagulation of the blood.

* Confer E. Brücke; über den Dichroismus des Blutfarbestoffs; in Sitzungsberichte der Wiener Akademie, vol. x. p. 1070.

2. Air, when introduced into the heart and vessels of living turtles, does not induce coagulation.

3. The blood of frogs, when deteriorated by the action of the heart, or the other tissues of the animal, and so deprived of its free oxygen, sometimes requires atmospheric air for its coagulation.

4. Normal blood needs not the presence of air for its coagulation.

Therefore, and chiefly from the last conclusion, it follows that air is not that general cause of coagulation for which we are seeking.

§ 7. We have before seen that the blood in the living body is not kept fluid by animal heat, nor merely by its motion; and we are therefore compelled to admit that it is effected by other forces acting in the living body. These forces may be considered as of two distinct kinds:—one arising from the corpuscles of the blood, and the other from the walls of the vessels and the surrounding tissues. The first of these is easily excluded. Thus the lymph, which contains only a small number of cells, is fluid in the living body, and when withdrawn, coagulates like blood; and I have kept fluid during several days the clear plasma of the blood, in which the corpuscles had subsided, as in Sect. 2.

In another experiment made in July, 1854, I tied, in the manner previously described, the heart of a Testudo Græca; and having suspended it in purified oil, I placed it in a cellar, of which the temperature was 50° F. Three days after, I carefully opened the auricles at the highest part, and by means of a glass tube gently removed the clear plasma, and blew it into a watch-glass; and then removed the red fluid below in the same manner, and placed it in another watch-glass. The two portions thus separated coagulated in the same time. On repeating this experiment at different times, I found that the clear plasma usually coagulated more slowly than the part containing the red corpuscles. Therefore we must not look to the corpuscles as the source of the fluidity of the blood.

§ 8. Thus, by excluding the first-mentioned cause, we are led to the idea that the influence which keeps the blood fluid arises from the surrounding tissues—that is to say, from the heart, and the walls of the vessels; and I can adduce strong evidence that this idea is the right one.

It was Sir Astley Cooper who made the first successful experiments in support of it. In the first edition of Turner Thackrah's work (London, 1819), these experiments are related from memory, after a verbal communication from Sir Astley; and in the second edition (London, 1834), after a letter from the same, in which he says:

Exp. 1. Having carefully excluded the atmosphere from the ureter of an ox, I tied one end, and put a cock upon the other. The cock was tied in the jugular vein of a dog, and being then turned, the blood rushed into it. The cock was then shut, and the blood in ten minutes was found coagulated.

Exp. 2. The same experiment was repeated upon the jugular vein of the ox, which was, by the same means as the ureter had been, introduced into the jugular vein of the dog; and the blood coagulated in ten minutes.

Exp. 3. Two ligatures were placed on the jugular vein of a living dog, and there left for three hours. The blood had not coagulated.

Exp. 4. Two ligatures were put on the jugular vein of a living dog, leaving a space between them of three inches. Then the lower part of the vein was cut through, and suffered to hang from the wound for four hours. The upper ligature was then removed, the blood admitted into the vein, and the ligature again tightened. The blood thus admitted into the dead vein was coagulated in a quarter of an hour.

Sir Astley Cooper induced Turner Thackrah to examine this subject, and the result was Thackrah's well-known work, in which he demonstrated by many experiments, that the blood in the excised veins of a recently-killed animal remains fluid at least half an hour, sometimes an hour, or even longer; while the blood of the same animal, received into the vein of another animal of the same species killed the day before or some hours previously, always coagulates in less than fifteen minutes. Therefore he agreed with Sir Astley Cooper, that "the vital or nervous influence is the source of the blood's fluidity; and its loss, the cause of coagulation."

His essay obtained a prize, and never was one more deserved. He found some followers, as Dr. Wright, Mr. Prater, and Mr. Ayres, but he never succeeded in making his views generally acknowledged. The chief reason, I think, was, that to most physiologists the difference in time did not appear sufficiently great to be striking. They objected, that a dead vessel allows diffusion rather than a living one; and that it was perhaps by the attraction of oxygen, or by the loss of carbonic acid, that the blood coagulated sooner in the former than in the latter; or, that possibly transfusion could not be performed with such care as completely to avoid the ingress of air, &c. Although these doubts were without foundation, they failed not of producing their effect on the minds of the medical public. It has also been objected that cold retards coagulation, and that blood may be frozen and become completely fluid again. If the theory of Cooper and Thackrah maintained that the *life of the blood* is the *only* source of its fluidity, then it might be asked, whether that life is able to bear a temperature of less than 32°; but, if the vital power of the *walls of the vessels* hinders the coagulation, by what reason is it *therefore* less conceivable that cold hinders, or at least retards it also? The coagulation of the blood has already been remarked by different authors, as the first step towards decomposition. The reasons are obvious. Coagulation is prevented by the influence of life, and retarded, although not absolutely prevented, by a low temperature. Blood which coagulates slowly, also decomposes slowly. Polli* bled a pneumonic man, thirty-seven years of age; the blood began to coagulate nine days after, and terminated within fifteen days; at the end of a month it began to putrefy. The temperature was between 8° and 11° Celsius (46°—52° F.).

But it cannot be denied that the want of complete success of Thackrah is attributable to his having confounded the action of the great nervous centres with the peculiar action of the walls of the vessels. He proclaimed that in inflammatory diseases the blood coagulates slowly, because vital action is increased; and that it coagulates rapidly when taken from a weakened frame, because the vital powers are reduced. He asserted, that in the case of an animal bled to death, the last blood which flows out

* Di un fatto relativo alla pretesa incoagulabilità del sangue in certe malattia.—Gaz. Med. di Milano, 1844, No. 3. Virchow, Gesammelte Abhandlungen zur Wissenschaftlichen Medicin part i. p. 114.

clots so rapidly, because the vital powers are vanishing. These explanations were, I think, erroneous; the difference not arising from the different state of the nervous system, but from the different quality and composition of the blood. Any one may convince himself that, if the animal, instead of being bled to death, is killed in any other way, the blood taken at the moment of death, or even one, two, three, or six hours after, coagulates not more quickly, but usually more slowly, than blood taken while life remained (§ 3). I cannot, therefore, support Turner Thackrah's views in general; but I can maintain, that the influence of the *living heart and vessels* is the source of the blood's fluidity, and its loss the cause of coagulation.

§ 9. It is an undisputed fact that the blood coagulates in the vessels of dead animals very slowly. In order to discover whether it is kept fluid by the lasting influence of the great nervous centre, I made the following experiment. On the 10th of May, 1854, at nine a.m., a terrier was suffocated by tying a hog's bladder over its mouth.* Forty minutes afterwards the brain was removed, care being taken to lose as little blood as possible, and the spinal cord was destroyed by means of a wire. At twenty minutes past five p.m., the blood in all, even in the largest veins, was quite fluid, and coagulated, when liberated, in the usual way. The temperature of the room during this experiment was between 62° and 65° F.

§ 10. I can adduce many instances in which the blood has kept fluid for a long time, without any connexion with the brain or spinal cord. I may refer to the experiments mentioned in Sect. 2, in which the blood was kept fluid for several days in the extirpated hearts of turtles and frogs preserved in oil. I also placed hearts, similarly treated, in glass vessels inverted over quicksilver, or in glass tubes, afterwards drawn out and hermetically sealed, while a stream of hydrogen was passing through. In all these cases the blood continued fluid. It is not even necessary to exclude atmospheric air. I suspended the heart of a turtle tied as before, and distended with blood, in a large bell-glass filled with atmospheric air, and inverted in water; and at the end of five hours the blood was quite fluid and coagulable. The temperature in this case was very variable, the room being heated for nine hours during the day, and cooling at night. Even the immediate contact of air does not induce coagulation of blood contained in the living heart; as I have shown in Sect. 3.

It may be asked, whether the fluidity of the blood depends upon the life of the heart, or not? Certainly if the heart undergoes decomposition, the contained blood is found to be changed into a viscous, uncoagulable fluid. I found the blood coagulated in the heart of a turtle which had been kept under oil twelve days in a constant temperature of 50°; but I frequently found the blood quite fluid, and in a state of perfect coagulability, in those hearts of turtles, frogs, and toads which were no longer affected by the most powerful electrical irritation. Notwithstanding, this fact cannot prove that a dead heart is capable of preventing the blood from

* It is to be observed, that in all my experiments wherein I inquired into the length of time during which the blood of dogs continues fluid after death, these animals were suffocated in the same manner; and that in most of my experiments the blood coagulated earlier than in this one.

coagulating; for we have seen that blood worn out by the frequent contractions of the heart, coagulates slowly, and may continue fluid a considerable time, even without the contact of the inside of the heart.

§ 11. In order to decide this question, I divided the great arteries of a turtle half-an-inch from the heart, and placed the animal in a cellar for three days. I then tied the arteries, and by means of a glass tube, transferred fresh blood from a living turtle into the heart so treated, then tied the veins, cut the heart out, and laid it in oil. Twenty-four hours afterwards the blood was completely coagulated, and had no buffy coat; which proves that it had coagulated at a very early period, for the blood of turtles usually becomes sizy during the first twenty minutes. I repeated this experiment several times, and on opening the heart one hour after, I always found the blood to be coagulated.

Thus the blood of turtles, as long as it is in a fresh and living heart, is always preserved fluid; but in a dead one it coagulates, as it would do in a glass or china vessel. But it must be carefully remarked whether the heart has really ceased to live; for often when it has remained quiet for a long time, if fresh blood is blown into it, it recommences its contractions. In one case I found this to happen eight days after death.

In another of these cases also I observed a very curious circumstance. I had bled a turtle to death, and kept it for three days in a room of from 65° to 71° F. At the end of that time, finding the heart quite empty and at rest, I tied the arteries, blew in the blood of a living turtle, and then tied the veins. The auricles of the heart began to act, and half an hour afterwards the blood had but partially clotted; but the fluid remainder when removed and placed in a watch-glass, coagulated almost suddenly, really in a few seconds, so rapidly as I had before seen only the last blood of warm-blooded animals—birds, rabbits, and dogs—when bled to death.

To this conclusion, that the power of keeping the blood fluid is dependent on the life of the heart, it may be objected, that the fresh blood which was blown into the dead heart had undergone the contact of air, and was thereby coagulated; but I have already shown, that in living hearts the entrance of air does not produce coagulation; and in order to avoid all sources of fallacy, I made the following experiments.

Having laid an open ligature round the great arteries of a turtle, I then cut the arteries below the ligature, and let the blood run into a cup. I then tightened the ligature, and by means of a glass tube blew some of the blood, through an opening in the left subclavian vein, into the heart again—tied it at the entrance of the great veins, then cut it out, and laid it in oil. It was kept in a temperature of 50°; and although some air-bubbles happened to enter with the blood, it was found quite fluid at the end of three days. The same result followed if I blew the blood of one Emys Europæa into the extirpated heart of another animal of the same species.

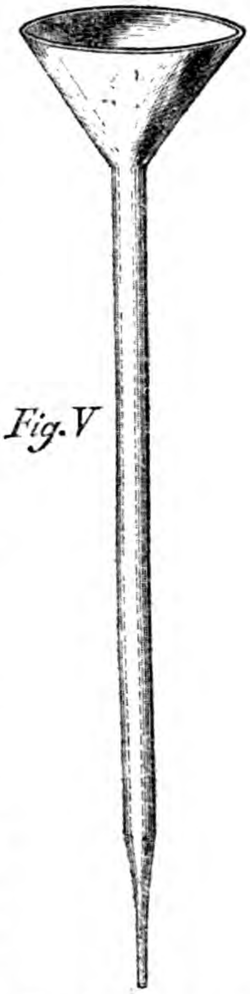
I then cut out the heart of a tortoise (Testudo Græca), and the blood being removed, I tied the arteries. I then cut the great arteries of an Emys Europæa, received the blood in a cup, and by means of a glass tube blew it into the heart of the Testudo Græca, and then tied the veins by tightening a ligature previously placed around them. This heart was kept in oil at a temperature of 50°, and fifty hours after I found the blood quite fluid; but after being liberated, it coagulated thoroughly and firmly in forty-five minutes.

I exposed the blood of an Emys Europœa to the air for *fifteen minutes*, and kept it fluid during that time by means of cold, placing the open glass vessel which contained it in a mixture of water and snow. I then blew it back again into the heart, having previously tied the arteries. This operation was performed by means of the glass tube (Fig. IV.), into which it was poured through the funnel (Fig. V.). The veins being not tied, I cut out the heart, and suspended it, by means of wax and a thread, in a large receiver filled with atmospheric air, and inverted in water. Five and a half hours after I liberated the blood; it was quite fluid, and began to film over in ten minutes, and coagulated slowly, but firmly.

Fig IV

Glass tube for introducing blood into the living heart of a turtle (reduced one-half).

Fig. V



Glass funnel for pouring the blood into the tube, fig. 4, (reduced one-half).

§ 12. My next experiments were made with a view to determining whether the walls of the arteries and veins of the turtle possess the same power as the heart. For this purpose I tied the great arteries of a turtle at the distance of half an inch from the heart; and when they were distended with blood, I placed a second ligature on the bulbus arteriosus itself. These arteries being then cut out, were kept in oil in a room heated for nine hours daily from 59° to 63° F. Three days afterwards the blood was found quite fluid, and coagulated when taken out of the vessels. I placed similar arteries in the same room, in an atmosphere of hydrogen gas, and after three days the blood was quite fluid, and in a state of coagulability.

I hung the arteries and the left subclavian vein of an Emys Europæa, each distended with blood, under a receiver filled with atmospheric air, and inverted over water; temperature 59°. Twenty-four hours after, the blood was fluid, and coagulated on being liberated.

I also kept the blood of carps (Cyprinus Carpio) fluid, placing in oil the bulbus arteriosus, full of blood, and secured by a ligature; but it always coagulated more quickly than that of turtles. The longest time I succeeded in keeping it fluid at a temperature of 50°, was twenty-five hours; oftentimes it was completely coagulated in twenty-four.

§ 13. As might be anticipated, when the contact of the blood with the walls of the vessels is cut off by a foreign body, coagulation readily occurs. In order to demonstrate this, I placed round the great arteries of a turtle the three ligatures (*a*, *b*, *c*, Fig. 6).* I first tightened the ligature *b*, but in such a manner that it could easily be loosened; and then opened the pulmonary artery at *d*, and having inserted the little glass tube *e*, loosened the ligature *b*. After having allowed some blood to flow from the opening *d*, I tightened the ligature *c*, and finally *a* also. Afterwards the veins of the heart were tied, and the whole was cut out and laid in oil. Twenty-four hours afterwards, I found the blood in the glass tube firmly coagulated; but in the heart and in the other arterial trunks it was still fluid.

Fig. VI.

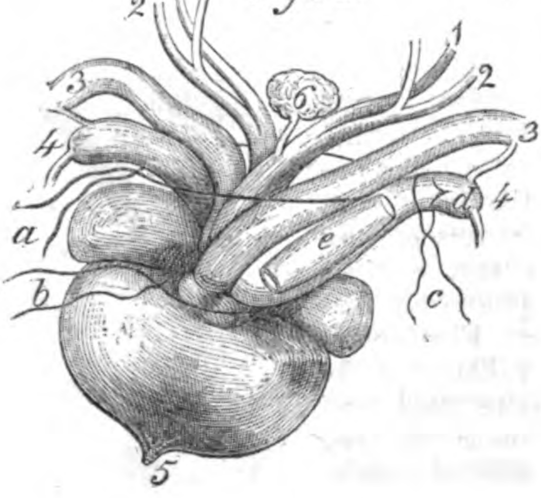
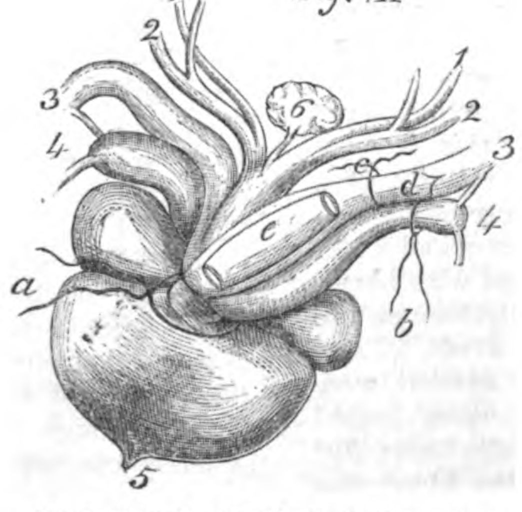


Fig. VII



Hearts of turtles, with the ligatures and glass tubes (natural size).

Again, I placed three open ligatures (*a*, *b*, *c*, Fig. VII.) round the bulbus arteriosus, the left pulmonary artery, and the arteria aorta sinistra, respectively. Having then tightened first *b*, and afterwards *a*, in such a manner that the ligature could be easily loosened again, I opened the aorta sinistra at *d*, introduced the glass tube *e*, unfastened the ligature *a*; and while the blood was pouring out at *d*, I tightened the ligature *c*. Fifteen minutes after, on fastening *a*, and opening the vessels, the blood in the glass tube was firmly coagulated, while that in the pulmonary artery was still fluid.

I think I have now plainly shown that the blood is kept fluid in the bodies of cold-blooded animals by the action of the walls of the heart and vessels; and that it coagulates out of the body, because it is then withdrawn from that influence. The difference of time in the results is here so striking, and the experiments are so much varied, that the objections

* Here, and in Fig. VII., the carotid is marked 1, the subclavian 2, the aorta 3, and the pulmonary artery 4; the vena cardio-pericardiaca 5, and the thyroid gland, with its artery, 6.

which were made to Thackrah's deductions cannot justly be applied to them.

Is it probable that the cause of the fluidity of the blood in mammalia and birds differs from that which obtains in amphibia?

§ 14. We have seen that the blood in the heart and vessels of a dog continues fluid for five, ten, and even thirteen hours after death; but that if removed, it coagulates generally in less than a quarter of an hour, and often in a few minutes. This difference cannot be ascribed to cold, for cold notoriously delays coagulation; nor to the influence of air, as was well known to Thackrah. He injected air into the jugular vein of a bitch. The blood, after death, flowed from the jugular vein, but coagulated on its effusion. Fifteen minutes afterwards, the blood in the vessels, though fully mixed with the injected air, remained fluid.

The experiment described in Sect. 3 may be also referred to, where blood mixed with air in the jugular vein of a dog, was but partially coagulated 4h. 30m. after; and on repeating the experiments of Professor Brücke,* I saw the blood of dogs coagulate in a few minutes in carbonic acid, nitrogen, and hydrogen gases, just the same as in oxygen gas, or in atmospheric air, although it entered immediately from the vessel into the glass tube filled with those gases.

Comparing these facts with the results of the fundamental experiments of Sir Astley Cooper and Turner Thackrah, it cannot be denied that the vessels of mammalia also possess the power of keeping the blood fluid; but it remains to be pointed out why this power in mammalia continues only a few hours after death, whereas in amphibia it lasts much longer. It is well known that the tissues of warm-blooded animals lose their irritability much sooner after death than those of amphibia; and I thought that possibly this might be the only reason, because I had found that in the class of birds which lose their irritability very early, the blood coagulates so quickly, that in the case of a cock which had been suffocated, the blood in the very heart itself, which had neither been removed from the body nor even laid bare, was coagulated one hour and a-half after death. I therefore attempted to keep the blood of mammalia fluid in the heart of a turtle, but in vain. For this purpose I opened the aorta sinistra of an Emys Europæa, and when it was as much as possible emptied of blood, I tied the wounded vessel, and by means of a glass tube, transfused into the left subclavian vein the blood of a rabbit. It passed through the arteries, but next morning it was found to have completely coagulated in the great veins and in the right auricle, notwithstanding that the irritability of the heart was not yet abolished.

Further, I received the blood of a horse in a glass cylinder, and placed it in a jar containing chloride of calcium and ice. Although I had to drive with it nearly a mile to my residence, the blood was quite fluid when it arrived there. I then filled with it four living hearts of turtles, two of which were kept suspended over water in an inverted glass jar, and in a temperature of 68°, and of the remaining two, one was kept in oil at 68°, and the other in oil at 50° F. Four hours afterwards, the three hearts kept at a temperature of 68° were opened, and the blood in them was found to be firmly coagulated. The heart kept in a tempera-

* Op. cit.

ture of 50° was opened twenty-four hours after, and the blood had also coagulated in it.

Thinking it possible that the blood had been too long withdrawn from the vessels, I determined to avoid this source of fallacy, and for that purpose kept a horse in a stall near my laboratory, and with his blood made four new experiments of the same nature, but so arranged that the blood, received in refrigerated glass cylinders, was, as soon as possible, injected into the hearts of the turtles. The first two of these experiments were made at a temperature of 71° or 72° F., and the hearts were opened seven hours afterwards. The remaining two were made at 70° or 71°, and the hearts opened six hours after; and in every case the blood was coagulated.

If, in these experiments, the blood of horses had remained fluid as long as the blood of turtles had done in the experiments mentioned in Sect. 11, they would permit the conclusion, that the blood of mammalia coagulates earlier after death than that of amphibia, only by reason of its higher temperature; and because the vitality of the heart and walls of the vessels disappears earlier after death in the former than in the latter; but as it is, they lead to no certain conclusion.

§ 15. A temperature approaching to that of the warm-blooded animals diminishes in a striking manner the time during which the blood of amphibia can be kept fluid. I have made some experiments with the hearts of turtles distended with blood, and placed in a hatching-oven. In the first case, the heart was kept in the oven during eight hours and three-quarters, the temperature meanwhile slowly decreasing from 95° to 89° F. At the end of that time the blood was fluid and coagulable. The second was kept for twenty-three hours in the oven, the temperature decreasing from 95° to 84°. In this case the blood was coagulated, but not firmly; nevertheless, the fluid which escaped when the clot was divided did not coagulate when it was exposed to the air. In the third experiment, the heart remained twenty-three hours exposed to a temperature decreasing from 95° to 89°. The result corresponded with that of the last. In the fourth case, the heart was kept in the oven for 12h. 40m. at a temperature decreasing from 97° to 91½°. At the conclusion of the experiment, some small flaky clots showed that coagulation had commenced, and the remainder of the blood clotted firmly when exposed to air at the ordinary temperature.

On a consideration of these facts, it must be acknowledged that, as far as regards the question before us, the difference between warm and cold-blooded animals is one of degree only, and not of kind. The blood of a dog just killed, which is reduced slowly from a temperature of 102°, remaining fluid, on the average, seven hours in the heart and vessels; and the blood of the turtle continuing fluid in the heart for about twelve hours, if kept in a hatching-oven, whose temperature gradually diminishes from 97 to 91½° F. Both are equally kept fluid by the action of the heart and vessels.

But it has not yet been demonstrated whether this difference arises solely from the unequal temperature, and unequal duration of vitality; or whether the blood of mammalia and birds has, by its own nature and composition, a stronger tendency to coagulate, and therefore requires a more powerful action of the heart and of the walls of the vessels in order to keep it fluid.

§ 16. This question was settled by other experiments. On Nov. 11th, I tied the heart of a hedgehog (Erinaceus Europæus), and the right ventricle being swelled with blood I cut it out, and suspended it in a large bell-glass filled with atmospheric air, and inverted in water. The blood of the hedgehog lost during the operation coagulated in less than five minutes; but this animal possessing great tenacity of life, and the irritability of its muscles continuing longer than in other mammalia, I might hope to keep it fluid for a considerable time in the separated but living heart. This expectation was verified; in three hours and a half I saw the last faint contraction, and then, having waited another hour, I opened the heart. Coagulation had already begun;—in the pulmonary artery was a soft clot, commencing at the point where it was tied, and extending down into the ventricle; but beyond that, about two-thirds of the whole bulk of the blood was completely fluid, and coagulated in a watch-glass in ten minutes. By means of Neef's magneto-electromotor I could yet excite contraction in the right auricle, and a faint and scarcely perceptible movement even in the right ventricle.

Thus have I preserved fluid the blood of a warm-blooded animal in the extirpated heart for four hours and a-half in a temperature decreasing from 68° to 64°; but coagulation commenced as life began to vanish; whereas, the blood of frogs, toads, and turtles, preserved in their own hearts, always remains fluid longer than contractions can be excited in them.

The blood of a kitten showed less tendency to coagulate. I performed a similar experiment with a kitten's heart, and opened it after three hours and a-half. In the pulmonary artery only was there a clot; the blood in the right auricle and right ventricle was fluid, and the corpuscles had so subsided, that they were all collected in the ventricle, and the auricle was filled with clear plasma. The blood coagulated in a watch-glass in ten minutes; but the heart could no longer be excited to action by the magneto-electromotor. The temperature was 66° F. In another experiment of the same kind, and conducted at the same temperature, the blood of a kitten was preserved fluid for four hours; but the blood of a puppy treated in the same manner, was found coagulated after four hours and a-half, although the heart had not yet entirely lost its irritability.

Therefore the different permanence of vitality in the tissues and organs of cold and warm-blooded animals, although the chief reason of the difference above mentioned, is not the only one; the blood of warm-blooded animals having generally, though not always, more tendency to coagulate, and therefore requiring a more powerful energy of life to counteract that tendency.

Here also we meet with an explanation of the results obtained by Sir Charles Scudamore, who, in his fiftieth and fifty-first experiments, found the blood of a sheep to coagulate very rapidly (in four and five and a-half minutes), if received in the fresh jugular vein of another sheep. But it cannot be doubted that the vein, being cut out or laid bare, had already lost too much of its vitality to keep the blood fluid. It must also be recollected that the blood of sheep generally coagulates very quickly, much more quickly, indeed, than that of horses, dogs, or oxen. Sir Charles himself does not deny the influence of life. He found blood intercepted by means of ligatures in the jugular vein of a horse, remain fluid an hour, and an hour and three-quarters, and yet coagulating, if let out, in five minutes.

§ 17. The blood is kept fluid by the walls of the vessels—the blood is also kept fluid by the heart—and lymph is fluid in the lymphatic vessels; it is therefore possible that blood can be kept fluid in the lymphatics. Warm-blooded animals were not fitted for experiment on that point, and I therefore again had recourse to turtles. I inserted a Cooper's forfex (having previously passed round its path an open ligature) between the lung and the stomach into the cisterna lymphatica, and divided the aorta sinistra near its anastomosis with the aorta dextra. I then withdrew the forfex, and closed the ligature. The great cisterna lymphatica being thus filled with blood, I tied the great veins and arteries, cut out the heart, and placed the animal in a temperature of 69° F. Seven hours and a half afterwards the blood in the cisterna lymphatica was quite fluid, and coagulated quickly and firmly when let out. This experiment, repeated at different times, and in different temperatures, gave always the same result. In order to determine whether the blood would likewise continue fluid in serous cavities, I passed a cutting cataract needle (also with an open ligature round its path), downwards from the shoulder through the cellular tissue in an oblique direction, into the pericardium of a turtle, and wounded the heart so that the pericardial sac was filled with blood; I then withdrew the needle, and tightened the ligature. One hour afterwards, I always found the blood in the pericardium to be firmly coagulated.

§ 18. It is frequently observed that the liquor pericardii contains liquid fibrin which coagulates when exposed to air. Therefore, although the pericardium does not keep normal blood in a fluid state, yet has it kept fluid fibrin when dissolved in liquids of a different composition. It cannot, however, be said that it does so by means of a peculiar action.

We have seen that in some cases (Section 5) abnormal blood did not coagulate until it had been some time exposed to the air. Therefore it is possible that the fibrin in the liquor pericardii may have continued fluid only because it was not exposed to the air. Virchow* has collected a number of cases in which fluid exudations were removed from the body long after death, and coagulated when exposed to the air; whilst, in a still greater number of cases, fibrinous exudations into the cavity of the pleura, or the pericardial sac, coagulate even in the living body. He never found coagula in the lymphatics of dead bodies, if they were in a normal condition; and he therefore thinks that the normal lymph in man never coagulates until it has been exposed to the air.

John Hunter relates a very remarkable instance in which blood, in a case of hydrocele, was kept fluid for sixty days in the tunica vaginalis; but there was in this case not blood alone, but blood mixed with the hydropic fluid.

In the tubuli uriniferi of the kidney, effused fibrin generally soon coagulates; but occasionally it does not do so, but is excreted dissolved in the urine, and afterwards coagulates if exposed to the air. I have myself seen a very interesting case of this kind. But there is no doubt

* Gesammelte Abhanlungen zur Wissenschaftlichen Medicln, p. 164.

that the composition of the fluid has more influence on the result, than the nature and action of the walls of the bladder, the ureter, and the tubuli uriniferi.

In the intestine of a recently-killed animal, blood has also been kept for some time fluid; but this experiment has not always produced the same result,* and leads to no certain conclusion, because the alkaline mucus which lines the internal surface of the intestine, may perhaps exert some influence.

§ 19. As it may happen that blood may be so altered in its chemical constitution as to continue fluid under circumstances in which normal blood would coagulate (Section 5); so it is also possible that blood may undergo an opposite change, and coagulate where normal blood would have remained fluid. No one, however, has given any explanation of the nature of that change, and I myself met with no opportunity of inquiring into it.

The hypothesis that blood rich in fibrin has a tendency to coagulate in the very vessels, is an ill founded one. No one has demonstrated a case in which the blood has coagulated during life by reason of the superabundance of fibrin. The white or yellow clots found in the hearts of those who have died of pleuritis or pneumonia, are notoriously formed long after death; and are indeed nothing but the buff of the blood which has coagulated in the heart, in the dead body;† they therefore rather prove that the blood coagulated in the heart unusually late after death, than that it did so during life. It is also matter of frequent observation, that blood drawn in cases of pneumonia or pleurisy, generally coagulates slowly, but is richer in fibrin than healthy blood. On the other hand, if an animal be bled to death, the last blood which trickles from it coagulates almost suddenly, and even that portion which yet remains in the vessels, clots with great rapidity. Such blood contains very little fibrin. I bled a dog to death, catching the blood in five different cups successively, and then carefully measured the quantity of fibrin which each cup contained. The result was as follows:—

	Blood, in grains.	Fibrin, in grains.	Fibrin, per cent.
I.	1588·5	3·55	0·223
II.	2146·7	4·28	0·200
III.	2381·9	4·22	0·177
IV.	2935·8	4·74	0·162
V.	1864·7	1·25	0·068

In a second experiment, also made upon a dog, but in which the blood was received in four cups, the result was as follows:—

	Blood, in grains.	Fibrin, in grains.	Fibrin, per cent.
I.	1670·7	4·86	0·291
II.	2086·5	5·64	0·270
III.	2491·6	6·07	0·244
IV.	930·5	1·71	0·184

* Sir Charles Scudamore relates, in his forty-eighth experiment, that the blood of man coagulated in the intestine of a recently-killed rabbit more quickly than it did in the intestine of a rabbit which had been killed the day before; and this result he rightly derives from the higher temperature of the freshly killed animal.

† Hewson. Op. cit. p. 44.

The blood of horses, which is by far richer in fibrin than that of dogs, coagulates much more slowly. All these facts are so striking, that some physiologists have thought that there is a direct relation between the quantity of fibrin in the blood, and the time which it requires to coagulate; but Nasse* found that it was not so. There is no doubt that the time required for coagulation is dependent upon so many different circumstances, that it is very difficult to determine the exact amount of influence exercised by any one of them.

Again, it is not proved that a constitution which causes the blood to coagulate rapidly when withdrawn from the body, produces likewise a tendency of the same blood to coagulate in the living vessels. Let us therefore only consider those cases in which blood coagulates in consequence of anomalous external conditions into which the very blood-vessels themselves of the living body may happen to fall. If we tie an artery, a clot forms from the ligature as high as the next branch springing from the same vessel. That blood, therefore, which was brought to a state of rest, has coagulated. Was rest the sole cause of this coagulation? We can imagine that although in amphibia, blood can be kept fluid for a long time in a state of rest, yet that in the blood of mammalia it may be necessary that fresh particles shall be constantly brought into contact with the walls of the living vessels, in order to keep it fluid. But we must first carefully inquire whether there may not be some other circumstances which exercise an influence in the case of man. Undoubtedly, the walls of the vessels are injured by the act of tying the ligature, and the internal coat is usually burst at the spot where the ligature is applied; and it is generally admitted that at *that* spot the coagulation begins, and increases upwards. M. Notta observed a very interesting case of a man, one of whose crural arteries was tied, and who died twenty-nine hours after the operation. On examination of this case, a branch was found springing from the artery above the ligature, so small as scarcely to admit an Anel's probe. This branch had not entirely prevented coagulation, being so small; nevertheless, the blood had not been quite at rest, a small quantity still continuing to run through the little branch. A small clot was found arising from the spot where the ligature was applied, and having a long filamentous appendix extending upwards to the next collateral branch, which sprang six centimètres above the first. Such cases show that the injury done to the walls of the vessels by a ligature, is not without its effect on coagulation. Every surgeon is aware that, in the ordinary cases, in which the clot fills the vessel as high as the next branch above the ligature, the walls of the vessel enclosing the clot undergo a change; the artery in this place becoming transformed into a solid string. Can the commencement of that change have any influence in bringing the blood to a state of coagulation? The formation of the clot is usually completed in thirty-six or forty-eight hours;† and in one case observed by M. Notta, in eighteen hours. Certainly in that period the change in the walls of the

* Rudolph Wagner's Handwörterbuch der Physiologie, tom. i. p. 105.

† It sometimes requires a longer time in tied *veins*. Having made a very small wound, I carefully tied the right jugular vein of four rabbits. On examining them forty-eight hours after, a clot was only found in two of them.

vessels cannot be far advanced; but yet it cannot be denied that their normal conditions are altered from the very moment that the blood within came to a state of rest. Therefore it is not impossible that the coagulation of the blood and the alteration in the nutrition of the walls of the vessels being contemporaneous processes, may promote each other by mutual action.

In general, if an arterial trunk be anywhere compressed by a tumour, or obstructed by a coagulum conveyed into it from the veins,* or by any foreign body whatsoever, the whole bulk of the blood reduced to rest, coagulates; but we do not exactly know how much time it requires; and we cannot with certainty decide whether rest alone, that is, the want of renewed contact with the walls of the vessels, is the cause; or whether the walls of the vessels are altered by the continued contact of the same portion of blood, and therefore allow it to coagulate.

In sound vessels, circulation may be greatly impeded, although not abolished, and notwithstanding, the blood remains fluid. For instance, long since, John Hunter observed that even in the most intense inflammation, blood does not coagulate, unless it terminates in gangrene.

On the other hand, blood will coagulate where the circulation is but slightly impeded, or somewhat slower than in the normal state; but in these cases the walls of the vessels have degenerated, or locally mortified. Then one thin layer of fibrin is deposited after another, until the vessel is obstructed.

But even when there is no impediment at all to the circulation, local disease of the walls of the vessels may produce depositions of fibrin; that part of the blood which touches the wall of the vessel being always more slowly propelled than the rest. It is extremely interesting to read Professor Virchow's descriptions of the various fibrinous depositions in the vessels, not only because he has collected a valuable and extensive series of cases, but also because these cases exhibit in a striking manner the influence which the walls of the vessels exert on the fluidity of the blood.

Any one who has read Professor Virchow's different papers on fibrin,† is aware how far he is from agreeing with the views of Sir Astley Cooper and Turner Thackrah, which I have supported; yet notwithstanding, he is compelled, by the irresistible power of facts, to appeal to the influence of the walls of the heart and vessels. In his paper on Arteritis,‡ he says:— "If the smooth surface of a globule of mercury is sufficient to make the blood coagulate round it; a spot in the inner coat of the vessel, when it is altered in its molecular condition, although still remaining smooth, must also be able to do so."

§ 20. It will necessarily be inquired, what idea I have formed of the nature of that peculiar action of the heart and vessels to which I ascribe the fluidity of the blood? Blood continues fluid in the sound and living vessels, but it coagulates in dead ones, and in jars or cups of every known substance,—glass, china, platina, silver, copper, &c., even without the

* Confer:—Virchow, Thrombose und Embolie, in his Gesammelte Abhandlungen zur Wissenschaftlichen Medicin. Erste Hälfte, 219.

† Archiv für Pathologische Anatomie, herausgegeben von R. Virchow und B. Reinhardt, vol. i. p. 321.

‡ Gesammelte Abhandlungen. Erste Hälfte, pp. 57 *et seq.*

contact of air. The inference drawn from these facts, is, that the vessels of the living body preserve the blood in the fluid state by means of a peculiar action which vanishes with their life. Would it not perhaps be better to say, that the contact of all bodies induces coagulation of the blood, except only the inside of the living vessels, which is so indifferent to the blood as not to do so?

This is a question which I have not been able to discuss at an earlier stage of the inquiry, because it necessitates a knowledge of many facts propounded in the foregoing pages. Undoubtedly the contact of foreign bodies promotes coagulation. In every cup or jar, coagulation begins from the surface, from the sides, and from the bottom, and proceeds towards the centre; and foreign bodies produce clots, even in the living vessels. But in blood withdrawn from the living vessels, coagulation proceeds from the foreign body throughout the whole mass; whereas a foreign body introduced into the living vessels, produces only a local clot, the remainder of the blood continuing fluid. Nobody doubts that the contact of air promotes coagulation; and notwithstanding, we have seen considerable quantities of air in the living heart and vessels, without any trace of coagulation. The blood of mammalia coagulates even in the living vessels, if brought to a state of rest; but motion *per se* does not preserve the blood fluid, nor does rest make it coagulate. Motion imparted to blood withdrawn from the vessels, hastens coagulation; and the blood of amphibia in the extirpated heart, and in a state of rest, remains fluid as long as that heart shows the slightest trace of irritability, and even longer. Yet we have seen that the blood of a rabbit requires sometimes more than forty-eight hours of rest to coagulate in a living vessel. If motion keeps the blood fluid in the living vessels only, it must be by means of the constantly-renewed contact with the walls of those vessels; and must therefore arise from some peculiar virtue in those walls. If the blood, being at rest in a living vessel, coagulates, it must be either because the blood requires a constantly-renewed contact with the walls of the vessels, or because the vessels require a constantly-renewed contact with the blood, and lose their normal quality if they are a long time in contact with the same portion of blood.

From these facts, I think we are compelled to assume that the walls of the vessels are not indifferent to the blood, but that they counteract its tendency to coagulate by a peculiar virtue inherent in them. In what this virtue consists, I am unable to say; and I wished not to waste time in planless experiments directed to that end. I thought it necessary that we should first understand the process which is impeded, and afterwards inquire into the nature of the power which impedes it. Therefore the next question which I proposed to myself was,—what changes does the blood undergo during its coagulation? Long did I labour in vain, being misled by generally-received, but erroneous opinions and prejudices; and I learned by experience how much truth there is in the remarks contained in the preface to the work of Turner Thackrah, who there says:—"The erroneous notions and unfounded theories which have been vainly adduced to remove the veil of Nature, have greatly obstructed the path of inquiry, and added darkness to obscurity." But latterly I believe I adopted a better mode of procedure, and I will briefly explain the results I obtained.

§ 21. If a solid body is produced from a liquid, then one of two things must happen : either it takes place in consequence of a change in the atomic constitution of the liquid, or it takes place without such a change. In the latter case, either the liquid itself undergoes a change in its cohesion—as, for example, when water freezes into ice ; or a substance dissolved in a fluid becomes again solid, either by a change of temperature, or by a diminution of the dissolving fluid. If blood coagulates, it is a case of a solid body arising from a liquid, at the expense of substances previously dissolved ; but it happens neither from a change of temperature, nor from a diminution of the dissolving fluid. But we know that a fluid can be so overloaded with a dissolved substance, that the molecules, even without any change of temperature or diminution of the dissolving fluid, suddenly rush from unstable into stable equilibrium ; as happens, for example, if, into a super-saturated solution of sulphate of soda, is thrown a crystal of the same salt. It might be imagined that the blood could be such a super-saturated solution, and therefore coagulate when removed from its ordinary conditions ; but there are various reasons which militate against this idea. We will here cite two of these, as sufficient to prove that it is inadmissible. First, if the blood has once thoroughly coagulated, and the clot contracted, no more fibrin can be obtained, either by increasing or diminishing the temperature, or by evaporation. Secondly, blood which contains a greater than ordinary amount of fibrin, does not therefore coagulate more easily ; even the blood of a pleuritic patient forms its dense and firm clot slowly ; and the last blood which flows from an animal bled to death, coagulates very quickly, although it produces very little fibrin. (Compare Section 19.) Therefore it must be admitted that blood coagulates in consequence of a change in its atomic constitution ; but since we know that it does so without either losing or acquiring anything, it must therefore occur by a mere change in the arrangement of its atoms.*

§ 22. We may be sure that the material for the formation of the solid fibrin has the general qualities of all albuminized substances ; but we can say no more,—we cannot even assert that it has more resemblance to coagulated fibrin than it has to albumen or to casein.

But although the coagulation of blood arises from the deposition of an albuminoid substance (or protein compound), this is not the only deposit formed during coagulation. I digested well-washed fibrin, procured by whipping the blood of oxen, in cold water containing $1\frac{1}{2}$ part of hydrochloric acid in 1000. Having evaporated the fluid in lukewarm water to the sixth part of its bulk, I added nitric acid, which precipitated a whitish substance ; this precipitate being washed and filtered, evaporated with nitric acid, and mixed with ammonia, gave the yellow colour of xanthoproteate of ammonia.

* It must be observed that blood, during its coagulation, does not alter its reaction with litmus. I have made some experiments on the reaction of the plasma of the blood of horses and turtles. I extracted the common litmus with hot water, and precipitated the concentrated and filtered fluid with alcohol—then filtered it again, washed it with alcohol, and finally dried it. The blue litmus thus obtained was freshly dissolved for every experiment, in order to prepare a blue and a violet tincture, which were mixed with the plasma. The plasma of horses was always alkaline : that of turtles, either slightly alkaline or precisely neutral; in one single case it changed the blue tincture to violet. In this case the blood had been a long time in a tied heart, but coagulated as usual when liberated. The lymph of all turtles had a decided alkaline reaction.

After having precipitated with nitric acid, I filtered and neutralized the solution with ammonia, which slowly produced a deposit. In this deposit I discovered by means of the microscope crystals of the double phosphate of ammonia and magnesia, and a greater quantity of amorphous granules. To show that this granular matter was phosphate of lime, I filtered the precipitate, and dissolved it in water containing some acetic acid; and then added some drops of a concentrated solution of oxalic acid. I thus obtained a crystalline precipitate of oxalate of lime. The fluid separated from the phosphatic deposit by means of filtration, when mixed with phosphate of soda or phosphate of ammonia, gave a still larger precipitate of the double phosphate of ammonia and magnesia, and phosphate of lime. Therefore the original fluid had contained magnesia, lime, hydrochloric and phosphoric acids; but the latter was in far too small proportion to form phosphate of lime and the double phosphate of ammonia and magnesia, with the whole quantity of magnesia and of lime that was present. No sulphuric acid was detected.

I made the same experiment with the fibrin of human blood taken from the hearts of dead bodies, carefully washed with water, and digested with alcohol and ether; and the result was the same. I then placed the fibrin of the blood of oxen in acetic acid which contained thirty-five per cent. of pure acid, and laid it aside for some weeks in a room whose temperature varied between 32° and 41° F. The liquid was then concentrated by evaporation, and treated as before; and the effect was the same, only the precipitate, which fell down on the addition of nitric acid, was greater in quantity. In the former experiments I found that if I exactly neutralized the hydrochloric solution with ammonia, the albuminous matter fell down with the phosphates, and nitric acid gave no further precipitate in the filtered fluid; but if I exactly neutralized the acetic solution in this experiment, only a part of the albuminous substance fell down, and the filtered solution still gave with nitric acid a white precipitate, which, when evaporated with the acid and treated with ammonia, gave the yellow colour of xanthoproteate of ammonia.

In order to get rid of the albuminous matter without the intervention of nitric acid, I put the fibrin which I used in the last experiment, still swelled by the acetic acid, into spirits of wine of 0·280 sp. gr., leaving it therein for some days, and frequently agitating it. The spirit was then poured off, and evaporated; the acid residue was dissolved in water, filtered, and neutralized with ammonia, gave a deposit of phosphate of lime, and the double phosphate of ammonia and magnesia. The filtered fluid, when treated with phosphoric acid and ammonia, threw down still more of these insoluble phosphates.

These experiments show that the fibrin of the blood contains, as is usually stated, phosphate of lime, and that the phosphoric acid found in the ash, is not, or at least not entirely, the product of the combustion of the phosphorus ascribed to fibrin. This phosphate of lime which exists in fibrin is little soluble in water, and probably the subphosphate ($3\,CaO, PO_5$). I say probably, because the inquiries of Dr. Heintz,* of Halle, have placed it beyond doubt that the phosphate contained in bones is $3\,CaO, PO_5$; and also, because with the microscope I never found in the fibrin, crystals of $(2\,CaO, HO)\,PO_5 + 4aq$. It is not neces-

* Poggendorff's Annalen der Physik und Chemie. Bd. 77, p. 267.

sary that I should bring this question to decision, because for my inferences I only need the fact that the phosphate of lime is one but slightly soluble. It is said that during life it has been dissolved by means of the albuminous substances; but how has it been dissolved? It is by no means proved that the fluid blood contains $3\,CaO, PO_5$, nor indeed $(2\,CaO, HO)\,PO_5$; but it is obvious that the substances could be dissolved if the phosphoric acid had for its base potash or soda, and the lime another acid, for instance, hydrochloric; or if another acid would combine with a part of the lime, the remainder being united to phosphoric acid, in the combination $(CaO, 2\,HO)\,PO_5$. All that is said with respect to the lime may be also advanced concerning the magnesia.

We have further seen that much more lime and magnesia was extracted from the fibrin, than could have been in combination with the phosphoric acid which was extracted at the same time. No evidence can be brought as to the nature of the combinations in which that lime and magnesia existed, but it is clear that these combinations were little soluble in water, and easily soluble in acids. It is not to be supposed, however, that they were so in the living body, but probably their combinations were more easily soluble in water.

On the other hand, although the pure albumen of Dr. Wurz is soluble in water, yet there are certain albuminates which contain albumen in such a modified state, that it is precipitated if the albuminate is decomposed by an acid. It may be thought possible that the blood contains these albuminates, which, during coagulation, are decomposed by the acids which dissolve magnesia and lime; and the result on the one hand should be, insoluble compounds of calcium and magnesium, and on the other, an insoluble albuminous deposit.

§ 23. The next inquiry to be prosecuted is, therefore, whether fluid fibrin is a peculiar substance, or whether it is formed at the expense of a part of the albumen of the living blood. The plasma sanguinis which was necessary for this inquiry, was easily obtained by bleeding a horse, and receiving the blood in a glass cylinder surrounded by a freezing mixture of ice and salt, not strong enough to freeze the blood, but only for the purpose of hindering the coagulation for some hours, in order to give the corpuscles time to subside; after which the plasma could be gently taken off by means of a long cylindrical pipette. I had observed that blood-plasma whose coagulation had been hindered for some hours by the addition of acetic acid, would not afterwards coagulate if the acetic acid was exactly neutralized by the addition of a few drops of ammonia. This observation led me to the following experiment:—I mixed some blood-plasma with an equal volume of water, to which I had added some acetic acid. Four hours afterwards I added as much ammonia as would nearly neutralize the acid, so that the fluid changed blue litmus-paper to a reddish hue. This fluid did not coagulate at the common temperature of the atmosphere, but when heated to between 140° and 150° F. it became opalescent; and at 160° milk-white, from the coagulation of albumen. Having heated it to 212°, I filtered it; the precipitate differed in no respect from the common albumen of serum coagulated by heat. The clear fluid which had passed through the filter became slightly turbid on the addition of nitric acid or bichloride of mercury, and gave a precipitate with tannin. But that precipitate was far too inconsiderable to admit of the supposition that the fluid could contain the whole of the albuminous substance which produces the fibrin by its coagulation.

I then diluted the serum of the same horse's blood with an equal volume of water, and added acetic acid until blue litmus-paper was changed to a reddish hue. Then heating slowly in the same manner as before, in a water-bath, a thermometer standing in the serum, the effect was precisely similar to that which happened in the case of the plasma. Also, on boiling, the filtered fluid gave exactly the same reactions with nitric acid, chloride of mercury, and tannin.

In order to avoid all suspicion of fallacy, I prevented a certain weighed quantity of plasma from coagulating, by the addition of dilute acetic acid; four hours after it was nearly neutralized as before, then coagulated by heat, and filtered. Another weighed quantity of plasma was whipped with a hooked platina wire, in order to catch the fibrin as long as coagulation proceeded. When it had finished, the serum was poured off; and the fibrin being well washed with distilled water, this wash was then mixed with the serum, which being very slightly acidulated with acetic acid, was coagulated by heat, and filtered. These two filtrates were then evaporated separately, each together with the whole bulk of the water with which the coagulated albumen had been washed. The residues were dried at a temperature of 260° so long as they continued to diminish in weight, and the weights of each thus obtained in grains were reduced into per-cents. of each corresponding bulk of plasma. The difference between the two was only 0·05 per-cents. In a second experiment conducted in the same manner, the difference was only 0·01.

From these experiments, there could be no doubt that the so-called fluid fibrin, the material for the formation of solid fibrin, had comported itself in the same manner as the common albumen of the serum. In another case, in which I prevented the coagulation of a quantity of plasma by the addition of tartaric instead of acetic acid, the result was the same; that is to say, after being neutralized, it did not coagulate at the common temperature, but at about 160° the whole of the albuminous substances became insoluble; however, on neutralizing the acid, the plasma became more turbid than in the former case. This disadvantage is even more striking with phosphoric and oxalic acids. In some specimens of plasma of horses, the same thing happened even with acetic acid; so that the experiment in these cases cannot be neatly conducted, although the plasma apparently differed in nothing from other samples but in that it became more turbid when slightly acidulated.

Having never known any other difference between fluid fibrin and fluid albumen, than that the one coagulates at the common temperature of the air, and the other only if the temperature is raised to 160°, we have no further inducement to suppose that there exists in the blood-plasma a peculiar substance, so-called fluid fibrin; but we are now compelled to admit that solid fibrin is formed at the expense of a part of the albumen dissolved in the plasma. Indeed, it has already been long known that there are no constant differences in the elementary composition of fibrin and albumen. The results of the elementary analysis of some samples of fibrin were not more different from those of some samples of albumen, than they were from other samples of fibrin, or than the results obtained from some samples of albumen were from each other. Professor Liebig has pointed out this fact in reference to the fibrin of the muscles and the albumen of the blood. The fibrin of the blood has scarcely even been analysed in the pure state, because it always entangles more or less of organized particles.

It is a commonly received opinion that fibrin contains phosphorus, whilst albumen, according to the recent analysis of Lieberkühn,* contains no phosphorus. But the assertion that phosphorus is a constituent of fibrin, is founded upon the same method and principles of analysis, on which all former chemists have based the assertion that it is a constituent of albumen also. And the method of Lieberkühn cannot be adapted to coagulated fibrin. It would, indeed, be possible to adapt it to the whole albuminous substance of the plasma, but it would then be liable to fallacy, because a part of the albumen passes through the filter. Therefore, some writers discovered the difference between albumen and fibrin rather in their different physical qualities, and their comportment with reagents, than in the results of elementary analysis. But I shall show, at the end of this Treatise, that such distinctions are also of little value.

Our next object will be to inquire how albumen is changed into fibrin.

§ 24. In the prosecution of this part of the subject, the first question which presents itself is, whether the plasma of the blood contains albuminates, which, when decomposed by an acid, give rise to an albuminous precipitate. It has often been debated whether acids hinder the coagulation of the blood or not. In the experiments I have made to elucidate this point, I have not used blood, it being an opaque fluid, but the plasma of the horse obtained in the manner before described. In the description of its conduct with acids, distinction must be made between three cases.

First case.—The acids are added of such quality, and in such quantity, that they would instantly coagulate the serum of the blood. In this case, they of course coagulate the plasma also.

Second case.—The acids are not added in such quantity and quality as instantly to precipitate the albumen, but yet in such quantity as to be considerably in excess. In this case, the plasma does not coagulate any more in the ordinary way, but exhibits different reactions with different acids, as I shall now describe. If there be added to the fresh plasma, mixed with three times its volume of cold water, dilute nitric acid, drop by drop, the precipitate thrown down by the first drops may be redissolved by agitation. If so much has been added as to render the fluid permanently turbid, and it be then boiled, it becomes clear; but on cooling, throws down a copious white precipitate, This is the reaction of the albuminous matter detected by Dr. Bence Jones in the urine, and in a case of Malakosteon.

If the same experiment is made with plasma not previously diluted with water, it coagulates, when boiled, like common albumen.

* Poggendorff's Annalen der Chemie u. Physik, Band 86, p. 119.

If the plasma be mixed with any quantity of phosphoric acid, between $\frac{1}{26}$th and $\frac{1}{3}$rd of its volume, and of a specific gravity of 1·117 (or with oxalic or tartaric acid), it usually becomes slightly turbid, but does not coagulate; after twenty-four hours, however, it will be found to have become a gelatinous mass. If this jelly be placed in a water-bath of the temperature of 212°, it becomes fluid, but on refrigeration, congeals again.

If fresh plasma is mixed with phosphoric, acetic, tartaric, or oxalic acid, and then boiled, it does not coagulate; but on cooling it is changed into a similar jelly to that which, in the former experiments, was produced by the protracted action of the acids. All these reagents produced the same result with the serum of the blood, the jelly being only less firm; because the serum contains less albumen than the plasma, a part of it being transformed into fibrin.

Third case.—The plasma was slightly acidulated with acetic acid, and the effect was, that it became more or less turbid, though often very slightly so. If water were added, it always became more turbid, and sometimes threw down a flocculent precipitate; but if more acid was added, it became clearer again.

No doubt there were here albuminates which, when decomposed, formed a precipitate; but its bulk was very variable, and the serum exhibited the same reactions—for although on the addition of the acid it became less turbid than the plasma, yet when water was subsequently added it became turbid enough. We cannot, therefore, assert that the albuminates which were decomposed in these experiments were the immediate or exclusive material for the formation of fibrin. But I would ask—is it not possible that coagulation may be a process of continual formation and decomposition of albuminates?

§ 25. It next occurred to me to endeavour to produce a substance analogous to fibrin, by the artificial decomposition of albuminate of potash. I prepared some of Dr. Lieberkühn's solid albuminate of potash ($C_{72}H_{56}N_9O_{22}S + KO$),* cut it into pieces as large as a bean, and put them into water, to which I added, from time to time, a solution of $(CaO + 2Ho) PO_5$, in order to keep the reaction always acid. The pieces became more and more milk-white, and began to shrink. At the end of the third day decomposition was complete, and the pieces of albuminate of potash had lost much in volume, and were milk-white, firm, and elastic. Under the microscope it appeared partly amorphous, while a part was delicately striated, and it could be torn more easily in the direction of the striæ than in any other. In water containing one-part in a thousand of hydrochloric acid, the pieces swelled into a translucent jelly, as they did also in acetic and phosphoric acid. In a solution of hydrate of potash they were easily soluble, and in ammonia they swelled rapidly, and became translucent. Every one is aware that these are all the reactions which fibrin exhibits, and which it preserves even if dried in the air, or treated with spirits of wine; whereas albumen, when coagulated by heat, neither swells in phosphoric acid of 1·117 sp. gr., nor in acetic acid, nor in ammonia, nor in water containing $\frac{1}{1000}$th part of hydrochloric acid. When swelled by this latter fluid, some pieces of the

* Poggendorff's Annalen der Chemie und Physik, Band 86, p. 117.

mucous membrane of a rabbit's stomach were added, and the decomposed albuminate was digested as easily as fibrin, and much more rapidly coagulated by heat than albumen.

I found, however, some differences between the reactions of the substance I had produced and those of fibrin, but they were differences more of degree than of kind. First, the new substance was more easily soluble in ammonia than fibrin. If I put in two different glass vessels ammonia of the same strength, and placed fibrin in one, and in the other the decomposed albuminate, and twenty-four hours afterwards neutralized the ammonia, the result was always a larger precipitate in the second vessel than in the first. It also appeared that the substance became more translucent in a solution of carbonate of soda than fibrin does. A third point of difference is, that it requires a more concentrated acetic acid to swell it than does fibrin. But in this latter respect differences are exhibited by various samples of fibrin, and the fibrin of horses always swells less in acetic acid than the fibrin of oxen. On the other hand, different samples of the substance I had prepared also showed differences; and I found that it was the more easily swelled in acetic acid, as the solution of biphosphate of lime, $(Ca + 2\ HO)\ PO_5$, which I used to decompose the albuminate, was more dilute. I soon afterwards learned that the same substance can be as easily prepared by means of phosphoric or acetic acid, as by phosphate of lime. The albuminate of potash is put into a large jar filled with distilled water, mixed with so much phosphoric or acetic acid that it will just turn blue litmus, red. When the acid is neutralized by the potash of the compound, fresh acid is added by degrees until the decomposition is completed. For this purpose, hydrochloric acid is less useful, because, when the decomposition has ceased, the slightest excess of acid causes the whole substance to swell up into a translucent, quivering jelly.

Every one who will prepare this substance for himself, and make himself acquainted with its properties, will say without hesitation that it has more resemblance to true fibrin than any other known substance.

I have hitherto directed the attention of the reader only to those points in which it differs from common coagulated albumen, and resembles fibrin; but of course it possesses all those properties which are common to both substances. Thus, with nitric acid it produces xanthoproteic acid; with concentrated hydrochloric acid in contact with the air, it makes a violet fluid, &c. I must however make especial reference to the connexions which it has with coagulated casein. It is a well-known fact, that a solution of the albuminate of potash so nearly resembles a solution of casein, that many chemists consider them as identical. Also, that the precipitate thrown down by acetic acid in the solution of the albuminate is rapidly dissolved by an excess of the acid, perhaps because it was precipitated in finely divided particles. With casein it is notorious that the same thing happens; but Bopp* observed further, that the casein precipitated by hydrochloric acid, swells, on the addition of water, to a quivering jelly, which could be dissolved in an abundance of warm water at a temperature of 104° F.

Fibrin, the decomposed albuminate, casein, and the melting jelly

* Annalen der Chemie und Pharmacie, Band 69, p. 16.

produced by the digestion of the plasma or serum of horses with phosphoric acid, are perhaps a series of substances even more nearly connected with each other, than has hitherto been generally admitted.

From what has been hitherto advanced, it will be seen that we cannot admit the existence in the blood of the living body of a peculiar substance which deserves the special name of fluid fibrin,—a name which necessarily presents the idea that it is a substance distinctly differing from albumen and its compounds; and that this fluid fibrin becomes coagulated fibrin by a simple change in the condition of its cohesion. It must, on the contrary, be acknowledged that a part of the albumen of the blood is changed into an insoluble substance—fibrin—which in several respects bears a close resemblance to the insoluble albumen obtained by means of common white of egg, in decomposing Dr. Lieberkühn's albuminate of potash.

The question remains, whether that change is produced in the same manner—namely, by the formation of insoluble albuminates, and their subsequent decomposition.

There can be no doubt that soluble albumen may be changed into insoluble fibrin in various ways with which we are not acquainted, and which no hypothesis explains. Even the fibrin, when separated from the blood, is a very variable substance; but there are two circumstances which seem to point to a formation and decomposition of albuminates. The first is, as I have already mentioned, the quantity of insoluble compounds of calcium and magnesium which are found in every sample of fibrin; and the second is, the contraction of the clot—a circumstance hitherto quite unexplained, and very remarkable, but nevertheless easily understood on the hypothesis that the clot is at first formed by albuminates, which are subsequently decomposed, and therefore have a tendency to contract in the same manner as the pieces of Dr. Lieberkühn's albuminate of potash contract when they are decomposed in acidulated water.

Art. II.

On the Pathology of Cancer of the Stomach. By William Brinton, M.D., Fellow of the Royal College of Physicians, Lecturer on Physiology in St. Thomas's Hospital, Physician to the Royal Free Hospital.

In bringing before the readers of this Review the following Essay on Cancer of the Stomach, it is only necessary for me to premise that I shall follow as closely as possible the method adopted in the article On the Pathology of Gastric Ulcer, which I was permitted to bring under their notice a year ago. The cancerous disease being far less frequent than the ulcerative, I have had, if possible, greater reasons for adding to my own personal experience whatever inquiries amongst Hospital records, Museums, Reports, and Journals could contribute respecting it. And although circumstances have made these researches somewhat less complete than I could have wished, I venture to hope that even where I can add nothing to what is already known concerning this lesion, my results will not be uninteresting to the student of pathology, as being based upon a broader (and therefore better) foundation of facts than such deductions have hitherto been.

The frequency of the malady I shall not attempt to deduce from mortuary returns; although the writings of Tanchou in Paris, D'Espine in Geneva, and the Registrar-General in this country, would afford a vast array of figures as materials for such a deduction. Unless founded upon necropsies, such returns are useless for pathological inquiries like that we are now engaged in. And though, from the fatal character of the malady, we might with little error regard the number of cases dying from it as exactly corresponding with the numbers it attacked, yet the vagueness and inexactitude of the diagnosis on which such returns are sometimes based, as well as the errors which are frequently made in the mere nomenclature of disease, invalidate all their conclusions. Indeed, to the ordinary uncertainty of diagnosis in general, we must add another special and obvious source of error which would, on the whole, greatly increase the apparent frequency of this particular disease. The frequency and fatality of the gastric ulcer, coupled with the close resemblance its symptoms often exhibit to the cancerous disease of the stomach—a resemblance which it sometimes baffles the most sedulous observation to unmask during life—justify conjecture that, except in returns which accurately distinguish and record the numbers of deaths produced by each of these maladies, the mortality attributed to gastric cancer is sure to be far too great. In like manner, that chronic inflammation of the stomach which is often (and I think most improperly) termed "hypertrophy," may easily be mistaken for cancer; and though a far less frequent source of error, is still by no means unworthy of notice.

Hence it is to large numbers of necropsies, the subjects of which have died from all causes indifferently,* that we are obliged to turn for information on this head. From various sources—but especially from the valuable records kindly placed at my disposal in Guy's, St. George's, St. Thomas's, and St. Mary's Hospitals, I have collected a total of 8468 necropsies, which include 81 cases of primary cancer of the stomach,—a proportion which is about equivalent to 1 per cent., or $\frac{1}{100}$th of the total mortality.

The ratio of deaths from cancer of the stomach to cancer of other organs is one which it belongs rather to the pathology of cancer than of the stomach to determine. Rokitansky† regards the stomach as only less frequently the seat of this disease than the uterus and breast of the female. Lebert,‡ in a promiscuous collection of 447 cases of cancer, gives

* Although not examined expressly with this view, the Hospital Records with which I am best acquainted afford little ground for doubting the substantial accuracy of the above term. Even in the largest general Hospitals, I suspect the mortality from the more frequent diseases—including pulmonary consumption—varies little from the proportions seen in the deaths of the population at large. While the disproportionate numbers of deaths by accident which figure in Hospital Report-Books, perhaps scarcely more than compensate those deaths from "unknown" or "uncertain" causes in the Registrar-General's Returns, which the more accurate diagnosis of the Hospital physician or surgeon renders a rarer explanation in these institutions. In any case, it is probable that such inquiries as those above alluded to would, with proper precautions, rather understate than overstate the frequency of the particular disease they refer to.

† Handbuch der Pathologischen Anatomie, Bd. 1, s. 347.

‡ Traité Pratique des Maladies Cancéreuses, p. 97. Paris, 1851.

the stomach the second instead of the third place in the order of frequency (breast 62, stomach 57, uterus 52, cases), and a proportion of 1 in 8, or $12\frac{1}{2}$ per cent. Tanchou, from the mortuary returns of the Department of the Seine, also assigns the stomach the second place in the order of frequency, but places the uterus first (uterus 2996, stomach 2303, breast 1147): and thus deduces for the stomach a proportion of 1 in 4, or 25 per cent. Lastly, D'Espine found 209 gastric cancers in 471 cancers generally : a proportion ($44\frac{1}{3}$ per cent.) nearly three times as great as that he attributes to the uterus, and five times as great as that of the breast. The later stages of uterine and mammary cancer would rarely be considered proper cases for admission to a British general Hospital; for the funds of such institutions are with more propriety devoted to the treatment of curable disease, and of infectious maladies in which each person cured probably represents many saved from the possibility of similar dangerous illness. But after allowing for these sources of disproportion, my own researches amongst the necropsies of the above metropolitan Hospitals give me the impression that Lebert's estimate would be nearer the truth, for an ordinary British population, than either of the others. Willigk,* however, who has made a more direct and trustworthy estimate than any of the above, inasmuch as it expresses the numerical results of a series of promiscuous necropsies, finds that out of 184 cases of cancer, 64 (or about 35 per cent.) were cancers of the stomach : while the mammary and uterine cancers amounted to but 12 and 42 cases respectively.

As regards the *age* of its occurrence, I have collected 601 cases which specify this fact. They may be arranged as follows:—

	Between the ages of								
	0 — 20	— 30	— 40	— 50	— 60	— 70	— 80	— 90	— 100
Number of cases of cancer of the stomach. . . .	3	32	80	140	162	133	38	12	1

These 600 cases afford an average age of 50 years.

Comparing the above numbers with the numbers of people living at these ages respectively, we obtain an estimate of the relative liability of these epochs of life to the malady. Reducing this to a maximum of 100, we get the following figures:—

	Between the ages of						
	0 — 20	— 30	— 40	— 50	— 60	— 70	— 80 — 100
Liability to cancer of the stomach, taking 100 as maximum .	·227	11·64	31·67	63·005	87·99	100	52·16 ...59·99

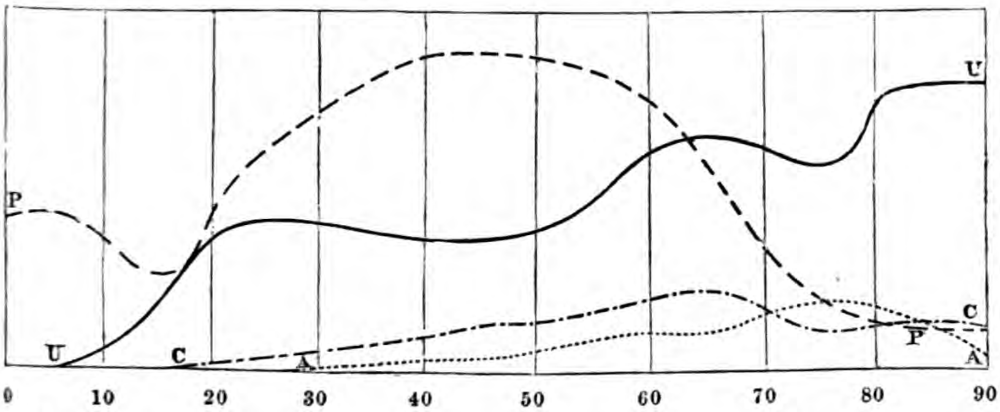
It would thus appear that, before the age of forty, the liability scarcely attains one-fifth of its total amount : in other words, four-fifths of this risk still remain, to be encountered in the succeeding years of life. At the age of sixty, nearly one-half the risk of the malady has already gone by: and at the age of seventy, two-thirds. The small number of cases, as well as of persons living, in the last two decades of life, render the conclusions that refer to these epochs somewhat less trustworthy. But, assuming the accuracy of these estimates, it becomes interesting to con-

* Sections-Ergebnisse, &c., Prag. Vierteljahrschrift, x. 2, 1853; and Schmidt's Jahrbücher, vol. lxxix. p. 90.

trast them with some other diseases of advancing life; and especially with those already compared with the ulcer of the stomach in my previous Essay (January, 1856).

	At ages between								
	0 — 10	— 20	— 30	— 40	— 50	— 60	— 70	— 80	— 90
Liability to cancer of the stomach	—	$\frac{1}{4}$	$11\frac{2}{3}$	$31\frac{2}{3}$	63	88	100	$52\frac{1}{6}$	60
To ulcer* of the stomach . .	—	20	51	49	47	56	80	75	100
To apoplexy (cerebral hæmorrhage?)	—	$\frac{1}{2}$	$2\frac{1}{6}$	7	16	40	61	100	69
To pulmonary consumption .	47	33	73	92	100	95	65	23	$7\frac{2}{3}$

But instead of chusing for all these diseases an arbitrary maximum of 100, which has a totally different and incommensurable value for each of them, we may profitably attempt a stricter comparison. In the accompanying diagram I have endeavoured to effect this, so that while the varying heights of each curve represent the relative liability of the individual to the particular disease during the epoch of life denoted by the ages (the widths) subjoined, the comparative heights of the different curves also represent the relative liability of the individual to the corresponding maladies at these epochs.†



For example, the line P P in the diagram represents the curve thus calculated for phthisis, or pulmonary consumption: the line U U, that of gastric ulcer: and the lines C C and A A those of gastric cancer and apoplexy respectively. It is the chief advantage of such delineations that they supply at a glance materials for much thought; and deductions out of which no two persons would probably select quite the same for especial notice. But, assuming the accuracy of the calculations on which these

* The reader must recollect that these ulcers differ from the other three lesions, in not being all primary or efficient causes of death. In other words, they are often found in the dead body without our being able to regard them as exerting more than a partial causative influence on the fatal event. (On this point, compare the author's Essays in this Review, No. xxxiii. p. 182; and No. xxxv. p. 160 *et passim*.)

† The deaths by apoplexy and consumption are reduced from the Registrar-General's Report for the year 1847. As mere death-rates, they are of course open to the objection of uncertainty of diagnosis. But errors would to some extent correct themselves, by being in opposite directions. And these maladies are (if we except pulmonary consumption in infant life) likely to be more correctly diagnosed than most others. It may therefore suffice to point out that, while "apoplexy" doubtless includes some cases of epilepsy and renal coma, the cerebral hæmorrhage which forms its most frequent cause would about as frequently end in deaths which are probably registered as "paralysis." Hence it is chiefly the *sudden* deaths produced by such hæmorrhage that are above referred to.

outlines are based, it would follow that, at about the age of seventeen or eighteen there is nearly as great a liability to be attacked by gastric ulcer as to die of pulmonary consumption: and that at about the age of sixty-three, the risk of attack from the former, and of death from the latter, malady, again culminates in the same manner.* While, with respect to gastric cancer, we not only see that the risk of this malady amounts to what is, on an average, barely one-fourth of the ulcer; but that, though much more distinctly and exclusively a disease of old age, its climax or maximum of risk occurs at least twenty years earlier than that of the gastric ulcer. Lastly, comparing gastric cancer with apoplexy, we notice that the risk of the latter disease, beginning about ten years later than that of the former, remains, with little deviation from the proportion of one-half the former, until the liability to cancer has reached its climax; when, by attaining its own maximum in the next ten years, it reverses the above proportion, and at length, between the age of 80 and 90, subsides to a death-risk which is nearly equal for the three very diverse diseases as phthisis, cancer, and apoplexy.†

Sex.—The question of sex is even more difficult than that of age to decide on any sufficient numerical basis. Death-rates are useless, for the reasons already mentioned. The personal experience of private practice is too small; and even if many observers were to group and unite their experience, still its results would be too promiscuous and uncertain. And even in public institutions for the treatment of the sick, the numbers, however large, are useless for all purposes of comparison, unless it be clearly stated what is the average proportion of male to female patients among whom the cases of gastric cancer have occurred.

Such considerations well explain the contradictory results arrived at by different authorities. D'Espine gives 116 cases from the mortuary returns of Geneva, of which 54 are male, and 62 female. But if these cases of supposed cancer included the ordinary proportion of gastric ulcer witnessed in this country, the disproportionate numbers (2 to 1) of females attacked by the latter malady, would explain the above result very differently. Lebert and Louis, who appear to have collected their cases promiscuously, deduce conclusions, the contradictory nature of which is less invidiously explained by the small numbers with which they deal. Lebert gives 42 cases, 19 male, and 23 female; Louis 33 cases, 20 male, and 13 female. Out of 224 cases collected by Dittrich and Willigk, 95 are males, and 129 females. While 233 cases which I have brought together, chiefly from British Hospitals, give 160 males, and 73 females.

The two latter groups of cases, however, are less contradictory to each other than might at first sight be supposed. At least, it is my impression that the London Hospitals receive on an average not less than six males to five females; and hence that the above proportion requires to be reduced

* The interest and the value of this method of pathological inquiry are little affected by the inaccuracies such deductions are sure to contain. Although the author has taken much trouble in the collection of materials, no one can be more sensible of their deficiencies.

† The climax which the risk of gastric ulcer reaches at the extreme age of ninety, seems also (in the diagram) nearly equal to that maximum danger of death from phthisis which occurs between the ages of forty and fifty. Whatever doubt the small numbers of the ulcers at these high ages throw upon this startling conclusion, I must say, that hitherto my clinical researches amongst persons at this period of life have but confirmed it.

to about 140 to 73. While Dittrich expressly states that the hospital at Prague, from which his (and Willigk's?) cases are derived, admits females in far greater numbers than males.

Adding together all the trustworthy returns I have been able to meet with, affords me 784 cases; out of which 440 are male, and 344 female. Such numbers leave little doubt that, whatever the exact proportions of the sexes, the male is the more frequently affected of the two: a fact which it is impossible to avoid connecting with the exclusive amenability of the female to the mammary and uterine localizations of cancerous disease. The above numbers nearly correspond to a proportion of 4 to 3, or 56 to 43 per cent. respectively.

The mode in which age is affected by sex I deduce from 223 cases, which I have collected chiefly from the London Hospitals before alluded to. Of these 151 are males, and 72 females. The epochs of life to which they belong, and the liability they afford when corrected for the numbers living at those epochs, are arranged as follows:—

	At ages between							
	0 — 20	— 30	— 40	— 50	— 60	— 70	— 80	— 90
Cases of gastric cancer { Male . . .	1	8	23	40	30	45	4	
Cases of gastric cancer { Female . .	—	5	12	24	19	8	2	2

	At ages between							
	0 — 20	— 30	— 40	— 50	— 60	— 70	— 80	— 90
Liability to gastric cancer, taking 100 as maximum { Male . .	$\frac{1}{5}$	$7\frac{4}{5}$	$24\frac{2}{3}$	49	46	100	17	
Liability to gastric cancer, taking 100 as maximum { Female .	—	$4\frac{4}{5}$	$12\frac{2}{3}$	$28\frac{1}{3}$	$26\frac{1}{2}$	$14\frac{2}{3}$	6	22

The numbers in the two last epochs are too small for any safe conclusion. But as a whole, this table of liability is interesting; because, whatever doubt may attach to its details, there can be little risk in accepting its chief conclusions. Thus there are obviously indications of a greater (double) liability of the male up to the age of sixty. And there is still more distinct evidence of a contrast in the epochs of rise and fall of risk in the two sexes. The influence attributed to the few years that follow the close of the fertile (or menstrual) epoch in the female, is contradicted by the precisely equal rise of risk which the same age exhibits in the male. The ten years between sixty and seventy, however, seem to bring about a converse alteration of risk in the two sexes,—increasing that of the male to double, diminishing that of the female to half its former amount. Whether this change really represents a corresponding alteration of risk from cancer in general, or whether it is in any degree equalized by an increased liability of the female to its mammary and uterine localizations, must be left to future researches to determine.

The average age in the two sexes is about 51 in the male, $40\frac{1}{2}$ in the female.*

* A larger number of cases for the above comparisons (394) might have been obtained by adding to the above summary of British necropsies, 160 given by Dittrich (Die Krebsige Entartung des Magens, Prager Vierteljahrschrift, 1848, B. i. s. 1) in his excellent though brief Essay; and 12 by Duchek (Id op., 1853, i. 3). But I have preferred a limited number of facts to a larger series, not only because they are likely to be more strictly comparable when drawn from kindred sources, but also because (as above mentioned) the Prague Hospital includes a very disproportionate number of females, while the British Hospitals exhibit something like an equality (6 to 5?) of the male and female cases. To this I must again add (what I have already conjectured in a previous Essay), that there can be little doubt that the Prague

The *situation* of the cancerous deposit is specified in 360 of the cases I have brought together. The pylorus was affected in 219 instances—a proportion of exactly 60 per cent., or three-fifths of the whole. The sex of the patients seems to have no influence on the frequency with which the lesion affects this part. Thus, in the 232 British cases, concerning which I have obtained fuller details than the others, the sexes are to each other nearly as 2 males to 1 female (159 to 73). And this group of cases yields 194 which specify the exact site of the cancer; and includes 125 lesions of the pylorus, which are divided in very nearly the same proportion into 86 males and 39 females.

The same group also affords me the basis of some specific conclusions, which are rendered important by the authority of the admirable pathologist whose opinions they somewhat modify. Rokitansky* states that "the cardiac orifice of the stomach is but seldom the seat of cancerous degeneration, and it is a circumstance worthy of note, that the pyloric cancer is exactly bounded by the pyloric ring, and never reaches beyond to the duodenum; while that of the cardia—even when it does not descend from the œsophagus above—always spreads itself over a considerable piece of the œsophagus."

It is only in a very limited sense that we can accept the above word "seldom." For out of the 360 cases already alluded to, no less than 36 are cancers of the cardia; a proportion amounting to exactly 10 per cent. of the total numbers, and to $16\frac{2}{3}$ per cent., or 1 in 6, of the pyloric cancers (216). And in the British cases which I have collected, the proportion of cardiac cancers is still larger, namely, 25 in 194, equal to 13 per cent., or 1 in 7 of the whole number; and 1 in 5 of the pyloric lesions. From these cancers of the cardia, I have excluded all cases except those in which the orifice itself was either solely or chiefly affected. And although it is possible that some of them may have been originally developed around the œsophagus, and only extended to the stomach, yet this objection, which applies equally to all the above cases, can scarcely be regarded as a frequent explanation in the absence of express evidence to that effect. We may therefore conclude that cancer of the cardia is *not* an unfrequent variety of this lesion in the stomach.

It is still more curious to notice how specifically the observations I have brought together contradict the next proposition of the above eminent pathologist quoted above. The 125 cancers of the pylorus included no less than 10 cases in which the disease was *not* bounded by the valve, but passed beyond it for a variable distance (often an inch or two) into the duodenum. Lebert† gives another instance of the same kind: making in all eleven exceptions to this supposed absolute and universal rule.

To the equally absolute statement, that the cardiac cancer *always* involves the œsophagus, I have also found two conclusive exceptions. Allowing for the much smaller number of these cardiac cases, it is not impossible that such exceptions may be almost as numerous as the preceding. Indeed the proportions in the cases I have collected, exactly

Hospital (like many of the German Krankenhäuser) would probably differ widely from our own hospitals in the number of aged persons it receives and contains: being in this respect much more akin to a workhouse infirmary than is an ordinary British metropolitan hospital.

* Handbuch der speciellen Pathologischen Anatomie, Bd. ii. s. 205. † Op. cit., p. 467.

correspond to such a conjecture; these exceptions being 11 in 159 pyloric cancers, and 2 in 30 cardiac cancers (or about 1 in 15 for each).

The rules, therefore, which Rokitansky has the merit* of having laid down, in these respects are (like many others in this branch of natural science) of general, but not universal, import. But their value is not much affected by occasional exceptions. For their significance, far from depending on any mere numerical ratio of 15 to 1, is much more essentially measured by the antagonism of the fact in the two classes of lesion —the limitation, and the spread, of the cancer beyond an analogous boundary or extremity of the organ. Hence it becomes interesting to inquire whether any reason can be assigned for it.

On the whole, the present state of our knowledge does seem to justify a conjecture of this kind. The character of the above laws—general, but not universal—sufficiently hints that they do not depend on any direct or single cause, such as would necessarily be of universal operancy, but rather on some secondary causation, which only indirectly brings about its result. And the first and nearest inquiry is one bearing on the anatomy of the stomach in relation to that of the cancerous deposit. The question naturally suggests itself—"Is there nothing about the structure of the pyloric and cardiac orifices which, by opposing or facilitating the continuity of the cancerous deposit in their tissues, respectively limits or permits its spread to outlying parts of the alimentary tube?"

The answer to this question appears to afford exactly the kind of explanation suggested. The minute anatomy of gastric cancer conclusively shows (as we shall presently have occasion to mention in detail) that the morbid cell-growth is generally first deposited in the sub-mucous areolar tissue; that it thence advances (by means of those partitions of the same tissue which separate and ensheathe the bundles of the organic muscle) into the subjacent muscular coat; that, at a later period, it engages the mucous membrane, by disorganizing which it soon produces symptoms of grave (if not fatal) import; and lastly, that the complete implication of the peritoneal coat is a still later phenomenon, and therefore is, in the main, generally anticipated by death.

Now it is precisely in the degree and kind of continuity by which these first and chief seats of the cancerous deposit—the sub-mucous and muscular tissues of the stomach—merge into their analogues in the adjacent segments of the digestive canal, that the cardia and pylorus differ most remarkably from each other. The cardia is so organized as always to concede to whatever food may be swallowed a transit in the onward direction,—a transit during which its contraction implies a muscular movement that is directly continued† from the œsophagus into the stomach. In accordance with these requirements, there is the most perfect con-

* The reader will hardly think me invidious in calling attention to any inaccuracies in a work so widely known and appreciated as the great Text-Book of Pathological Anatomy cited above. The less so, perhaps, that few books of the present day demand or repay so thoughtful and minute a study. It is because every sentence evidently sums up a wide series of accurate observations, that we may justifiably apply to it a criticism of unusual severity: a criticism which, even if it weighs every word, will scarcely do more than the author's terse and weighty propositions really deserve.

† For fuller details on this and other points connected with it, the reader may be referred to the article STOMACH, by the Author, in the Cyclopædia of Anatomy—Supplement, pp. 311 *et seq.*

tinuity between the sub-mucous and muscular coats of the two segments. The latter, in leaving the œsophagus, radiates its longitudinal layer on all sides, and with special distinctness along the lesser curvature; while its circular or transverse layer has an almost equal continuity with both the circular and oblique layers which represent it in the stomach.

On the other hand, the pylorus is constructed so as to resist the onward transit of the contents of the stomach by its powerful contraction during the whole period of gastric digestion—a contraction that appears to be slightly over-balanced by every wave of the powerful peristalsis which gradually engages the strong muscular layer of the pyloric region, so as to strain off a little of the more liquid contents of the stomach during each of these undulations. The violent action into which the pylorus thus passes from the time of entry of food into the stomach, requires its complete isolation from the adjacent duodenum. And a careful dissection of the pylorus shows that this is attained, to some considerable degree, even for the sub-mucous areolar tissue, by its small quantity, and its close and dense attachment, at the line of junction of the two mucous membranes. But it is especially in the muscular structure that this isolation is most distinct. The mode of attachment of the duodenum to the stomach may be best expressed by the statement that, instead of being continuous with the pyloric extremity of this organ, it is attached around it, at a short distance from its termination,—in short, that the commencement of the bowel receives the stomach, just as the vagina receives and embraces the neck of the womb at some distance* from its mouth. And hence not only is the continuity of the muscular coats of the two organs diminished to the junction of the thin linear layer of the duodenal coat, but this delicate layer comes off from the stomach at an angle, and at a distance from its pyloric extremity, which render it far more likely that the deposit should engage the massy pyloric valve beyond this attachment, than that it should diverge—through what seems to be chiefly a second or outer process of areolar tissue—towards the duodenum. Indeed such a view receives a frequent confirmation in the ordinary phenomena of the distribution of cancer; in the way, for example, in which sub-peritoneal cancer almost always involves the continuous membrane that covers two or three organs or segments of the digestive canal; or the converse frequency with which the disease, when primarily affecting a gland, leaps over, as it were, a scanty medium of areolar tissue, to fix on a more congenial organ at some distance from its original seat.

It may be interesting if we sum up the other situations of the above 360 cancers of the stomach, by a table, comparing them with as many ulcers of this organ—the more so, indeed, that some authors appear to regard these two diseases as affecting the pyloric and cardiac extremities of the stomach in nearly equal proportions.

* I am aware that the illustration I have selected may seem to militate against the very conjecture for which it helps to lay the foundation: by showing an equally discontinuous structure, where, nevertheless, cancer *does* often spread. But we must recollect that, owing to the less direct influence exercised by these sexual organs on the life of the patient, the cancerous lesion is prolonged to a much later period; to one which, at any rate, might well suffice to convert the ratio of continuity of cancerous deposit seen in the stomach (1 in 15 cases), to that which seems to prevail in the uterus (1 in 3).

	Total.	Pylorus.	Lesser curvature.	Cardia.	Stomach generally.	Greater curvature.	Posterior surface.	Anterior surface.	Middle segment.
Number of examples of gastric cancer.	360	219	38	36	13	11	11	11	4
Number of examples of gastric ulcer.*	360	52 (25 ?)	98	(5?)	0	8	177	18	0

Such a comparison of the two diseases shows that the remarkable preference of the cancerous lesion for the orifices of the stomach (which together form the site of 71 per cent., or nearly three-fourths of the gastric cancers), is not at all shared in by the ulcerative disease. In the recorded necropsies of the latter, indeed, the terms "pylorus" and "cardia" so often refer to the mere neighbourhood of these valves, that a large deduction (probably at least one half, as indicated by the notes of interrogation in my table) must be made from the moderate proportion (16 per cent., or less than one-sixth) which their numbers would together imply.

The contrast of the remaining situations does not seem to call for any comment. Of course, the absence of all cases of ulcer of the whole stomach, or of the whole of its middle region, is sufficiently explained by the death that necessarily intervenes as soon as the process of ulcerative absorption has destroyed a large proportion of a mucous membrane so essential to life as that of the stomach. I may add, that I have taken every care to exclude from these cancers of the whole stomach, those cases of chronic inflammation of the gastric parietes sometimes mistaken for scirrhous disease—have generally, for instance, either obtained direct evidence derived from its microscopic or anatomical characters, or indirect evidence equally trustworthy in the shape of the presence of secondary cancer in other organs.

The *anatomy* of gastric cancer is most conveniently subdivided, according to the three chief forms of this deposit which it affects—the scirrhous, medullary, and colloid cancer; to which we may add a fourth, the villous cancer of the mucous membrane.

Of these varieties of cancer, the scirrhous is by far the most common. Out of 180 cases which name the species of cancer present, 130 (a proportion equivalent to about 72 per cent., or nearly three-fourths of the whole) belong to this variety. The same group affords 32 instances of medullary cancer, 17 of colloid, 3 of melanotic pigment, and 1 of villous cancer. These numbers about correspond to per-centages of 18 and $9\frac{1}{3}$ for the medullary and colloid cancer respectively; or, in fractions, to rather more than one-sixth and one-twelfth. The melanosis was in one case diffused generally over the whole body, in the form of small tumours; in the two others was such a superficial colouration of the gastric mucous membrane covering the cancerous tumour, as would scarcely merit this title, unless substantiated by very careful histological examination.†

As my limits oblige me to confine myself strictly to the local or gastric features of the malady, I am spared the necessity of dwelling on the his-

* These cases (rearranged, and raised by multiplication from 220 to 360) are derived from the sources specified in the author's Essay in this Review for January, 1856.

† In order to prove that it was not due to any of those alterations in colour which mere ecchymoses in this situation so often undergo from the action of the digestive fluids.

tology of these three forms of cancer. The more so, indeed, that they merge into each other by gradations of almost infinite variety. For not only may we regard the scirrhous, medullary, and colloid cancer of the stomach as being what they are in other parts of the body—manifestations, in a different form, of one and the same disease; but as constituting, with even greater frequency, mere consecutive phenomena of one and the same morbid process. In other words, in a great many cases, what is originally scirrhus becomes admixed with more or less medullary or colloid cancer, if it be not absolutely metamorphosed into it. While from collateral circumstances, we are entitled to conjecture that in many more a similar complication would occur, but for the occurrence of death in an earlier stage of the disease. Whatever may be the apparent temerity of such a view, it does but state, in a somewhat circuitous way, a proposition the truth of which is obvious at first sight—namely, that our knowledge of the pathological anatomy of this (or any other) disease is built up from a number of observations; and that of these observations, many of the most valuable in respect to the origin and succession of morbid changes in the organ that forms the seat of the disease, are only afforded us either by casual deaths, by intercurrent or secondary maladies, or finally by an effect of the disease on the powers of life which is so complex and variable, that we may fairly view it as somewhat independent of the local mischief.

In the vast majority of cases, the cancer begins by a deposit in the submucous areolar tissue. This loose but thick layer—which, by intervening between the mucous and muscular coats, shares in every movement of the two, and especially concedes and limits that free play of the mucous membrane by which the passive contraction of the muscular coat throws it into folds, to be effaced by any distension of the stomach—is composed of its ordinary constituents of white and yellow fibrous tissue, the elements of the latter being of large size, and in great quantity. And the first rudiments of a cancer are generally deposited among these fibrous structures in the form of a dense knot, of a dull white colour, and a firm and hard (rather than tough) texture. This opaque mass includes the normal fibrous elements of the tissue, but in such an intimate state of fusion that it is almost impossible to detect them in any quantity. Indeed, their proportion is so small that, even allowing for the mechanical difficulties that oppose their isolation, it is difficult to avoid the conjecture that they are compressed and killed by the new growth; in which, so to speak, they remain dead, as well as buried. And it is to the strictly retrograde changes of this kind which these healthy original structures undergo, that I am disposed to attribute the fatty molecules which are found in even the freshest specimens of scirrhus, and in the earliest stages of its growth. In many cases, at least, I believe this fat to be produced by the mere decay of the ordinary tissues.

Blastema in any quantity is rarely met with, save as an element of the whitish juice which exudes from the scirrhous mass when it is squeezed or scraped. The irregular warty character of the layer into which these knots soon expand, seems to be often quite unconnected with any peculiarities in the arrangement of the scirrhous growth with respect to the plexuses of vessels that occupy the sub-mucous areolar tissue; though occasionally the protuberances seem to be received into vascular loops or meshes of this kind. Finally, in most instances, however small or recent the mass of scirrhus, a large proportion of its substance consists either of fibres, or of cells, the fibrous extremities of which are so long as almost to deserve this title. Mere nuclei are in but small proportion.

It is often difficult to determine by examination whether the scirrhous deposit has originally predominated on the muscular or external aspect of this layer of sub-mucous tissue. But there are good grounds for supposing that this is generally the case. At any rate, it is in this direction, and towards this constituent of the stomach, that its progress in the succeeding stage of the disease is almost exclusively directed. The sub-mucous and muscular strata become inextricably fused into each other at their line of contact; while as yet the mucous membrane itself is separated from the disease by an interval of healthy areolar tissue. The muscular coat is then transformed into scirrhus by an extension of the same process as that by which the sub-mucous tissue was itself at first affected.

The appearances of the scirrhus in this stage are too specifically connected with the organ it affects, to be altogether passed over. In rare instances, we may find a dense white semi-translucent mass, of tolerably uniform structure, occupying the whole thickness of what was formerly the muscular coat, and quite undistinguishable from the original focus of the disease in the sub-mucous areolar tissue.

But in the majority of cases, the cancerous deposit is much less homogeneous, and exhibits characters which may almost be regarded as relics of that differentiation of tissue present in the healthy textures which it has replaced. On cutting through the diseased mass, we find that the deeper portion which has replaced the original muscular substance, exhibits two varieties of tissue, so arranged as to give it more or less of a honeycombed appearance on section. There is a brownish, reddish-yellow, or at any rate darker and somewhat softer mass, enclosed in small polygonal meshes formed of whiter and denser tissue. Of these meshes, which are irregular in shape and size, those are in general strongest and thickest which run transversely to the axis of the stomach from the sub-mucous to the sub-serous areolar tissue. They are evidently the transformed septa of the bundles of fibre-cells which unite to form the thick muscular coat of this part of the stomach; and they inclose cavities which, originally occupied by these muscular bundles, now contain, in addition to their fatty and decayed relics, a cancerous deposit which is often visibly less fibrous, and richer in nuclei and blastema, than that of the septa themselves. Their arrangement, however, does not always precisely recal that of the original sheaths of the unstriped fibres. The thickest septa are, as above mentioned, often directed transversely to the mucous surface; and, from the direction of the circular fibres, are best seen by cutting lengthwise through the mass in the same plane. But irregularities in the deposit of the cancerous matter, as well in the septa as in their inclosed cavities, easily affect the uniformity of this arrangement, so as to render the meshes comparatively larger or smaller, or even to obscure and obliterate them altogether. And as a pretty equable deposit around the narrow pyloric end of the stomach is one of the commonest varieties of scirrhus in this organ, the first effect of the commencing disease often presents an obstacle to further growth in the direction of the centre of the tube, such as even aids the natural tendency of the disease to spread outwards; so that the septa seem to diverge (or even radiate) in passing from the centre to the periphery of the tumour, and include an increasing amount of their darkish or gelatinous-looking contents as they approach towards the tissues of the peritoneum.

There is one circumstance which renders it especially important to appreciate these characteristic appearances of the gastric scirrhus in this stage of its growth. It is probably not uncommon for specimens which present little more than the average distinctness of this differentiation seen in ordinary scirrhus, to receive the name of "colloid" cancer. And though it is true that in many instances of the kind a careful and accurate histologist could distinguish the tissue as really fibrous or scirrhous cancer —a decision in which its physical as well as microscopic characters would alike concur—still in some cases, in which the meshes are large, and enclose much fluid exsudation, it is not very easy to say to which of the two species the cancer really belongs. To some examples, indeed, it would be very difficult to deny the appellation of colloid. At any rate we may assert that a large proportion of so-called colloid cancers of the stomach are either essentially scirrhus, or are developed out of a cancerous deposit which was originally of this species. And hence that the ordinary way in which this name is at present used, obliges us to deduct from the recorded cases of colloid cancer of the stomach a very considerable percentage as modified scirrhus; and (what is even a more practical point for the pathologist) to regard such cases as having little or no weight in the decision of an important problem in the natural history of gastric cancer—namely, the average duration of the three forms of the malady, or the rapidity with which they severally destroy life.

The further progress of the disease conducts it to the mucous and serous surfaces of the stomach; still, as it goes, fusing into itself, and confounding together, the structures previously present.

In the serous membrane, the tenacity of its tissues, as well as its pathological tendency to inflammation, soon give rise to adhesion of the cancerous segment of stomach to some neighbouring viscus. How far the cancer itself lends the aid of any specific irritation to this process it is difficult to estimate: although, from the analogy of the ulcer, one may suspect that we have no great need to assume such an explanation. Of course the particular portion of the tumour thus attached, as well as the viscus to which it is fixed, are subject to considerable variety. But while (for obvious reasons) the adhesion of the ulcerous stomach accurately corresponds to the site of the lesion, and to the viscus which is normally in contact therewith, that of the cancerous organ appears to be regulated by circumstances less obvious. The diaphragm, liver, pancreas, and spleen, constitute the most frequent sites of such adhesion; and in nearly the above order of frequency. In short, it is at the upper and posterior aspect of the stomach, and with a frequency that is quite disproportionate to the predominance of the cancer itself in these positions, that this fixation chiefly occurs. It is probable that the chief cause of this peculiarity is to be found in the relative immobility of the organs which occupy this part of the cavity of the belly; and that, so far, it is analogous (if not strictly parallel) to the infrequency or tenuity of adhesions between an ulcer of the anterior surface of the stomach and the corresponding wall of the belly. Indeed, the chief difference seems to be, that the contrast, which we can only verify in different cases of ulcer, is often illustrated by opposite surfaces of one and the same cancerous tumour.

In advancing towards the mucous membrane, the disease inaugurates a series of changes which, however secondary in their essence to the deposit that has long preceded them, are in the majority of cases probably more intimately connected with those symptoms which attract the notice and claim the skill of the physician, than any other of the numerous details which the morbid anatomist has to study. The destruction of the gastric mucous membrane not only directly involves the partial ablation of an organ essential to life, but leads to a train of indirect results of at least equally serious import. The certainty of hæmorrhage and ulceration; the probability of a grafting of cancerous germs into distant organs by means of the current of the lymph or blood; together with an increased possibility of obstruction, dilatation, and hypertrophy of the stomach; or of fistula opening into other parts of the canal, the chest, or the surface of the belly:—all these risks are now superadded to those of the cachexia which the cancer itself expresses, and to those of the cachexia which it can produce.

As the cancer approaches the immediate proximity of the mucous membrane, its first effect often appears to be a slight though perceptible increase in the thickness and firmness of this membrane, such as a mere increase of its healthy nutritional fluid could produce. It resembles, in short, a stomach taken from a younger and healthier person, or from a fresher corpse, than that to which it belongs.* Then follows a fusion of the cancer with the under surface of the mucous membrane, giving to this latter a complete immobility upon the subjacent textures, and a dull, white, thickened appearance. The local anæmia which this state seems to imply, is probably often due to a complete occlusion or compression of the blood-vessels of the membrane: a state which, by the pressure it throws on the obstructed current of the blood, occasionally appears to give rise to an hæmorrhage strictly analogous to that which so often causes death in cirrhosis. And it is probable that this interference with the vessels (quite as much as any specific tendency of the cancer itself) is answerable for the destructive changes that now ensue in the membrane. This destruction, whether effected by the intermediate stage of dark pulpy or ashy softening that merges into an ulcer, or by the more rapid death of the tissues piecemeal by a process of sloughing or gangrene:—in any case speedily leads to the same result, to the production of a solution of continuity, which is bounded exclusively by the exposed or denuded cancerous growth.

The subsequent phenomena still permit of considerable variety in different specimens. On the whole, the most frequent change is that

* And may be compared in so far to the stomach of diabetes; which, as I described many years ago, from a specimen I examined for Dr. Todd, is best summed up as remarkably healthy, and unusually resistant of cadaveric changes. Subsequently, I believe, Dr. Todd and myself were both misquoted to mean that the tubes were in a state of abnormal distension.

in which the periphery of the cancerous mass, and especially that part of it which is denuded by the removal of the mucous membrane, becomes the seat of an infiltration of medullary or areolar cancer. This new deposit generally protrudes into the cavity of the stomach by spongy masses of variable size and shape, which constitute what is (κατ' ἐξοχήν) a true *fungus hæmatodes*: a soft pulpy fungus, that sometimes breaks down by a process, half sloughing, half suppuration (*verjauchung*); but oftener associates with a similar process such an amount of hæmorrhage as materially to disguise and conceal it. Hence, unless to a strict scrutiny (or even a careful miscroscopic examination), there is often no perceptible difference between the coffee-grounds vomit ejected in such cases during life, and that expelled from the merely ulcerous stomach. And in like manner the surface and edges of the cancerous fungus itself are sometimes occupied by coagulated and altered blood, that bears no testimony whatever to the character of the lesion which has procured its extravasation.

This secondary deposit of medullary cancer seems to be precisely analogous to that secondary form of colloid already mentioned, than which it is, on an average, more frequent, and to which it not unfrequently serves as a basis. Both of them suggest the question as to how far they are real transformations of the original scirrhus, how far they are merely admixed with it. But the morbid anatomy of a large number of cancerous stomachs probably indicates a somewhat different answer for each of the two. If we are to accept the term "colloid" in its ordinary sense, it will certainly be difficult to exclude from this category cases in which it seems to have been developed, by something very like a genuine transformation, out of both scirrhous and medullary cancer. In the former of these two species we have seen that it occasionally (though very rarely) appears to perpetuate a normal differentiation of tissue; that it is the morbid analogue (so to speak) of the sheaths and bundles of the unstriped muscle.* In the latter it amounts to the introduction, into a comparatively homogeneous cell-growth, of an exactly similar differentiation: describable in most cases as a formation of delicate balks or partitions of fibres, which are arranged so as to enclose irregular cavities containing a softer mass of cells or rudimentary fibres. On the whole, the ordinary situation of the deposit (whether medullary or colloid) seems to show that it is rarely or never produced by any genuine transformation of the preexistent scirrhus; and that though it is not at all improbable that a moderate interstitial deposit of new cancer often admixes a certain quantity of medullary or colloid with even the deepest parts of such a scirrhous tumour, no real conversion of the previous scirrhus ever takes place. At any rate, we have no evidence of such a process; and all the softening that

* It may be objected, that definite instances of this kind are too rare to justify the confusion of terms that would arise from calling them colloid. The validity of such an objection I quite admit. But I think it would be impossible for any one to analyse and study the records of even modern pathology, without coming to the conclusion that (rightly or wrongly) the name "colloid" cancer of the stomach is often applied to a very moderate exaggeration of an appearance which few cases of scirrhus of this organ are altogether devoid of. Hence I offer the above remarks chiefly as a clue to these records, and as a hint with respect to the physiology of cancer; though I should be happy if they called attention to a vagueness (if not inaccuracy) from which it is but justice to our German fellow-labourers in the field of Pathology to say they seem comparatively free.

occurs in the scirrhus itself seems quite explained by the degeneration of those healthy tissues which it has enclosed within its mass, and by the true cancerous softening to which it is liable, in common with all other forms of this adventitious growth.

With respect to the origin and growth of the two other and rarer varieties of gastric cancer, in their uncombined form, I have little to add (certainly nothing material) to the excellent account given by Rokitansky,* and to Dittrich's† valuable and original commentary upon it. I think, however, that it might be laid down more definitely than they have done, that the medullary deposit, as a rule, begins more immediately beneath the mucous membrane, and the colloid beneath the serous membrane, than does the ordinary scirrhus or its combinations. But the exceptions to this rule are so numerous as to deprive it of much claim to our notice.

The villous cancer of the stomach seems to be strictly a deposit at or near the basement membrane; not merely (as has been suggested) an isolated medullary deposit in the sub-mucous areolar tissue growing by continual accretion on its mucous side, and thus sometimes enlarging to a tumour here, of which the original basis remains a mere peduncle or stalk—but rather a cancer of the mucous membrane itself, the very proximity of which is the chief cause of its undergoing so great a change of form, while it suffers so little direct injury to its structure. That the deposit is, in the majority of instances, *within* the basement membrane, the microscope leaves little reason to doubt; a proposition which, if true, establishes an important distinction between this and the epithelial cancer which has been sometimes regarded as its analogue.

It is only in conjunction with the preceding remarks that we can accept or interpret numbers like those I have already mentioned, in alluding to the frequency of the three forms of cancer in the stomach. In point of fact, the numbers given probably represent little more than instances in which the corresponding form of cancerous deposit formed a large or predominant portion of the whole mass. Of the 32 instances of medullary cancer, for example, it is probable that several at least were merely large combinations or admixtures of this growth with what was originally and essentially a scirrhus. And of the 17 cases of colloid, I suspect even a larger proportion ought to be referred to a similar category. This conjecture is confirmed by the observations of Dittrich,‡ who found only 3 out of 11 colloid cancers to be pure and uncombined examples of this variety; the remaining 8 being combined, 7 with scirrhus, (2 of these also with medullary cancer) and 1 with medullary cancer.

The numbers I have collected include altogether 34 examples of colloid in 417 cases of gastric cancer. If the proportion observed by Dittrich were applied to these, it would reduce the above number (which about equals $8\frac{1}{6}$th per cent.) to 9 cases of pure alveolar cancer, equivalent to a proportion of $2\frac{1}{6}$th per cent.

The destructive process which ultimately engages the free or internal aspect of a gastric cancer, after its mucous membrane has been removed by the processes already mentioned, is of especial interest, from its close

* Op. cit. † Op. cit. ‡ Op. cit., p. 22.

connexion with the appearances by which the cancerous and ulcerous stomach sometimes resemble each other.

Perhaps the commonest method by which cancer simulates ulcer of the stomach, is that sometimes brought about in the course of the process described by Rokitansky; in which, generally with little or no previous formation of medullary or areolar cancer, the scirrhus, denuded by ulceration or sloughing of the mucous membrane, gradually sloughs in round circumscribed patches, so that its tissues exfoliate in successive strata, and thus excavate tolerably smooth pits or fossæ in the crude cancerous mass. Here, however, the distinction is rarely by any means difficult. The bottom of the pit remains more or less sloughy; or even if this characteristic appearance is disguised by a casual hæmorrhage, such as might equally complicate an ulcer, still the quantity of the surrounding mass, and especially its histological characters as seen under the microscope, would rarely allow any room for doubt. Besides, in the ordinary ulcer, thickening is scarcely compatible with sloughing; and is almost always associated with a history that sufficiently refutes the notion of malignant disease.

But while there can be no doubt that in the majority of cases a careful examination would scarcely allow any room for hesitation as to the ulcerous or cancerous nature of a given specimen, it seems probable that there are rare instances, in which nothing that the most sedulous examination of the dead body could reveal, would justify a decision. The smooth circular excavation of a scirrhus may obviously imitate the ulcer surrounded by a hard and thickened mucous membrane. If the cancerous hollow be for the time denuded of slough, or covered with blood, we lose another of the means of distinction that a mere inspection could afford us. Would the physical and microscopical characters of the hardened periphery of the sore always decide the question? I am afraid not. In other words, though my own limited experience has never yet left me in doubt, I have known instances in which the dense cicatrix around an ulcer has yielded portions respecting which, had I seen them separately, and been obliged to decide their character solely from their appearance, their incision, and even their microscopic anatomy, I should have had great difficulty in coming to a decision. In respect to the microscope, indeed, the great variations in the quantity of cell-growth contained in a scirrhous tumour, and in the developmental stage attained by its fibres, will sometimes render its diagnosis from the more complete forms of cicatrix-tissue (containing a fair proportion of fibres and long fusiform cells) anything but an easy task.

Such equivocal cases have suggested themselves in a still more marked form in some of the numerous records and specimens to which my researches have led me. This form, however, I would rather regard as a contingency, than definitely announce as a fact. Is it not possible, I would ask, that the sloughing or ulceration of a limited deposit of scirrhus may not only imitate a mere ulcer, but produce or become one? May it not happen (if only in infinitely rare cases) that the destructive process destroys all the malignant deposit present, leaving behind it neither more nor less than a circular ulcer, bounded by healthy, or at most inflamed, tissue?

(*To be continued.*)

PART FOURTH.

Chronicle of Medical Science.

ON THE DEVELOPMENT AND STRUCTURE OF THE TEETH OF MAMMIFERA.

BY DR. ADOLPH HANNOVER, OF COPENHAGEN.

(*Translated by* T. WHARTON JONES, F.R.S.)

THE following is a translation in abstract of Dr. Hannover's admirable monograph, Ueber die Entwickelung und den Bau des Säugethierzahns, recently published in the 'Nova Acta Acad. Cæs. Leopold. Nat. Cur.,' vol. xxv. part ii.

In the mammifera, the dental sac contains four distinct elementary structures.

1. *The Dentine Germ.*—This is a soft body at the bottom of the sac, which early presents the permanent form of the crown of the tooth, and is, by a process which Dr. Hannover names *dentification*, converted into the dentine or ivory of the tooth.

2. *The Enamel Germ.*—This immediately invests the dentine germ, and consists of cells vertically disposed, which are at first soft, but afterwards converted by the deposition of earthy matter into the hard columns composing the enamel.

3. *The Cement Germ.*—This, the outermost of all the structures of the dental sac, is, by a process of true ossification, formed into the cement or bony substance of the tooth.

4. *Membrana Intermedia.*—The cement germ is not in immediate contact with either the enamel or the dentine, but is separated from them by a particular membrane, which, at the crown of the tooth, has attached to its inner surface the enamel cells, and consequently is interposed between the cement germ and the enamel; whilst, at the root of the tooth, where the enamel germ ceases, the membrane in question lies between the cement germ and the dentine germ. This membrane, which Dr. Hannover names *membrana intermedia*, and to which he considers due attention has not been hitherto directed, is found, in the fully-developed tooth, metamorphosed into the *stratum intermedium*.

PART I. — HISTOLOGICAL HISTORY OF THE DEVELOPMENT OF THE TEETH OF MAMMIFERA.

1. *Dentine Germ.*

In the new-born infant, the dentine germ of the permanent incisor tooth consists of a homogeneous, reddish-yellow, semi-transparent substance. The edge of the dentine germ being older than the base, the earliest conditions are best seen, the nearer the base the germ is examined.

At the base the germ consists of very small cells, *dentine cells*, deposited without any definite order in a transparent intercellular substance. This being in small quantity, the cells lie closely pressed together, and are thus rendered partly angular. The nucleus, which is very little smaller than the cell itself, is rather dark, coarsely granular, round, oval, or angular.

Whilst the cells of the dentine germ are in this very early stage towards the base, their development is already much further advanced in the free dark border of the same germ.

The first change perceptible in the cells of the free border, consists in their elongation and arrangement in rows, one behind the other. This change is not perceptible in the cell membrane, on account of its transparency, but is very distinct in the darker nucleus. The nuclei thus become more slender, and four to five times longer than before, lie, on the whole, parallel with each other, and are disposed perpendicularly at the border of the germ. From the ends of the elongated nuclei there are fine prolongations, which appear light or dark according to the change of the illumination. Dr. Hannover has not been able to determine whether these be prolongations of the nucleus or cell itself, but from analogy, thinks the latter more likely. The prolongation of the posterior end of one cell coalesces with the anterior prolongation of another; and in this coalescence of the prolongations and processes, the *tubes* of the *dentine*, together with their ramifications, originate.

When the nucleus is at last fully drawn out in its length, the distinction between it and its prolongations no longer exists. The nucleus forms the lumen and the contents of the permanent dentine tube; the walls of which, on the contrary, are formed of the cell membrane and cell contents.

When the tubes have been thus formed, they become hardened by the deposition of earthy matter. This process, which Dr. Hannover names *dentification*, takes place very soon after the first appearance of the germ, and commences in the oldest part or the free border of the germ, so that the cell series in the border may be found dentified, whilst the back-lying layer is still in a soft state.

Dentification, or the deposit of earthy matter, takes place first around the lumen of the dentine tubes, later in their walls, and in the intercellular substance.

It is to be noted that the lumen of the tube is very often looked upon as the tube itself, whilst the walls of the tube have been altogether overlooked, or mistaken for interspaces between the tubes.

The contents of the tubes are, in the fresh state, limpid like water, fluid, and of the same nature as in the still soft prolongations of the cells; yet, in all probability, impregnated with earthy salts quite as early as the walls of the tubes.

Contemporaneously with the dentification of the tubes, proceeds the solidification of the transparent intercellular substance. It appears in this state structureless and clear, but somewhat darker than the walls of the dentine tubes. In the hardened state it merits the name of intercellular substance.

From the preceding demonstration, it is seen that the dentine tubes have their origin immediately in the coalescence of the dentine cells; and that there is nothing like a secretion of dentine by the germ. This being the case, the ground for the admission of a particular *membrana præ-formativa* is taken away.

2. *Cement Germ.*

Although the enamel attains its full development earlier than the cement, Hannover has found it more convenient to give the description of the cement germ before that of the enamel germ.

The nature of the cement germ has been mistaken by many of even the most recent observers, because they have incorrectly attributed to it a part in the formation of the enamel. The cement germ has nothing to do with the formation of the enamel, and is, indeed, wholly separated from it by a particular membrane —that called by Hannover *membrana intermedia*.

The formation of the cement or bony part of the tooth takes place through the cement germ in a manner peculiar, and hitherto unknown. Whilst the dentine tubes are immediately developed from the dentine cells, the cement, on the contrary, attains its permanent form only by a threefold process. In the first stage of development, the primordial cells, of which the cement germ consists, change completely into fibres; in the second stage, true cartilage cells form in the now fibrous primordial germ; the third stage is that of ossification.

For the investigation of the earliest conditions of the cement germ, Hannover has found the cement germ of the permanent incisor of the new-born infant likewise the best adapted. The cement germ surrounds, like a cap, the dentine germ on all sides, with the exception of the base. From the dentine germ, however, the cement germ is everywhere separated—first, by the *membrana intermedia*, and second, by the enamel cells of the crown.

In the earliest period the cement germ is a limpid or slightly reddish fluid. The only solid bodies which occur in this substance are the primordial cells. After the cement germ has attained a slightly gelatinous consistence, the cell-membrane of the primordial cells shoots out on all sides numerous prolongations, so that the cells become branched or stellate. From the prolongations are given off finer branches. At last, the branches of several cells meet each other and inosculate.

The cell membrane, which, as well as the ramifications, is very pale and delicate, is expended in the formation of the increasing number of prolongations; so that the appearance is presented as if the latter proceeded from the nuclei. The nuclei, however, during the changes of the cell, remain unaltered. After the still isolated primordial cells have become stellate, the previously gelatinous germ begins to become more consistent; and a fine, transparent, and structureless intercellular substance is seen, in which the stellate cells are deposited.

The primordial germ now approaches its last stage; the prolongations of the cell membrane have become so long and fine as to be actual fibres. The primordial germ forms a semi-transparent, homogeneous, viscid tissue, in which there are bundles of fibres, very fine and smooth. The fibres run parallel, seldom irregularly or intercrossing, and do not ramify. On the individual fibres are long, fine, fusiform nuclei, evidently the remains of those nuclei found in the primordial cells, which were at first round, and afterwards stellate and ramified.

Hereupon closes the primordial stage of the cement germ. From being a fluid, limpid mass, the germ acquires a gelatinous consistence and colour, and is at last changed into a fibrous cartilage: for in the fibrous substance at last formed in the primordial germ there appear cartilage cells. Hannover has observed distinct cartilage cells in the back teeth of new-born infants, or infants some months old; but in human teeth the further development of the cement germ, at least around the crown of the tooth, is arrested, and the fibro-cartilage is not converted into true cartilage and bone, as in animals. For the study, therefore, of the second stage, which comprises the appearance of cartilage cells, Dr. Hannover takes the tooth of the new-born foal.

The cartilage cells appear isolated in the fibrous primordial mass, and without connexion with it. They are of different sizes, round or slightly oval, have coarsely-granulous contents—or rather, coarsely-granular nucleus—but no distinct nucleolus. As they increase in number, which they do first towards the *membrana intermedia*, the fibrous structure of the substance disappears, and it becomes more uniform.

Endogenous development, the presence of two cells in one mother-cell, or of several nuclei in the same cell, Dr. Hannover has not observed. Dr. Hannover remarks, in a note, that what are usually called cartilage cells, he does not consider cells, but only nuclei. Complete cartilage cells, he says, occur rarely; most frequently they are met with in enchondroma. Nevertheless, to avoid confusion, Dr. Hannover continues to use the name, *cartilaginous cells.*

The transition of fibro-cartilage into *bone*, in the third stage of development of the cement, commences soon after the appearance of the cartilage cells.

The ossification of the cement begins with an earthy deposition in the intercellular substance. This takes place partly in the form of a coarsely-granular crumbly mass, partly in the form of shorter or longer fine fragments, arranged in definite directions. The fragments are, however, perhaps only fragments of branches of bone-corpuscles which lie in another plane, and have been cut through in making the section for examination.

As the earthy deposition increases, the cartilage cells (nuclei) are p ssed together, and become small and angular. Pointed prolongations shoot from them, being the first beginnings of the future branches of the bone-corpuscles.

The bone-corpuscle is not a cavity in which can be distinguished a surrounding membrane and proper contents; it is, on the contrary, a solid body arising from the conversion or metamorphosis of the gelatinous firm substance of the cartilage cell.

The Haversian canals make their appearance in the primordial germ as soon as the formation of the cartilage cells begins, or perhaps somewhat later.

The walls of the Haversian canals ossify at the same time with the ossification of the rest of the intercellular substance.

3. *Enamel Germ.*

The investigation of the development of the enamel is also most conveniently made on the teeth of new-born infants.

When the dental sac is opened under water, without tearing the *membrana intermedia*, the cement germ escapes like a fine cloud in the water; but if the *membrana intermedia* is at the same time opened, the cloud is mixed with the enamel cells, or earliest elements of the enamel. These cells are usually united into a sort of membrane.

The enamel germ consists throughout of cells. There is no intercellular substance. In its earliest stage—e.g., in the first permanent back-tooth of the new-born infant—the cells are found still isolated.

The isolated enamel cells are usually round, seldom oval, somewhat smaller than the primordial cells of the cement germ, but much more coarsely granular and darker. They also appear softer. The nucleus is small, round, oval, or angular; has a sharp linear contour, and a peculiar glance. One or two punctiform nucleoli are commonly visible. Two cells enclosed in a mother cell are sometimes seen, and the occurrence of two nuclei in the same cell is likewise not uncommon.

The first change which the enamel cells undergo, after being at first isolated and round, consists in their becoming closely aggregated together, and assuming an angular form, so that, in their membraniform union, they present almost the appearance of a tessellated epithelium. The cells lie pressed together, but each individual cell maintains its independence without coalescing with its neighbours, so that their boundary contour-line is always visible.

The membraniform expansion of the enamel cells is at first very soft and thin, but afterwards becomes firmer, so that it may be separated under water, in small flakes, from the *membrana intermedia*, or from the dentine germ.

The cells, on account of the complete want of intercellular substance, being continually pressed against each other, their angular form is changed into that of longish rectangula, the ends of which are abruptly cut off or rounded. One end is thicker than the other, or one end is pointed—a disposition which becomes more marked in a subsequent stage. In the middle of the rectangulum is the nucleus.

These elongated rectangular cells are, for the most part, disposed vertically between the *membrana intermedia* and the dentine germ. They adhere more firmly to the *membrana intermedia;* and to the end corresponding to this membrane, their nucleus is nearer than to the end next the dentine germ.

The cells are continually growing. The end fixed to the dentine germ increases in length, whilst the nucleus end always remains nearest the *membrana intermedia.*

Even at an early period of their development, the enamel cells are, at the end next the dentine germ, pointed, or even drawn out into a filament; or, as the late Mr. Nasmyth described it, "furnished at one extremity with a delicate prolongation."

The origin of this filament is unknown to Dr. Hannover. It usually appears as a continuation of the cell, and the longer the filament, the more slender the cell itself; but, on the other hand, it is to be observed that the contour of the filament is sharp, and does not appear to be formed of the cell-membrane, the contour of which is soft-looking.

After the cells have attained a certain length by continued growth of the end next the dentine germ, their development passes into the last stage, that of *calcification.* The deposition of earthy matter into the cells takes place first in the ends next the dentine, and then spreads in the direction towards the *membrana intermedia.* Even when one part of the cell is calcified, the remaining part is still soft, and always recognisable by the nucleus at the end next the *membrana intermedia.* The nucleated end is the last to become calcified.

As there is every reason to admit that the same individual enamel cell extends from the dentine germ to the *membrana intermedia*, it follows that the enamel cells are the longest cells in the human body, their length corresponding to the thickness of the completely-formed enamel.

As the enamel cells become calcified they assume the form of six-sided columns, and most probably they assume this form even some time before calcification.

It has been above stated that the enamel cells are disposed vertically, but this is not exactly the case always, for there are parts of the enamel in which the cells form a more or less sharp angle with the *membrana intermedia* and the dentine germ. Moreover, the columns are not always straight, but very often undulating, and sometimes they even cross each other.

4. *Membrana Intermedia.*

This is a fine thin membrane, which must not be confounded with the membraniform expansion of the enamel cells, and which lies on the inside of the cement germ, between this and the enamel cells.

It consists of a homogeneous substance, in which numerous small round or oval, angular or pointed nuclei, without distinct nucleoli, are embedded. The boundary towards the cement germ is sharp and linear, and the cells of the cement germ lie compressed on it. The boundary towards the enamel cells which sit on the opposite surface, is likewise sharp. The enamel cells admit of being easily detached from the inner surface of the *membrana intermedia*; whilst, on the contrary, it is not without difficulty that the *membrana intermedia* can be separated from the cement germ.

The *membrana intermedia* does not belong exclusively to the crown or the enamel, but is continued on the root, where it separates the dentine from the cement; thus lying, as in the crown, on the inside of the cement. Dr. Hannover has, however, not been able to isolate it here, because, immediately on the formation of the outermost stratum of dentine, it forms an adhesion with it, and can only be recognised in the fully-formed tooth as the *stratum intermedium.*

General Remarks on the Order of Succession in the Development of the Dental Elements in the Mammifera.

According to the view, that the development of the teeth begins with the formation of papillæ at the bottom of primitive grooves in the jaws—grooves at first open, but which afterwards close and become subdivided by transverse partitions—according to this view, the papilla is the future dentine germ; the cement germ is formed in the follicular stage of the development, after that the previously open papilla-cavity has closed, by deposition of a granular substance. The formation of the cement germ takes place, therefore, subsequently to that of the dentine germ.

If we view the enamel cells as a continuation of the epithelium of the mucous membrane of the mouth and of the dental groove, the foundation of the enamel must exist before the formation of the cement germ; and this would argue that the formation of the enamel is independent of the cement germ or the previously so-called enamel organ.

Although, however, the order of succession in the development of these three dental substances appears to be as stated, it is not a uniformly progressive one, and there are periods when the development of the one substance appears to anticipate that of the other.

The dentine germ is found at first as a solid body at the bottom of the dental groove and of the dental sac. The point or free border appears earliest, is composed of cells at first isolated, but which subsequently elongate, meet each other, and coalesce into long rows; lastly, the dentification of the tubes commences also at the point, and this takes place before any one of the other substances has attained its permanent form.

The resemblance of the enamel cells to a tessellated epithelium is unmistakeable, and in so far as the epithelium of the dental groove exists before the papilla, it may be asserted that the enamel is the first formed substance of the tooth. Nevertheless, the cells in question first acquire the character of enamel cells only after the papilla has sprung forth. It is here also the point of the papilla on which the enamel cells, after having acquired the elongated form, first arrange themselves beside each other, and become united into a fine membrane, the peripheral part of which is always found in a less advanced stage. Likewise, the calcification of the individual cells commences first on the point or the free border of the dentine germ. The end towards the dentine germ calcifies first, is supported only on the dentified dentine germ; whilst the root end of the cell, which contains the nucleus, is in close union with the *membrana intermedia*. With the calcification of this end the formation of the whole enamel column is completed.

Dr. Hannover has not yet satisfactorily traced the first formation of the *membrana intermedia*.

The cement germ arises from the primordial germ, which is at first fluid, subsequently gelatinous, then converted into fibro-cartilage, and lastly ossified. In its development, it is always behind the other dental substances. The ossification commences first in the part lying next the *membrana intermedia*, but the ossification can only commence at the period when the enamel cells are completely calcified, and the enamel has acquired its permanent thickness. The slower and later growth of the cement germ is easily observed.

In man, the crowns of the teeth are covered by a cement germ, but it never ossifies there. In ruminants—the elephant, &c.—the crowns are covered with a strong development of cement, both around the circumference generally, and down between the pointed parts. The difference is owing to this,—that in man, the crown, by its growth, pushes through the cement germ in cutting the gum, and a stop is put to the further development of cement. It is thus also that we find, in the ruminants and horse, in teeth which have just cut the gum, large openings in the middle of the cement, which are owing to the arrested ossification of the cement germ, occasioned by the protrusion of the crown through the gum.

When the formation of the enamel is completed, and the root commences to form, the dentine germ is very closely surrounded by a firm membrane, which consists of the condensed remains of the *membrana intermedia* and cement germ, and contains small nuclei and fibres closely pressed together, the latter belonging to the cement germ. Cartilage corpuscles Dr. Hannover has not observed in it—at least, not in the upper part of the root of the human incisor.

All the dental substances increase in hardness during their growth, which is probably occasioned partly by the loss in water, partly by the stronger pressing together of the elementary parts.

Part II.—Histology of the Teeth of the Mammifera.

1. *Dentine.*

The dentine forms the largest portion of the tooth, is non-vascular, white, semi-transparent, and intermediate in hardness between the cement and the enamel. It is composed of thick-walled tubes, imbedded in an intertubular substance.

The walls of the dentine tubes are very thick in proportion to their lumen; they cannot, however, always be distinguished, because they usually coalesce with the intertubular substance. The thickness varies in different animals; in the horse they are very thick; thinner in oxen and in man. The greater apparent fineness of the tubes towards the periphery of the tooth, depends on the narrowness of the lumen, not on a greater thinness of the walls of the tubes themselves.

The contents of the tubes are in the fresh state composed of a transparent fluid, with calcareous matter in solution; in dry teeth—such as are commonly made use of for making sections—the tubes appear empty, or filled with earthy particles. When the tube is empty, the lumen is seen, even in fine tubes, bounded on either side by a dark line. The lumen itself is clear, and the clearness increases on the addition of a fluid, without, however, being accompanied by a disengagement of air globules. The same tube may be at one place empty, at another filled with earthy particles.

A circulation of nutritive fluid in the perfectly-formed dental tubes, such as has been supposed by Krukenberg, cannot, Dr. Hannover says, be admitted.

The peripheral end of the dentine tubes calls for some detailed consideration. According to Dr. Hannover's observations, the outermost ends of the trunks and branches are lost in the surrounding intertubular substance, their walls coalescing with it. Loops, or anastomoses with neighbouring or more distant trunks and branches, are only exceptionally formed. Many branches, however, sink into small calcareous cavities.

The admission of an anastomosis between tubes is often founded on the appearances presented in those cases in which the original globular basis of the dentine has remained still visible, the interspaces and passages between the globules in such a case being mistaken for branches of dental tubes.

Contrary to a common opinion, Dr. Hannover insists that there is nowhere any connexion between the dentine tubes and the ramifications of the bone corpuscles of the cement, the dentine and cement being separated from each other by the *stratum intermedium*.

Dr. Hannover can as little admit a communication of dentine tubes with the enamel. The idea of a transition of dentine tubes into tubes between the enamel fibres, is completely opposed by the development of these two substances. The appearances which have been supposed to indicate a transition, Dr. Hannover thinks must have been in some cases owing to the sections having been oblique, so that a part of the dentine overlapped the edge of the enamel; in other cases, the passages or striæ into which the dentine tubes appeared to pass may have been merely an optical expression of the angular form of the enamel columns.

Earthy deposits, sometimes met with in the substance of the dentine, having some resemblance to bone corpuscles, have been mistaken for such, and the erroneous notion of a transition of dentine tubes into bone corpuscles founded thereon.

The intertubular substance in which the dentine tubes are embedded is, as is known, clear, homogeneous, and structureless.

The substance in which the lumina of the dentine tubes are embedded, also appears in a form which recals to mind the development of the dentine tubes from round cells, and most probably does depend on this original form.

This is a point not yet completely cleared up; Dr. Hannover therefore expresses himself cautiously in regard to it, and speaks of the substance only as it has appeared to him in many cases between the lumina of the dentine tubes. In man, Dr. Hannover has often observed it, but not nearly so distinctly as in the ox, the horse, and most cetaceans. In the ox, for example, Dr. Hannover found, near the enamel, a globular structure in which the individual globules were separated by very clear passages from each other. By the anastomosis of the passages, there was produced a retiform appearance, which has erroneously given origin to the admission of a retiform anastomosis between the ramifications of the dentine tubes: the proper ramifications ran past these passages without forming any junction with them. The globular structure consisted of round or oval bodies, of the size of a human blood-corpuscle to that of a frog's; sometimes, when the bodies were pressed against each other, they were angular. Their substance was somewhat more granular and less transparent than the rest of the dentine. On and between the globules were shapeless earthy masses, similar to those above noticed. They lay partly close to the boundary of the enamel, partly at some distance from it, and had a deceptive resemblance to bone corpuscles, for which they have been mistaken by those observers who admit a transition of the ramifications of the dentine tubes into bone corpuscles.

The innermost part of the tooth, or that part of the germ which is latest of dentifying, presents in most animals indications of imperfect development. Very generally it consists of an irregular aggregation of imperfectly-formed dentine tubes. This is the case in almost all teeth, and the difference from the rest of the dentine is perceptible even to the naked eye, on account of the greater transparency of the parts. The lumina of the tubes are seen in a structureless, clear mass, frequently in branches irregularly winding or suddenly interrupted, in fragments of different length and form, diverging in their direction, however, usually from within outwards. Their thickness does not appear, on the whole, changed; the branches, on the contrary, which are sometimes absent, are in other cases very numerous, and finer than elsewhere. Dr. Hannover thus found it in man, the dog, the bear, sow, horse, &c., in which the fragments frequently form large loops. Often the inner transparent part, in which these irregular tubes occur, is sharply defined from the rest of the dentine, and forms a centre of greater or less extent, outside which the regularly-arranged dentine tubes begin; for example, in the sow, horse, ox, &c. In the otherwise uniform clear mass in the axis of the tooth, the globular form of the intertubular substance may also, in rarer cases, be recognised, as in the dugong. In other instances, there is a total absence of dentine tubes in the innermost part of the dentine, and the dentified germ consists in the middle of only a uniform clear mass.

True Haversian canals of the same nature as in the cement do not occur in the dentine. There are, however, in the middle of the dentine, canals which much resemble the Haversian canals, but which have probably a different origin. They are met, for example, in the ox, in the form of round or oval, though more frequently irregular, sections, filled with a yellow granular mass, or they appear black and opaque when extraneous matter has penetrated them in grinding. Dr. Hannover believes them to be remains of vessels. The canals mentioned by Owen and Tomes, in the inner part of the dentine of several rodents, should not be looked upon, Dr. Hannover thinks, as Haversian canals, especially as, according to Tomes, they run parallel with the dentine tubes, and do not anastomose, but branches of the dentine tubes open into them. It is therefore improper to speak of the vascularity of dentine in the same sense as the vascularity of bone is spoken of. The admission of Haversian canals in the dentine is founded only on the incorrect comparison of the dentine with bone, and the process of dentification with that of ossification; therefore, also, the name of *osteo-dentine*, which Owen has given to the central clear substance, with free and irregularly-disposed dentine tubes, in several mammifera, is objectionable.

As in the interior of the dentine, canals occur which have been taken for Haversian canals; so there also occur in it forms which are very similar to bone corpuscles, and which have been mistaken for them. They occur in the ox and bear, but are most frequent in cetaceans. Dr. Hannover believes them to be of the same nature as the pulp and pulp-cavity.

A similar origin, though a somewhat different structure, most probably belongs to the various forms which occur in the so-called granular substance (*Poudingue*, Cuvier). This substance is of very general occurrence in the interior of the teeth of the sea mammifera; in other animals it is only exceptionally present. In no respect, Dr. Hannover says, can the structure of the granular substance be compared, as Retzius thinks it may, with the structure of the cylindrical bones. Both the anatomical and histological structure of the substance in question, Dr. Hannover insists, is opposed to any resemblance with bone.

When the tooth does not remain open throughout the whole period of life, the point of the root is gradually closed by the external cement; the growth of the dentine is ended, and bloodvessels and nerves can no longer penetrate the pulp-cavity. In some teeth, a cavity is still found filled with the remains of the dentine germ; but in most cases the whole germ is dentified. The consequence of arrested nourishment is especially evident in the point of the root. Not only with the microscope, but also with the naked eye, it is frequently seen that the dentine and the cement are not sharply separated, as in the other parts of the tooth; but that the transition between the two takes place in an unmarked manner. Therefore, the irregularly-disposed dentine tubes mingle with bone corpuscles, without, however, this exceptional simultaneous presence being anything more than juxtaposition or accidental mingling. The ramifications of the dentine tubes, which are here often in extraordinary number, do not pass into the ramifications of the bone corpuscles. Sometimes the dentine tubes, sometimes the bone corpuscles, have the predominance. When the cement has been formed in great masses around the point of the root, the Haversian canals characteristic of this substance are also met with. The granular substance, likewise, together with its irregular earthy deposits, may be found mingled in the point of the root with the cement; and here the best opportunity is presented of observing the distinction between those earthy deposits and the bone corpuscles. Lastly, irregular smaller and larger cavities are met with, which are owing partly to an incomplete dentification, partly to the remains of bloodvessels, but which are not to be mistaken for Haversian canals.

2. *Cement.*

In the history of the development of the tooth, it has been shown that each *dentine germ* is originally surrounded by a *cement germ*, and is separated from it in the crown by the *enamel germ* and the *membrana intermedia*, but in the root by the *membrana intermedia* only. When the different dental substances have acquired their permanent form, the cement can therefore never be in immediate contact with the dentine; it is only at the point of the root, as has just been shown, that a mingling of the two substances takes place. Towards the enamel the cement is distinctly limited; it never mingles with the enamel columns, and easily separates from them completely, whilst the connexion with the dentine in the root is very firm.

Cement exists around every root, but not around every crown; for in teeth with conical dentine germs, the cement germ in general aborts round the crown, and does not ossify. The quantity of cement formed around the root of the conical dentine germ is very variable. Whilst the cement around the root of the teeth of man and the dog forms only a thin layer, its quantity in the dolphin is very considerable, and in the physiter almost as great as that of the dentine; as in these animals the crown of the tooth is very small, and soon worn away, it may with truth be said of them that they masticate with the roots of the teeth. In teeth with notched dentine germ, the cement, which is considerable in quantity, first surrounds the enamel of the crown, and then the dentine of the root. In teeth with a cup-shaped dentine germ, the outer cement exists only in small quantity, the inner predominates, and is distinguished, as before mentioned, from the outer by its yellow colour and opacity, the greater number and the size of the less-branched bone corpuscles, and the wide-spread Haversian canals. The inner cement, in particular, resembles still more closely true bony substance than the outer. In teeth with foliated dentine germs, the quantity of cement between the leaflets is considerable; in the periphery, small. Although the cement is not everywhere of the same thickness, still its quantity on the whole increases in all teeth from above downwards.

The cement, which is opaque, is softer than dentine, and is therefore always most worn away on the masticating surface; on the contrary, it is harder than bone. Its colour is dull white, grey, or yellowish, and at the same time speckled. In other respeots the structure of the cement in general is similar to that of osseous substance, and both are distinguished by bone corpuscles imbedded in a ground substance, and by Haversian canals; the bone corpuscles in bone are, however, usually more numerous, larger, and darker; the arrangement and number of the Haversian canals and the ground substance are also different.

As the cement does not lie in immediate contact with the dentine, there can be no communication of the ramifications of the bone corpuscles of the former with the ramifications of the tubes of the latter; in the dentine itself there are no bone corpuscles, as has been above insisted on.

Although the form of the bone corpuscles of the cement is very irregular, still their direction is, in general, such that their longest diameter is perpendicular to the length of the tooth. Around the Haversian canals the bone corpuscles lie, sometimes in no definite order, sometimes in concentric series, as in the bones; this is the case, for example, in the inner cement of the ox.

The number of bone corpuscles corresponds to the quantity of cement. They are entirely absent where the cement is thin, as in the neighbourhood of the crown of teeth with conical dentine germs—e.g., in man, dog, trichechus; in man, the part of the cement next the crown is quite clear and very brittle, so that fissures occur in it, as in the enamel.

The ground substance, in which the bone corpuscles are imbedded, is, in most cases, clear and transparent, but not so much so as the dentine. In other cases it is granular. In many it has the appearance as if stratified.

The Haversian canals of the cement do not completely agree with those of bone, in respect to their structure, number, and arrangement. They appear to have walls distinct from the rest of the cement. They serve for the passage of bloodvessels, which penetrate from without inwards. Where the cement forms only a thin layer, they are wholly wanting in all animals. In man, Haversian canals are met with only at the point of the root where the cement is in great quantity. In the horse and ruminants the cement, especially the inner, is pervaded by numerous and large Haversian canals. The opening of the ramifications of the bone corpuscles into the canals, Dr. Hannover has not been able to observe, such as is stated to be the case by Owen in the megatherium, in which the canals at the same time anastomose with each other.

The irregular cavities often seen in the point of the root above-mentioned in the description of the dentine, must not be confounded with Haversian canals. These cavities, first observed by Czermak in human teeth, are branched, thick, varicose canals which pervade the cement in different directions, penetrate it from without inwards, and reach as far as the dentine by their blind end. They appear to be abnormal.

Different also from Haversian canals are fine passages which occur even in thin layers of cement, and have a distant resemblance to dentine tubes, but which are seldom or never branched, and usually run transversely. The nature of these passages is still unknown. According to Tomes and Kölliker, they are frequently connected with the dentine tubes and bone-corpuscles. That they are connected with the dentine tubes, Dr. Hannover most decidedly denies, because the dentine is everywhere separated from the cement by the granular *stratum intermedium*. As to their connexion with the bone corpuscles, Dr. Hannover has observed this only in the dugong; in all other animals he has expressly remarked that they have no communication with the bone corpuscles.

3. *Enamel.*

The enamel characterizes the crown of the tooth. It is always interposed between the dentine and the *stratum intermedium*, outside which is the cement; where, however, the cement does not ossify, the enamel is covered only by the *membrana intermedia*, which then appears as the so-called enamel cuticle. This is the case in teeth with conical dentine germs; and Owen has probably confounded the *membrana intermedia* with the cement, when he speaks of the occurrence of a very thin layer of cement on the crown of the tooth in man and the ape. The enamel cuticle is soon worn away after the crown has broken through the gum, so that the enamel is laid bare. The enamel is in general thickest above, especially on the masticating surface; below, it is thinner, and ends with a free border, which is frequently covered with a thin layer of cement.

Although it cannot be said, with Owen, that the enamel is the least constant substance of the tooth, because every tooth possesses a crown, still Dr. Hannover remarks that the normal relations are frequently changed at an early period by the wearing away of the crown. Thus, very commonly the teeth of trichechus, dolphins, edentata, the incisors of the elephant, &c., are found without enamel—i.e., the crown, being so small, is soon worn away, and only the root remains; in the teeth of the young animals, however, the enamel of the crown is distinct enough.

The enamel is composed of columns developed from the enamel cells. The typical form of these columns appears to be six-sided. This form is, however, usually changed by the mutual pressure of the columns into a polygonal or flattened one. The thickness of the columns is variable. The columns present more or less distinct transverse markings, having some resemblance to those of muscular fibres. The cause of these markings lies in the calcification of each individual cell taking place in strata, hence the appearance is often more distinct in young than in old animals.

The direction of the enamel columns is either perfectly straight, or bent, or slightly tortuous. In regard to this, however, there is no definite rule, for in the corresponding tooth of the same order of animals, or of a different order, sometimes one, sometimes another direction of the enamel columns is observed. In the human tooth, for example, the columns are found sometimes straight, sometimes bent, sometimes wavy or intercrossing. As to the cause of this difference: the origin of the straight or wavy course in one direction it is easy to explain, but not so the crossing of straight or spiral columns.

Between the enamel and dentine there is no special membrane. The enamel columns directly touch the dentine. Dr. Hannover also denies that there are any such depressions on the surface of the dentine for receiving the ends of the enamel columns, as have been supposed by Lessing and Owen. In all the teeth of different animals examined by Dr. Hannover, he found the limit between the enamel and the dentine always sharply defined, and formed of a single dark line. Owen mentions a layer of cells between the dentine and enamel, in regard to which Dr. Hannover remarks that it is not quite clear to him what could have deceived that observer into this notion, unless it was the globular formation of the intertubular substance of the dentine, which in the ox, for example, may, like small protuberances, project into the enamel.

4. *Stratum Intermedium.*

This forms in the crown the limit between the enamel and the cement; and in the root, the limit between the dentine and cement. It is the metamorphosed *membrana intermedia* which, during the development of the tooth, is closely connected by its outer surface with the cement germ, and on the inner has attached to it the nucleated ends of the enamel cells.

The structure in question attains its permanent form after the enamel cells have become completely calcified in their entire length; as it is situated between the enamel cells and the cement, the ossification of the cartilaginous cement germ can commence only after the complete development of the *membrana intermedia*. It, therefore, always separates, in the crown, the enamel,—in the root, the dentine, from the cement. But as the cement germ round the crown of teeth with conical dentine germs does not in general ossify, but becomes aborted, so the *membrana intermedia* lies free on the surface of such crowns, and forms on the as yet unworn teeth the so-called enamel cuticle above noticed. This name, which was given to it by Kölliker, Dr. Hannover observes, the membrane merits neither by its origin, nor its structure, nor its nature. For the *membrana intermedia* does not belong exclusively to the enamel, nor does it lie superficially like a cuticle; whilst its structure, as before shown, has nothing in common with that of epidermis.

Erdl first demonstrated this membrane by means of the application of diluted hydrochloric acid; according to him, it has an epithelium-like aspect, and appears to be composed of small cells. By Owen it was probably mistaken for a thin layer of cement. Nasmyth supposed that he was able to trace it not only on the outer surface of the enamel of the human tooth, but also on the outer surface of the root; he called it "the persistent dental capsule," a name, Dr. Hannover observes, which might well be adopted, as the *membrana intermedia* does indeed represent a sac-like structure in the dental sac, and can be also still demonstrated in the fully-formed tooth. As, however, the membrane, though it covers the surface of the enamel, does not cover the outer surface of the root, as Nasmyth supposed, but is situated between the cement and the dentine of the root, Dr. Hannover does not consider it proper to retain the name of "persistent dental capsule," but proposes instead that of *stratum intermedium*.

What becomes of the *membrana intermedia* in the crown between the enamel and the cement, Dr. Hannover has not clearly made out. The limit between those two substances is formed of a dark, well-defined, but irregular line, sometimes double. There is no interspace between the enamel and the cement, only where they have split from each other—which readily takes place in drying or in making the preparation — there is formed an empty space, which is often filled with foreign matter. It thus appears that the *membrana intermedia* in the crown, which is so evident in the early stage, is in the completely-formed tooth no longer to be observed.

It is otherwise with the *membrana intermedia* in the root between the dentine and cement. Here it is converted into a particular stratum, having the aspect of a clear line of variable breadth, but always narrower than the *membrana intermedia,* whence the *stratum intermedium* has arisen, and opposes any communication of the dentine tubes with the ramifications of the bone corpuscles of the cement.

The stratum usually presents itself as a clear streak running along the whole length of the root; a frequent variation in its aspect, however, is occasioned by the deposition in it of masses of finely or coarsely granular earthy matter. These masses are usually collected towards the dentine in greater quantity, whilst the limit towards the cement is more defined. At the same time, the commencement of the dentine tubes is concealed among them. Such an appearance has especially occasioned the already often-mentioned erroneous supposition that dentine tubes pass into bone-corpuscles to which those masses very often have a great resemblance. The perfectly clear or the granular and dark aspect of the *stratum intermedium* may be observed in the same tooth; much depends on the thickness of the preparation, and it is especially in thick preparations that the shapeless, earthy masses of the *stratum intermedium* are most readily mistaken for bone corpuscles. This, according to Dr. Hannover's experience, was the case with Retzius's preparations of the teeth.

From Dr. Hannover's observations, it is seen that the *stratum intermedium* may present itself with a very variable aspect, but it always serves for the separation of the dentine from the cement. Only in the point of the root it is frequently indistinct, and there arises that previously-mentioned mingling of dentine and cement, but which is not accompanied by a real communication of the dentine tubes and the ramifications of the bone corpuscles.

The *stratum intermedium* has been seen by some previous observers, but its nature not recognised, because the *membrana intermedia,* whence the stratum arises, had been overlooked.

HALF-YEARLY REPORT ON PHYSIOLOGY.

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I. Food and Digestion.

1. F. S. Smith: *Experiments on Digestion.* (The Medical Examiner, vol. xii., July and September, 1856. Philadelphia.)
2. Hanlo: *De Extracti Carnis Frigide Parati, virtute nutrienti.* (Trajecti ad Rhenum, 1855, and Schmidt's Jahrb., vol. xci. p. 145, 1856.)
3. Jos. Jones: *Digestion of Albumen and Flesh, and the Comparative Anatomy and Physiology of the Pancreas.* (The Medical Examiner, vol. xii., May, 1856. Philadelphia.)
4. Skrzeczka: *Quæritur, quomodo Caseinum et Natrum Albuminatum pepsino afficiantur.* (Diss. Inaug., Regiomonti, 1855, and Canstatt's Jahresber. d. Physiologie für 1855, p. 184.)
5. F. Hoppe: *On the Influence of Cane Sugar on Digestion and Nutrition.* (Virchow's Archiv, vol. x. pp. 144 ss., 1856.)
6. Colin: *On the Formation of Sugar in the Intestinal Canal, and its Absorption by the Chyliferous Vessels.* (L'Union, No. 41, 1856; and Schmidt's Jahresb., vol. xc. p. 273.)
7. Funke: *Contribution to the Physiology of Digestion.* (Siebold and Kölliker's Zeits. für Zool., vol. vi. p. 304, and vol. vii. p. 315.)
8. Zenker: *On the Chyliferous Vessels in the Intestinal Mucous Membrane.* (Zeits. für Zool., vol. vi. p. 321.)
9. C. E. E. Hoffmann: *On the Absorption of Mercury and Fat, &c.* (Würzburg, 1854; and Canstatt, l. c. p. 80.)

Alexis St. Martin, the Canadian, so well known through Beaumont's experiments, has again been made the subject of scientific observation by Dr. Smith, of the Pennsylvania College. This author occupies himself, in the present essay, principally "with the nature of the acid contained in the gastric juice, and the influence of this secretion upon the various alimentary principles as classified by Prout, to wit, saccharine, oleaginous, and albuminous food." Regarding the temperature of the stomach, Smith observes, that it was during the progress of digestion about 100° to 101° F.; in the state of fasting, 98° to 99°. The reaction of the gastric fluid, while digestion was going on, was invariably, and with all kinds of food, acid; while the fluid obtained from the empty stomach was always neutral. Concerning the cause of this acidity, repeated experiments and analyses (the latter performed by Professor R. E. Rogers, of the Pennsylvania University) lead to the following conclusions:—1. That the acid reaction is not due to the presence of *phosphoric acid,* or acid phosphate of lime:* 2. That if *hydrochloric acid* is present, it is in very small quantity: 3. That the main agent in producing the acid reaction is *lactic acid.*

In regard to the influence of the gastric juice upon *albuminous* substances, the author offers the following experiment:—"Four ounces of rarely-done beef-steak were given to St. Martin at 10 A.M., May 5th, after a light breakfast of bread and coffee, at 6 A.M., of the same day. No fluid was allowed to be taken in connexion with the beef, nor any other article of food. At 12 M., of the same day, St. Martin was subjected to examination. On pushing back the fold of mucous membrane which acts as a valve to the fistulous orifice, a considerable amount of fluid was readily distinguishable in the stomach, mixed with bubbles of air, but no solid matter was visible. About a fluid ounce and a-half of this fluid was withdrawn from the stomach by a catheter, with the effect of producing nausea, which pre-

* Blondlot: Traité Analytique de la Digestion. 1843.

cluded the possibility of obtaining more. . . . . Specific gravity, 1·009. Numerous flocculi were visible to the naked eye. . . . . It was almost entirely inodorous, viscid, and to the taste decidedly acid. The microscope revealed amorphous granular matter, mucous corpuscles, granular cells, and a few epithelial cells; a few transversely-striated muscular fibres, some almost uninjured, some broken down, and with the sarcous elements liberated. Numerous oil-globules were also distinctly visible, and a few fibres of yellow elastic tissue. The bulk of the material consumed as food had undergone entire solution, and had wholly lost its characteristic appearance. A portion of the supernatant fluid was boiled actively, without, however, presenting the slightest trace of coagulation. The mineral acids had no effect upon it while cold, but when boiled with strong hydrochloric acid, the purple colour of the protein bodies was distinctly manifested. The addition of acetic acid rendered the fluid rather more clear than before. The action of alkalies on the fluid was not tried; but Trommer's test gave no evidence of the presence of glucose." (*loc. cit.* p. 515.)

It is easy to recognise in this description the characters of Mialhe's *albuminose*, or Lehmann's *peptone*.

"The conclusion from this observation is, that *the gastric juice is a true solvent for animal food.*" (*loc. cit.* p. 515.)

From the number of oil globules visible under the microscope, the author is inclined to support the view "that *fatty matters* undergo no change in the stomach beyond that of disaggregation."

In order to show the influence of gastric juice on the *amylaceous* articles of food, Smith relates two experiments. In one of them a portion of wheaten bread was given to St. Martin while fasting, which was deliberately masticated by him; in the other, a portion of bread moistened with water, was introduced into the fistulous orifice, "and St. Martin was requested to swallow as little saliva as possible, which, as he used tobacco, he had little difficulty in complying with." After two hours and a half in the former, and after an hour and a half in the latter experiment, the contents of the stomach were withdrawn and submitted to examination; the result of which lead the author to the following inferences:—"That starchy materials are digested in the human stomach; that human gastric juice does not prevent the conversion of starch into grape-sugar, and that this conversion may take place in the stomach independently of the action of saliva." (*loc. cit.* p. 518.)

We have purposely apportioned more space to the description of these three experiments, on which the author's conclusions regarding the action of the gastric juice are based, and have given these conclusions in his own words, in order that the reader may be better enabled to judge for himself of the value of these observations on so important a subject.

Hanlo made on himself experiments with the extract of meat prepared according to Liebig's prescription (i.e., without heat); with broth (bouillon) prepared from the same quantity of meat; with roasted meat itself; and with the "decoctum album" (a decoction of scraped hartshorn, bread, and sugar): the food taken besides these substances, and the general mode of living, remaining the same during all the experiments. By comparing the quantity of urea and chlorides excreted through the kidneys during the experiments with the four above-mentioned articles of diet, Hanlo comes to the inference, that the body assimilates more nutrient material from the extract of meat than from the "bouillon," and much more than from the "decoctum album." The quantity of urea excreted during the consumption of the extract, exceeded even that obtained during the experiment with the roasted meat! But whether this is a result constantly met with also in other individuals, and whether it justifies the conclusion, that we derive more nutritious matter from the extract than from the meat in substance, we must as yet consider as undecided. The albumen and hematosin are the principal substances to which the extract owes the preference over the "bouillon."

In opposition to Lehmann, Bidder and Schmidt, and other physiologists, Jones maintains the view, "that meat is entirely digested in the stomach." This view is based on the fact, that the author, in examining the contents of the stomachs of fishes, reptiles, birds, and mammalia, in every stage of the digestive process, never has discovered undigested particles of flesh in the small intestines (p. 259). He has convinced himself "that, in the normal process of digestion, the matters dissolved by the gastric juice are almost immediately absorbed or pass into the duodenum." Jones considers it therefore "evident that this process is far more energetic than that of artificial digestion," and that therefore "the argument founded upon artificial digestion falls to the ground."

Skrzeczka gives the result of a series of experiments on casein and soda-albuminate under the influence of pepsin (rennet). The coagulation of the milk is considerably accelerated by containing more fat, by the addition of a larger quantity of rennet, by increase of temperature. Boiled milk coagulates later than unboiled. The whey obtained by filtration contains albumen free from alkali, similar in reaction to the white of eggs. Skrzeczka's experiments with alkali-albuminates show, 1, that they are not coagulated by rennet alone; but 2, that coagulation takes place when sugar of milk or butter are added; 3, that this coagulation takes place most quickly when sugar of milk or butter are added in the proportion in which they are contained in the milk; 4, that an artificial milk, prepared of soda-albuminate, sugar of milk, and butter, coagulates after some time spontaneously, quite like genuine milk. Analogous to the soda-albuminate is the reaction of Panum's serum-casein, and of casein obtained by Rochleder's method.

Hoppe's investigations are made principally on a dog, that received daily from 20 to 200 grains of cane-sugar, with or without other food. The author himself draws the following inferences:—1. Cane-sugar was not altered by the saliva and gastric juice within one hour. 2. Larger doses of cane-sugar excited vomiting in one or two hours. 3. Neutralization of the gastric juice, by means of chalk, effected no change in the just-mentioned two observations. 4. When the gastric juice was thus neutralized, yeast did not seem to develop fermentation in the stomach. 5. No trace of sugar was found in the urine or fæces during continued feeding with sugar. 6. The quantity of lactic acid in the urine does not become increased by feeding with sugar. 7. When sugar and meat were given together, the weight of the animal increased much more rapidly than when meat alone was given. 8. When sugar and meat were consumed, urea was excreted in smaller quantity than when meat alone was taken. 9. By exclusive sugar-diet the excretion of urea was depressed to its lowest amount. 10. The excretion of nitrogen with the fæces was not much altered by the addition of sugar to the meat. 11. By the presence of much sugar in the blood, the albuminous substances are preserved from oxidation. The albumen thus stored up appears to become decomposed under the development of fat. In this manner sugar produces fattening only when, at the same time, albuminous substances are liberally supplied. 12. Bernard's* conjecture, that the ingestion of sugar excites only an increased formation of sugar in the liver, while the sugar ingested as such is supposed to be transformed into fat, is untenable; it is likewise unproved that the production of sugar in the liver forms the principal source of animal heat. 13. The temperature of the body did not become increased by the addition of sugar to the allowance of meat. 14. The health of the dog was in no way injured by feeding upon large quantities of cane-sugar in addition to a liberal meat-diet.

Colin found sugar as well in the thoracic duct as also in the larger chyliferous vessels of the mesentery, in animals that had been fed exclusively with animal food. He is thus led to conclude, that sugar is formed in the intestinal canal, at the expense of the animal constituents of the food.

Funke maintains the view, that there are no actual and constant channels for the passage of fat through the villi, with the exception of a central canal; that the fat enters the epithelial cells of the villi, according to the laws of endosmosis, and finds from thence its way through the parenchyma to the central canal. Funke adds, that frequently several globules, on their way towards the central canal,

* Leçons de Physiologie Expérimentale, &c. 1855.

follow each other closely; that through this succession of globules lines are formed; that the existence of various such lines in different directions produces sometimes the appearance of a network of vessels; but that this appearance is deceptive, as the lines just described do not possess any well-defined membranous walls, and can therefore not be considered as vessels.

Zenker, on the other side, who made his observations on two individuals that had suddenly died soon after meals, holds the view that the network witnessed in the villi is formed by channels with distinct boundaries; that also the mucous membrane between the villi contains many such narrow channels, which lead without interruption into the larger chyliferous vessels.

Hoffmann's researches lead to the inference, that a considerable quantity of fat is absorbed, without any previous change, by the lymphatic and chyliferous vessels. Experiments on endosmosis, made with pieces of small intestines and of the urinary bladder of oxen, show that fat passes through these membranes under a lower degree of pressure, when they have been previously steeped in bile, than without this.

II. Blood; Circulation; Respiration.

1. Bischoff: *Calculation of the Quantity of Blood in a Decapitated Criminal.* (Zeit. f. Wissens. Zool., vol. vii. 3, 1855; and Schmidt's Jahrb., vol. xc. p. 286.)
2. Lehmann: *On the Constitution of the Blood in different Vessels.* (Bericht. d. saechs. Gesells. d. Wissenschaften, Nov. 1855; and Canstatt, l. c., p. 188.)
3. Jos. Jones: *Physical, Chemical, and Physiological Investigations upon the Vital Phenomena, Structure, and Offices of the Solids and Fluids of Animals.* (Hay's American Journal of Medical Science, July, pp. 15 ss. 1856.)
4. Bamberger: *Contribution to the Physiology and Pathology of the Heart.* (Virchow's Archiv, vol. ix. pp. 328 ss. 1856.)
5. Chaveau and Faivre: *New Experimental Researches on the Normal Movements and Sounds of the Heart.* (Compt. Rend., Septemb. 1855, pp. 423 ss.; and Canstatt, l. c., p. 82.)
6. Soire: *On the Circulation of Man and Animals.* (Canstatt, l. c., p. 83.)
7. Heidenhain: *Disquisitiones de Nervis, Organisque Centralibus Cordis, &c.* (Berolinae, 1854; and Canstatt, l. c., p. 130.)
8. Poiseuille: *Researches on Respiration, &c.* (Compt. Rend., vol. xli. p. 1072; and Canstatt, l. c., p. 8.)

The researches of Arnsperger, Fano, and Schiff are noticed under 'Nervous System.'

Bischoff calculated the quantity of blood in circulation by means of Welker's method. This consists in taking, first, a sample of the normal blood of the body to be examined, and in washing out afterwards the bloodvessels and the minced organs of the whole body, in order to collect all the blood contained in them. By measuring the whole of the fluid thus procured, and by diluting the sample of the normal blood previously obtained until it has gained exactly the same colour as the fluid of the maceration, one may calculate, through the quantity of water required for this dilution, the quantity of blood contained in the fluid of maceration.

In the case examined by Bischoff, the individual weighed—

	Grammes.
With clothes, before the decapitation	65,780
„ after „	62,280
The loss of blood	3,470
Blood contained in the clothes	291
Blood in the fluid of maceration	994
Blood from the hepatic and portal veins	20
Total amount of blood	4,775*

* About nine pounds and a half.

According to this examination, therefore, the weight of the whole of the blood in circulation would be only about $\frac{1}{13}$th of the weight of the body; while Valentin had assumed it to be $\frac{1}{5}$th, E. Weber and Lehmann, $\frac{1}{8}$th. In Weber's case, the loss of blood alone from the decapitation amounted to about 5200 grammes, although the total weight of the body was smaller than in the present instance. For this reason, Bischoff is inclined to doubt the parallelism between the weight of the body and that of the blood, assumed by Valentin. It is however to be observed, that the subject of Bischoff's examination had been affected with symptoms of scurvy for some weeks previous to his death. We may add that, by means of the same method, Welker has found the quantity of blood in animals of different classes, to be about $\frac{1}{13}$th of the weight of the whole body.

Lehmann's researches show that the per-centage of *fibrin* in the blood from the small veins is larger than in that from the arteries; while the blood in the vena cava is poorer in fibrin than that obtained from the arteries. Lehmann therefore considers it probable that fibrin is principally formed in the arteries, that its quantity is increased in the capillaries by the influence of the oxygen, that it perishes again in the larger veins. The per-centage of *salts* is found, by the same author, larger in the arterial blood than in the venous: this circumstance is ascribed to the destruction of organic substances in the lungs, and particularly to that of the extractive matters, but partly also to that of albumen, which is constantly disappearing in a considerable degree; a circumstance manifested by the fact, that the solid residue of the arterial serum contains about 2% less albumen than that obtained from the serum of the venous blood.

The blood from small veins contains about 6% more *water*, and about 6% less dry blood-globules, than that from arteries. The blood from the vena cava (before the inosculation of the hepatic veins) yields, likewise, 2% of globules less, and 2% of water more, than the arterial blood.

The comparison of the blood from the vena portæ with that from the hepatic veins, in dogs and horses, shows that the former contains more serum than the latter; that the serum of the former is richer in albumen, salts, and water than the latter; that therefore a part of these substances seems to remain in the liver. The quantity of extractive matters, on the contrary, is found much increased in the blood from the hepatic veins. The cruor of the latter exhibits a great augmentation of salts, which seems to point to an important alteration, if not new formation, of blood-globules.

Jones's observations relating to the comparative anatomy and physiology of the vertebrate and invertebrate animals, are, to a great part, not new in this country; we give, therefore, only a few of his inferences. Concerning the specific gravity of the *blood*, the author's tables show that, "as the organs, and apparatuses, and intelligence of animals are developed, the blood becomes more concentrated." Regarding the single constituents of this fluid, Jones observes:—The proportion of *water* is greatest in the invertebrata; amongst the vertebrate animals, it is greatest in fishes and aquatic reptiles, smallest in serpents, birds, and mammalia. "It may be laid down as a general law, that, as the organs and apparatuses of the animal economy are developed, and the temperature and intellect correspondingly increased, the blood becomes richer in *organic* constituents." (p. 55.)

In the invertebrata, the number of blood-corpuscles is very small in comparison with the number which exists in the blood of the vertebrata. The increased development of the cerebro-spinal system and the organs of vertebrate animals, is attended by a corresponding increase in the solitary gland-cells of the blood. The office of the *blood-cells*, taken collectively, is that of an immense gland, which elaborates the constituents of the blood. The *fibrin* presents a remarkable index of the vital and intellectual endowments of animals. (p. 59.) It is absent in almost the whole of the invertebrate kingdom; it is soft and unstable in the lowest orders of the vertebrata. The collation of the amount of *fixed saline constituents* in the blood of various animals leads the author to the observation, that their proportion is remarkably uniform throughout the whole animal kingdom.

Regarding the *circulation*, the table giving the proportional weight of the heart shows that, among the vertebrate classes, this organ is smallest in fishes, and largest in birds. The comparison of the frequency of the heart's contraction in the different classes of animals leads him to the deduction, "that the rapidity of the circulation depends upon the structure, habits, age, and development of animals. If the vital forces are of a low grade, either from original conformation or the depressing influences of old age, the circulation is correspondingly sluggish and feeble." (p. 35.)

Bamberger had the opportunity of observing the contraction and impulse of the heart in a healthy man, thirty years of age, who had inflicted on himself, by means of a knife, a deep wound at the inferior margin of the fifth left rib, somewhat below the nipple and nearer to the middle line. Through this wound, the author believes, the pericardial cavity had been opened, without lesion of the substance of the heart. By introducing the index finger, he could distinctly feel how, with every systole of the heart, the apex, hardened and slightly pointed, moved along the anterior wall of the chest, from above downwards, slightly inclining to the left; and with every diastole, returned to its former position. The duration of the former of these two acts appeared to the author a trifle shorter than that of the latter. He could, however, not perceive either a lever-like movement of the apex towards the front, or a rotation round the longitudinal axis. By this observation, Bamberger was induced to institute, with the assistance of Professor Kölliker, a series of experiments on rabbits, which led to the following results:—1. The change taking place in the shape of the heart with every systole, consists in the shortening of the longitudinal, and in the increase of the antero-posterior diameter; while that from one side to the other becomes probably likewise smaller. 2. The perceptible impulse of the heart is produced merely by the systolic vaulting and hardening of the anterior ventricular wall. 3. An actual locomotion of the heart, in the direction from above downwards, takes place with every systole—as Skoda had observed it in a child without sternum—and at the same time, the large vessels are seen stretched. The elongation of the vessels appears to be the cause of the downward motion of the heart. This inference became particularly probable in a rabbit, in which the sternum had been longitudinally divided, and both halves of the thorax drawn aside; there the pulmonary artery appeared, during the systole, elongated to such a degree, that a piece of it, between two and three lines long, became visible with every systole, and disappeared again with every diastole. 4. The systole is accompanied by a rotatory movement round the axis of the heart, from the left to the right. By the simultaneousness of the rotatory and the descending motion, the heart has the appearance of moving in a spiral direction along the wall of the chest. 5. The heart descends considerably with every deep inspiration, probably in consequence of the stretching of the great vessels. 6. The margin of the left lung, bordering the heart, exhibits two distinct motions: the *respiratory*, showing itself in the gliding down of its inferior margin along the inner wall of the chest with every systole (elongation of the diameter from above downwards), and by the retraction with every diastole; and the *systolic*, which consists in a short and quick movement of the thin anterior border (covering the pericardium), in the same direction and synchronous with the heart, the extent being about one line. 7. The diastolic movements are, as Haller already observed, in every respect the opposite of the systolic. It may be added, that chloroform had been used to facilitate these observations, the inhalation of a few drops having been sufficient to render the respiratory movements and the contractions of the heart so slow, that the single periods could be easily watched.

Chaveau and Faivre have examined the movements of the heart by laying it bare, after having previously divided the medulla between the atlas and occipital bone, resorting at the same time to artificial respiration. They distinguish three periods:—*a*. The systole of the auricles; and simultaneous with this, the diastole of the ventricles. *b*. The systole of the ventricles and diastole of the auricles. *c*. The diastole of both ventricles and auricles. To the second period belongs the first, to the third the second sound. Both sounds are explained by the tension of the valves. The impulse is attributed to a change in shape, and to an increased firmness of the substance of the heart.

Soire, whose observations are made on frogs, denies the existence of the short pause between the diastole of the ventricles and the following systole. He says that the systole is seen to commence immediately after the diastole, when, in opening the cavity of the chest, loss of blood has been avoided; that the pause in question is observed only when the animals have become anæmic from lesion of large vessels during the operation.

Heidenhain's researches lead him to adhere to the view, that the ganglia are the nervous centres of the heart; that the sympathetic nerve is its real motor nerve; while the medulla oblongata and the pneumogastric nerve have the function of regulating its pulsations. Heidenhain does not confirm Wagner's observation, that the contractions of the heart become more frequent by section of the sympathetic nerve on the neck.

Poiseuille found, by means of injections, that the capillaries of the expanded lung (inspiration) are more stretched, and therefore narrower, than those of the lung containing less air (expiration). He concluded from this, that, during inspiration, the flow of blood in the capillaries of the lungs is rendered slower, and that the whole circulation becomes retarded. This conclusion the author confirms by microscopic observations in the living frog.

III. Skin; Secretion; Excretion; Metamorphosis of Matter.

1. Duriau: *Experiments on the Absorption and Exhalation of the Skin.* (Arch. Génér. 1856, t. ii.; and Prag. Vierteljahrs., vol. iv. p. 53, 1856.)
2. Poulet: *On the Capacity of the Skin to absorb Water and Substances dissolved in it.* (L'Union, No. 33, 1856; and Schmidt's Jahrb., vol. xci. p. 278.)
3. Kletzinsky: *On the Faculty of Diffusion of the Skin.* (Wien Wochenschr., Mai, 1855; and Canstatt, l. c., p. 4.)
4. Schlossberger: *Does the Milk become Acid by Stagnation in the Gland?* (Annal. d. Chemie und Pharm., vol. xcvi. p. 76; and Canstatt, l. c., p. 192.)
5. Gubler: *On the Secretion and Composition of the Milk in New-born Children.* (Gaz. de Paris, No. 15, 1856; and Schmidt's Jahrb., vol. xci. p. 8.)
6. Kölliker and Müller: *Second Report of the Physiological Institution at Würzburg.* (Verhandl. d. Würzb. Gesellsch., vi. 3, pp. 436 ss. 1856.)
7. Schwarzenbach: *On the Presence of Copper in the Human Liver.* (Verhandl. d. Würzb. Gesellsch, vii. 1, p. 19. 1856.)
8. Jos. Jones: *Digestion, &c.,* l. c.
9. Kaupp: *Contribution to the Physiology of the Urine.* (Vierordt's Archiv, pp. 385 ss. 1855.
10. Dunklenberg: *On the Quantity of Phosphoric Acid and Earthy Phosphates in the Urine.* (Liebig's Annal. xciii. p. 88; Canstatt, l. c., p. 202.)
11. Neubauer: *On the Quantity of Ammonia in the Normal Urine.* (Erdmann's Journal, v. lxvi.; and Canstatt, l. c., p. 202.)
12. H. Blot: *On the Physiological Glycosuria of Women in Childbirth, &c.* (L'Union Médic., No. 126, tome x. 1856.)
13. Frerichs and Staedeler: *Further Contributions to the Knowledge of the Metamorphosis of Matter.* (Müller's Arch., p. 37, 1856; and Schmidt, vol. xc. p. 146. 1856.)
14. Frerichs and Staedeler: *On the Metamorphosis of the Cholic Acids into Colouring Matter.* (Müller's Arch., p. 55, 1856; and Schmidt's Jahrb., vol. xc. p. 147.)

The results of Duriau's experiments confirm several of the inferences drawn by Homolle,* who, as well as several other authors, had maintained that the

* See this Journal, No. 27, p. 256. 1854.

skin absorbs water during bathing; Duriau proves not only the correctness of this assertion, but shows at the same time, that the amount of the water absorbed changes with the temperature of the bath. He found that exhalation and absorption are almost equal, when the water has the temperature which allows the body to feel it neither cold nor warm. This point is not quite the same in different individuals, but it lies in general between 89° and 94° F. ("point isotherme," "limite thermique.") If the water is warmer, the exhalation exceeds the absorption: the body therefore loses weight. If it is colder, the absorption preponderates: the body gains weight. Both these changes become increased with the increased duration of the bath. In a bath of 71·5°—77° F., the absorption amounted in the average to 16 grammes in a quarter of an hour, to 139 grammes in three-quarters of an hour. At a temperature 105·8° to 107·6°, the loss of weight was equal to 135 grammes after seven minutes, to 378 grammes after fifteen minutes; at 113° F. the loss was as much as 432 grammes after ten minutes.

The inferences regarding the absorption of saline or organic substances dissolved in water, are based on the examination of the urine before and after the use of the bath. Iodide and ferrocyanide of potassium, carbonate of potash, sulphate of quina, and other salts, were employed. The reaction of the urine after the bath was always alkaline, even when nitric acid had been added to the bath. Potash and soda were the only bases found in the urine—no trace of iodine or cyanogen, &c.

Poulet draws the following inferences from his experiments :—1. That the loss of weight of the human body in a bath of 82·4° F. is very trifling after the first hour, but that it never amounts to less than 50 grammes after the second hour. 2. That this loss is produced,—*a*, by increased elimination of water through the lungs; *b*, by the perspiration of parts of the skin not immersed in the water. 3. That the urine becomes alkaline after acid, as well as after alkaline baths.* 4. After friction of the skin with a solution of tartrate of antimony or extract of belladonna, none of these substances were found in the urine. 5. The skin absorbs, therefore, neither water (?) nor substances dissolved in it, as long as the epidermis is entire.

Kletzinsky's experiments likewise confirm the non-absorption of salts through the healthy epidermis. Gaseous or volatile substances, however, may, according to this author, pass through the skin into the body.

Schlossberger, in opposition to Hermbstaedt and Fraas, found the milk always neutral or alkaline, even after long stagnation in the gland—i. e., after between twenty-nine hours and four days in cows, and after between two and eight days in women. Schlossberger, however, has not examined diseased milk; he thinks it probable that in those cases in which the fresh-drawn milk has no acid reaction, it had been secreted in a sour state.

Gubler gives the result of his observations concerning the secretion of a milk-like fluid, which may be squeezed out of the nipples of new-born children, as Morgagni and Natalis Guillot had already mentioned. In 435 children under three weeks of age, the secretion was almost always met with; it was small in quantity, and more serous during the first three days; on the third and fourth day the breasts became turgid, the fluid more abundant, and of an opaque colour; the quantity increased during the following days, and on the tenth day, only one child amongst sixty examined did not yield the fluid; between the tenth and twentieth day there is scarcely any change; the fluid is rarely met with after the lapse of the first month. Regarding the chemical nature of the fluid, the author found it always alkaline—even more so than the milk of nursing women. He gives an analysis by Quevenne, which we subjoin, together with the corresponding figures for the milk of women and asses :—

* See Homolle, Duriau.

	Children's milk.	Women's milk.	Asses' milk.
Butter	1·4	2·6	1·4
Casein	2·8	3·9	1·7
Sugar of milk	6·4	4·9	6·4
Water	89·4	88·6	90·5
	100·0	100·0	100·0

Saline Ingredients.

Earthy phosphates from the casein	0·120
Soluble salts from the casein	0·040
Soluble and insoluble salts from the sugar of milk	0·180
	0·340

The resemblance of this fluid with the milk of women and asses is confirmed by Donné's lactoscope.

Kölliker and Müller's report contains valuable additions to our knowledge of the *secretion of bile*. The inferences are drawn from experiments performed on dogs. 1. As regards the influence of *meals*, a considerable increase was found from the third hour—the greatest amount, in general, between the sixth and eighth hour, the lowest between the nineteenth and twenty-fifth hour after moderate meals, while the increase continued for sixteen or seventeen hours after large meals. It will be remembered that Arnold found the quantity of bile largest soon after meals, decreasing again after the fourth hour.* 2. The quantity of bile secreted per one kilogramme of dog in twenty-four hours, is estimated at 36·1 grammes, with 1·162 grammes of solid residue: which nearly agrees with the observations of Bidder and Schmidt, while the figures are higher than those given by Nasse† and Arnold. 3. Of physiological as well as pathological interest are the researches made on dogs, after the closure of the external fistula: cases, therefore, in which, as the ductus choledochus had been previously obliterated, *artificial icterus* had been produced. The first signs of the icterus were observed in the urine, and not till several days later it appeared in the conjunctiva and mucous membrane of the mouth. In spite of the most intense icterus, one of the animals remained for several months very lively, and gained weight; sudden death ensued, however, in the midst of apparent health. The examination exhibited the signs of peritonitis, and a perforating ulcer of the duodenum. It follows that the mere retention of bile does not appear to exercise so injurious an influence on digestion and nutrition, and on the nervous system, as is generally assumed. 4. Amongst the *post-mortem phenomena* of dogs affected with biliary fistula, our attention is particularly arrested by the comparative frequency of perforating ulcers of the duodenum, and of incrustation (ossification) of the branches of the cœliac axis and the mesenteric artery. Two of the five dogs experimented upon died of these perforating ulcers; a third of them manifested the signs of gastro-intestinal catarrh. The incrustation of the arteries was discovered likewise in two out of the five dogs, and has not been looked for in the remaining three. These pathological alterations, which had been formed without obvious symptoms during life, must make us careful in asserting that the bile may be drawn off without material injury to the constitution; the more so, as all the dogs experimented upon, as well by the authors as also by Schwann,‡ Nasse, and Arnold, died more or less suddenly, although they had been well provided with food. Regarding the etiology of the incrustation of the arteries, we must wait for further observations; we have already other pathological facts before us which make it probable that diseased states of the bile-conducting apparatus are apt to cause this morbid condition of the bloodvessels of the intestines.

* See this Journal, No. 33, p. 232.

† Commentatio de Bilis quotidie a Cane secreta copia et indole. Marburgi, 1851.

‡ Müller's Archiv. 1844.

Schwarzenbach confirms the existence of *copper* in the human liver, but he found only 0·009 grammes of oxide of copper—i. e., 0·004 of pure copper in 2100 grammes of liver; while Orfila had found ten times as much. The same quantity of the same liver yielded about 0·017 grammes of pure lead.

Regarding the physiology of the *pancreas*, Jones defends the assertion of Bernard, that its chief office is to prepare fatty matters for absorption, adducing in favour of this view the following facts:—1. In the garfish (Lepidosteus osseus), the emulsion of the fatty matters takes place in the duct and cæca of the pancreas and their immediate vicinity, and nowhere else in the alimentary canal. 2. The pancreas of carnivorous, is relatively much larger than that of frugivorous and granivorous animals; the amount of oil consumed by the former is much larger than that consumed by the latter. The size of the pancreas amongst carnivorous animals is in a measure proportional to the amount of the oleaginous matter consumed. 3. The pancreas of carnivorous chelonians, fed upon vegetable matters, degenerated in its structure.

Kaupp's observations, performed with much attention to diet and manner of living, show that, as a general rule, the quantity of *chloride of sodium* excreted by the *urine* is in proportion to the quantity of this salt ingested with the solid and fluid nutriments. If, however, after several days of abstinence from culinary salt, it is introduced in larger quantities, the amount excreted through the kidneys is smaller than that ingested; but this is only an apparent exception of the rule, as the body seems to store up a certain quantity, of which it had been deprived by the preceding abstinence. Concerning the connexion between the ingestion of culinary salt and the excretion of other constituents of the urine, Kaupp's tables show, that the increased ingestion of salt causes an increased excretion of water, and also of urea, while the other solid substances exhibit a decrease.

Dunklenberg found the quantity of *phosphoric acid* contained in the urine of twenty-four hours varying between 2·144 and 2 657 grammes; that of the *earthy phosphates* between 0·763 and 0·972 grammes.

According to Neubauer's examinations, the urine of a healthy young male person contained in twenty-four hours, in the average, 0·8351 grammes of *ammonia*, that of another likewise healthy man 0·6137 grammes, which figures correspond to 2·6361 and 1·9305 grammes of hydrochlorate of ammonia. The highest figures in the two subjects were 3·8038 and 2·3025 grammes; the lowest, 1·4272 and 1·5987. Exercise appeared to have no marked influence, while increased ingestion of water was accompanied by increased excretion of ammonia.

H. Blot, of the Clinique d'Accouchements de la Faculté de Paris, has made the interesting observation, that the urine of all lying-in, of all nursing, and of about half of the pregnant women, contains sugar. The glucose commences, in general, to appear as soon as the secretion of milk is established, sometimes even before this. The quantity secreted in twenty-four hours is not given, but it is stated to be much smaller than in diabetes. The urine of the best nurses was found richest in sugar. The glucose begins to disappear from the urine as soon as lactation ceases either permanently or becomes transitorily suspended from disease or other causes. Blot infers, therefore, that this glycosuria is in an intimate relation with the secretion of milk. He further adds the observation, that the same physiological phenomenon exists also in the cow.

Frerichs' and Staedeler's further researches on the occurrence of *leucin* and *tyrosin* in the animal body, lead to the following inferences:—1. That leucin and tyrosin in some diseases and functional disorders of the liver, accumulate in this organ in considerable quantity, while they are absent in the normal state. 2. Leucin is always found in the juice of the spleen; tyrosin not as regularly. 3. Both are met with in the pancreas; no other organ contains leucin in so large a proportion. 4. The salivary glands do not yield so much leucin as the pancreas. 5. The lymphatic glands exhibit an ample amount of leucin, but no tyrosin. 6. The examination of the thyroid gland and of the thymus of calves, ten weeks old, leads to the same result. 7. In the brain, leucin does not appear constantly to occur. 8. In the muscles and lungs it was not discovered. 9. The urine of typhus patients exhibited leucin in large amount, and also some tyrosin.

The authors are of opinion that these two bodies are the products of albuminous substances; that they are formed principally in those glands which contain a ferment-like element; that they are thence carried by the circulation to other organs, especially the liver, where they are farther decomposed, entering into the formation of bile, perhaps also volatile fatty acids.

The experiments made by the same authors on *glyco-cholalic acid* and *glyco-cholalate of soda*, when acted upon by sulphuric acid, lead them to the conclusion, that the acids of the bile may be transformed into colouring matter of different shades, according to the degree of temperature employed and other circumstances. The following experiment makes it probable that the same metamorphosis may take place in the living organism. A drachm of colourless ox-gall dissolved in distilled water was injected into the vein of a dog; three ounces of urine were obtained six hours later; this urine formed a considerable green sediment, which exhibited under the microscope green granules, and with nitric acid the characteristic change of colour.

IV. Animal Heat.

Cl. Bernard: *Experimental Researches on Animal Heat.* (L'Union Médic., tome x. No. 108. 1856.)

In order to ascertain the influence exercised on the temperature of the blood during its passage through the digestive apparatus, Bernard examined:—*a*, the temperature of the abdominal aorta—i.e., of the blood before its distribution to the digestive apparatus; *b*, of the portal vein—i.e., of the blood after its passage through the intestinal canal, spleen, pancreas, &c., but before its entrance into the liver; *c*, of the hepatic veins—i.e., after its passage through the liver and the whole digestive apparatus. From these necessarily very delicate experiments, for the description of which we refer to the original, the author draws the following conclusions:—1. The heat of the blood is constantly increased by its passage through the digestive apparatus, in such a manner that it is warmer in the portal vein than in the abdominal aorta, and still more so in the hepatic; the process of digestion exercising apparently no influence over this phenomenon. 2. The blood of the hepatic veins is a constant source of calorification for the blood conveyed through the inferior cava to the heart. This may be considered even as the principal source, for nowhere else is the blood found so warm as in the hepatic veins, where it raises the thermometer in vigorous dogs even to 106°·88 Fahr. 3. Among the organs which contribute to the augmentation of the temperature of the blood in its passage through the digestive apparatus, the liver maintains the highest rank. This organ must therefore be regarded as one of the principal sources of animal heat.

V. Nervous System.

1. Arnsperger: *On the Cause and Pathological Nature of the Alteration of the Lungs after the Section of the Nervi Vagi on the Neck.* (Virchow's Archiv, vol. ix. 1, 2, 3. 1856.)
2. Fano: *Section of the Right Nervus Vagus in Man.* (Arch. Génér., Févr. 1856; and Schmidt's Jahrb., vol. xci. p. 19. 1856.)
3. Budge: *On the Movements of the Iris.* (Braunschweig, 1855; and Canstatt, l. c., p. 116.)
4. Schiff: *Researches on the Physiology of the Nervous System.* (Frankfurt, 1855; and Canstatt, (l. c., p. 133.)
5. Heidenhain: (l. c., cf. ii.)

Arnsperger distinguishes the effects of the section of the recurrent branch alone from those of section of the entire pneumogastric nerve. After division of the *recurrent laryngeal* nerve, he observed:—1. Narrowing and even closing of the rima glottidis. 2. Relaxation of the ligaments of the glottis. 3. Change in the number and depth of respirations. 4. Incomplete deglutition, allowing the passage of particles of food into the trachea of rabbits. 5. Death through inflammatory affection of the lungs in rabbits. 6. Loss of voice as the only consequence of this operation in dogs, the larynx of which does not permit the entrance of particles of food into the bronchi. Regarding the section of the *pneumogastric nerves* on the neck, the author remarks:—1. Division of one nerve causes no functional derangement of importance. 2. Division of both nerves causes as well functional disorders as also a series of pathological alterations. 3. The former consist in loss of voice, retardation in the respiratory movements, acceleration of the contractions of the heart. 4. The anatomical alterations are:—Consolidation of the parenchyma of the lungs, serous exudation, emphysema, sometimes coagulation of the blood in the bloodvessels of the lungs. 5. Death, with very rare exceptions, in rabbits sooner than in dogs. Only one dog survived the operation by rapid reunion of the dissected nerves.

The principal anatomical alteration after section of the *recurrent* nerves consisted in lobular inflammation (catarrhal or broncho-pneumonia, or "pneumonie lobulaire mamelonnée" of Rilliet and Barthez), tending to the formation of abscess round the foreign bodies.* The condition of the lungs after section of the *pneumogastric* nerves is described as bronchitic condensation ("bronchitische Verdichtung" of Hasse), a state analogous to, but not quite identical with, atelectasis. In consequence of the diminished respiratory action, the vesicles collapse; this collapse leads to stasis and serous exudation (œdema). The analogy of this state with atelectasis, and its difference from inflammation, is shown by the redness, the condensation, the firmness, and the sunken state of the affected tissue, its irregular distribution over the lobes, and, lastly, its capability of being inflated.

Fano describes the details of the case of a man, aged forty-five, in whom, together with a carcinomatous tumour on the right side of the larynx, a piece of the pneumogastric nerve was removed. The only symptoms attributable to this lesion, were increased hoarseness and difficulty of expectoration, which is quite in accordance with the observations made by Longet, Horner, and Robert.

Budge's valuable contribution to the physiology of the *iris*, contains the results of the author's own researches, as well as those of other physiologists. We can here of course give only a few of the principal results. Budge attributes the sensation of the iris exclusively to elements from the *fifth* pair, questioning the influence of the optic, oculo-motor, and sympathetic nerves. Irritation of the *third* pair, in mammalia and birds, produces contraction, that of the *sympathetic*, dilatation of the pupil; galvanism applied to the iris itself causes in birds rapid general contraction, while this is slower in birds. The reflex action which leads to contraction of the pupil, in consequence of irritation of the optic nerve, takes place by the medium of the *corpora quadrigemina*, after the extirpation of which, this reflex action is no longer observed. Section of the *fifth* nerve, as well before as after the ganglion Gasseri, causes contraction of the pupil of the same side. Budge concludes from this, that the first branch of the nervus trigeminus contains motor fibres for the sphincter of the iris. The author describes two central organs of the iris in the *medulla:* (*a*) the centrum cilio-spinale inferius, situated, in rabbits, between the sixth cervical and the fourth dorsal vertebra; extirpation of this part causes contraction, irritation causes dilatation of the pupil; irritation of either of the lateral halves, when separated by a longitudinal section, causes dilatation of the corresponding pupil. The fibres to which this influence is attributed, leave the medulla by the anterior fasciculi. (*b*) The superior centre of what Budge calls the "iris sympathicus" lies near the point of exit of the ninth pair, and is connected with the superior cervical ganglion by the anastomotic fibres between the ninth pair and the sympathetic nerve. Budge denies that the vagus exerts any influence on the iris.

* See this Journal, No. 33, p. 235. 1856.

Concerning the influence of the *distance of shining bodies* on the *width* of the pupil, Budge's experiments show, that the degree of contraction of the iris is not in a direct proportion to the distance of the shining body; the distances being in the proportion of 1 : 2 : 3, the diameters of the iris were 1 : 1·096 : 1·165. Similar experiments on the influence of a varying intensity of light on the width of the pupil, gave for the intensities of 30 : 9 : 4 : 2, the diameters of the pupil, 3 : 3·47 : 3·94 : 4·73.

Schiff presents the result of his experiments on the function of the fifth pair, of the sympathetic nerve, and on the influence of paralysis of nerves on the increase of animal heat. As we have no space for a condensed extract of all three subjects, we offer only a few points from the third of them. The increased fulness of the bloodvessels after the section of nerves, is attributed to the paralysis of the vaso-motor nerves; the elevated temperature is considered to be caused by the presence of the larger quantity of blood. We have related, in former Reports, the experiments of other observers regarding the change of the temperature of the head following the section of the sympathetic nerve on the neck.* Schiff shows that the sympathetic is not the only vascular nerve of the head, that the nervus "auricularis cervicalis" always supplies a part of the vessels of the ear; section of this nerve always causes an increase of warmth in the corresponding ear, which, however, disappears after some days; galvanic irritation of the sympathetic nerve, and of the auricularis respectively, cause contraction of a different set of vessels. The fifth pair sends fibres to the vessels of the conjunctiva, the mucous membrane of the nose, and the gums; its section causes slight increase of temperature in these parts. Section of the facial nerve, several days after the extirpation of the superior cervical ganglion, gives rise to a further augmentation of warmth in the ear in rabbits; it must therefore contain vaso-motor fibres, which are not derived from the sympathetic, but from the anastomosis with the pneumogastric nerve, as section of the facialis immediately in front of the stylo-mastoid foramen is not followed by any rise of temperature. Section of the ischiatic nerve occasions the corresponding foot to become warmer than that of the other side; a similar effect is produced on the hand and forearm by section of the brachial plexus. Schiff also gives a series of experiments showing the influence of the nervous centre on the tone of vessels. Thus section of either half of the cervical portion of the medulla, on any spot whatever, effects increased heat of the ear of the corresponding side; at the same time, the arterial pulse is felt larger and fuller. The author confirms Budge's view, that the vaso-motor nerves of the head contained in the sympathetic nerve, are derived from the medulla; those of the thighs appear to have their origin in the dorsal medulla.

VI. Senses.

1. Krause: *On the Refractive Indices of the Transparent Media of the Human Eye.* (Hannover, 1855; and Canstatt, l. c. p. 29.)
2. Helmholtz: *On the Accommodation of the Eye.* (Graefe's Archiv, vol. i. 1855; and Canstatt, l. c. p. 12.)
3. Dubrunfaut: *Observation on Vision.* (Comptes Rend., vol. xli. p. 1087; and Canstatt, l. c. p. 12.)
4. Budge: (l. c. cf. Nervous System).
5. Kramer: *Contribution to the Physiology of the Human Ear.* (Deutsche Klinik, Sept. 1855; and Canstatt, l. c. p. 115.)
6. Czermak: *Physiological Studies. Section II.—Sense of Touch.* (Sitzungsber. d. Wiener Academ., März, 1855; and Canstatt, l. c. p. 124.)

Krause measured the refractive indices of the eyes of dead human bodies, after having previously convinced himself on the eyes of animals, that there is scarcely

* See No. 33, p. 231; and No. 35, p. 230.

any difference between the figures found immediately after death, and those found at the period when the human eyes were examined by him. Thus he obtained, as the average of twenty experiments, the refractive index of the cornea=1·3525 (that of distilled water being 1·3358); the index of the exterior lamellæ of the crystalline lens, 1·4071; that of its middle lamellæ, 1·4319; of the nucleus lentis, 1·4564; of the humor vitreus, 1·3506; of the humor aqueus, 1·3435.

Helmholtz examined, by an ingenious apparatus, for which we must refer to the original, various phenomena connected with the process of accommodation. No alteration is to be observed in the curvature of the cornea during the accommodation for different distances; the iris becomes more prominent by the accommodation to near objects; the radius of the curvation of the anterior surface of the crystalline lens is, during accommodation for near objects = 8·6 millimètres; for remote objects = 11·9 millimètres. During the accommodation for near objects, also, the radius of the curvation of the posterior surface appears to become diminished; while, therefore, the lens becomes more curved on both sides, the posterior vertex remains in its place, while the anterior advances considerably. The lenses of dead bodies have the shape of those adjusted for near objects; they are in general even thicker than those. Helmholtz is therefore of opinion that this is the form of the lens in its state of equilibrium. Regarding the influence exercised by the iris in this alteration of shape and position of the lens, Helmholtz maintains a similar view to that of Cramer.*

Dubrunfaut calls attention to the circumstance, that bright objects are not seen brighter with both eyes than with one. He explains this by the observation, which he believes to have made, that if we close one eye, after having looked on a bright object with both eyes, the pupil of the other becomes dilated exactly to such a degree that its entire area becomes doubled. Thus the central organ would receive through one eye the same amount of impression as it had previously received through both eyes.

The *cartilaginous part* of the *external ear*, according to Kramer, leads into the external meatus, the third part of all the acoustic waves reaching the tympanum. If we cover the external ear, with the exception of the entrance in the meatus, by means of wet flannel, the sounds of a watch perceived by the uncovered ear at a distance of twenty-one to twenty-two inches, are heard only when not further removed than between thirteen and sixteen inches.

Czermak's experiments, made according to Weber's method, by means of a pair of compasses, show that the *sense of space* (i.e., of touch) is in boys more delicate than in grown-up persons, the differences being greatest in those parts of the skin where the sensibility is least acute. There is, however, also amongst children a great difference. Czermak's experiments on blind persons confirm the common belief, that the sense of touch, on the whole surface of the body, is considerably more developed in the blind than in those who see.

VII. The Supra-renal Capsules.

Brown-Séquard: *Experimental Researches on the Physiology and Pathology of the Supra-renal Capsules.* (L'Union Médicale, tome x. No. 108. 1856.)

Brown-Séquard, induced by Addison's work on the supra-renal capsules, made experiments relating to the physiology and pathology of the supra-renal capsules, on rabbits, dogs, cats, and guinea-pigs. These organs, the author infers, are essential to life, at least in the animals just named, as none of those experimented upon survived the extirpation of either one or both of the capsules for many hours. The symptoms—which were the same in almost all the animals—consisted in a considerable sinking of strength, in various derangements of the respiration and circulation, in convulsions, turning round (*turnoiement*), delirium, and coma. The

* See this Journal, No. 29, p. 272. 1855.

convulsions are, like those from strychnia, easily excited by reflex action. The extirpation of only one organ is frequently followed by more violent convulsions of the other side, and often also by a more contracted pupil of the side operated upon. Death follows more rapidly upon this operation than upon extirpation of the kidneys. The author attributes to the supra-renal capsules a near relation to the cerebro-spinal nervous centre. He ascribes the symptoms enumerated in part to the lesion of several filaments of the great sympathetic nerve going to these bodies, but this alone he considers insufficient to account for the rapid death after the extirpation.

Brown-Séquard adds, that he has frequently met with acute inflammation of the supra-renal capsules in rabbits, always accompanied with a rapidly fatal termination, preceded by symptoms similar to those consequent on extirpation. He has further witnessed the occurrence of congestion and hypertrophy, in some instances also of inflammation of these organs, after the dissection of the lower part of the dorsal and the upper part of the lumbar medulla.

VIII. Generation and Development.

1. Dareste: *On the Influence of Impermeable Coating of the Shell of Eggs on the Development of the Chicken.* (Comptes Rend., Nov. 1855; and Canstatt, l. c. p. 154.)
2. Küchenmeister: *On the Cœnurus Cerebralis of Sheep.* (Bulletin de l'Acad. de Bruxell., 1855; and Canstatt, l. c. p. 148.)
3. Van Beneden: *On the Development of Cœnurus Cerebralis in Sheep.* (Bullet. de l'Acad. de Bruxell., 1855; and Canstatt, l. c. p. 148.)
4. Ankermann: *De Motu et Evolutione Filorum Spermaticorum Ranarum.* (Regimonti, 1854; and Canstatt, l. c. p. 149.)
5. T. Moleschott and Richetti: *On means to Revive the Spermatozoa of Animals.* (Wien Med. Wochenschrift, Mai, 1855.)
6. Kölliker: *On the Vitality and Development of Spermatozoa.* (Würzburg Verhandlungen, vol. vi. 1855.)
7. Kölliker: *Physiological Studies on the Spermatic Fluid.* (Zeitschr. für Wissensch. Zool., vol. vii. 1855.)

Dareste in his experiments applied varnish to different parts of eggs, and at different periods of incubation. When the thick end of the egg had been varnished at the commencement of the incubation, the chicken-embryos were either destroyed, or when they had remained alive, the allantois was found attached to an unvarnished part of the egg. The author thus corroborates the view, that the allantois forms an organ of respiration.

Küchenmeister confirms the observation, that the tænia cœnurus of the intestines of dogs, is the full-grown individual of the cœnurus cerebralis of sheep. The sturdy of the sheep is propagated by the circumstance, that the heads of sheep thus affected are taken as food by dogs, in the intestines of which tænias are developed; the excrements of these dogs, containing the proglottides filled with eggs, are deposited in the grass on which the sheep feed. Damp meadows are particularly favourable to the development of the cœnurus, because they prevent the proglottides and eggs from becoming dry.

Van Beneden, who produced the sturdy in sheep by feeding them with proglottides, found in the heads of these sheep not only the passages of the cœnurus, but also the animals themselves in the cortical substance of the brain in different stages of their development.

Kölliker, Ankermann, Moleschott, and Richetti have made very careful researches on many substances influencing the motion of the spermatozoa of various animals. Serum of blood, lymph, the secretion of the prostatic gland, of Cowper's glands, a solution of albumen, &c., favour the motion of the spermatozoa.

Bile, milk, mucus, if not too viscid, do not prevent the motion; while this is checked by viscid mucus from the collum uteri. The just-named fluids, however, when too much diluted, act like water—i.e., they then cause the motion of the spermatozoa to cease, without destroying their vitality. Amongst the solutions of alkaline and earthy salts, there are some that act favourably when little concentrated, as the chlorides of sodium and of potassium, and nitrate of soda (1%); others, which act injuriously when so much diluted, while they become innocuous or even favourable to the motion by greater concentration (5%), as phosphate of soda, sulphate of soda, and sulphate of magnesia. The caustic alkalies in much-diluted solution possess the greatest power in effecting motion, even where it had long ceased. Concerning the manner in which these substances act, Ankermann is of opinion that the motion is due to the process of diffusion; Kölliker seems disinclined to ascribe this phenomenon merely to endosmosis or imbibition, but attributes to it a vital character. Mineral acids, metallic salts, and narcotics, act in a decidedly injurious manner.

HALF-YEARLY REPORT ON MATERIA MEDICA & THERAPEUTICS.

By Robert Hunter Semple, M.D.,
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I. *On the Therapeutical Applications of Glycerine.* By Dr. W. Lauder Lindsay. (Edinburgh Medical Journal, September, 1856.)

Dr. Lindsay used glycerine himself, to the extent of two or three teaspoonfuls daily for several weeks, in order to test its nutrient properties. He found the most palatable mode of using it was when it was mixed with coffee. The result was, a gain of weight to the extent of two pounds at the end of four weeks; and on discontinuing the glycerine, the weight gradually fell. The glycerine is readily miscible with fluids of all kinds. Coffee may be sweetened by it instead of sugar; and if the somewhat peculiar taste which it imparts should be objectionable to fastidious stomachs, a small quantity of sugar may be superadded. It has the characters of a syrup, and does not betray its presence by oil globules or otherwise. It may be added to tea, and it sweetens milk and cream very pleasantly; but its mixture with water is very palatable, and is the readiest and cheapest mode of administration. Dr. Lindsay carefully observed its effects as a nutrient and alterative in eight patients, to whom it was given in doses of two or three tea or table-spoonfuls daily for a month. All the patients before taking it were more or less anæmic, emaciated, and feeble; in all the diet, exercise, and occupations were otherwise the same. At the end of the month all of them appeared greatly improved in their general condition; they seemed plumper and stronger, and in some the countenance was even ruddy. In most of the cases there was a marked increase in weight at the end of the month.

Dr. Lindsay has also given glycerine internally in a variety of affections, in combination with several alteratives and tonics, such as iodine, iodide of potassium, quinine, and iron, or as the basis of expectorant or demulcent mixtures. It was found to answer extremely well as a solvent or suspending agent, or a vehicle. The author thinks that all the alteratives or tonics which have recently been combined with cod-liver oil, might be administered more agreeably if dissolved or suspended in glycerine. Such are iodine and quinine; the iodide, lactate, and bromide of iron; the protiodide, biniodide, and bichloride of mercury; the iodides of arsenic and sulphur; and the valerianate of zinc. By the majority of patients, to whom it was given as a nutrient, it was much relished; and its sweet taste would probably render it a favourite with children. The advantages of glycerine over cod-liver oil consist in its pleasant sweetness, and its freedom from all disagreeable odour; in its ready solubility in, or miscibility with, ordinary fluids; in the absence of the principles which, in animal and vegetable oils, so frequently nauseate and purge; and in its solvent and other properties, which render it useful as a vehicle or basis for pharmaceutical preparations. Its great disadvantage is its present comparatively high price. In opposition to Dr. Garrod, who has suggested that cod-liver oil acts simply in virtue of its oleine, Dr. Lindsay believes that glycerine is the active principle. Glycerine appears already to have been tried somewhat extensively in phthisis as a substitute for cod-liver oil, but the results described hitherto are contradictory. In other strumous cases, however, it appears to have proved serviceable.

Dr. Lindsay has used glycerine frequently as a dressing to wounds, ulcers, and abrasions of various kinds, with marked good results; and in these respects it seems to be equal, or even superior, to collodion. In the treatment of bed-sores, he regards it as superior to gutta percha, but inferior to collodion. In the treatment of skin diseases it has been found useful, not only by keeping the skin constantly moist, but by allaying the irritation which so frequently accompanies cutaneous eruptions.

Glycerine may also become very serviceable in pharmacy; and particularly in the preparation of extracts, pills, syrups, and infusions, it promises to be very useful. It has been proposed as a substitute for syrup in such cases as the syrupus ferri iodidi; and as a vehicle for medicines, it combines the properties of a syrup and a mucilage.

II. *On the Endermic Application of Iodide of Glycerine.** By Dr. FERDINAND SZUKITS. (Wochenblatt der Gesells. der Aerzte zu Wien. Sept. 1, 1856.)

The author of this paper, after enumerating the several forms in which iodine has hitherto been endermically applied, proceeds to remark, that all the solvents in ordinary use take up only a small quantity, with the exception of alcohol. It was therefore desirable to discover a solvent which, without affecting the skin like the alcoholic tincture, should take up as large a quantity as possible of the iodine. This solvent was found, in 1854, by Cap, in glycerine. Cap attributed to glycerine the part of a simple solvent, and he proposed it, among others, for the solution of bromine, iodine, oxyde of lead, strychnia, veratria, atropia, morphia, &c. To Dr. Richter belongs the credit of having first introduced into practice the solution of iodine in glycerine. He combined the iodine with iodide of potassium in order to facilitate the solution of the former; combined with this, it may be dissolved in any quantity up to the proportion of almost three to five. But in this concentrated state it is a caustic solution, and too strong for common endermic use; and the author has proposed a proportion of one part of iodine and five parts of glycerine, as a solution which may be applied for a long time to the parts about the neck and to the female breast, without any inconvenience except a slight burning. In the neck and the female breast, the application, after two or three paintings, causes smart burning; and after four or five it produces more or less large excoriations, which require the discontinuance of the remedy and the application of cold fomentations. On the abdomen and in other parts, these symptoms occur much later. After a longer application of the iodide of glycerine, the epidermis peels off on the painted parts. The paintings were performed once a day in the author's cases, and paper of gutta percha was laid over the painted places to prevent evaporation. The paintings may be continued for a month without producing *iodism*, and without causing the slightest disturbance in the well-being of the patient. According to the experiments of Bonnet, the absorption and elimination of iodine may take place to the amount of a gramme of iodine (15·4 grains) per diem for several weeks, without any injury to the general health. The number of the cases in which Dr. Szukits has employed the iodide of glycerine were 24, in some of which the most satisfactory results were obtained.

* See British and Foreign Medico-Chirurgical Review, July, 1856, p. 239.

III. *On the Effects of the Tincture of Iodine applied locally on the Mucous and Serous Membranes, in relation to Pain.* By Dr. BOINET. (L'Union Médicale, June 14th, 1856.)

Dr. Boinet remarks that the contact of tincture of iodine with the mucous membranes is not at all painful; and that it is possible to paint, almost without the consciousness of the patients, the pharyngeal and buccal mucous membranes, the tonsils, the neck of the uterus, the vagina, &c., without causing any pain: on condition, however, of not allowing the tincture to touch the orifices of the mucous cavities—namely, the points where the mucous membrane terminates and the skin commences; for the pain is very severe, and is prolonged for a considerable time, whether the tincture is applied to the lips, the anal orifice, or the female external parts of generation. In these cases the patients experience a pain as intense as when the tincture of iodine is applied to the skin denuded of its epithelium, or to a recent wound. There is the same pain when the ocular or palpebral conjunctiva is touched for the treatment of certain inflammations of the eye, the removal of granulations, &c. If several successive paintings take place, the same change ensues on the mucous membranes as on the skin—namely, that desquamation having taken place, the pain becomes then very severe after the subsequent application. As to the serous membranes, the tincture of iodine always produces in them very severe and cutting pains, and in an instantaneous manner. But this pain is much less severe upon the articular membranes than on the peritoneum. The acute pain produced by the contact of the tincture of iodine with the peritoneum is, in fact, a certain sign which indicates that an ascites has been mistaken for an ovarian dropsy; inasmuch as, in the latter affection, the iodine injection is never painful. This pain is also a proof, when it arises with less intensity in injecting an ovarian cyst, that a certain quantity has penetrated into the peritoneum.

IV. *On the Febrifuge Properties of Apiol.** By Dr. JORET. (L'Union Médicale, June 26th, 1856.)

The author of this paper considers that apiol possesses all the advantages of arseniate of soda in the treatment of intermittents, without the inconveniences which often attend the use of the arsenical compounds; that the safety of its action is also quite manifest, and that the dose of it may be increased without fear of producing any other effects than those which are habitually observed after the administration of the sulphate of quinine. Three cases are adduced in support of the statements advanced, and care was taken not to administer the febrifuge until the fever was completely developed, nor until the succession of the fits, each time brought nearer together, caused a fear of greater severity in the fits about to follow. The author remarks that the sulphate of quinine will long remain the best anti-periodic, and that recourse must always be had to it by preference whenever the object is to cut short a dangerous intermittent, the approaching paroxysm of which might be fatal; but he remarks that the apiol acts with the same safety, that it may be administered with the same advantage, in all fevers where it is not of much importance to put a stop to the paroxysms a day earlier or a day later, and that there is nothing to offer any obstacle to its therapeutical employment.

V. *On the Rottlera Tinctoria as an Article of the Materia Medica.* By Assistant-Surgeon THOMAS ANDERSON, M.D. (Indian Annals of Medical Science, No. 5, October, 1855.)

The *rottlera tinctoria* is a species of euphorbiaceous plant found in the hilly parts of India, as along the base of the Himalayas from Assam to near Peshawur,

* See British and Foreign Medico-Chirurgical Review, Jan. 1856, p. 247.

in Central India, at the Northern Cercars, in Mysore, and at Parell Hill, near Bombay. In its habit it is almost arborescent, growing to twenty or thirty feet high. The substance called kamila, obtained by brushing the powder off the capsules of this plant, has long been known in India as a dye, and it is also occasionally used by the natives as a vermifuge; this latter property is supposed by Dr. Royle to depend upon the stellate hairs found in the powder. Dr. Anderson mentions that his attention was first called to the medicinal properties of this substance by Dr. Gordon, of the 10th Regiment, who had met with great success in employing it as a remedy for tapeworm. Dr. Anderson afterwards employed it himself for the expulsion of the same parasite in the case of several men of his own regiment. The powder is of a dark brick-red colour, with a peculiar heavy odour, increased on its being rubbed between the fingers. Its physiological action is very simple: on an adult the powder in a dose of ʒij. or ℥ss., besides purging, very often causes nausea and vomiting, and in some cases griping; its action on the bowels, however, is very variable, producing from four to ten or fifteen stools even when a dose of ʒiij. has been administered. A strong ethereal or alcoholic tincture, besides acting more mildly, is followed by more uniform effects. Dr. Anderson found that an amount of the tincture sufficient to produce the full anthelmintic effect of the drug was never followed by more than six stools, and always acted without griping. After ʒiij. of the powder have been administered, the worm is usually expelled in the third or fourth stool. It is generally passed entire, and almost always dead, and in about fifteen cases examined by Dr. Anderson he was unable to detect the head. The vermifuge properties of *rottlera tinctoria* have been attested in a large number of cases. Dr. M'Kinnon has mentioned sixteen successful cases in a paper published by him, and he has since administered the powder to nearly fifty patients, out of whom there were only two cases in which no worm was expelled. Dr. Gordon has tried the remedy in thirty cases of tapeworm with uniform success. The dose of the powder of the kamila which seems to act most satisfactorily is ʒiiss. to ʒiij. in an adult; and ℥ss. of the alcoholic tincture is the dose which is followed by the most successful effects.

VI. *Poisoning by Strychnia successfully Treated by Camphor.* (American Journal of the Medical Sciences, October, 1856.)

Professor Rochester has communicated to the Buffalo Medical Association the case of a person, aged thirty-two, who had taken strychnia for the purpose of self-destruction. He stated that the quantity taken was four grains. When brought into the hospital he had several tetanoid convulsions. A large sinapism was directed to be applied to the epigastrium, and two grains of powdered camphor were given with half a teaspoonful of tincture of camphor suspended in water. The sinapism had hardly been applied and the camphor taken, when a spasm commenced, first showing itself in the cervical muscles, then in those of the arm and chest, the latter producing slight opisthotonos, and lastly in those of the face, turning the eyes into their orbits, and setting the lower jaw firmly. The pulse was eighty-eight, and regular; respiration seemed to be entirely suspended; no respiratory murmur was detected, but the heart's sounds were quite audible. The paroxysm lasted about three minutes, and at its termination the camphor was repeated, with the addition of half a grain of morphia. About half an hour after the paroxysm just described, the patient was seized with another, and the camphor was directed to be given every fifteen minutes. The spasms returned at intervals, but they finally ceased about three hours after his admission into the hospital. The next day he was much better, had had some sleep, and said he was hungry. The camphor, of which he had taken about ʒj., produced neither cerebral nor gastric derangement. Dr. Rochester remarked that this was the second case reported by him this year where camphor had been successfully employed to counteract the effects of strychnia, and he thought that there was no doubt as to its properties as an antidote.

VII. *On the Therapeutical Action of the Galvanic Current on the Human Nerves and Muscles.* (L'Union Médicale, October 4th, 1856.)

M. Remak of Berlin, in a series of experimental researches, has found that the application of a galvanic current to contracted muscles rendered them softer and more obedient to the will. He therefore directed his attention to the therapeutical effects produced by the galvanic current upon rheumatic contractions, as well as those which are combined with cerebral hemiplegia. In the course of his researches, he observed several times that the paralysis of the face or of the tongue, or even intellectual weakness, was benefited by the currents, although they had been conducted only through the extremities; and he was therefore convinced that the action of the current was transmitted to the nervous centres. He accordingly tried its operation in the cure of partial and general chorea, and then of certain cases of paraplegia, and of paralysis of the bladder and the rectum. The greatest success has attended this treatment, but as the number of patients does not yet exceed two hundred, M. Remak is not at present prepared to enter into the details of the methodical application of constant currents in several diseases; but he throws out a suggestion that the galvanic current may eventually be found beneficial in the cure or amelioration of spinal distortions, and of the shrinking of the pectoral cavity, which so often arises in youth by contraction and weakness of the respiratory muscles.

VIII. *On Hyoscyamia.* By Professor Schroff. (Wochenblatt der Zeitschrift der Gesellschaft der Aerzte zu Wien, June 16th, 1856.)

The hyoscyamia employed by Professor Schroff was supplied to him by Meek of Darmstadt, and presented the following properties: it was not crystallized, but presented an amorphous form of crystal, inasmuch as it formed a half-transparent, tough, viscid mass. It presented a yellowish-brown colour, and smelt strongly of the æther and alcohol which were used in its preparation, and had a sharp, biting, nauseously bitter taste. It was gradually dissolved in water by trituration, and also in alcohol and ether, but unchanged by exposure to the air. Concentrated nitric acid dissolved it without changing its colour. When sulphuric acid was poured on it it swelled up, became coloured reddish-brown at the edges, and dissolved into a brown solution. The solutions in acids and the salts were precipitated by tannic acid. The result of the experiments made by Professor Schroff on rabbits proves that hyoscyamia is a poison to these animals, although nearly twenty times the quantity of a very powerful alcoholic extract of the seeds of henbane does not injure them. This circumstance therefore proves the great affinity which exists between hyoscyamia, atropia, and daturia; atropia given to two rabbits caused death, after inflammation of the lungs, while a considerable quantity of the most powerful powdered belladonna did not hurt them. The appearances presented during life by animals treated with hyoscyamia and with atropia, were the same as concerns the inflammation of the lungs, but there were some differences in their respective operation upon the nervous system. In poisoning by hyoscyamia the animals remained quiet, and exhibited no effort to move, which was not the case with animals poisoned by atropia. In the physiological experiments on the human subject with atropia and daturia, the efforts to move were uncommonly heightened, and even amounted to a desire for fighting, in spite of the weakness of the extremities; but neither large doses of the preparations of henbane, nor of hyoscyamia, produced any inclination to increased movement; but, on the contrary, there was a great propensity to sleep, which was subsequently fast and deep. In henbane, also, there was wanting the inclination to delirium, such as resulted from the operation of the other two alkaloids. The author describes the following effects produced on himself by a small quantity of hyoscyamia, which he took before one of his lectures. At the end of the lecture he felt a sensation of dryness in the mouth, throat, and larynx; the pulse fell several beats, and the pupils of both eyes were a little dilated. He felt his head confused, and he had some vertigo, so that he reached home with some difficulty, and was obliged to use a stick in order to keep himself upright. All these symptoms increased remarkably at home; he was unable to read anything attentively, the pulse rose to 90 in three hours, although he was in a perfectly quiet sitting posture, and the natural pulse was 75. The dryness in the throat increased so much that he was unable to bring any morsel at dinner to the œsophagus without moistening it by abundant drinking. The sensation of dryness in the mouth, throat, and larynx, and the indisposition to mental exertion, continued till the period of sleeping; the sleep was quiet and deep. The next day he was quite well. The second and third experiments were performed upon a friend, who took on one occasion a two-thousandth part of a gramme, and upon another occasion, a five-thousandth part of a gramme, of hyoscyamia. The pulse was diminished in frequency in both cases, but in the second it fell at first from 79 to 18, and afterwards suddenly increased in frequency. In both experiments the head was confused, the secretion of saliva was diminished, the mouth and throat were very dry, and the power of swallowing in the second experiment much impaired; there was a feeling of feebleness, dilatation of the pupils; in the second experiment the taste and smell were diminished, headache supervened; and in both cases there was great propensity to sleep, which was quiet and deep.

As a medicinal agent, this alkaloid is serviceable when employed for the purpose of alleviating the irritation of coughing, and promoting sleep. In the latter circumstance it is inferior to morphia, and cannot easily be substituted for it when sleeplessness is caused by violent pain; but it has the advantage over morphia in promoting rather than retarding the evacuation of the bowels. The proper dose of hyoscyamia is one-sixtieth to one-twentieth of a grain, and is best administered as a powder with sugar. One-tenth of a grain is, according to Professor Schroff, too strong a dose. Hyoscyamia surpasses all other drugs in its local operation upon the iris. It acts more rapidly, and produces a more intense and continued dilatation of the pupil, when it is dropped into the eye, than any other medicine, and has besides the advantage of being soluble in water, forming a solution which is less exciting to the iris than daturia and atropia, which are soluble only in alcohol.

IX. *On the Use of Acetate of Lead in Yellow Fever.* By Dr. G. B. Wood. (American Journal of the Medical Sciences, October, 1856.)

Dr. Wood believes that the black vomit of yellow fever is not altogether attributable to the state of the blood, but somewhat also to the condition of the mucous membrane; and that an important indication in the treatment is to obviate the phlogosed condition of the membrane, and to induce a healthy action in its inflamed bloodvessels. These indications, he thinks, may be fulfilled by the acetate of lead, which is at the same time an energetic astringent and a decided sedative. He had tried it in three cases with decided success, but attention is necessary as to the period and circumstances of its administration. It should be commenced with at the earliest signs of the approach of the second stage, for after this it would probably be useless. It should be given in doses of two grains every two hours, without the accompaniment of any other substance that might tend to decompose it, and should be continued steadily until thirty-six grains have been taken.

X. *On the Treatment of Neuralgia by the Valerianate of Ammonia.* (L'Union Médicale, July 8th, 1856.)

Dr. Declat has warmly recommended the valerianate of ammonia in the treatment of neuralgia, and quotes the following very remarkable case in proof of its efficacy in that disease. A lady had been affected ever since six years of age with a most severe facial neuralgia. The pain first appeared on the occasion of her cutting a wisdom tooth; the tooth was extracted, but without any relief of the neuralgia. All the ordinary means were tried in succession: internally, sulphate of quinine, opium, belladonna, sulphate of strychnia, iron, gold, &c.; externally, opium fomentations, blisters, morphia, chloroform, collodion, aconitine, &c. M. Jobert de Lamballe performed cauterization with a redhot iron in the course of the inferior maxillary nerve. This treatment diminished a little the acuteness of the pains, without making them disappear; and, although suffering less, the patient could neither eat nor speak. She was obliged for at least six months to have recourse to nutritive injections and tonic baths to support her health and her life. She was afterwards ordered to take twelve drops of a diluted Fowler's solution of arsenic three times a-day, and this treatment was followed by a little improvement; but the tongue became red, and the stomach painful, and on continuing the medicine at the urgent request of the patient, there were vomiting, diarrhœa, cramps in the stomach, and a return of the neuralgic pains. The arsenious course was then discontinued, and the valerianate of ammonia was ordered. On the 3rd of January, 1856, a teaspoonful taken in the evening rendered the night endurable; two spoonfuls the next day procured relief. On the 6th of January, the patient was able to go out and to converse; on the 19th she opened her mouth and began to eat. The dose of the remedy was successively raised to a dessert spoonful night and morning; the improvement was so great that the countenance assumed a totally different appearance, and the appetite returned. At last, on the 6th of May, the pains having completely ceased for several days, the use of the medicine was discontinued. From time to time some twinges of pain occurred, but each time the valerianate caused them to disappear, and Dr. Declat believes that there is no reason why the remedy should lose its efficacy in case of relapse.

In a subsequent communication,* Dr. Declat has stated that the valerianate of ammonia which he employs is a brown liquid, not very limpid, of a disagreeable taste, and smelling strongly of the peculiar odour of valerian; of this liquid he employs a teaspoonful for a dose in continued neuralgia and hysteria; but he gives two or even three teaspoonfuls in paroxysmal neuralgia, at the period of pain. Dr. Declat was first induced to try the curative effects of the valerianate of ammonia by observing the benefit which he experienced himself from its use when he was suffering from frequent headaches resulting from a severe attack of meningitis. It produced in himself the sedative effect of opium without the cerebral inconveniences which the latter drug always induced.

It should be mentioned, that the composition and properties of valerianate of ammonia are not yet accurately ascertained, and that different specimens obtained from a variety of sources are far from being uniform. It is therefore necessary that some standard preparation should be established before this remedy can be brought into general use.

XI. *On the Use of Chlorate of Potash*† *in Mercurial Stomatitis.* (L'Union Médicale, July 8th, 1856.)

At a meeting of the Société Médico-Pratique de Paris, M. Perrin described the good effects of the chlorate of potash in a case of mercurial stomatitis. In a lady, thirty years of age, attacked with acute inflammation of the uterus, mercurial frictions abundantly employed for several days on the abdomen, had produced very painful mercurial stomatitis, with impossibility of opening the mouth, of swallowing, of moving the tongue, and of speaking; and the whole of these symptoms complicated with an abundant and offensive salivation, which deprived the patient of sleep. Two grammes of chlorate of potash were prescribed in an

* L'Union Médicale, Aug. 30th, 1856.

† See British and Foreign Medico-Chirurgical Review, July, 1856, p. 244.

ordinary gum-potion, and given in spoonfuls every hour. The next day there was considerable improvement; the patient was able to open her mouth and to speak distinctly for the first time for two days. Four grammes instead of two were given in a second portion. After the use of this second potion, the improvement was so complete, that there was no occasion to continue the treatment any longer.

It should be mentioned that at a recent meeting of the Société Médico-Pratique de Paris, Dr. Otterbourg,* without denying the good effects produced by chlorate of potash, has expressed an opinion that the borate of soda possessed equal efficacy in stomatitis.

XII. *On the Use of Cod-liver Oil in some Cases of Dropsy.* By Dr. FILIPPO SINIBALDI. (Bulletino delle Scienze Mediche, December, 1855.)

Dr. Sinibaldi was led to prescribe the cod-liver oil in two cases of dropsical effusion, both of which terminated favourably. The first case was one of hydrothorax, in which the effusion was in the left cavity of the chest, pushing the heart over to the right side. The cod-liver oil was prescribed in the dose of two drachms twice a day after meals, the only other treatment being the use of tamarind pulp, and milk with a decoction of Iceland moss. The oil was increased in quantity to an ounce and upwards each day, and after about two weeks, an improvement was plainly manifested; the patient abandoned his lateral decumbency, the respiration was less difficult, some purulent matter was coughed up, the respiratory murmurs re-appeared with some mucous râles, percussion gave a less dull sound than formerly, and the heart removed from its abnormal situation. The oil was continued, and a nutritious diet was ordered, and the symptoms of disease gradually disappeared, and the patient left the hospital. The second case was that of a boy, aged eight, who was suffering from scrofulous disease, and after an attack of gastro-enteric fever, a fluctuation was perceived in the abdomen. The cod-liver oil was recommended, together with the administration of pills composed of soap, potash, and extract of cicuta. After four or five days of this treatment, the secretion of urine was increased, and the measurement of the abdomen showed a diminished quantity of liquid poured out into its cavity, and after a short time, all traces of œdema disappeared. The patient pursued the cod-liver oil treatment for some months, to which were added the iodide of iron and a meat diet, together with sea air and bathing. It was remarkable that in this case the urine was slightly discoloured and turbid, and gave out an ammoniacal odour. When exposed to heat, it presented a whitish, flocculent substance, analogous to the physical character of albumen; but the same effect was not produced by nitric acid.

XIII. *On the Subcutaneous Application of Medicinal Substances.* By PROFESSOR KURZAK. (Wochenblatt der Zeitschrift der Gesellschaft der Aerzte zu Wien, June 2nd, 1856.)

The inoculation of medicinal substances is performed very little by practical physicians. In Germany, Professor Langenbeck has tried a method somewhat modified by himself in a great number of medicines. The results are very interesting in their practical bearings. He calls his process the "hypodermic subcutaneous method."

The purpose of this proceeding is twofold: first, to bring a medicine immediately to bear upon a particular organ or part of the body, and secondly, by this application, to induce a derivation or revulsion. The absorption of the inoculated matter is facilitated by the very act of inoculation. But there are a number of substances, such as those which cannot be introduced in a fluid or semi-fluid state, the absorption of which is difficult and tedious; and these are mixed by Langenbeck with some exciting vehicle—such as croton oil, or tartar emetic, or both

* L'Union Médicale, October 21st, 1856.

together. In the *inoculation by needle*, the instrument must pierce deeper than in vaccination—namely, into the cellular tissue beneath the skin—and a much larger quantity of matter must be introduced. The inoculation-needle, therefore, presents a two-edged point, has the shape of a myrtle-leaf, deeply hollowed on one side in the form of a spoon. This two-edged point passes into a narrow, blunt stem or neck, which is so fastened by a hinge to a handle with double plates, that the needle can be pushed in like a lancet. The course of inoculation by needle is that the part becomes more or less red, and the skin warmer and harder. When the tissues are normal, there is formed a *passage* of the length of the needle introduced, and which feels hard and stringy, and is generally closed when the absorption into the deep part is completed. If the needle has been sunk from one point in several directions, there are several such *passages* formed, which often combine to form a small cavity. If the openings of the passages lie close together, an ulcer is formed by their union; it is then only necessary to introduce the medicine to be inoculated into this cavity; the substance is taken up in a short time into the canals passing from the ulcer, and generally in from four to six hours it has entirely disappeared. In the *inoculation by plaster*, the medicinal substance is laid upon a small skin-wound by means of a little thread of charpie, and kept in its place by a piece of sticking-plaster. The operation of this mode of subcutaneous inoculation is not so intense as in the needle inoculation.

A great variety of substances are enumerated by Langenbeck as having been employed by him in subcutaneous inoculation; and those were especially selected which are soluble in the cellular tissue or in the parenchyma of organs. For instance, strychnia was inoculated near the vertebral column for weakness of the spinal cord and paralysis; veratria for various skin diseases, as lepra, pityriasis, and scabies; quina for intermittent fever, inoculated into the breast or abdomen; digitalis for dropsy and palpitation, into the scrobiculus cordis; extract of squills as a diuretic, into the neighbourhood of the kidneys; cantharides, for incomplete paralysis of the lower extremities, into the lower part of the spine or the sacrum; cubebs and copaiba, for gonorrhœa, into the inguinal region, &c. &c.

XIV. *On the Chemical, Physiological, and Therapeutical Properties of Iodoform.* (L'Union Médicale, Sept. 4th, 1856.)

This body, discovered by Sénellas, presents itself in the solid form in the shape of glittering spangles, of a sulphur-yellow colour, friable, soft to the touch, of an aromatic persistent smell; it contains more than nine-tenths of its weight of iodine; its taste is sweet, and it has no corrosive property. Administered to dogs, it kills in a weaker dose than iodine, after having given rise to more or less marked depression, and rarely to vomiting. To the depression succeeds a period of excitement, convulsions, contractions, &c. Iodoform is quite destitute of any local irritant action, and does not occasion the slightest vascularity of the mucous membrane of the stomach or of the intestine. The therapeutical properties of iodoform, according to MM. Moretin and Humbert, are the following:—1. In consequence of the great quantity of iodine which it contains, it may be substituted for iodine, and the iodides in all the cases in which the latter agents are indicated. 2. The absorption of iodoform occurs with the greatest facility. 3. Iodoform, applied to therapeutics, possesses over other iodic medicines the advantage of not exciting any local irritation, or any of those symptoms which require the suspension of the latter preparations. 4. Besides the properties which are common to it with iodine, iodoform possesses special virtues: it calms the pain of certain neuralgic affections, and causes a kind of local and partial anæsthesia of the rectum when it is deposited in that part. 5. The doses to which it may be carried are 5, 10, 15, 25, 50 centigrammes *per diem*. 6. The principal diseases in which iodoform has been employed with advantage are, endemic goître, scrofulous disease, rachitis, syphilis, certain affections of the neck of the bladder or of the prostate, and some kinds of neuralgia. It may also be used in phthisis, in those cases in which iodine is employed with advantage.

XV. *On the Employment of the Silicate and Benzoate of Soda, combined with the Preparations of Aconite and Colchicum in the Treatment of Gout, Gravel, Chronic and Gouty Rheumatism, &c.* (L'Union Méd., Sept. 9th, 1856.)

In a memoir lately presented to the Académie Imperial of Paris, MM. Socquet and Bonjeau have proposed, in gouty and rheumatic affections, the employment of the silicate of soda and benzoate of soda. Silicate of soda facilitates the elimination of uric acid, and its influence may extend so far as to render the urine alkaline. This salt, moreover, by its tonic action upon the digestive functions and its diuretic properties, is said to be far superior to the carbonates of soda or potash, which are so constantly employed in the rectification of the uric acid diathesis. The benzoate of soda transforms uric acid into hippuric acid, the combinations of which are extremely soluble, while those of uric acid are hardly soluble at all. This medicine, in thus modifying the part of the acid which may have escaped the action of the silicate of soda, will thus contribute also to diminish its quantity. Colchicum will rapidly carry away, by the urinary passages, the remains of the uric acid which the blood may still contain. Aconite is used to act specially upon the painful part.

XVI. *On the Medical Properties of the Mineral Waters of Pougues.* (L'Union Méd., June 19th, 1856.)

The beneficial effects of the waters of Pougues were known to the Romans. There are two springs: the oldest, called St. Léger, is intended for drinking; and the second, discovered in 1833, serves for the administration of baths. The drinking water is cold and very gaseous; its temperature is 12° (Cent.?), and the specific gravity, 1003·12. Examined at the source, it appears to boil, an ebullition which is dependent on the disengagement of carbonic acid which exists in it in great quantity. When poured into a glass, it is limpid, inodorous, of a rather sharp taste, and alkaline. The water of Pougues belongs to the acidulated calcareous class; it contains a little iron. Its predominant bases are lime and magnesia; but as it contains also carbonate of soda, it is joined in this respect with the acidulous alkaline mineral waters, so designated because the predominating base is soda. The diseases in which the Pougues water is said to be most efficacious, are dyspepsia, chlorosis, especially when connected with alteration of the digestive functions; diseases of the liver and the spleen, particularly the engorgements of those organs; diabetes, which is said to have been often modified, sometimes cured, by its use; gravel and vesical catarrh, which are said to be peculiarly benefited; and gout and scrofula, for both of which the water has often been recommended with great advantage.

XVII. *On the Correct Appreciation of the Curative Powers of Ischl as a Watering-Place.* By Dr. Joseph Polak. (Wochenblatt der Zeitschrift der Gesellschaft der Aerzte zu Wien, May, 1856.)

Chronic catarrh of the respiratory passages, simulating tuberculosis, is said by Dr. Polak to be arrested or removed by a residence of a few weeks. Persons suffering from pulmonary tubercle also visit the valley of Ischl for its remedial powers, and this they do before their autumnal and winter journeys to the south; and although the deleterious influence of the tuberculous process is often accelerated by mountain air, yet in certain cases, the patients have been known to regain their flesh, and to walk without fatigue; and this improvement has extended into the autumn and the winter, so that patients of this kind, when circumstances did not allow them to visit the south, have been exposed in a much less degree to the irritation of coughing and the pain of pleuritic seizures; and in women especially, the catamenial functions formerly interrupted have again assumed their regular course. Dr. Polak also thinks that exudations from the pleura and the peritoneum resulting from inflammation of those membranes, are absorbed more rapidly than would otherwise be the case by the course of treatment pursued at Ischl. Swellings of the ovaries and the uterus, resulting from chronic inflammation, and hysterical forms of disease, are found to be remarkably benefited by a residence at this watering-place. Scrofulous diseases are benefited by the saline baths, especially as they are combined at Ischl with good air, and the opportunities afforded for all kinds of strengthening exercises. Dr. Polak opposes the general opinion, that Ischl ought only to be visited in the height of summer, for he states that at this period of the year the weather is often changeable and treacherous, while in spring and autumn, especially in the months of May, September, and October, the weather is more constantly fine, and exercises a most powerfully beneficial influence upon both body and mind.

XVIII. *On the Employment of Tartarized Antimony and Sulphate of Quinine in Acute Articular Rheumatism.* By M. FELIX BARBEAU. (L'Union Médicale, Sept. 4th, 1856.)

The idea of employing sulphate of quinine in large doses in acute articular rheumatism originated with M. Briquet in 1842; but although this treatment has succeeded in many cases, it has frequently failed. M. Barbeau attributes this failure to the neglect of due attention to the digestive organs in the treatment; and he suggests the employment of tartar emetic, with a view to causing an evacuation of the digestive passages, before commencing the administration of the sulphate of quinine. He adduces five cases of this mode of treatment, all of which terminated successfully. The tartarized antimony carrying away the noxious matters contained in the stomach, the sulphate of quinine is rapidly absorbed, and arrives in a short time at its destination; a fact which is indicated by vertigo, singing in the ears, and deafness. Care must be taken not to stop too quickly the administration of the sulphate of quinine, or even to lower the doses too suddenly, for in that case the fever will return immediately with great intensity. It is necessary to continue the same dose until the pulse returns to its habitual frequency. The use of sulphate of quinine is attended with some inconvenience: it sometimes happens that it cannot be borne, and that it excites vomiting. In the first case recorded by M. Barbeau, the patient was improving, but his dose of quinine was omitted by accident, and there was an aggravation of all the general and local symptoms; but on resuming the medicine, the symptoms again subsided, and the patient rapidly recovered.

QUARTERLY REPORT ON SURGERY.*

By JOHN CHATTO, Esq., M.R.C.S.E., London.

I. *On the Duration of the Incubation of Syphilis.* By Professor SIGMUND. (Wien Wochenschrift, No. 18.)

Researches made in great numbers, under most varied circumstances, in all parts of the world, have pretty exactly determined the *shortest* period within which secondary symptoms first plainly appear after the production of the chancre. This is found to be the sixth week, or soon after, an earlier period being very rare, and a much later very uncommon. But when we wish to ascertain the *longest* period within which such symptoms may become developed, the most opposite views prevail; the so-called *latency* lasting, according to some, for a few months, and to others, for some years. The solution of a problem as difficult as it is important, can only be obtained at the hands of specialists dealing with a series

* **Press of matter compels us to postpone the Quarterly Report on Medicine.**

of varied and long-observed cases. The differences of opinion upon the point are due to defective examination and observation of the patient, erroneous computation, varying ideas as to which constitute secondary symptoms, and the difficulty with which such symptoms are in some cases recognised.

As to defective examination of the patient, daily experience shows that eruptions, glandular swellings, excoriation of fauces, &c., are either overlooked or are attributed to other causes. A careful examination of the lymphatic glands is of the rarest occurrence, unless the patient complain of pain. But acutely painful and rapidly suppurating glands are of far less significancy, in relation to secondary syphilis, than the slowly enlarging, hard, non-suppurating, nearly painless glands. Even an accurate examination of the chancre does not always take place, providing that this has but cicatrized over—this being, unfortunately, in the idea of most practitioners and patients, identical with a cure; and yet the condition of this has an essential signification. Not the slightest value is to be placed upon the assertions of the patient, careful and repeated examination alone serving as a guide.

Although specialists may agree as to the signs of secondary syphilis, this is far from being the case with practitioners less familiar with the disease. To the first series of symptoms belong induration of the chancre, and indurated, almost painless, enlargement of the glands nearest to the chancre; enlargement of the other glands accessible to the touch, as in the axilla, neck, &c., forming a continuation of the first series of symptoms. Papulæ on the skin and mucous membranes, especially near the genitals and anus; spots on the skin, and sharply circumscribed redness; swelling and superficial ulceration of the fauces, are soon added to the first symptoms. Such appearances are found in pretty constant succession within the first three months. Examining 1473 cases that have occurred to the author during the last five years, with reference to the period of incubation, 293 have been selected, either because copulation had occurred only once or only after a very long interval had elapsed, and because the characteristic chancre, still present, had undergone no essentially influential treatment. In these 293 cases, the lymphatic glands were affected in all, the chancre was indurated in 261, the fauces were affected in 248, spots on the skin existed in 204, and papulæ, pustules, or condylomata in 134, besides undergoing various combinations with each other, that we need not transcribe. The following is the scheme of the period of the respective appearances of these symptoms:—

	In the 4th	5th	6th	7th	8th	9th	10th	11th	12th week
Enlarged glands	31	44	56	74	46	20	13	9	— times.
Spots of skin	—	—	2	41	68	45	22	11	2 „
Papulæ and pustules	—	—	—	3	10	11	24	27	31 „
Affection of fauces	—	—	—	7	22	34	41	41	42 „

	On the 9th	10th	14th	17th	19th	21st day.
Induration of chancre	71	84	76	15	12	3 times.

Secondary symptoms may, however, exist, without being thus distinctly exhibited, and these difficult cases are only possible of recognition when we have known the patient, and carefully examined him prior to his illness, or from the beginning of the affection. They consist in changes in the skin, which becomes of a pale, white, fawn, yellow, or brownish colour; it is wrinkled, dry, harsh, rough, and hard, its soft and elastic turgor having disappeared. The muscles lose their hardness, elasticity, and energy. Nutrition is defective, and there is often rapid emaciation; various kinds of disturbance of the digestive organs being present. Pains are present in various parts of the body, especially in the head, neck, joints, and muscles; sleep is disturbed; and the spirits are dull and devoid of tone. All these symptoms may be due to other causes, but it is the office of the circumspect practitioner, in these less marked pictures of disease, to form his diagnosis by combining and carefully weighing all appearances. Although such cases are rare, yet is their occurrence not to be denied; and they are of importance, because, at a later period, and sometimes very late, other series of secondary and tertiary appearances may be developed, and then the interval that has elapsed is reckoned as part of the incubation-period. In these cases the author has almost always found the glands affected, though this may be to a slight extent. It is, in fact, in cases in which the forms are not very tangible, but much oftener because the patient has not been subjected to exact and well-timed examination, that instances of prolonged incubation-period of syphilis have been said to occur. The existence of such instances are ignored by the author; and he lays down his position, that when a chancre has healed, and manifests no induration within the first three months, and when none of the already-mentioned appearances are manifested, the patient has no cause to fear further syphilitic symptoms, unless he have, in the mean time, exposed himself to a new infection.

II. *On the Valves of Abscesses and Fistulæ.* By Professor ROSER. (Vierordt's Archiv, 1856, pp. 349—354.)

In an abscess that has spontaneously opened, we often may observe a valvular mechanism, permitting the pus to flow out, but not allowing the entrance of air. This may be the case, not only with abscesses opening upon the skin, but those discharging upon internal membranes. Thus, we sometimes find an empyema emptying itself through the bronchi, the most careful examination failing to detect the admission of air into the cavity of the abscess. The same thing occurs in several abscesses within the abdomen. Collections of matter may be discharged through the intestinal canal, the intestinal gases or fæcal matters not reaching the abscess. So also in respect to the rectum, the bladder, the urethra, and the trachea. Indubitably, in many of these cases, a valvular mechanism must be supposed to be present; and to this, and its prevention of the putrefactive decomposition of the contents of the abscess, many a patient has owed his life.

But all valves of abscesses are not thus useful, some being injurious in their operation, as when they obstruct the issue of the pus, and prevent the complete emptying of the abscess. Such abscesses keep filling again and again. Under the influence of forcible distension or acute suppuration, the mouth of the abscess becomes from time to time widened, and the valves are pressed aside or torn through; but they are soon reproduced, and the obstruction to the flow again occurs, so that such collections may last for years. The valvular condition of such abscesses can be demonstrated; for the pus cannot be pressed out, although the opening may be large. But if a catheter or other tube be introduced, or the opening be enlarged by a knife, a considerable quantity of pus is often suddenly discharged. These valves may be often temporarily displaced by a sound; and by its daily introduction, many abscesses of this description may be healed; but in more obstinate cases, the introduction of tubelets or repeated incisions are required.

As a general rule, it is not in subcutaneous abscesses that we find this valvular mechanism, but in the more important and deep-lying collections—as in empyema; deep cervical, post-mammary, or axillary abscesses; and in those of the abdomen and pelvis; and in periosteal and perinæal collections. The valve may not always be found near the orifice, and the deeper it is placed the less are we disposed to pursue it with the knife. In such cases we must endeavour to widen and maintain the opening by means of metallic or caoutchouc tubelets, or sponge tents. In many places, as in the deep parts of the thigh, the neck, and the pelvis, it is preferable to produce forcible dilatation with a forceps to penetrating too far with the knife. Professor Roser has often had recourse to dilatation of this kind in deep-seated acute or chronic abscess, in the removal of osseous sequestra, and in operations for hernia.

Three kinds of these valves may be distinguished:—1. The *obliquely-placed valves* are the most common, and they may be called from their describer, Abernethy's valves. As physiological examples of such, the entrance of the ureter into the bladder, and of the parotid ducts into the mouth, may be adduced. In the lip-form fistulæ a similar valvular mechanism is brought into play, an example of which occurred in Dr. Beaumont's case of fistulous opening into the stomach. In a case of lip-form vesico-vaginal fistula which occurred to the author, a catheter could be easily passed along; but if air or water were injected into the rectum, no portion, owing to the oblique direction of the valve, entered the vagina. 2. Among the obliquely placed valves, the *wart-like* may be included. The mouth of the abscess is surrounded by spongy warts, from the midst of which the pus issues, while the admission of air is prevented by the warts pressing inwards so as to close the orifice. Such openings are not infrequently observed at the surface; and Professor Roser believes that, in many intestinal perforations, a similar mechanism prevails. 3. The *pad-like* valve, which is analogous to the valve of an air-pump, and an example of which is found in the valve of the prostate. It is this form which especially opposes the exit of pus, and calls for surgical interference.

III. *On the most eligible Spot for the Performance of Amputation of the Leg.* (Gazette des Hôpitaux, Nos. 116, 117, 120, 126, 129, and 131.)

A prolonged discussion upon this subject has recently taken place at the Société de Chirurgie. M. Larrey took occasion to observe, that the soldiers who have of late arrived from the Crimea, having had amputation performed at the middle third or lower part of the leg, were in so bad a condition as to lead to the conclusion, that amputation at the place of election must in the end prevail. The difficulty in employing artificial limbs is so great, and the accidents which result are so numerous, that the patients at last find themselves obliged to resort to the wooden leg. M. Chassaignac, believing our first duty to be the preservation of life, thinks we should never resort to the place of election when we can perform supra-malleolar amputation. M. Verneuil stated that he had paid much attention to the ulterior effects of amputations, and he thinks that supra-malleolar amputation has been too exclusively recommended. There can be no doubt but that the immediate mortality is far less than after the old mode; but we should also take into account the amount of ulterior benefit derivable by the patient. Startling as the assertion may seem, he thinks that in certain cases it is better to run the chance of a greater mortality, than to perform an operation that may prove useless and require repetition. Supra-malleolar amputation is much oftener followed by conicity and other defective states of the stump, than is amputation high up; while osteitis, caries, or necrosis of the bones of the leg, is a more frequent result. This last usually has occurred when the operation has been performed for disease of the tibio-tarsal joint, the osteitis of the bone having spread from the disease of the joint. The first results of the operation are deceptive—for it has an antiphlogistic effect—and for some months the patient may seem cured. But later, either spontaneously or from slight causes, the osteitis is reproduced, and may necessitate secondary amputation. Therefore, whenever amputation is performed for disease of this joint, it should be practised at the upper third. But in traumatic affections, and in disease of the bones of the foot, in which those of the leg but little participate, the supra-malleolar operation is preferable.

M. Guersant has found, in operating upon children, that the mortality is the same in both localities; but from his patients having in after-life to provide for their living, and finding difficulty in getting artificial limbs, he prefers operating at the place of election. M. Huguier dwelt upon the relative safety of the supra-malleolar operation, having lost only one patient in 14 cases; but he admits that the predilection for this operation which his success imparted to him, has undergone considerable modification on observing its ultimate consequences. These never follow when the operation is performed for traumatic lesions, and he does not recommend it in the case of white swelling. M. Broca admits that many patients who have undergone supra-malleolar amputation, have suffered severe accidents from want of a suitable prothetic apparatus; and great is the inconvenience produced by the long stump when a wooden leg is resorted to. Still these effects are as nothing when compared with the greater safety of the operation; and while it is admitted that $\frac{5}{6}$ths of these patients recover, more than half of those die who are operated upon at the place of election. Even in those cases when necrosis demands another operation, secondary amputation is less fatal than primary. As to the question of the ultimate effects of the two operations upon the stumps, after amputation at the place of election, the patient rests upon his knee, which gives him a firm support, but he is deprived of the power of flexion and extension of the joint. After the supra-malleolar amputation, the artificial limb is supported at the ischium, and a hinge-joint allows of such movements at the knee, that it is quite surprising how perfect a substitute the apparatus becomes. It is true that the poor only obtain ill-made apparatus, which frequently get out of repair, and often ultimately produce irritation and ulceration of the stump. Still it is the duty of the surgeon to perform that operation which saves most lives, and leave the supplying these defects to others.

M. Robert observed that if the relative amount of mortality were to decide the question, there could be no doubt about the preference. In children, however, amputation at the place of election is preferable, for the mortality is not greater, while there is difficulty in fitting a prothetic apparatus and necessity of changing it. Even in the adult, the question of preference is doubtful, when the occupations of the patient are laborious, for he then often forsakes the artificial limb for the greater solidity afforded by the wooden leg. Then, again, the nature of the lesion should exert great influence upon our decision. When it affects the foot, but not the joint, the supra-malleolar operation is preferable, but it should not be had recourse to in the case of white swelling of the joint. M. Giraldès thought that the instances of the soldiers coming from the Crimea, given by Larrey, were hardly fair examples of the effects of supra-malleolar operations, inasmuch as such patients had suffered much in the ambulances, and in shifting from hospital to hospital. He believes that some of the evil results are due to the application of apparatus prior to complete cicatrization. M. Hutin stated that during the eleven years he had been at the Invalides, he had had more than two hundred soldiers under his care who had undergone amputation. In the great majority it had been performed at the place of election, or above this, and in not a single case had he observed any rupture or ulceration of the cicatrix. Among those patients, however, in whom it had been performed at the lower third, these were common. The fusiform disposition of these stumps, the almost constant presence of ulceration, and the inconvenience produced by the constriction of artificial limbs, induce the patients to reject these in favour of the wooden leg. With this, the large projection of the stump behind is most inconvenient, and gives rise to the production of great irritation. During winter, the stump becomes cold, violaceous, tense, and painful, while ulceration of the delicate and unsupported cicatrix is almost constant.

IV. *On Orchitis.* By M. Velpeau. (Gaz. des Hôpitaux, No. 122.)

In the course of his annual clinical review at La Charité, M. Velpeau made some interesting observations upon the cases of orchitis. These were 50 in number, 48 being acute and 2 chronic. It was remarkable that 24 occurred on the right and 24 on the left side, two cases being double—one of these being an example of tubercular disease. M. Velpeau observed that examples of tubercular testis should teach us the caution necessary in laying down absolute laws in pathology. Louis has laid down such a law in stating that when tubercles are found in any other organ, they will also be found in the lung; but the testis offers numerous exceptions to this, which it is necessary to bear in mind, lest our prognosis be needlessly unfavourable.

Of the 48 acute cases, 3 were parotidean, 2 were due to masturbation, 6 occurred without appreciable cause, and 37 arose from gonorrhœa. The variety of orchitis due to mumps, of which there were three examples, should be distinguished from the others, as it has neither the same duration, mode of progress, or appearance. The epididymis is moderately swollen, the testis is increased in size, and the scrotum is slightly erysipelatous, while there is generally no fluid in the tunica vaginalis. This form is rapidly developed, reaches its height almost at once, and then decreases spontaneously, resolution soon being completed. It is evidently quite a special kind of inflammation.

In several cases masturbation was suspected, and in two was ascertained to be the cause; and it is easy to see how orchitis may arise from irritation induced at the lower part of the urethra by this practice. It is, however, only of late years, after close interrogation of the patients, that M. Velpeau has admitted this as a cause of orchitis. It is a variety also requiring to be studied apart. There is less swelling of the epididymis, and little or no fluid. If the cause ceases, resolution takes place in three or four days.

Six of these cases are said to have occurred without appreciable cause,—that is, independently of all inflammation or irritation of the urethra. The patients often attribute the occurrence to a strain, but the data furnished by anatomy have led to the denial of the influence of this cause, inasmuch as compression of the cord cannot be produced by the external ring. This doctrine has prevailed since the time of Winslow, but then the external ring only was taken into account. Since then it has been shown that a bundle of fibres extends from the external edge of the aponeurosis of the rectus to the crest of the ileum. These form an arch with its concavity upwards, upon which the cord lies, making a more or less acute angle at the internal orifice of the inguinal canal. It is the compression exerted by this fibrous arch during straining that may become an occasional cause of orchitis, when it has been carried far enough to notably impede the circulation through the cord.

Of the thirty-seven cases of gonorrhœal orchitis, in seven or eight there was no notable quantity of fluid in the tunica vaginalis; and in employing punctures in orchitis, there never flows out a quantity of fluid equivalent to the volume of the tumour. We may always observe swelling of the epididymis or of the testis, or of both. The fact of simultaneous swelling of the epididymis and of the testis shows the impropriety of the term epididymitis that has been applied to orchitis. It is, indeed, often difficult to determine the presence of fluid when there is swelling of the testis, or even when the testis itself is healthy. The testis gives to the finger, in fact, a sense of fluctuation. But if we grasp the scrotum, so as to cause the tumour to project forwards, if there is even but a thin layer of fluid, we find it presenting a non-resisting plane to the finger, which, giving way, allows us to come upon a more resistant plane, in which we still perceive fluctuation. This last is the testicle; but to distinguish slight accumulations, it requires that the finger should be well exercised. The vas deferens is affected in the majority of cases, being swollen and painful; and this is of importance, for such a condition of the canal implies a longer duration of the orchitis. The testicle may indeed be compared to an inflamed gland, and just as sometimes we do not perceive the inflamed absorbent vessel, so here there may be an absence of swelling of the vas deferens. Swelling of the epididymis also implies a longer duration of the affection; and it may be stated that this will be less in proportion as the testis is more affected than the epididymis and the vas deferens.

The mean duration was in these cases sixteen days; forty-six of the patients were cured, two were not so when they left, and one of these afterwards died. In this case the orchitis was not very severe, and succeeded to a mild gonorrhœa, contracted by a young man having hypospadias. He died of peritonitis; and on examination, all the seminal passages were found to be the seat of blennorrhagia.

The vesiculæ seminales were in a state of suppuration, and the peritonitis had originated at the recto-vesical cul-de-sac. This is a rare case; but, as M. Velpeau has long since remarked, inflammation of the vesiculæ seminales is by no means a rare affection after gonorrhœa. The treatment of these patients has consisted in the employment of rest, cold, suspensories, mercurial inunction, and either single or multiple punctures with a lancet, abstaining from leeches. Punctures, by giving issue to the fluid, give great relief to the patient, certainly abridges the duration of the disease, and is exempt from inconvenience. In appreciating various modes of treatment, we must never lose sight of the varieties of the affection, for these will explain much of the success said to follow some of the modes proposed.

V. *The Hæmostatic Douche.* By M. GAILLARD. (L'Union Médicale, No. 127, 1856.)

M. Gaillard gives this name to a contrivance he has often had recourse to during the performance of operations, especially upon arteries, which, without giving rise to an amount of bleeding that can be termed hæmorrhage, yet induce it in sufficient quantity to obscure the steps of the operation, imparting the same colouring to all the cut parts. The more the coagula are wiped away, the more does the tomentous cellular tissue become infiltrated with blood, forming a kind of pseudo-membrane, beneath which all distinctions of muscle, aponeurosis, and vessels disappear. The means consists in placing a vessel full of cold water upon an elevation near the patient, and connecting with it a flexible syphon, terminated by a straight canula. An assistant, watching the steps of the operation, guides a continuous stream of water over the surfaces as they are divided, regulating the force of this by means of a small cock. The surface of the incisions thus made under water are kept quite free from blood, and the distinctive colour of the various parts is easily observed.

VI. *On Auscultation of the Ear.* By M. GENDRIN. (Comptes Rendus, tome xliii. No. 9.)

M. Gendrin, believing the diagnosis of the diseases of the deep-seated portions of the ear may be facilitated by the employment of auscultation, communicates the results of six years' examination of the subject. The auscultation may be either mediate or immediate, and the patients' nares should be closed.

In the physiological condition every expiration produces in the tympanum a deep, gentle, distant *bruit de souffle*, which passes away before the end of the expiratory movement. If the *membrana tympani* is perforated, this sound becomes acute, dry, sometimes even sibilant, and is more prolonged. When the Eustachian tube is narrowed it becomes intermittent, consisting of several successive *souffles*, which are usually accompanied with bullar crepitation due to mucosities. Crepitation may be heard when there is caries of the internal ear, or when there is a collection of matter on the middle ear, or in the mastoid cells, in communication with the tympanum and the open tube; but here the crepitations are deep and moist. Coughing renders these abnormal sounds shorter and more clear, so that they are more easily recognised.

Inspiration, in a sound ear, does not give rise to any perceptible sonorous vibrations; but if the membrane is pierced, the Eustachian tube remaining pervious, a very sharp sibilant *souffle*, mingled with moist crepitation, is heard, the patient himself often being conscious of the sound.

The voice, heard in the ear, appears deeper and slightly vibrating, and is interrupted by frequent and sudden intermissions. When the tube is narrowed, or the tympanum is filled with mucosities, by pus or by a central exostosis of the petrous bone, it degenerates into a confused and inarticulate murmur. It is not heard when the tube is obstructed; and it becomes whistling, and is accompanied by crepitating bullæ, when the membrane is ruptured. In the normal state, the labial hissing is transmitted by the ear like a distant acute *sibilant souffle.* It is much weakened or almost silent when the tube is narrowed, and is not heard at all when this is obstructed. When the membrane is destroyed, the tube remaining free, the hissing becomes very acute, and is so near that the patient seems to be whistling in one's ear. In most cases the abnormal sounds may be verified by a comparative auscultation of the ears, as it is rare to find the same degree of the same lesion in the two ears.

VII. *On Galvanism in Opacities of the Cornea and Nervous Palpebral Palpitation.* By Dr. A. Quadri. (Annales d'Oculistique, vol. xxxvi. p. 41.)

Dr. Quadri is of opinion that one reason why electricity is not oftener serviceable, arises from its employment being reserved for those cases in which all other means have failed, cases of confirmed paralysis due to changes in the nervous substance, over which it exerts no control. If it were resorted to for the treatment of slight neuroses, success would be more prompt and easy; and patients might not require dosing with medicines which are as often dangerous as useful. On the present occasion, Dr. Quadri first relates the results of the application of galvanism by means of Bunsen's pile in two cases of marked albugo. Very few applications, each continued for five minutes, effected marked amelioration. In old and very thick leucoma he has found electricity of hardly any avail.

While engaged in trying the remedy in amaurosis, for which disease it seems to have been of very little avail, he observed that it exerted remarkable power over the nictitation which so often accompanies amaurosis. It then struck him that it might prove of utility in the nervous affections of the muscles of the eye, nerves to which a direct application could be made, impossible in the case of the optic nerve. Four cases are given in which the remedy seems to have been of very considerable avail. In the first, persistent convulsive movements of the eyelid, accompanied by great photophobia, had resisted other means. In the second, involuntary contractions due to bad acquired habits, occurring in a girl ten years of age, were relieved. The third was another example of convulsive motion of the eyelid, with photophobia; and the fourth was a case of ptosis.

VIII. *On Vicious Cicatrices.* (Gazette des Hôpitaux, No. 116, 1856.)

At a recent discussion at the Société de Chirurgie, some interesting observations were made upon this subject. M. Chassaignac, speaking of the cicatrices from burns, called attention to the importance of operating for these when occurring in children at as early an age as possible, inasmuch as the parts influenced by cicatricial bridles fall into a state of atrophy, or rather, suffer from an arrest of development. Thus, in a case in which the little finger had been kept flexed by a bridle, the finger, after rectification, never attained to more than half the size of the other. What is very remarkable in these cases is, that the joints remain uninjured, in spite of their long disuse; but nevertheless the motions of the limb cannot be at once re-established after the division of the bridle, for the insufficient development of the soft parts beneath the cicatrix prevents the necessary extension being made without laceration. In the adult, in whom development was completed at the time of the accident, such complication is not to be feared. The rule for these operations, therefore, is, that while in the adult they may be deferred without inconvenience, in children they should be performed as early as possible. M. Guersant also had found, after complete division of cicatrices at the bend of the arm in children, great resistance made to extension; but he never attempts to use force to overcome it. He relates a case occurring in an adult, who was twenty years old when burned, and in whom the limb offered no resistance to extension made six years after. M. Vernueil observed, that it is proper to wait before operating until cicatrisation is quite completed, a year sufficing to confer all its properties on a cicatrix; but we must not overlook the consecutive lesions that are developed with time. M. Desormeaux related an interesting case of a young girl of about sixteen, in whom the knee was powerfully flexed and the foot extended, in consequence of a bridle formed after a burn occurring in infancy, and which extended from the thigh to the heel. By division of this, and of several of the flexors, the position of the limb was rectified; but the limb was atrophied, small, and short, great lameness resulting. Restored to its normal position, however, it rapidly increased in size, so that in two or three years it had become almost as large as the other, and had so increased in length that lameness no longer existed. M. Larrey noticed the great difficulty military surgeons have in deciding whether the amount of immovability in certain contracted limbs is wholly or in part simulated, and he recommends the following plan for determining this. The two limbs are placed side by side, and gentle movements are imparted to them. Presently, and all of a sudden, this movement is rendered rough. If the affection be simulated, the patient being unable to completely separate in his mind the movements of the two sides, the two limbs yield or resist together, it being sometimes the sound side that resists the most.

IX. *On the Treatment of Bubo.* By M. Broca. (Bulletin de Thérapeutique, tome li. p. 208.)

M. Broca observes that a bubo undergoes two stages of development, during the first of which the inflammatory engorgement is confined to the gland itself, this containing a small central cavity filled with semi-fluid pus. In the second stage, suppurative inflammation is propagated to the surrounding cellular tissue; and it is by such extension that the ravages of bubo are produced. The object of the proposed means of treatment is to prevent the production of this secondary abscess, by attacking the bubo during its first stage, and evacuating the pus before this has extended beyond the limits of the gland itself.

M. Broca prefixes some observations upon the diagnosis of the form of bubo that should be so treated, these being based upon Ricord's doctrines. Such bubos are indurated, rounded glands, the skin over which is not discoloured, and they have very much the appearance of the indolent bubo met with in the first stage of constitutional syphilis, but which, never suppurating, requires no local treatment. This indolent, *constitutional* bubo is in fact one of the first symptoms of secondary syphilis which follows indurated chancre, and is amenable to mercurial treatment. The *local* suppurating bubo never appears but in glands which are in direct communication with the part that is the seat of chancre, which chancre is never indurated, and never gives rise to constitutional syphilis. It is amenable only to local treatment, and the existence of a glandular abscess is sufficient to conclude that the syphilis is local, and that mercury is inexpedient. When this *local* bubo has reached its stage of complete development, there is therefore no difficulty in its diagnosis; but at first, prior to the propagation of the suppurative inflammation to the cellular tissue, it may be confounded with constitutional bubo. But, as has been stated, this last almost constantly arises from indurated chancre, which is never the case with the local bubo. The constitutional exists on both sides, the local is very often unilateral. The latter is never accompanied by symptoms of constitutional syphilis, while in the former there are always more or less evident signs of a general infection, which gives rise to other analogous glandular engorgements, and especially at the postero-superior cervical region. The tumour in constitutional bubo is quite indolent, while the other is always more or less painful, especially upon pressure. In the former there are generally a considerable number of glands engorged, which are scattered over the whole extent of the bend of the groin; while in the latter, but two or three glands, placed close to each other, and often only one, are affected. In constitutional bubo the tumour is very hard and entirely solid; but in local bubo it is somewhat less hard, and imparts a sense of fluctuation similar to that furnished by a small cyst with very thick walls. This fluctuation alike differs from that of an ordinary abscess, and from the resistance of solid tumours. It is due to the semi-fluid purulent matter contained in the centre of the gland.

The accurate diagnosis is of importance, as the treatment recommended is applicable only to the local bubo. When the gland has acquired the size of a small hazel nut, it should be firmly fixed by two fingers of the left hand, and a bistoury plunged into its centre. Without letting go of it, the bistoury should be removed, and a grooved director passed in. On employing strong lateral pressure, a small quantity of semi-fluid, ill-elaborated pus is forced along the groove; and the pressure must be continued until the blood comes, so as to secure the entire discharge of this pus. It is rather a painful procedure, and must be repeated on each affected gland. The tumour becomes a little reduced in size, but next day it has somewhat enlarged again, and the small quantity of pus that has again formed must be discharged by passing in the director and using pressure. This must be done every day until either suppuration ceases, or a small fistulous opening has become established for the discharge. In some of his cases, M. Broca has injected tincture of iodine by means of a small syringe, and he thinks this may exert some effect in neutralizing the virulent properties of the pus when this is inoculable. At present but 9 cases have been treated by this new mode, no ill effect having resulted in any of them; while extension of suppuration to the cellular tissue, with the consequent ulceration, detachment of skin, &c., has been avoided. In 5 out of the 9 cases, less than a week sufficed for a cure; the other cases requiring twelve, thirteen, thirty-seven, and fifty days; a small fistulous opening alone remaining during that period, in place of the large purulent collection usually observed. This mode of treatment, therefore, even when it does not abridge the duration of the bubo, materially restrains its extension.

Since the above paper appeared, M. Gély, surgeon of the Hôtel-Dieu, Nantes, has published* an account of some observations he made upon the subject in 1852–3. He states that he has derived great advantage from making punctures with a lancet at an early period, sometimes as soon as the third or fourth day. He introduces no conductor, and employs no pressure, but makes a puncture large and deep enough to allow of a free escape of the pus; and applies a tepid cataplasm if there is much inflammation.

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D. (Lond.)
LETTSOMIAN LECTURER ON MIDWIFERY, ETC. ETC.

I. Diseases of the Generative Organs.

1. *Case of Vesico-Uterine Fistula cured by Operation.* By M. Jobert (de Lamballe). (L'Union Méd., Nov. 22, 1856.)
2. *Case of Rupture of an Ovarian Cyst, and Absorption of the Fluid.* By Dr. Gautier. (L'Union Méd., Nov. 25, 1856.)
3. *Two Cases serving to illustrate the Therapeutical History of Ovarian Cysts.* By Dr. Th. Herpin. (L'Union Méd., Nov. 8 and 11, 1856.)

1. M. Jobert's case of vesico-uterine fistula is remarkable. A woman, aged twenty-seven, entered the Hôtel-Dieu on the 4th June, 1856. She was in tolerable health. Menstruation had appeared at the age of twenty. She bore her first child at twenty-two; nothing remarkable followed. She married at twenty-four, and conceived soon after. Six weeks after marriage, she suffered from inflammation of the lower abdomen; abortion of a two-months' ovum followed. Eight days

* Moniteur des Hôpitaux, Nos. 133, 136, 138.

after this abortion, she first perceived that her urine came away involuntarily. Menstruation was suppressed. A curved sound introduced through the os uteri met a silver catheter passed into the bladder by the urethra.

M. Jobert performed the following operation:—With toothed forceps and bistoury the borders of the os uteri were bared, and freshened so as to make two bleeding surfaces; then, by means of curved needles borne on handles, and carrying waxed threads, three points of suture were applied, so as to bring together the lips of the os uteri, and completely to obliterate the opening. A catheter was placed in the urethra. The result was cicatrization of the os uteri and closure of the uterine communication with the vagina. Henceforth the incontinence of urine was removed: the urine came by the urethra; and the catamenial discharge flowed through the same canal. The operator, considering it impossible to close the vesico-uterine fistula by direct proceedings, had resorted to the operation described, which had the effect of diverting the menstrual flow from its usual course into the bladder and urethra, at the same time that the urine was made to take the same course. He conceives that the patient was relieved of a loathsome affection at a moderate cost, by the loss of the reproductive function.

2. A single woman, bearing a large ovarian cyst which had been developing for some years, followed, under Dr. Gautier, a long course of purgative, diuretic, and ferruginous medicines. One day the patient was carrying a full kitchen boiler, when she tripped against a chair, and fell with her left side on the edge of the boiler. She fainted; great pain followed; but she was not conscious of any sensation of laceration. On external palpation, however, the resistance of the tumour had vanished; fluctuation was freely discerned, and the liquid always fell to the lowest point on moving the patient. The liquid was gradually absorbed; and there remained nothing of the tumour but a hard nucleus.

3. Dr. Herpin relates two instructive cases of ovarian dropsy, of which the following is a summary:—1. On the 27th June, 1838, a woman, aged twenty-nine, had been married fifteen months; her husband stated that she bore at that time a small tumour over the right groin. During the last month, this tumour increased rapidly, and began to disturb the intestinal functions. On the 3rd August, pregnancy, which had not existed at first examination, was suspected; the size of the abdomen had greatly increased. On the 4th, the patient was seized with colic and diarrhœa, followed by intense abdominal pain; other attacks came on at intervals of a few hours, and caused collapse. On external examination, it was discovered that the elastic resistance of the tumour was gone, that the tumour had lost much of its volume and consistency. Symptoms of peritonitis came on. This was subdued by leeching, mercurial inunction, and opiates. The cyst grew again, so that at the end of the term of gestation, it had reacquired the size it had before rupture. Labour came on at the period prognosticated, but proceeding slowly, M. Mayor, under whose care she had come, fearing some accident from the violent efforts made by the patient, applied the forceps, and brought forth a living child. She made a good puerperal recovery. The characters of the ovarian cyst became clearly defined. Four years later, the cyst had enlarged considerably, and caused great functional disturbance. Tapping was resorted to repeatedly. After the third, it was perceived that another tumour was springing up to the right of the ovarian cyst; the patient was again pregnant. Several other punctures were made, but the patient sank, with all the signs of advanced phthisis, at the end of seventeen months from the first tapping.

CASE 2.—Cure of an ovarian cyst by absorption, through sudorific treatment and purgatives.

On the 14th May, 1844, a woman, aged forty-five, still menstruating, having had two children, had perceived for some months a tumour, recognised to be an ovarian cyst. M. Herpin prescribed, for every two days out of three, a packing, after the hydropathic fashion, in blankets, and every two hours, until sweating, Dover's powder. Profuse sweating was caused; the blankets were dripping. After fifteen days of this treatment, the tumour had sensibly diminished. After a week's repose a purgative course was adopted. Anderson's pills were given every two or three days. Her health improved; the tumour decreased. Three years afterwards, the tumour could not be felt, and after six years it was ascertained that there was no relapse.

II. Diseases of Pregnancy.

A Case of Vomiting during Pregnancy. By Dr. Turnbull. (Australian Medical Journal, No. 2. April, 1856.)

The case of vomiting during pregnancy related by Dr. Turnbull of Victoria, is interesting on account of the clearness of its pathology. On the 22nd of September, 1855, he saw Mrs. S., who was suffering from persistent vomiting, with quick weak pulse; no pain on pressure over the epigastric region; she was between seven and eight months' pregnant. Hydrocyanic acid; sinapisms to the abdomen; mistura creosoti (Ph. Ed.); chloroform, at first in doses of five minims, half-hourly, afterwards increased; were successively tried and totally failed. The patient was unable to retain even a teaspoonful of cold water. On the next day vomiting persisted, patient almost pulseless; premature labour determined on; os uteri found partially dilated, feet presenting. The membranes being ruptured, the vomiting ceased almost immediately. A living female child was extracted. Placenta removed easily. The vomiting returned five or six hours after delivery. Half a grain of opium and two and a half grains of bismuth were ordered frequently. A small clot was discharged, and on examination a substance was felt protruding through the os; this removed, was found to be a portion of placenta. On its extraction every disagreeable symptom disappeared. (That the cause of the obstinate vomiting was reflex irritation, having its starting-point in the uterus, seems without doubt. Was this irritation before delivery set up by the peculiar nature of the presentation? Had the feet descended into contact with the cervix uteri in consequence of conversion from an originally different presentation?)

III. Labour.

1. *On a Case of Artificial Premature Labour by Scanzoni's Method.* By Spaeth. (Wochenbl. d. Zeitschr. der Ges. d. Aerzte zu Wien. 1856.)
2. *A Case of Inversion of the Uterus.* By Professor White. (Buffalo Med. Journ. March, 1856.)
3. *Case of Inversio Uteri.* By Samuel P. Brown, M.D., of Greensburgh, Pa. (Medical Examiner. June, 1856.)
4. *The Numerical Relations of the Occurrence of Placenta Prævia in Kurhessen.* By Dr. Schwarz, of Fulda. (Monatsschr. f. Geburtsk. August, 1856.)
5. *Extra-uterine Gestation outside the Abdominal Cavity.* By Dr. Genth, of Schwalbach. (Verhandl. der Ges. für Geburtsk., Berlin, 1855.)

1. (In a former Report the method of Scanzoni for inducing premature labour was described. The following note contains the results of its application in practice.)

In Spaeth's case the application of suction to the breasts brought on strong pains, which exerted a marked influence on the cervix uteri, but had to be intermitted on account of the disagreeable effect upon the breasts. Out of eight cases in which the method has been tried, it succeeded in four. In the quickest, labour followed in seven hours; in the slowest, on the thirteenth day after twenty-one applications. The result depends partly on the reflex excitability of the individual, partly on the condition of the nipples. One advantage attending it is the freedom from hurtful influence on the fœtus.

2. Professor White's patient had been attended in her labour by a German midwife, who stated that after a brief labour she had given birth to a male infant weighing upwards of ten pounds. The placenta very soon came away, accompanied by the inverted uterus, which descended into the vagina. The flooding at this time was described as terrific, and caused protracted syncope. No medical assistance was requested for several days afterwards, by which time the tumour had descended through the os externum. The first attempt to replace it was unsuccessful, in consequence of the fainting of the patient from loss of blood during the effort. Next day (the eighth after accident), with the aid of chloroform, a more successful attempt was made. The dimpling of the fundus was preserved by a rectum bougie, whilst the hand was allowed to rest. The re-position was effected by great perseverance under great difficulty. The hæmorrhage ceased entirely after replacement. The patient, however, died two days afterwards. Autopsy revealed great anæmia. There were slight marks of inflammation with exudation, in the peritoneum. The uterus exhibited nothing abnormal.

3. The case of inversion of the uterus related by Dr. Brown serves to illustrate the mode of occurrence of this accident. Mrs. E., a healthy young woman, in her second confinement, was in labour at three A.M. on the 3rd of April, 1856. The pains were recurring, at intervals of fifteen minutes, short, and without trismus. At eight A.M. they became more frequent and effective. Whilst walking about the room a strong bearing-down came on, during which she got on her knees close by the bed; with a violent expulsive effort the child was expelled. She immediately complained of exhaustion, and pain in the abdomen, and desired to be put to bed. In a few minutes after she was found to be flooding profusely. Whilst Dr. Brown's hand was on the abdomen she was seized with a violent pain, which he thought expelled the placenta, but on introducing his hand he found the uterus inverted and the placenta adherent. Dr. Brown attempted the re-position of the uterus with the placenta attached, but failed. Shortly after Dr. R. Brown peeled off the placenta, grasped the uterus in his hand, reintroduced it into the vagina, carried it through the os uteri, and, indenting the fundus, pushed it forwards with the fingers in a conical shape, and thus without much difficulty replaced it. Strong uterine contractions followed, and on examination the fundus was found again slightly depressed. The patient rallied and did well.

4. Those statistical summaries which are based upon complete observation, are clearly the only ones which can furnish laws. In the district of Kurhessen, *every* delivery is recorded, and the nature of the labour related, by the medical attendant. We thus possess the necessary elements for estimating the comparative frequency of particular obstetric occurrences. In the course of twenty years, there were 519,328 births, and placenta prævia occurred 332 times. The numbers vary remarkably in different years—namely, from 8, the lowest number, to 76, the highest. Of these 332 cases, 246 women recovered; 86 died. 251 children were still-born; 85 lived. 40 women were primiparæ. Turning by the foot was practised in 259, and in 7 turning by the hand; 23 children had to be extracted by forceps; 13 were extracted by Cæsarean section after death of mother. The placenta was artificially removed 8 times, and 16 times plugging was resorted to.

5. The case of Dr. Genth is of a very unusual kind. A woman, aged thirty-four, had borne from her earliest childhood an easily-moveable oval tumour, the size of a hazel-nut, in the left side, near the external abdominal ring, under the skin. This tumour gradually reached the size of a walnut after the establishment of menstruation. It was always tender on pressure. At twenty-four she married, and fell pregnant immediately. Without known cause, she suffered in the fourth month a prolapsus uteri, which towards the end of pregnancy had attained the size of a child's head. The tumour in the side remained unchanged. The labour was normal. The prolapsus continued, and had to be supported by a band. After

some years another pregnancy, with prolapsus uteri, took place, without any influence upon the tumour in the side. The third pregnancy resembled the preceding, and ended in 1850. She menstruated regularly until February, 1852.

After menstruating as usual, on the 22nd of February, 1852, the catamenia ceased. Some weeks later, the patient perceived an enlargement of the tumour in the side; it became painful and inflamed; the inflammation was removed by local antiphlogosis; but the tumour grew rapidly. Sixteen and a-half weeks from the cessation of the catamenia the tumour was as big as two fists, uniformly tense, so that its contents could not be determined. It had extended under the skin to the labium majus. In the inguinal canal was felt a pedunculated prolongation, of the thickness of the little finger. The tumour was so distinct from the abdominal cavity, that the horizontal branch of the pubes could be felt between. The patient was now very weak, anæmic, and forced to keep her bed. She begged earnestly for the removal of the tumour. An exploratory puncture was made; some ounces of water followed, and then a quantity of arterial blood. The tumour lost nothing of its tension, and so its contents could not be made out. A longitudinal incision was made. Under the outer coverings a tendinous membrane was seen, which being slit, a capacious round cavity was exposed. In this the finger detected an easily moveable fœtus, which lived for a moment after extraction. It was about four or five months old, and well-formed. The placenta was implanted all round the cavity, and the greater part was removed by the forefinger, with much loss of blood. The wound was closed, and hæmorrhage stopped by cold. On the seventh day another piece of the placenta was removed, which had occasioned another bleeding. In three weeks the wound had almost entirely closed. She recovered her strength quickly. After complete cicatrisation of the wound, the tumour was felt, as before, easily moveable under the skin.

The woman has had a natural pregnancy since. Dr. Genth has no doubt that the tumour was formed by the ovary, which, like the testicle, had escaped from the abdominal cavity, drawing the Fallopian tube after it. The passage of the impregnated ovum into the abdomen was somehow stopped, perhaps by compression of the abdominal muscles, and thus the development of the ovum took place not only outside the uterus, but even outside the abdomen. The falling of the uterus at each gestation is explained by the dragging of the ligamentous part of the ovary preventing the uterus from rising up towards the thorax.

[The Reporter would remark that a stethoscopic examination of the tumour would have revealed the presence of a living fœtus. It is also a matter of question whether removal of the entire tumour, including the ovary, would not have been proper, in order to prevent the possible recurrence of this hernial gestation.]

IV. Puerperal and Post-Puerperal Diseases.

1. *Account of a Puerperal Epidemic raging concurrently with Cholera in the Lying-in Hospital at Trient.* By Professor Braun. (Zeitschr. der k. k. Ges. d. Aerzte zu Wien. Aug. 1856.)
2. *Remarks on the Consecutive Diseases of the Puerperal Condition.* By Dr. Mikschik of Vienna. (Zeitschr. der k. k. Ges. d. Aerzte zu Wien. Nos. 3 and 4. 1856.)

1. We extract from Professor Braun's Report of the movement of the Trient Lying-in and Foundling Hospital for 1855, some brief notes relating to the spread of a form of puerperal fever during the prevalence of cholera. During the year 1855, 279 pregnant women were admitted, 40 remained over from the preceding year, so that 319 were treated. 30 went out undelivered. Of the women delivered, 191 were discharged well; 66 were transferred to the Poor-house, 6 to the Trient Hospital, and 12 died; some remained undelivered. The mortality of the puerperal women was as 1 to 28, or 3·5 per cent. Twenty per cent. of those delivered in August died, whereupon the most speedy separation of the sick from the healthy was effected. With the exception of August, the health-condition of

the house was good. There died in February, 1 woman of asthma of the lungs; in March and May, in each, 1 of pleuritis and peritoneal exudations; in June, 1 of apoplexy of the brain; but in August, when the cholera epidemic had reached its culmination-point in the town, 6 died of a puerperal disease. In this summer, out of a population of 330,000, more than 6200, or 1·9 per cent., were carried off by cholera. From the 10th to the 18th August, 8 women out of 12 delivered, or one daily, sickened, of whom 5 died within a few days. On the 16th August, a man belonging to the house fell ill of cholera, and died on the 20th. Two days after, a perfectly healthy woman, who had been delivered nine days, took the cholera, and was transferred on the same day into the Cholera Lazaretto. Concurrently with this intense puerperal-fever epidemic, cholera infantum spread amongst the children. There appears to have been a serious want of water; a great terror seized the pregnant women; in consequence, a temporary hospital was procured, to which some of the patients were removed, whilst others were sent to their homes, and no more were admitted, except extreme cases, until the 30th September. The first child died of cholera infantum on the 16th August, and the last on the 12th September. The wards were cleansed by chlorine gas and heating, after Busch's method, and fourteen days later, re-occupied without ill consequence.

The following summary of the cases gives a general view of the origin, cause, and symptoms of the fever:—

No.	Day of delivery.	Day of sickening.	Result.	Remarks.
159	Aug. 10th	Aug. 10th	Aug. 12th, died	Strong fever, meteorism, diarrhœa and vomiting.
178	Aug. 10th	Aug. 12th	Aug. 16th, died	Peritoneal exudation, vomiting, and diarrhœa.
175	Aug. 6th	Aug. 12th	Aug. 22rd, died	Septicæmia without localization; no diarrhœa or vomiting.
183	July 30th	Aug. 12th	Aug. 20th, recov.	Metrorrhagia, continued fever: quinine.
151	Aug. 12th	Aug. 15th	Aug. 19th, died	Septicæmia, no vomiting, diarrhœa, or meteorism.
171	Aug. 14th	Aug. 16th	Sept. 19th, recov.	Perimetritis, Aug. 28th; miliaria, Sept. 3rd; metastatic abscesses.
140	July 29th	Aug. 16th	Sept. 22nd, died	Metrorrhagia, then pleuritic exudations.
105	Aug. 15th	Aug. 18th	Oct. 18th, died	Septicæmia, Sept. 1st, choleraic diarrhœa and metastasis. Autopsy: large purulent deposit in apex of right lung.

It was remarked that the cases of puerperal fever preceded those of cholera. It is in the highest degree probable that this puerperal fever arose out of the influences and relations with the ruling contagious miasmatic diseases; for since the 21st July, no autopsy was performed by either of the attending physicians; and after July, every patient on admission was supplied with clean linen, and her own clothing purified by chlorine gas for twenty-four hours. External communication was cut off; the school was closed on 15th July; no obstetric operations were called for in this period; and several pounds of sulphate of iron were daily thrown into the privies. The deficiency of water was supplied at great cost, and good diet (*without vegetables and fruit*) allowed.

During the winter of 1856, no case of puerperal fever appeared.

2. Dr. Mikschik refers to the labours of Heschl, Rokitansky, Wedl, Virchow, Retzius, Rainey, &c., on the fatty transformation of the uterine tissues after labour, preparatory to the return of this organ to its normal state. He observes that, when unfavourable circumstances arrest this transformation, the womb remains in the state of fatty degeneration, and will be found either increased or diminished in volume. This latter condition, more rare than the first, was noted in 1842 by Rokitansky, who pointed out a remarkable brittleness of the uterine fibre. The organ remains porous, pale red, of a slaty-grey here and there, yellowish, and pale yellow. Amongst the causes which induce these changes of volume of the uterus, must in chief be noted puerperal diseases, which fill the organ with exudation-liquids, and envelope it and its appendages with thick

exuded masses. The causes next in frequency are tubercular diseases; but of 16 cases, Mikschik found the volume of the uterus increased in 13. In the 3 cases of atrophy, the connexion was with tuberculous disease. The time that had elapsed between delivery and death was from ten weeks to two years.

Can a fatty uterus conceive, and become the seat of menstruation? This question cannot be answered positively. But the possibility is doubtful; for autopsies have shown that, in intense puerperal affections, the exudations are deposited, not in the uterus alone, but in the ovaries also, and that the Graaffian follicles are thus often destroyed. But there exists sometimes a sanguineous discharge that may be mistaken for menstruation: this is because the vessels of a fatty uterus remain open, and the blood is able to escape the more easily from the friable condition of the capillaries. Later, when the fatty metamorphosis has made progress, the researches of Wedl have shown that the vascular walls and the small capillaries which have no *vasa vasorum*, disappear for want of nutrition. Then results a local anæmia which dries up the source of hæmorrhage. The atrophied womb causes no disorder; but since amenorrhœa is the inevitable consequence, and since the primitive lesion is always accompanied by other alterations, the patients seek for the cause of their illness in the absence of menstruation, and torment the physician to prescribe in this direction. It is therefore necessary to be on our guard not to do mischief, in trying to restore impossible menstruation.

Amongst the other consecutive diseases, the most frequent and the most severe are the consequence of peritonitis. This easily and frequently returns. Each one of these forms of partial peritonitis has a course and gravity which depend upon the nature, quantity, and situation of the exudations caused by the primitive disease. Such are the deposits in the pelvis, around the womb; the transformation of the exudation into tubercles; its organization into false membranes, bands, which determine the most various modifications in the situation and relations of the abdominal organs; the shortening of the mesentery; incurable displacements with fixing of the womb, dragging the bladder and rectum in the empty state of the womb, and giving rise to more severe accidents in the event of pregnancy; lastly, all the disorders which result from disturbance of the functions of the intestines, and the sterility following upon a faulty position of the Fallopian tubes.

MEDICAL INTELLIGENCE.

The Imperial Society of Medicine of Constantinople.

Whatever may be the ultimate results of the late war, on the civilization of the Turkish Empire, it appears that medical science and the state of the profession are likely to be advanced and benefited.

Nowhere is such a motley crowd of medical practitioners (qualified and non-qualified) to be found as in Constantinople: English, French, Italians, Greeks, Armenians, Jews, Turks, &c., may fairly be said to represent every system that has ever been propounded in medicine. Though it had been long felt that it would be of the greatest interest to bring all these different physicians together, and of great advantage in promoting a good understanding among them, the scheme of forming a Medical Society, though sometimes contemplated, could never be carried out, owing to the jealousy of so many conflicting nationalities, and the local difficulties of habitation and practice.

It having struck Dr. Pincoffs, one of the physicians of the Scutari hospitals, that the circumstances which during the late war brought an additional influx of medical officers, offered peculiar opportunities for carrying out such an object, he succeeded, on the 13th of February, 1856, in effecting a meeting of the chief medical officers of the English, French, and Sardinian military hospitals, and of the leading physicians of Constantinople; they then resolved that they would assemble regularly so long as the war should last, with a view of ultimately establishing a permanent Medical Society at Constantinople.

M. Baudens, Inspector-General of the French hospitals in the East, was elected President; Dr. Linton, Inspector-General of the Scutari hospitals, and Dr. Fauvel, Physician to the French Embassy and Médecin Sanitaire de France, Vice-Presidents of the Society.

The Society have ever since met regularly every fortnight; the all-engrossing feature of the French hospitals—typhus fever—has formed the topic of the principal papers and discussions; and, if we say that physicians of all nations, and belonging to all schools, have spoken on the subject, we are sure that our readers will look forward with interest to a detailed account of the proceedings, shortly to be published by the Society itself, and which will contain a vast amount of individual observation. We take from the 'Union Médicale (Procès Verbaux de la Société Impériale de Méd. de Const., Juin—Septre. 1856), the names of the different medical men who have read papers and have spoken at the meetings:—Of the French Military Hospitals, Doctors Cazalas, Thomas, Nettes, Jacquot, Valette, Pastureau, Barudel, Baudens, Quesnoi, Garreau, and Grellois; of the Naval Hospital: D. Arnaud; of the English Hospitals: Drs. Dryce, Freund, Pincoffs, Temple; of the Turkish Army: Dr. Bonelli; of the Constantinople Physicians: Drs. Fauvel, Caratheodory, Sotto, Pardo. The names of two Russian physicians, sent by their Government to observe the typhus fever in various localities, are now also found: Drs. Alferieff and Moering.

Since the departure of the allied armies, Dr. Fauvel, a man of acknowledged scientific and professional standing, has been the President of the Society; the Council is formed of some of the principal physicians of Constantinople, and the Society numbers above forty resident, and several honorary and corresponding, members. One of the chief statutes of the Society, that none but properly qualified practitioners are to be members, will contribute not a little to raising the standard of the profession. The Society has been fortunate enough to obtain the patronage of Fuad Pasha, the Minister for Foreign Affairs. This distinguished statesman, who has the most enlightened views on all matters, is fully able to appreciate the benefits likely to result from such an association for Turkey, the more so as he has himself studied medicine; he has accepted the title of honorary member, and procured from the Sultan a *Bérat*, granting to the Society the title and privileges of an Imperial Society of Medicine (*Djemiéti Thebiei Chabané*). It is also probable that the Society will obtain a considerable pecuniary grant from Government; and it intends publishing a periodical account of its transactions.

The interest of such a publication will be all the greater, from the circumstance that the members of the *conseil de santé* (board of health) are members of the Society. This board has paid medical correspondents throughout the Levant, and as far as the borders of Persia and India; it will thus be easily understood that much valuable information on those countries (involving questions of epidemics, quarantine, and other matters of scientific and commercial importance) may be imparted and diffused by this channel.

Edible Seaweeds.

Some very interesting experiments were published in the July number of the 'Edinburgh New Philosophical Journal,' by Dr. John Davy, on the constitution of certain edible seaweeds,—the Chondrus crispus, the Rhodomenia palmata, Porphyra lacunata, Laminaria digitata, and Fucus vesiculosus. The main result was the determination of the presence of a larger quantity of nitrogen than is contained in the best flour. We merely allude to Dr. Davy's observations, that we may draw attention to the fact that two prizes of 50*l.* and 20*l.* respectively have been offered by Sir C. Trevelyan, Bart., for the best essays on the applications of the marine algæ and their products as food or medicine for man and domestic animals. We regret that we have not space to do more than advert thus briefly to the matter.

BOOKS RECEIVED FOR REVIEW.

On the Composition of Food, and how it is Adulterated, with practical directions for its analysis. By W. Marcet, M.D., F.C.S., &c. London, 1856. pp. 178.

The Science of Mind; or, Pneumatology. Vol. I. 1856. pp. 312.

Recherches sur les Monorchides et les Cryptorchides chez l'Homme. Par M. Ernst Godard. 1856. pp. 38.

The American Journal of Insanity. July, 1856.

The Dublin Practice of Midwifery. By Henry Maunsell, M.D. New Edition, revised. London, 1856. pp. 272.

Lectures on the Principles and Methods of Medical Observation and Research. By Thos. Laycock, M.D. Edinburgh, 1856. pp. 218.

Norsk magazin for Lægevidenskaben. Band x. Hefte 7, 8.

Remarks on Vesico-Vaginal Fistula. By N. Bozeman, M.D. of Montgomery, Ala. 1856. pp. 29.

An Address to the Graduates of the University of Edinburgh, 1st Aug. 1856. By J. Miller, F.R.S.E., Professor of Surgery. (Reprint.)

Advice to Officers in India. By John M'Cosh, M.D. London, 1856. pp. 310. 2nd ed.

Handbuch der Systematischen Anatomie des Menschen. Von Dr. J. Henle. In drei Bänden. Erster Band, Erste und Zweite Abtheilung. Braunschweig, 1855–1856.

The Census of Ireland for the Year 1851. Part V., Vols. I. and II. Presented to both Houses of Parliament by command of her Majesty. Dublin, 1856.

The Principles of Surgery. By James Syme, F.R.S.E. Fourth Edition. London, 1856. pp. 486.

On Some Points of the Anatomy of the Liver of Man and Vertebrate Animals. By Lionel J. Beale, M.B., &c. London, 1856. pp. 80.

Guy's Hospital Reports. Edited by Samuel Wilks, M.D., and Alph. Poland. Third Series. Vol. II. pp. 428.

A Letter addressed to the Gentlemen of the Medical Profession, on the Nature and Properties of the Aluminous Chalybeate Water at Sandrocks, in the Parish of Chale, Isle of Wight. By H. Waterworth, Surgeon. Newport, 1853. pp. 31.

Introductory Address delivered at the Middlesex Hospital at the commencement of the Medical Session, Oct. 1, 1856. By Campbell De Morgan, F.R.C.S. London, 1856. pp. 31.

The Australian Medical Journal. Edited under the superintendence of the Medical Society of Victoria. January and April, 1856.

On Eczema Infantile. By Erasmus Wilson, F.R.S. London, 1856. (Reprint.)

The Assurance Magazine and Journal of the Institute of Actuaries. No. XXV. Oct. 1856.

Tubular System of Supporting Artificial Teeth. By C. Stokes, M.R.C.S., Surgeon-Dentist. London, 1856. pp. 24.

Etudes Médicales et Statistiques sur les Principales Sources d'Eaux Minérales de France, d'Angleterre, et d'Allemagne. Par le Dr. J. C. Herpin (de Metz). Paris, 1856. pp. 371.

Military Sanatoria. Letter addressed to H.R.H. the Duke of Cambridge, on the Introduction of Mineral Water Establishments for the use of the Army. By P. Pincoffs, M.D. London, 1856. pp. 37.

The True Art of Healing. A Discourse on the Rational System of Medicine. Delivered Oct. 1, 1856. By Thomas K. Chambers, M.D., &c. London, 1856. pp. 32.

On Self-Training by the Medical Student. The Introductory Lecture delivered in University College, 1856. By E. A. Parkes, M.D. pp. 36.

Researches in Pathological Anatomy and Clinical Surgery. By Joseph Sampson Gamgee, Staff-Surgeon, &c. London, 1856. pp. 216.

The Life of Cornelius Agrippa von Nettesheim, Doctor and Knight, commonly known as a Magician. By Henry Morley, Author of 'Palissy the Potter,' &c. 2 vols. London, 1856.

Handbook of Physiology. By W. S. Kirkes, M.D. Third Edition. London, 1856.

Statistics of Two Hundred and Fifty-eight Cases of Intestinal Obstruction, with Remarks. By S. Foster Haven, Junior, M.D. Philadelphia, 1855. (Reprint.)

Revue Etrangère Médico-Chirurgicale, No. I. Oct. 16, 1856.

The Vocation of the Medical Scholar. Being the Oration delivered at the Eighty-third Anniversary of the Medical Society of London. By B. W Richardson, M.D. London, 1856. pp. 26.

Lectures on the Varieties of Continued Fever, and their Discrimination. By Thomas B. Peacock, M.D., F.R.C.S.

Surgical Anatomy. By Joseph Maclise, F.R.C.S. Fasciculus XIII. Second Edition. (Conclusion.)

A Manual of Physiology, including Physiological Anatomy. By W. B. Carpenter, M.D. Third Edition. London, 1856. pp. 652.

New Remedies, with Formulæ for their Preparation and Administration. By Robley Dunglison, M.D., &c. Seventh Edition. Philadelphia, 1856. pp. 769.

Clinical Lectures on Paralysis, certain Diseases of the Brain, and other Affections of the Nervous System. By R. B. Todd, M.D., F.R.S. London, 1856. pp. 474.

Human Physiology. By Robley Dunglison, M.D. 2 vols. pp. 729, pp. 755. Philadelphia, 1856. Eighth Edition.

A Manual of Materia Medica and Therapeutics. By B. I. Forbes Royle, M.D., F.R.S. Third Edition, revised by F. W. Headland, M.D. London, 1856. pp. 809.

A Manual of Elementary Chemistry; Theoretical and Practical. By Geo Fownes, F.R.S. Sixth Edition, revised and corrected. pp. 714.

Transactions of the Pathological Society of London. Including the Report of the Proceedings for the Session 1855–6. London, 1856. pp. 420.

Illustrations of the Pathology of Cancer. By J. Z. Laurence, F.R.C.S. London, 1856. pp. 59. (Reprint.)

Seventeenth Annual Report of the Registrar-General of Births, Deaths, and Marriages in England. London, 1856. pp. 112.

Principles of Medicine. An Elementary View of the Causes, Nature, Treatment, Diagnosis, and Prognosis of Disease. By C. J. B. Williams, M.D., F.R.S. Third Edition. London, 1856. pp. 603.

Egypt, its Climate, Character, and Resources as a Winter Resort; with an Appendix, &c. By A. H. Rhind, F.S.A. Edinburgh, 1856. pp. 151.

Calisthenics; or, the Elements of Bodily Culture on Pestalozzian Principles. A contribution to Practical Education. By Henry de Laspée. London, 1856.

Medico-Chirurgical Transactions. Published by the Royal Medico-Chirurgical Society of London. Vol. XXXIX. London, 1856. pp. 360.

De la Rue's Medical Memorandum Book and Indelible Diary. 1857. Edited by a Physician.

Hygienic, Medical, and Surgical Hints for Young Officers of the Royal and Merchant Navy. By W. M. Saunders, M.D., R.N. London, 1856. pp. 96.

On Artificial Digestion as a Remedy in Dyspepsia, Apepsia, and their results. By E. Ballard, M.D. London, 1856. pp. 46.

The Introductory Address delivered at St. Bartholomew's Hospital, Oct. 1st, 1856. By A. M. M'Whinnie, F.R.C.S. London, 1856. pp. 43.

Gesammelte Abhandlungen zur Wissenschaftlichen Medicin. Von Rudolf Virchow. Erste und Zweite Hälfte. Frankfurt, 1856. pp. 1024.

Die Cholera in der Schweiz. Von Hermann Lebert. Frankfurt, 1856. pp. 93.

Pathologische Physiologie. Grundzüge der gesammten Krankheitslehre im Zusammenhange dargestellt. Von Dr. G. A. Spiess. Frankfurt, 1857. pp. 709.

Die Einflüsse der Vaguslähmung auf die Lungen und Hautausdünstung. Von G. Valentin, Professor in Bern. Frankfurt, 1857. pp. 157.

A Treatise on Therapeutics and Pharmacology, or Materia Medica. By George B. Wood, M.D. 2 vols. Philadelphia, 1856. pp. 840, 901.

Clinical Lectures on Diseases of Women and Children. By Gunning S. Bedford, A.M., M.D. Fourth Edition, carefully revised. New York, 1856. pp. 602.

A Few Remarks on the Perforating Ulcer of the Stomach and Bowels. By J. B. Harrison, M.D., F.R.C.S. London, 1856. pp. 68.

Charleston Medical Journal and Review. September.

Obstetric Aphorisms for the use of Students commencing University Practice. By J. G. Swayne, M.D. London, 1856. pp. 124.

On the Origins of the Visual Powers of the Optic Nerve. By Jos. Swan. London, 1856. pp. 40.

Investigations into the Primary Laws which determine and regulate Health and Disease. By Jacob Dixon. London, 1856. pp. 24.

Suggestions in reference to the Means of advancing Medical Science; being the Opening Address delivered before the Members of the Harveian Society on Nov. 6th, 1856. By F. H. Ramsbotham, M.D. London, 1856. pp. 32.

Address to the Liverpool Literary and Philosophical Society. By T. Inman, M.D. Liverpool, 1856. pp. 20.

On the Pathology and Treatment of Contagious Furunculoid. By Thomas Laycock, M.D. (Reprint.)

Handbuch der Krankenwartung. Zum Gebrauch für die Krankenwartschule der K. Berliner Charité. Von Dr. C. E. Gedike. 3te Auflage. Berlin, 1854.

On the Constitutional Treatment of Female Diseases. By Edward Rigby, M.D., &c. London, 1857. pp. 324.

On Poisoning by Strychnia, with Comments on the Medical Evidence given at the Trial of William Palmer. By A. S. Taylor, M.D. London, 1856. pp. 152. (Reprint.)

The Mechanism of the Gubernaculum Testis. By John Cleland, M.D. Prize Thesis. Edinburgh, 1856. pp. 40.

THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

APRIL, 1857.

PART FIRST.

Analytical and Critical Reviews.

Review I.

The Treatment of the Insane without Mechanical Restraints. By John Conolly, M.D. Edin., Hon. D.C.L. Oxon., Fellow of the Royal College of Physicians of London, Consulting Physician to the Middlesex Lunatic Asylum at Hanwell.—*London*, 1856. pp. 380.

The manifest object of Dr. Conolly's treatise, which we propose to examine in the following pages, is to make sure the continuance, and to afford the means for imitating, a beneficent work, in the anxious prosecution of which the writer's best energies of heart and soul have, for many years, been engaged and spent; and the book carries with it all the tokens, showing it to be a labour of love.

The author opens with a touching allusion to the time when "night cometh" and he shall work no more, in these words:

"When the close of active professional exertions is felt to be approaching, and the pressure of that period *aut jam urgentis aut certe adventantis senectutis* becomes perceptible, a natural wish arises in the mind of any man who has been especially engaged in what he regards as a good and useful work, to leave the work, if not finished, yet secure; or if not yet secure, at least advanced by his labours, and as little incomplete as the shortness of his life and the limitation of his opportunities permit. The accordance of such a privilege must have imposed obligations which his imperfect powers can never have fulfilled satisfactorily; and consolation under a consciousness of deficient performance can only arise from a trust in that Higher Power which allows men to be the instruments of any kind of good. Influenced by some feelings of this kind, I am anxious in these pages to explain, as distinctly as I am able, the nature, as well as the rise and progress, of that method of treating the insane which is commonly called the non-restraint system; so as to contribute to its preservation and further improvement, and perhaps to its wider adoption; or at least to prevent its being abandoned, or imperfectly acted upon, or misrepresented, when those by whom it has been steadily maintained in its early days of trial and difficulty, can no longer describe or defend it." (pp. 1, 2.)

We pass over the First Part, entitled The Last Days of the Old Method of Treatment; it concerns us not, except as a chapter of painful history, a huge black page in the life of mankind, notable chiefly for a proof how long what is useless and embarrassing, as well as inhuman and wrong, may hold its ground, supported by universal sanction.

It seems, indeed, a curious phase in the nature of human affairs, as indicative of the extent to which independence of thought and action in individuals, are repressed in society by the communication of rigid ideas—so-called education—that it remained for men of the last century to conceive, and of the present to thoroughly apply, principles in the treatment of insane persons, the reverse of those which had been so long esteemed.

The wonder increases when it is observed that, only to forbear the old management of insanity is to strip the disorder of all its terrors, and, by mere passive endurance, to make way for many a spontaneous cure. That the observation should never have been made, of the immediate aggravation due to the imposition of mechanical restraint on an individual in a state of raving, is beyond conception; and that no one mind in all those ages, reasoning from effect to cause, should have been tempted to essay an entire plan, consistent in all its parts with the avoidance of mechanical coercion, may be considered as one more to the many wonders of the world.

We beg to recommend this very interesting sketch to the general reader. Not only is it a necessary prelude to the main performance of the book, but from it he will be enabled to estimate what mankind owe, and for all time will owe, to a little band of philanthropists, amongst whom our author, the latest member, must unquestionably be held the chief. We have used our last assertion advisedly. The position of one who teaches how to utilize a discovery or an invention capable of benefiting mankind, is, we have no hesitation in saying, morally higher than that of the inventor. By attempting his great experiment at Hanwell, a metropolitan asylum on a large scale, Dr. Conolly obtained those effects of a conspicuous example, which would probably have ensued in no other situation. No matter how great the intrinsic value of a discovery may be, the conviction thereof must often be brought home to the very doors, made to take root against the will, or rather the prejudices, of the people, before society can reap the fruits of its acknowledgment.

We picture to ourselves, with a feeling of deep respect, the courage, faith, perseverance, and anxiety, under various and great discouragements, through which Dr. Conolly's great experiment was made to succeed and prove the truth of his foregone convictions.

As we would avoid the temptation to excessive quotation, we string together some of the most important sentences from Dr. Conolly's Second Part, The First Days of the New, or Non-restraint System. Our extracts will be sufficient to display the nature of the new method of treatment, and to them we shall have occasion to append some critical observations.

(*a*) "It is indeed, above all, important to remember, and it is the principal object of this work to explain, that the mere abolition of fetters and restraints constitutes only a part of what is properly called the non-restraint system. Accepted in its full and true sense, it is a complete system of management of insane patients, of which the operation begins the moment a patient is admitted over the threshold of an asylum." (p. 34.)

"It is a part of the non-restraint system to remember, whatever the state and circumstances of a newly-admitted patient may be, that he comes to the asylum to be cured, or, if incurable, to be protected and taken care of, and kept out of mischief and tranquillized; and that the strait-waistcoat effects none of these objects." (p. 38.)

(*b*) "I should not advert to this mistaken description of what is really resorted to, if short occasional seclusion was not a medicine, and if the padded rooms, the real substitutes for restraints in very violent cases, were not of the highest importance—offering, indeed, an auxiliary without which it is questionable whether or not restraints could be entirely dispensed with in any large asylum." (p. 42.)

"Violent attacks, serious accidents, and even homicides, have been the consequence of such delusions, in many asylums; and the best security against such accidents is quietness, or temporary isolation of excited patients; or in other words, seclusion in a padded room, which includes both advantages.

"The great advantage of a padded room in all these cases is, that it renders both mechanical restraints and muscular force unnecessary for the control of even the most violent patients." (p. 44.)

"Every possible evil of seclusion was then" (in the time of mechanical restraints) "combined with every suffering incidental to the confinement of the arms and the legs, and the whole body; and the patient, excited and feverish from his malady, and heated and exasperated by the previous struggle, was left to lie in a constrained and comfortless position, and to suffer thirst, and to become subjected to all the miseries of unavoidable uncleanliness. With such treatment the patient commonly became furious. All kind attentions being incompatible with such disregard and neglect of him, there was no avenue to a good understanding between him and the attendants, whom he then, and long afterward, only looked upon as enemies and tormentors. The superintendents who speak of padded rooms as useless, do not explain their present mode of treating very violent patients in the recent stages of mania." (pp. 47–8.)

(*c*) "In the old asylums, every arrangement was principally made for security and control; in the new, every arrangement is made for the cure of the malady, or the comfort of the insane. In many of the old asylums there were even no infirmaries." (p. 53.)

(*d*) "We seek a mild air for the consumptive, and place the asthmatic in an atmosphere which does not irritate him, and keep a patient with heart disease on level ground; and on the same prophylactic and curative principles, we must study to remove from an insane person every influence that can further excite his brain, and to surround him with such as, acting soothingly on both body and mind, may favour the brain's rest, and promote the recovery of its normal action." (p. 55.)

"The officers should dismiss all their own cares from the mind on commencing the morning inspection, and be ready to hear with patience, to investigate with justice, and to remedy with kindness, all the little or great causes of dissatisfaction laid before them; so that the patients may be tranquillized, and at the same time the attendants not ruffled and discomposed, and left in an unfit state themselves to show kindness or exercise patience towards others through the rest of the hours of the day." (pp. 56–7.)

"Among the improvements yet to be made in the practical department of public asylums, arrangements for what may be called an *individualized* treatment are particularly required. None but those daily familiar with the events of asylums, can duly appreciate the great effects of such treatment in special cases." (p. 64.)

"The physician must be able to command the services of a staff of kind and conscientious attendants trained by himself. If the attendants are accustomed to the sight of their patients in the humiliating condition of restraints, and allowed to impose restraints whenever a patient is wayward or irritable, for every irregular action, and for every violent word, they cannot be trained to treat the same patients with any show of respect, much less with any constant manifestation of humane regard." (pp. 94–5.)

"Non-restraint system; the great principle of which is to exclude all hurtful excitement from a brain already disposed to excitement." (p. 101.)

"By these various appliances—some of them singly of small significance, and perhaps almost wearisome in detail, but conjoined forming a complete system directed to one object—the whole constitution of an asylum and the transactions and incidents of every day, are made remedial. Everything done by every officer, and every word spoken by the sane to the insane, is in conformity to one plan, directed by a chief physician, carried out in all its details by efficient and faithful officers, and having for its sole object the happiness of the patients, the relief or cure of all the griefs and troubles of the heart, and the restoration of composure and power to the mind. These, in their union, constitute the system of managing the insane without mechanical restraints." (p. 106.)

Upon this brief summary of non-restraint, which by no means conveys an adequate idea of Dr. Conolly's masterly allusion to every point, we would remark (*a*), that if "the mere abolition of fetters and restraints constitutes only a part of what is properly called the non-restraint system," as is indeed true, then the practice of a system embracing all the resources of non-restraint, cannot have its good neutralized because expedients allied to some extent in form, but so dissimilar in nature, object and effects, both immediate and remote, as to bear no real resemblance to restraint, are occasionally superadded. (*b*) Dr. Conolly calls padded rooms "the real substitutes for restraints in very violent cases;" in fact, he speaks of such chambers as the *sine quâ non* to non-restraint. He further describes the great advantage of them, in "rendering both mechanical restraints and muscular force unnecessary for the control of even the most violent cases."

We are unable fully to agree with him in this estimate. We acknowledge the great, the indispensable utility of seclusion in a room so furnished; but we shall proceed to detail the limited variety of circumstances in which, in our opinion and experience, seclusion in a padded, as distinguished from a common, chamber, is requisite.

It is in some few cases of excitement only (not being delirium), that the instinct of self-preservation is blunted or lost. In the great majority of paroxysms of mania, there is no disposition to batter the head against the floor or wall; none to fall foul of furniture in a room, such as the bedstead. But there may, frequently, be the inclination to break the latter, or anything else which can be destroyed.

We can hardly help associating the peculiar addition of padding to a room, with the sole purpose in harmony with it—namely, the protection of the patient from reckless injury, with or without design, inflicted on himself. Further, in some suicidal cases, the padding and absence of furniture will certainly deprive the patient of several means of accomplishing his destruction. We have witnessed extensive cutting of the scalp by a patient suicidally beating her head, almost pulpifying the integuments against the bedstead, and have found the ease of dealing with such an incident, by removing her to a padded room. When a padded room is named as a great substitute for mechanical coercion, we begin naturally to inquire what mechanical coercion was expected to do, or to prevent, and what we should seek to have embraced by its substitutes? Prevention of injury to self, to others, and to property. The padded room greatly diminishes the practicability of the first; it does, not as a room padded, but as a room of seclusion, prevent the second; and can by no means prevent the last. We should prefer to rank systematic kindness and forbearance as the first substitute, then, direct treatment, medical and moral; and to reckon next, seclusion simple, then padded seclusion, in the order of frequency and importance in which they are respectively necessary or applicable.

Far from intending to speak of padded rooms as useless, we acknowledge their invaluable aid, sometimes in the delirium of epileptics; in states of extreme exhaustion, with jactitation of the limbs, occasionally occurring before death from acute mania; in some suicidal cases, and, sometimes, in the conduct of patients suffering a paroxysm of acute mania.

They possess, however, one more property which is of general advantage—that of deadening sound, and diminishing the disturbance of many patients by one.

(*c*) Dr. Conolly's remark, "that in many of the old asylums there were even no infirmaries," invites inquiry as to the degree in which such separate departments are serviceable for insane patients,—by whom we mean, the mixture of persons in any asylum which may not have become a sort of almshouse, in respect of the age and infirmity, and the reduced movement, by discharge and death, of its inmates.

It would appear that every part of an asylum is, or ought to be, an hospital; and granting the full value of an infirmary, in the event of epidemic or infectious disorder, when every general consideration may properly be made to give place to the emergent call for a separation of the sick from the sound; we incline to the opinion that, for the great majority of the ordinary sick persons of an asylum, an infirmary is not a better place than the wards. In a general hospital we have many beds in a large room, and, consequently, every occupant always in the presence of the nurses. This, we apprehend, is the peculiar arrangement on which the special character of a common hospital or infirmary depends. Change this, substituting a distinct room for each patient, without the means of devoting a nurse to each, and it will appear that that peculiar and beneficial characteristic is gone. We obtain, in fact, a ward of a lunatic asylum. Who are the ordinary sick, keeping their beds, in a lunatic asylum, and what are their general states? Premising that their number is extremely small, we answer: persons bedridden with the paralysis of the insane, or with other forms of paralysis, or dying of exhaustion in mania, or enfeebled by pulmonary consumption and a few other diseases; all of which maladies, be it remembered, are of the chronic form. We think one and a half per cent. of invalids keeping their beds would be a large proportion; nor would the sick, able to be up, make an appreciable addition to the whole number of the invalids.

The bed-ridden paralytics, and those sinking under acute mania, ought not, in our opinion, to be associated, for their own sakes. The paralytics often cry out—particularly at night—and grind their teeth painfully; and in the last stage of mania, the dying disease may be lighted up, and the patient agitated by rage, and swearing, with a force which his physical condition might have seemed to render impossible. Insane persons dying of consumption, are not always blessed with composure of mind and the non-appearance of acute symptoms. We remember the late case of a female patient, occupying her last moments in this disease, with loud imprecations and attempts to strike and kick, which were pitiable indeed to behold from their impotency.

Our own observation, then, teaches us not to associate insane persons who may be sick or dying, as a rule, and therefore not to make general use of an infirmary for them; since, if the arrangements therein cannot, with comfort, be approximated to those in a general hospital, nothing is to be gained by the removal of the patients from the ward and class to which, in view of their mental malady, they may belong.

In looking to obtain the uses of an hospital in the wards of an asylum, we stipulate, of course, for a desirable separation or classification of the patients, since, wherever the sick may remain, those who make no noise themselves and who would be disturbed thereby, certainly ought not to be submitted to that infliction. Further, we assume the existence of the proper means of nursing the sick, as well in their own wards as in any other situation.

But for a very small number of the sick, we recognise the superior advantage of distinct infirmaries. It has happened that a patient mentally convalescent, and soon to be discharged, has fallen ill and died of acute bodily disease; and it does occur that a portion of the ordinary sick present no obstacle to their association in an infirmary.

Whether in an asylum, full or other, a space shall be reserved empty, and a staff of nurses comparatively idle, for the patients who would so seldom come, is, we think, more an economical than a medical question, always saving the desirableness of possessing an infirmary, or a part of the asylum which may suitably be vacated, at any moment, in order to receive patients who have been attacked with epidemic or infectious disorder.

Of the non-restraint principle as a medical truth, the most easy and natural comparative proof is afforded in the preceding quotation, marked *d*. The soundness of the doctrine of forbearance and soothing, as the principal sedative to the irritability and excitement in insanity, is indisputable, and receives invaluable support from the established laws of general medicine; and the analogy affords a most satisfactory reason why the non-restraint system once recognised on this basis, shall flourish and become a rooted necessity in every mind.

The whole series of the substitutes for restraint, used in the practice of the new system, may seem few in number. In fact, forbearance and a soothing method of personal intercourse with the insane are, in a word, the great alternative. That phrase, however, embraces an infinite variety of minute constituents—not less, indeed, than all the elements which compose practical humanity. Any particular expedient, like the padded room, is not frequently necessary or serviceable, notwithstanding the indispensableness of it when really required. Compared with the universal application of the old restraints, the occasions demanding a resort to the modern alternatives of a material kind are, in practice, few indeed. The explanation of this is very simple: restraint was indiscriminately applied (one of the chief abuses of it), but the substitutes are limited to the cases really calling for them.

Having extracted from our author such a summary description, of the nature of the system called non-restraint, as we think justifiable, we beg to refer the reader to the whole of the beautiful exposition contained in Part II., a chapter replete with the fruits of an experience directed by earnest solicitude; and we now proceed to notice remarks illustrative of treatment in general and of remedies in particular.

We deem this not the least in importance of the firstfruits of the non-restraint method:

"The general effect of the first judicious management of patients is, that it becomes practicable to examine their whole condition carefully, and decide on the best plan of medical treatment." (p. 66.)

Medical examination and treatment must have been impossible before restraints were laid aside; and therefore one main effect of the new system is, the lifting of asylums from the low grade of cruelty-prisons, to the noble one of hospitals for the cure of mental disease. The progress of medical and pathological science is, however, slow, and the new field for its culture in insanity, is but newly cleared and fitted to receive the germ of observation:

"If," says Dr. Conolly, "the limitation of the direct therapeutical means applicable to mental disorders is so unsatisfactory, it is to be ascribed to the extreme obscurity in which the origin of cerebral disturbance is involved, and to the narrowness of our knowledge of the mental functions of the brain. In a great majority of cases of mania and melancholia, the condition of the brain in the commencement of the malady is entirely unknown; all conjecture about it is vague, and dissection reveals nothing. In older cases, the appearances found after death are the consequences of an anterior disturbance, of the nature of which we cannot always form a reasonable conjecture. In cases in which we are justified in concluding that a vitiated condition of the blood is the immediate cause of the disturbance of the brain, and in others in which plethora, or inanition and debility, are the evident causes, our indications of treatment are clearer. But even in these cases, as in all others, we speak of increased or diminished nervous energy as manifested in certain results, the nature of which is dimly comprehended by the most diligent mental physiologist." (pp. 76–77.)

Of the shower-bath, among other particular remedies, we read—

"The full use of the shower-bath can only be cautiously obtained by repeating the shower at short intervals (in a bath supplied by a cistern), and until decided prostration ensues. Employed in the ordinary manner, its effects are rather exciting than depressing. Blisters are occasionally useful; setons and moxas seldom or never. Generally speaking, whatever reduces the strength of the patient acts unfavourably on the malady; and the superintendents of asylums are unanimous in maintaining that the usual treatment must be tonic and generous, and the diet of the insane liberal and nutritious." (pp. 67–8.)

We are very glad to obtain Dr. Conolly's opinion of the shower-bath, and to find coupled with the recommendation, caution in the use of it. We ourselves purposely abstain from the remedy altogether—not from entire want of faith in its powers—not wholly from fear of its accidental abuse—but because we think any virtues it may possess for the insane, are as nothing to the terror which would be occasioned in persons submitted to it against their will. Sane persons dread the shower-bath, with the check-string in hand; but insane persons would have no choice; and for them we cannot approve of a remedy involving the depressing, and therefore unsuitable, action of fear. Again, the shower-bath has been made an instrument to punish with, and might, at any time, become a terrible engine of vengeance in the hands of a vindictive attendant. In the administration of the shower-bath, superintendents ought always to be present, which would be difficult with female patients. In a medicinal point of view, benefit would not be expected from the shower-bath, unless its use should be quickly followed by reaction—a skin made warmer by a quickened circulation. In our experience amongst insane persons, an effect of this kind is neither so readily nor safely obtainable by agents exercising an indirect action on the heart, like the shower-bath, as by internally administered stimuli, exciting at once the centre of the circulation. And, as a general rule, we find occasion to esteem a stimulant, rather than a tonic plan of treatment for the insane.

"It is scarcely possible to predicate which of the several sedatives will have the best effect in any particular case; and the chief benefit of any of them has always seemed to me to be most conspicuous in chronic or recurrent cases, and far more remarkable in melancholia than in mania." (p. 68.)

Agreeing in the general truth of this statement, we are yet rather surprised to find no mention of the practice recommended by Dr. Seymour, as derived by him from Messrs. Beverley and Phillips, of Bethnal-green, and announced in his 'Thoughts upon Several Diseases,' a criticism of which work was contained in the 'Medico-Chirurgical Review,' for July, 1847. This absence, however, may be attributable to Dr. Conolly's design, for, he says, "Of the medical or direct treatment of the patient, it is not the particular object of this work to speak." (p. 66.)

For ourselves, we immediately commenced the practice Dr. Seymour recommends; and although we have made no reckoning and analysis of the many cases so treated in the subsequent years, we are confident of having met with great success in effecting many cures. The plan consists in administering small doses ($\frac{1}{4}$ or $\frac{1}{2}$ gr.) of the acetate of morphia, nightly only, during a long, steady continuance, in certain cases of melancholia, marked by continued sleeplessness, with fear and apprehension, or with self-abasement and reproach. Once or twice we have been called upon to treat functional jaundice, after the mental recovery, apparently the consequence of the long use of the opiate; otherwise, we have observed no ill effect from the practice.

The following cannot be too widely made known:

"It is always desirable to keep in view that the preternatural excitement of a patient affected with acute mania, his violent action, and his loud voice, are not indications of strength; and that the more violent the symptoms, the greater is the danger of sudden prostration and death. In young persons, maniacal symptoms are not unfrequently the first in the train of those belonging to pulmonary disease, of which the ordinary symptoms are long masked. In old persons, an outbreak of mania is often the mere precursor of general decline and death. (pp. 69, 70.)

"There will soon be accumulated, I believe, in many asylums, very singular proofs of the general benefit of a tonic and nutritive plan of treatment in most of the forms of chronic insanity, and in all cases attended with obvious debility, in consequence of the recent extensive introduction of the use of the cod-liver oil into practice. The effects are scarcely more gratifying than the principle is important." (p. 76.)

"All practitioners in medicine whose experience extends, as my own does, to more than thirty years, must have observed, even within that short period, a striking change in the extent to which ordinary means formerly considered remedial, and even indispensable, are employed. Large and frequent bleedings, once so common as almost to be universal, are now wholly unknown. Violent purgative medicines, and the excessive employment of mercury, by which rude attempts were made to force the performance of languid functions, have been desisted from in all climates where scientific practice prevails; and it is admitted that the restoration of the general powers of the system is a more successful way to repair partial irregularities of action. The physician whose practice is especially directed to diseases of the brain and nerves, has no reason to be dissatisfied if such views already influence the minds of those whom careful observation has led to conclusions which daily experience confirms." (pp. 77–8.)

"By far the greater number of agents which are found to be eventually remedial in insanity, are indirect in their operation, gradually influencing the mind itself. To all these the physician who wishes to maintain the non-restraint system must constantly and earnestly direct his attention. Under the ancient plan of treatment, medical means were often inapplicable, or not applied, and were sometimes used more for punishment and subjugation than as remedies for physical causes of malady. The resort to instruments of mechanical coercion was inconsistent alike with any medical consideration of the various forms of mental disease and their causes, as it was with attention to the numerous auxiliary or moral means of cure which are so greatly relied upon where the ancient methods of control have ceased to be employed." (p. 81.)

We are anxious to endorse the great general truths expressed in the foregoing extracts. A knowledge of them is fundamental to the only appropriate medical treatment of insanity in the mass; and we purposely introduce these quotations here, with the view of bringing them the further under the notice of general practitioners, in whose hands the prospects of persons becoming insane may so often be influenced, in the interval which elapses before placing them in an asylum. Many instances have come to our knowledge, in which, to the best of our judgment, acute and once hopeful cases, had been rendered the reverse by general bleeding, into which the surgeon had been misled, probably by delusive appearances of plethora and turgescence of blood-vessels, with accelerated radial pulse and over-heated skin. In such cases we beg him to be guided by the pulse at the wrist, but by its force, not by its frequency, and to treat the flushing and suffusion of the countenance, which he may observe, together with capillary congestion of the conjunctival membranes, with a diffusible stimulant, combined with a sedative medicine. By thus assisting the heart's labour he may cause this passive congestion of the conjunctivæ to disappear, and, at the same time, equalize the circulation in the superficial and deeper vessels of the brain and head; and it is probable he will, sooner or later, have induced tranquillity in the patient. Dr. Conolly's observation on the changes in general medical practice of late years, may perhaps excuse our mentioning, for the sake of corroboration, the fact of our having, in no instance, employed general bleeding in a practice extended over, probably, two thousand insane persons.

In the following terse lines we find, in the form of an aphorism, the law for all persons occupied about the insane :—"No one is qualified to be an officer, a keeper, a nurse, or a servant in a lunatic asylum (in which the non-restraint plan is pursued), who is not able and disposed to make every part of personal conduct more or less conducive to one great end—the comfort and cure of the lunatic inmates." (p. 216.) So much do we admire the precept, so deeply recognise its essential truth, as the sole basis of all right action, whilst at the same time, experience has so fully taught us the difficulty of obtaining uniformity of conduct at the hands of many individuals, with their too frequent change, that we have caused this brief quotation to be printed in a large type on cards, and displayed in many situations, so that all may be perpetually reminded of the great end to which their exertions must be directed. We do not consider this a substitute, in any degree, for the personal guidance and instruction of attendants by superior officers; but deem it a useful monitor in case of the unintentional omissions of the latter. The many cares of a superintendent hardly permit of his marking, and making an illustration of every instance in which he may observe a deviation from the principle of non-restraint.

We would say, the effects of non-restraint treatment applied to the insane, resemble those produced by civilization on the healthy but untrained man. To soften manners and evolve or restore the power of self-control, are the proper functions of both systems. As amongst human beings to be trained and educated there exists the widest diversity both of capacity and character; so amongst the inmates of a lunatic asylum (all passing under the common denomination of insane persons), mental disease is met with, from the most partial to the complete, in form and degree. We find selfishness and vice in general, under the name, and claiming the consideration due to disease, and we have a varying contribution of persons of criminal and degraded character, either truly insane, or feigning insanity, from gaols.

We are by no means intending to raise, here, the important question of distinguishing between disease and error; but we think the non-restraint doctrine ought to be examined as to its fitness to be made, in practice, the portion of those who may possess a power of self-control, and who may therefore be properly deemed responsible for their licence of conduct. The system of non-restraint appears to us to embrace no resource for any vice or misconduct, not the involuntary offspring of disease. It would seem to be solely applicable to those patients who are, by affliction, rendered irresponsible; whilst every superintendent knows that the inmates of an asylum, in the mass, cannot be so esteemed.

We cannot suppose that all the patients in an asylum, have lost the power of controlling, more or less, their language and actions. As a rule, we see that they exercise a correct judgment of what is right or wrong in the behaviour of their fellow patients, we hear their shrewd remarks and experience the power of their observation, as regards any difference in treatment between themselves and others, and we notice, in them, a strongly marked appreciation of their own rights. Does the acuteness of these faculties argue nothing in favour of a general answerableness for improper conduct, even, if more direct evidence of possession of the power of self-control were always wanting? What resource has non-restraint for evolving and strengthening the power of self-control? Indulgence and forbearance can hardly be expected to have that effect, unless we attribute to insane persons a refined sensibility, highly at variance with their usually selfish disposition. A soothing manner displayed towards angry excitement, gives the best and speediest chance of a restoration of tranquillity; just as, on the contrary, a harsh one would add to and prolong irritation. But the question here is, how is the power of self-control over explosions of temper and acts of aggressive personal offence, without provocation, to be regained or strengthened? Surely by the exercise of it. So much would it appear, sometimes, that the will alone is in fault, in many of the conditions under insanity, that it is impossible not to be forcibly struck by the resemblance of some insane patients to unruly children. Dr. Conolly insists much, and justly, on the necessity of gaining the confidence of the patients, as the key to successful management; and he argues that the influence thus obtained over a patient in the intervals, will not be without its good effects in the paroxysms of insanity. This argument, then, supposes the propriety of looking for the power of self-control even in the paroxysms of true insanity. Our preceding remarks, however, apply in strictness, not to such, but to the coarse insensibility to the feelings of others, the vicious disposition so often accounted to be insanity, displayed by some inmates, and to the general conduct of some patients transmitted from prison. If non-restraint treatment be the only proper law for asylums, it must contain within itself, the lines of treatment fitted to every description of case in the miscellany of inmates.

In questioning the whole adequacy of non-restraint, as hitherto developed, to the government of asylums *as they are,* we wish it not to be supposed that we contemplate, or would tolerate, the infusing thereinto, of any element irreconcilable with that pure system of kindness, sympathy, and of incitement to self-control, by example and reward. We merely raise a point which, in the actual management of an asylum on purely non-restraint principles, occasionally asserts itself with some inconvenience, and from time to time renews the conviction, that there is something for non-restraint to yield in principle, and something more for it to embrace in practice.

Dr. Conolly's description of non-restraint is, we apprehend, to be taken as the desirable standard we are to endeavour to reach. This can only be attained through considerable difficulties. We have alluded to the first difficulty which is personal to the patients themselves, and in which is involved the universal applicability of non-restraint to the inmates of an asylum. The next difficulty belongs to the providing, educating, and retaining of suitable persons as attendants. The social position and education of persons filling this office generally fit them for little more than menial duties, to their service in discharging which their remuneration is adapted. It is not to be expected that persons are to be had, at once not too refined for menial service, yet refined enough to be at all times exercising the judicious influence of sound and well-regulated, over unhealthy minds. Until attendants are doubled in number, so as to permit of two classes, a working and a thinking, to every division of the patients, this difficulty cannot be overcome. Attendants, as they are, require long training, yet are frequently lost when becoming really serviceable.

A third difficulty is remarked upon by Dr. Conolly as arising out of "the knowledge possessed by attendants that they are independent of the physician," since, by law, they can only be appointed and discharged by the committee of visitors of an asylum. In practice we have not found serious inconvenience from the operation of this needless device of legal jealousy; and we are satisfied that the issue of it, whether it shall lead to obstruction, or be a thing of no practical consequence, is wholly in the hands, and dependent on the general principle of management adopted by the committee themselves. This evil merges itself in the last-mentioned, and the real remedy would be found in the employment of an upper class of attendants; with whom, better fitted by their mental superiority to ordinary nurses, when once animated by the proper spirit, we think there would be no cause to complain of the exhibition of a wrong independence towards the physician.

Dr. Conolly with justice complains of a pertinacious misrepresentation of the non-restraint system, by which is attributed to it the practice of holding fast refractory patients as a substitute for other restraints. The real fact is, at Hanwell—

> "That repression by holding the patient's hands and arms is never resorted to *except* when some sudden impulse requires such immediate interposition for a few minutes, after which the impulse usually passes away; or the patient is removed, and his attention occupied with something that makes him forget it. Against such sudden impulses it would never be right to resort to mechanical restraint; and any continued holding, or struggling, or violent over-mastering of an irritable patient, belongs to the older system of treatment, and is quite inconsistent with the new." (p. 41.)

We are a little surprised to find even the admitted exception; so obviously objectionable is any holding whatever, so intolerant is irritability of personal handling, or even touching. But in the early days of non-restraint, before the series of alternatives to mechanical coercion had been developed, we must look upon such an expedient as not merely excusable, but as perhaps the best thing to be done under a novel exigency calling for instant attention.

At page 89, amongst many others, indeed as the ever-recurring idea of the book, we find declared the incompatibility of the new system with any mechanical restraint of the person.

It appears to us to be in the interests of the insane, and within the province of a rational moderation, to inquire in what degree this is to be taken for true. Is every sort of coercion, beyond deprivation of liberty within walls and the fastening on of clothing, quite irreconcilable with non-restraint? May no other agent of compulsion be used in conjunction with non-restraint, provided the all-governing spirit be the spirit of the modern system? Is non-restraint, as expounded by Dr. Conolly, always sufficient for every emergency, both therapeutical and of danger? And, if any superintendent, free from attachment to the former system of management by restraints, but, on the other hand, impressed with the most ample practical conviction in favour of the modern method, should feel called upon to use any sort of mechanical appliance to a patient, is he thereby necessarily and justly to be deemed to be upholding and perpetuating the abuses of former days? Spots on the sun do not change, nor perceptibly diminish, his light; and we can confidently leave the answer to the last question in the hands of our readers.

But we cannot think we are called upon to express approval of those parts of Dr. Conolly's book through which there runs, as through his previous writings, an implied assumption that no restraint whatever can be used without re-introducing an old and abhorred system, to the exclusion of the system of non-restraint.

We cannot but regard such assumption as unfair as well as unreasonable, and calculated, rather, to injure than to promote the true cause of non-restraint. And we would prefer Dr. Conolly's openly taking, to his merely insinuating, this very questionable position.

Beyond this exclusiveness—a prominent feature in the system of non-restraint—we see occasion to apprehend a tendency to inaction, imposed by the rigidity of its terms on all devoted followers. We allude to the danger of *laissez aller*, when things are to be done contrary to the perverted will and general resistance of the patient. There are, occasionally, patients to be fed who would be starved by our forbearance; and we have reason to think that the dislike entertained by thorough-going non-restrainers, to any and every kind of coercion, extends even to the use of the stomach-pump for the introduction of food. We do not mean that any superintendent would withhold this instrument when he believed it to be necessary; but that the pre-occupation of his mind with anti-coercive doctrines will influence his opinion of its occasional value, and lead him to let slip, or overpass the moment when instrumental feeding may prevent or postpone the fatal issue. Dr. Conolly places reliance on varied and tempting food, which we must deem a very poor resource in a description of case which he must, as well as ourselves, have repeatedly, though not very often, seen.

What we have said of feeding, applies also to the administration of medicines; and this, if less immediately vital, may be second only in importance. In the cases we are recalling, all human persuasion *must* fail: the eyes of the patient see not; the ears hear not; the sensorium is not susceptible to objective impressions, because the whole mind is absorbed in the contemplation of one morbid conception, under which the patient is wasting quickly, and may be destroyed unless the rapid loss of vital power and bodily substance is timely made up. In other words, the disease must be fed in order to save the patient. In these cases, all the avenues to the faculties from without are closed; the brain is like a sealed book, either filled with one distressing theme, or blank, in a state of vacancy, whilst the patient effectually resists every proffered assistance to those acts, necessary, not for comfort only but for existence.

Must we hesitate to treat such a patient as one devoid of individuality? Must we not rather, with judgment, tenderly supply the office of the instincts temporarily lost? Shall we be doing our duty if, with such a patient before us, we suffer our hands to hang idle for the sake of consistency? How many things amongst a number of the insane are required to be performed against the will of the patient! The least of these is unjustifiable, if the greatest be inconsistent with the true principle of non-restraint. We have to consider our duty as physicians, equally with that as general managers of the insane; and it does not appear to us that any amount of excellence in the latter capacity, can excuse a too passive discharge of the former. It cannot be that any resource duly applied, in the treatment and cure of an insane patient, in a spirit of true kindness, can be other than proper and humane, let it be what it will; nor can it be right to attribute to those who cherish their independence for duty's sake, a general course of management deficient in common humanity. Such an imputation can serve no purpose higher than that which is the aim of the mere partisan. No resource whatsoever, applied by the physician as succour in states of danger and difficulty, the existence of which are patent to the eyes of all, can be misapprehended as to the purpose; can have the effect of ill example for attendants; or can unfavourably influence the character of an asylum. We regard Dr. Conolly's argument of this sort, against the abuse of coercion, as the strongest that can be raised against the old system; but we discover no force in it to deter the physician from any expedient on which, to the best of his judgment, he may determine; and we hold it of no real consequence what any such particular expedient may or may not appear to resemble, so it exceed not the purpose, so the purpose be a good one, so the means itself be the best attainable under the circumstances. We have no sympathy for any expedients off the tram-road of non-restraint, that may not stand the test of these three requirements.

There are many other parts in this excellent book on which we would dwell, but that we have already occupied much space. Not the least interesting is the Fourth Part, consisting of extracts from a series of Dr. Conolly's Annual Reports during his great experiment at Hanwell, and after it had been proved and become established.

We had intended quoting a most excellent remark at page 159, on ordinary education, in order the more to attract attention to the way in which the invasion of mental disorder is unfortunately almost bid for, by defective training in early life, in defiance of the laws of body and mind. But our object has been chiefly, the examination of the nature and capabilities of non-restraint, and we must forbear; however intimately questions, seemingly distinct, are, in their mutual relations and reaction, allied to the general subject of insanity and to each other.

Although many years a superintendent, we do not hesitate to confess that we have derived no less of advantage than satisfaction from the perusal of this noble work. The principles on which we have so long endeavoured to act, are fortified by this able display of the whole system to which they belong; and we have only to regret, for the general good, that this competent treatise should have been so long withheld. It is true there have been the Annual Reports of Hanwell itself, from the same pen, and there have been circulated, also, the yearly testimonies of numerous public asylums worthily pursuing a similar course. Still, the completeness of a work like this, must give the force and weight of a whole argument, and leave a satisfaction in the mind of the reader, hardly otherwise attainable. And we cannot but think that the long opposition, and the many counter-allegations with which the system of non-restraint has been compelled to struggle, would have been earlier routed in presence of a superior force like this. We cannot over-estimate the general obligation to Dr. Conolly, for the care which has provided this elegant memorial, and we desire to tender him our most sincere thanks. We cannot join in the apprehension he somewhere displays, of retrogression from the great principle which will be ever most associated with his name. Non-restraint as a principle, as a thing developed, cannot be associated with any other name than his; although, as an experiment, it may belong to others, and men like Pinel, Tuke, and Hill, his predecessors, deserve mention as having essayed to open out this new path. Non-restraint may possibly be modified under extended experience, but the modification will not lop one branch of charity, gentleness, humanity from the noble tree; such modification must consist in expansion and emancipation, where a finite boundary may have been proved too narrow.

We heartily trust the *senectus*, to which Dr. Conolly alludes in his first page, may be, if *certe adventans*, still very far off; and that he may yet meet, on earth, the only reward to which, we feel, he must be looking—the knowledge that these fruits of his propagating have become the inheritance, not of Englishmen only, but of civilized man throughout the earth.

Review II.

Ueber die Todtenstarre und die ihr nahe verwandten Zustände von Muskelstarre, mit besonderer Rücksicht auf die Staatsarzneikunde. Von Dr. Adolph Kussmaul, Docenten der Medizin in Heidelberg. (Vierteljahrschrift für die Practische Heilkunde. xiii. Jahrgang, 1856. Zweiter Band, s s. 67–115.)

On Cadaveric and the nearly Allied States of Muscular Rigidity, with special reference to Forensic Medicine. By Dr. A. Kussmaul, Lecturer on Medicine in Heidelberg.

The series of alterations which the constituent parts of the human body undergo on their final abandonment at death to the uncontrolled operation of the physical and chemical laws, opens up a study of considerable interest to the medical practitioner, and one of indispensable importance to the forensic physician. Without an intimate acquaintance with the successive changes in the solids and fluids of the dead body, no accurate discrimination can in many cases be made betwixt the traces of ordinary disease, toxical agents, or violent injury, on the one hand; and those, on the other, which may have originated in merely natural causes; both sets of phenomena being admittedly liable at times to mimic, mask, or obliterate each other in a variety of ways.

Of the class of phenomena referred to we select one for consideration which, though not entirely overlooked or wholly undervalued amongst ourselves, has been studied with more care and greater consequent success amongst our Continental brethren, especially within the last ten years. We allude to the so-called *rigor mortis*, the *Todtenstarre* of the Germans, the *raideur* or *rigidité cadavérique* of the French, the *cadaveric rigidity* of British writers.

It is well known that, in the great majority of instances of death, from whatever cause it has arisen, the previously firm and elastic state of the limbs and joints gives place to a relaxed condition of these parts; that this relaxation of the limbs and joints, after a longer or shorter interval, is in its turn succeeded by a rigid condition of both; and that this rigidity, at the end of a period, usually of more lengthened duration, is again replaced by flaccidity of the same parts, more or less pronounced as it is met with earlier or later after the advent of the putrefactive process. That this rigidity, preceded and followed by flaccidity, has its seat in the muscles, has, since Nysten's time, been the settled opinion of physiologists. We need only remind our readers that this experimentalist showed that the fixity of the dead limb continued after the division of the cutaneous tissues, the articular ligaments, and the synovial capsule, but disappeared at once on the division of the muscles.

Medical men are well aware how eagerly the ordinary attendants on the dead will watch the proper moment for the decent disposal of the corpse, before the limbs have had time to fix themselves in what they consider to be unnatural or unbecoming postures. On an occasion of this sort, it would not perhaps be beneath the dignity of the practitioner to step in to the aid of the nurse and the undertaker. Both these parties will invariably be found to be ignorant of the fact that a little simple manipulation will suffice to secure, at any period after death, the object which both have so much at heart, and thus secure to the one a well laid-out body, and to the other a symmetrical coffin.

From this careful tutoring of the corpse by the nurse before it has come under his notice at the post-mortem inspection, the medical attendant being but rarely present at the death of his patient, opportunity is seldom afforded him for forming any practical acquaintance with the ordinary physiognomy of the dead body as it would appear if left to take its natural position. Hence, without due consideration, he may be led to give in to the popular notion that the body of a person whose death has been sudden and secret, if in position or attitude it presents any decided departure from the artificial or typical corpse, has previously been subjected to violence or unfair play, and *vice versâ*.

Now, the truth we believe to be that, in the far greater number of instances—whether after natural disease, poison, or violence (apart from the immediate indications of these)—the aspect of the corpse will in all be found to present a considerable degree of uniformity: the position assumed by it, if allowed to follow its natural direction, differing less than might be expected from that of a decently disposed corpse.* In a smaller, but still not very limited number of cases, it will, on the other hand, be found to hold equally true that the dead body will in the same circumstances be met with in what must be considered as constrained and unnatural postures, evidently from the operation of normal causes, exclusive altogether of the idea of its having suffered either injury or violence at the time of its death.

1. The following are the broad features which a somewhat extended study of cases of sudden death has shown us to be the natural aspect, which the corpse, if left to itself, may be expected to present in the great majority of instances.

(1) *Prior to the approach of the usual rigidity*—the body on its back; absence of expression from the countenance; the eyelids closed, or one or both partially open; the mouth not quite shut;† the lips slightly apart; the pupils more or less dilated; the limbs extended; the hands prone; the thumbs straight; the fingers straight, or very slightly flexed; the position of the arms in relation to the trunk somewhat uncertain, but usually pretty close to its sides. At this precise period, it matters little whether the death has been a natural or a violent one. The aspect of the corpse in these respects was seen by us to be the same in two executed criminals, when cut down from the gallows; in several instances of suicidal hanging, where the bodies had not had time to cool; after cut-throat or other fatal wounds; and after drowning, strangulation, suffocation, &c.

(2) *After rigidity had come on*, though the physiognomy of the corpse was not quite so uniform, the mass of cases were very much alike. The body was on its back. The eyelids and lips were, with rare exceptions, closed, or but very little apart. Whatever had been their previous state, the pupils were now about midway between dilatation and contraction. The elbows and knees very slightly bent; the fingers more or less flexed; the thumbs oftenest straight, occasionally slightly flexed, rarely drawn in to the palms; the toes, except sometimes in the case of infants, straight. The expression of the countenance varied rather more than the other features of the cases. In the majority,

* We were much struck with this in one of those cases which are but rarely met with:—suicidal strangulation.

In July, 1825, the body of W. McD., a man about sixty years of age, was found in a wood, a napkin round his neck, tightened by a walking-stick twisted through a loop in it. The circumstances attending his disappearance and death were such as satisfied the law authorities that the man had voluntarily strangled himself. When found, the corpse was lying on its back, the lower limbs extended, and the arms straight and close by the sides; the whole exactly as if the body had been laid out artificially after death.

† We have on two occasions seen the mouth wide open at short periods after sudden deaths.

before tumefaction had come on, whatever the immediate cause of death, the countenance expressed calmness and placidity. In a large number, it presented a perfect blank. A very few faces had some peculiar expression. Thus, in three persons accidentally drowned, the expression was that of a near-sighted individual in one, that of a blind woman in a second, and that of terror in a third. Rarely was anything like anxiety marked in the face. Equally rare was it for the relatives who came to identify a body in the dead-house, to trace the features distinctive of the character of the living individual. Whatever assistance the fixing of the features at this period may give to the identification of the dead, such identification is oftener effected from the examination of the clothes or other accessories than from the features of the naked corpse.* The occasional presence, however, of expression in the features of the dead face may neutralize the charge of inaccuracy which has been brought against Dévergie, in stating, as he has done without qualification, that the latest thoughts of the dying man will be imprinted on his physiognomy: that, for example, in passing through an hospital dead-house, the body of a person who has had an easy, may be thus distinguished from that of one who has had a painful death; that the ferocity of the living face of the hardened criminal will reappear on his features after death; and that the dull hebetude of the drunkard, in winter especially, will be depicted even on his dead countenance.†

(3) *After the rigidity had disappeared*, the eyelids were occasionally seen open, the mouth frequently shut, though the joints were relaxed. The limbs frequently retained the appearance of stiffness, either from infiltration of bloody serum into their tissues, the development in these of putrid gases, or from the commencement of saponification. The shrivelled soft parts on the one hand, or, on the other, their subsequent tumefaction, rendered the character of the faces a perfect blank. We have seen the bent toes of the child, or the closed hand of the adult, remaining after the disappearance of muscular rigidity.

2. In a limited number of cases of sudden death, the corpse has been seen lying on its face, the arms crossed under some part of the face,

* A curious instance of the mistakes occasionally committed in the identification of the dead, is given in the number of the Edinburgh Monthly Journal for February, 1854. A somewhat serio-comic illustration of the same point, which occurred in Aberdeen about thirty-five years ago, we owe to one of the public prosecutors, now deceased, who conducted the trial:—W. D., a medical student, along with some others, had disinterred a body from the churchyard of Newhills, five miles off, and were taking a circuitous route by the seaside to bring it to town, when they were met by the coast-guard, who captured D., and, with the corpse, conveyed him as a prisoner to town, where he was lodged in jail. A day or two after, the wife and sons of a weaver at the Spital, a suburb of Aberdeen, came forward and claimed the body as that of a relative of theirs, who had been missing for some days, and whom they accused D. of having enticed away and murdered. D. now seeing the serious charge thus preferred against him, found it necessary to state how he had come into possession of the body. On this the grave at Newhills was opened, and found empty, when the relatives of the Newhills person offered to swear to its identity, while the Spital people as positively refused to admit that they had made any mistake. Matters were in this state when the Spital weaver reappeared in life; but so incredulous were some of the lower orders amongst his neighbours, that the magistrates had to parade him through the streets; but even this failed to satisfy a few, who insisted that they had shown a mere effigy dressed up in the weaver's clothes. D. was tried for body-snatching, and sentenced to a fine and short imprisonment.

† Médecine Legale, tome i. p. 77.

neck, or chest, or more rarely under the belly or pelvis. This we have encountered after syncope from the bursting of an aneurism, or rupture or other disease of the heart; in apoplexy, with or without attendant cardiac affection;* in acute alcoholic poisonings; after blows on the epigastrium, &c. In these instances the parties had been in the standing posture immediately before death.

3. In an equally limited number of cases of sudden death, we have met with the body on its back or sides, with the limbs mechanically retained in an unnatural posture, the corpse having slipped off a chair† or the steps of a stair in its fall, the inequality of the ground at the spot, or the narrowness of the locality, preventing the limbs from being extended.

4. In some instances of sudden death, it has been long known that the limbs at the moment fail to relax, no interval whatever being observable betwixt the continuance or departure of the vital tension of the muscles, and their fixture by rigidity. Cases of this sort should not be confounded with those we have last mentioned, nor with the rigidity at the moment of death from the freezing of the fluids in some cases of death by cold. In themselves they are deserving of the closest consideration of the medical jurist. Commencing at the latest instant of life, the rigidity continues uninterruptedly till the muscular tissues have begun to alter under the influence of the putrefactive process. Though no line of demarcation has yet been drawn betwixt this form of muscular stiffening and ordinary post-mortem rigidity, we may take it for granted in the meantime that the two phenomena are different, and consider the one in question under the term which has been applied to it of *cadaveric spasm*, employing the designation, however, in its *generic* sense.

The instances of cadaveric spasm which we have oftenest encountered in cases of sudden death, were those which had this feature in common with well-developed cadaveric rigidity; that when in either the fixity of the limbs was once overcome by their forcible extension, they did not again return to their previous positions. A few examples will show the varying features which instances of cadaveric spasm of this sort are liable to present in actual practice.

J. S., a gentleman of middle age, and in the last stage of consumption, went on board a packet-boat ready to sail. Going into the cabin, he was observed to sit down on a camp-stool, with his elbows on a table before him, and his face buried in his hands. An hour or more had elapsed, and the vessel had sailed, when on going up to him, one of the passengers found that it was a corpse that sat before him.

By referring to the case of death by chloroform, reported in the

* If we remember rightly, the late Dr. Abercrombie, of Edinburgh, was found dead in his room in this position.

† We once witnessed a death in this way. A man of about sixty was seized in the street with sudden sickness and palpitation, but seemed, after being assisted into a house, quite recovered. While sitting on a chair, a few minutes after, conversing cheerfully, his face in an instant was seen to become turgid and dusky-red, and before he could be interfered with he had slid down on his back on the floor, with his limbs extended. The face as suddenly as it reddened had assumed the collapsed and pallid hue of death. No inspection was obtained, but the man's previous history pointed to hypertrophy of the heart as the cause of death.

forty-first volume of the 'London Medical Gazette' (p. 318), by my colleague at the inspection, Dr. R. Jamieson, it will be seen that the young man had died in a standing posture, with his trunk resting on a shop counter.

J. B., a man about sixty years of age, in September, 1853, went into an eating-house and ordered dinner. On entering the room about twenty minutes after, the attendant found his body on a chair in the sitting posture, the hands on the table before him. On inspection, a large morsel of beef was found in the upper part of the œsophagus, which had suffocated him.

The body of J. S., a seaman, aged thirty-five, was found (November, 1843) in a sitting posture on the floor of his room, the head and arms leaning on a chair. Deceased was of intemperate habits. Putrefaction had made some progress in the body at the time.

A. L., a man about sixty, entering a tavern (June, 1842), was left in a room by himself for from twenty to thirty minutes. His corpse was then found seated on a couch, one arm over the arm of the couch in an easy attitude.

A robust and muscular prisoner, under forty years, hung himself in jail. The free end of the ligature had been attached to a bar in the window of his cell, which was so low that his knees, when suspended from it, almost touched the floor, while his toes rested on it. Near him lay a stool overturned, on which it was conjectured he had been kneeling previously. When discovered hanging, there was a Bible betwixt his knees, retained there by the closely adducted thighs.

L. J., aged thirty-five, the mate of a vessel in harbour, one morning in April, 1838, shortly after having left the deck, was found in the cabin of the ship, dead from a large wound which he had inflicted with a razor on the front of his neck. The body was in the sitting posture on the floor, the back resting against the locker, the trunk inclining to the right side, and the head drooping a little in the direction of that shoulder. There was very little blood on his clothes, or on his hands, or immediately under the wound on the floor; but on the left-hand side of the body was a large pool of it.

Mrs. P., aged forty-five, the keeper of a brothel, was found accidentally smothered in bed one morning in April, 1837. The body rested on its elbows and knees, both of which, as well as the hip-joints, were half bent. The wrists were extended to the utmost, and the hands clenched. The face was buried in a pillow, the nose and lips flattened, the tongue protruded, and the neck bent far forwards. On lifting the body, the pillow where the face had lain was seen to be covered with a quantity of blood. She had been carried to bed on the previous evening, much intoxicated, and thrown recklessly down on it without being undressed.

M. B., a female of about thirty-four years of age, of intemperate habits, was found, at four one morning (February, 1832), lying in a prone position in bed, her chin touching the top of the chest, and the joints of the extremities much flexed. Though the heat had not left the trunk, all the joints were rigid. The tongue was protruded, and fixed betwixt the clenched jaws. She had gone to bed intoxicated at a late hour, and had, about half-an-hour before four, been heard going to a cupboard for more whisky. The woman's shift, and the bed-clothes near the mouth, were wetted with fluid of a spirituous odour.

M. McP., about thirty-six years of age, of intemperate habits, was found dead in a house of bad fame betwixt eleven and twelve at night (15th April, 1833). The corpse lay on its back above the clothes in bed, her own clothes turned up above the knees; the lower limbs extended and widely separated, the arms by the sides, the hands closed, the temperature of the body but little reduced, and the joints rigid. A few minutes before some young men had gone into her apartment, one of whom subsequently stated that, while he was attempting to have connexion with her against her will, he found her insensible—as he said, either dead or drunk—when he and his companions ran out of the house in a fright. The woman had not been drinking that night, but was known to have swallowed two drachms of laudanum, to which she was accustomed. The principal morbid appearances at the subsequent post-mortem inspection were, some extent of pneumonia in both lungs, dilatation with attenuation of the right heart, and lymphy fringes on the margins of the mitral valve.

We have detailed elsewhere* the particulars of the finding of the body of Mary Smith, aged sixty-three (April, 1849), for the rape and murder of whom a man was subsequently tried and executed. In this instance, the woman's body had been fixed at death in much the same position as in the last case. One of the knees and one elbow were half bent, one of the upper extremities entirely extended at a right angle with the trunk.

The case of Hugh Gauld, referred to in the same paper (p. 78), was that of a powerful man of the same age as Smith, who died by congestive apoplexy, and whose body was found fixed by cadaveric spasm in much the same posture.

An additional instance of the same occurrence was witnessed in May, 1854, in the body of Ann Harvey, aged twenty-three, for the suspected murder of whom a man was subsequently tried at Edinburgh. The corpse was found at Cults, five miles from Aberdeen, with the throat deeply cut, lying on its back; the left hand extended, and a little under the trunk; the right arm above the head, with the elbow half bent; the right knee half flexed; the feet in contact with a basket or reticule, which the slightest additional pressure would have overturned.†

* London Medical Gazette, vol. xlv. pp. 38, 73.

† This case presented a variety of particulars both of legal and medico-legal interest. The person apprehended and tried had made an appointment to accompany the woman to town on the previous evening. Several witnesses testified to having seen him in her company both in Aberdeen and on the road to Cults; others as positively swore that the man had passed the evening in their company at a house near the place. Besides several incised wounds on the face, neck, and across the insides of the fingers of the right hand, the principal wound of the neck had involved the soft parts down to the bodies of the vertebræ, dividing also the invertebral cartilage betwixt the second and third cervical vertebræ and the spinal cord at the same part. As if to give a colour to the idea of a rape as well as murder, the woman's underclothes were turned up as far as the knees, and stained with blood, there was blood on the insides of both thighs and on one groin, there was an incised superficial wound on the right labium pudendi, and a second wound, penetrating an inch and a half inwards, and dividing the clitoris and the left nympha. From

On more than one occasion, bodies removed from the water after death by drowning have been found by us in a state strongly suggestive of the idea that they had stiffened in the falling posture—the elbows remaining half bent; the arms projected directly forwards, with the fingers spread out; and the knee and ankle-joints more or less flexed.

The body of J. W. or S., an old woman, was found (Sept. 1845) within a few minutes of the outbreak of a fire, in her room, with the clothes on the upper part of her person consumed; deep burns on the side of the face, on various parts of the trunk, thigh, and one arm; and the tongue protruded and fixed between the clenched jaws. The joints of both the upper and lower extremities were all rigidly and closely contracted.

The same unnatural and rigid contraction of all the joints with extensive burns, was noticed, in 1835, in the bodies of five individuals (three adults and two children) discovered on the extinction of the flames of a burning house, who were found, at the post-mortem inspection, to have died of suffocation. The same thing occurred in 1836, in the case of a woman who, while in drink, had set her room on fire.*

The form of post-mortem rigidity termed *raideur tétanique*, or tetanic spasm, has been found, like the ordinary cadaveric spasm, to pass into the usual cadaveric rigidity without any noticeable interval of previous relaxation of the muscles and joints. It has been observed however, in some instances, to differ both from cadaveric rigidity and from the usual form of cadaveric spasm, in the circumstance that, when the rigidity in this case is forcibly overcome, it again speedily returns. This at least appears to hold true generally in the rigor following fatal doses of strychnia—a fact which we have verified in the frog, and in an adult male, on the second day after death.†

the genital wounds only a reddish serum had trickled directly downwards between the nates. There was but a small quantity of blood under the woman's head, but large pools of it, and indications of a severe struggle, on the opposite side of a wall, seven feet high, over which the body must have been lifted after it had been drained of most of its vital fluid, and previous to the infliction of the genital wounds. There were several incisions in the woman's clothes, and much blood on them, and on the top of the wall and on the face of it on the side farthest from the body. The suspected murderer, when apprehended, had spots of blood in front of his dress, but was proved to have been very subject to bleedings from his nose, and, at the time of his capture, had his nostrils full of moist blood. There was no wound or scratch on his person. A shirt found in his lodgings had had one of its sleeves newly washed, notwithstanding which there were found traces of blood on the still moist sleeve. The shirt was claimed as the property of a second party. The prisoner was known to have for some time cohabited with the deceased.

* As was to have been expected, we have met with instances both of fatal burns and of death from suffocation in a burning building, without the cadaveric spasm of the limbs.

In April, 1849, the body of M. D., a woman of intemperate habits, was found in her room, the back deeply charred, and the clothes on her back and head consumed. The corpse was lying in an easy attitude on the hearth, one shoulder in contact with the fire-grate, the fire of which was extinguished. The joints at this time (seven hours after she had been last seen in life) were all flexible.

In May, 1851, on the extinction of a fire in a room in which a boy of five years had been left shut up, his body was discovered crouched up in a cupboard, where he had been suffocated. In this instance, too, the limbs were all supple.

† The constancy of this form of rigidity after death by strychnia was called in question by some of the witnesses in the late celebrated trial of William Palmer, but its occasional occurrence both in man and in the lower animals was sufficiently proved on this occasion. By the way, we were struck with the difficulties thrown in the way of the prosecution of

Kussmaul refers to Sommer and Clemens, as authorities in proof of the occasional passage of rheumatic and ordinary tetanus, without any appreciable interval of relaxation, into this form of cadaveric spasm; and states that this may also at times occur after death from carbonic acid gas. In this last instance, however, he may have confounded this special form of post-mortem rigidity with the ordinary cadaveric rigidity, as in the cases we have adduced above, in the six individuals removed from burning buildings.

The subject of post-mortem tetanic rigidity is deserving of a more complete elucidation than we have the means of bestowing upon it. During life, we know that convulsions not, *per se*, to be distinguished from those produced by ordinary tetanus, are liable to occur in other abnormal conditions of the cerebro-spinal system, whether arising from ordinary disease or the effects of poisons. We have ourselves encountered well-marked opisthotonos in the convulsions from teething in children, in a case of hydrophobia, in a case of hysteria in a young man, and in several severe epileptic seizures. Several of our known poisons besides strychnia are capable of giving rise to the same symptoms. What we chiefly desiderate is, information regarding the continuance or otherwise of the tetanic rigidity after death, in cases of this sort, and whether it may occur with or without an interval of previous relaxation of the muscles and joints. On this subject, Kussmaul has promised (p. 108) some valuable information obtained by him in experiments on animals.

It seems that the muscular contractions which have been occasionally observed after death from cholera, agree with the tetanic spasms caused by strychnia and traumatic tetanus, in being liable to return after the rigidity has been forcibly overcome, but differ from most of these cases in the circumstance that they are usually preceded by a short interval of muscular relaxation after death. They farther differ in their spontaneously ceasing and recurring in cholera, and in their attacking the muscles, singly or in groups, in succession.* In one

Palmer by the alleged fact that Cook had shown a degree of command over the voluntary muscles, during the action of the presumed poison, inconsistent with the usual character of strychnia poisoning. In connexion with this point we may be permitted to adduce verbatim from our notes taken at the time, the following case:—

"Nov. 26th, 1836.—Called by the police at two P.M. to see Ann Euston, aged twenty-four, who had just swallowed two drachms of nux vomica, in powder, diffused through water. Her face was flushed, her eyes fiery, her pulse small and quick, and she was in a very excited state. On preparing to use the stomach-pump, she could not be restrained by a powerful policeman, who was unable to hold her. She requested to be left at liberty for an instant, when she darted at a table-knife near her, and, before it was noticed, had it at her throat. The knife was with difficulty obtained, and her arms secured by the assistance of a third party, she was strapped to a chair, and the contents of the stomach removed. The face during all this time continued much flushed, and the pupils moderately dilated; the pulse 110, full, with considerable firmness, and she had constant clonic spasms of the extremities and opisthotonos; whites of the eyes at times turned up. After the operation she raved furiously for nearly an hour, during which time the pupils continued dilated and fixed, the eyes fiery, and she disliked the light. After this the pulse fell gradually to 84, and by half-past four, to 72, at which time the spasms had ceased, except at the wrists, where there were still startings of the tendons. When left at half-past four, the lips and cheeks were of a vermilion-red, and she complained of pain in the stomach and throat.

"27th.—No spasms after last visit. Complained a little of pain in the abdomen."

* See Barlow; London Medical Gazette, vol. xliv. p. 798.

instance of cholera, however, we witnessed the spasm of the muscles continue without intermission after death, as in ordinary cases of cadaveric spasm.

It is now ascertained that the ordinary cadaveric rigidity, besides being liable at times to be masked by or to become indistinguishable from the different forms of cadaveric spasm, may also occasionally escape the notice of observers altogether, either from its not having had time afforded for its development at the moment, from its having previously disappeared, or, if present, from having had its existence overlooked by them on account of its limited extent or partial sphere of operation. These admissions afford the means of reconciling the conclusions of Nysten* with the observations of later writers.

Kussmaul (p. 80) contends for the accuracy of Nysten's first rule in regard to the unvarying constancy of this phenomenon after death, in opposition to the instances recently advanced as subversive of it by others. Mashka's statement† that cadaveric rigidity is wanting after poisoning by mushrooms, is met by an experiment on the rabbit, in which, two minutes after the animal's death from eating two of these fungi, the usual stiffness was noticed in the neck, jaw, and limbs. In four dogs destroyed by large doses of acetate of morphia, the rigidity was also observed. This author likewise combats other prevalent opinions on this subject, to the effect that rigidity fails to manifest itself in the flesh of over-driven animals; in the bodies of those who have died by lightning; in those of premature infants, either still-born or who have breathed after birth; and in children removed from the uterus by the Cæsarian section.

Our own experience, so far as it goes, tends to corroborate the views of Kussmaul. We have had repeated occasion to remark the unusually early approach of putridity in those instances of narcotic poisoning in which the rigidity at the time of the inspection was either entirely absent or but faintly marked. In the case of J. F., a woman of nineteen—for the death of whom, from the reckless administration to her of between nine and ten grains of muriate of morphia, a medical student was tried in Aberdeen, in 1842—fourteen hours after death the joints had become flaccid, and the body emitted a putrid odour. We have noticed the same state of matters in several instances of poisoning by laudanum, at periods of from twenty-four to forty-eight hours after death. In a case of poisoning by snuff, some of the particulars of which were recorded by Dr. Christison in the last edition of his work on Toxicology,‡ at the post-mortem inspection, seventeen hours after death, the rigidity had disappeared from all the joints except the fingers and lower jaw. On the other hand, we have occasionally met with well-marked rigidity on the second and third day in instances of poisoning by laudanum, and in the case of a female child of five and a half months, for the death of which by an overdose of vinum opii, an irregular practitioner was tried in Aberdeen, in 1853. At the inspection, about forty hours after death, the lower jaw and

* Recherches de Physiologie et de Chimie Pathologique, p. 419.

† Prager Vierteljahrsschrift, Band ii. 1855; British and Foreign Medico-Chirurgical Review, vol. xvi. p. 506.

‡ Treatise on Poisons, p. 850.

the joints of the lower extremities were found rigid, the remaining articulations flaccid.

Boudin* states that marked rigidity of the limbs has been met with after death by lightning; a fact which we have witnessed in the case of a boy of fourteen, who died from this cause.

In the greater number of our inspections of premature and still-born infants, the period of rigidity had been passed prior to the examinations. In some others, however, this phenomenon was sufficiently well marked. In lately removing the body of an infant from the abdomen of its mother, who had died twenty-four hours before from rupture of the uterus during labour, the rigidity was sufficiently developed, especially about the neck and trunk of the child.†

The nearest approach to the entire absence of rigidity after sudden death, where opportunity was afforded for informing ourselves sufficiently on the subject, was witnessed by us in the case of a lad of seventeen, who, about seven o'clock one morning in the summer of 1840, while on his way to his workshop, dropped down in the street, and was picked up dead. Immediately before, he had left his mother's house, apparently in good health. On the afternoon of the same day, we were sent for by the mother, who stated that she had been deterred from proceeding to arrange the corpse by observing that it did not cool as she would have expected had he been really dead; that the limbs were still supple; and that, within the previous half-hour, the colour had returned to his cheeks;—in short, she believed that her son was going to come to life again. These statements were undoubtedly correct; and except that the lower jaw had not dropped, there were no further traces of rigidity about the body, the trunk and limbs of which felt warm to the touch. Of course, it was our painful duty to make the poor woman aware that this state of matters was owing to the unusually rapid approach of decomposition. Next morning, this was sufficiently evident, as the temperature of the body was even increased, and it emitted a putrid odour. The limbs were still flaccid. With the exception of the return of colour to the cheeks, a parallel instance happened to a scorbutic seaman in July, 1827, who had died suddenly from the effusion of a large quantity of serum into the chest.

It may not be deemed too trivial a circumstance to deserve being noticed, that, in some instances of sudden death, the forcible extension of the limbs by the attendants, in the removal of the clothes, causes the disappearance of the rigidity in the dead body. This we have repeatedly noticed at the dead-house.

That the rigidity may be occasionally not only imperfect in its kind, but also partial in its extent, seems to be sufficiently demonstrated in practice. In the cases which have been adduced in illustration of this, individual parts have shown no tendency (as observed in others) to become rigid after death, the usual cadaveric softening consequent on the commencement of putrefaction taking place in some of the limbs without their having previously passed through the intermediate stage of rigidity. This has been witnessed by Sommer and Kussmaul

* Annales de Hygiène, tome iii. pp. 287, 290.

† Since writing the above, we have examined the body of an infant which had perished from fracture of the skull before it had fully breathed, where the joints, with the exception of the lower jaw, were all rigid on the fifth day after death.

(p. 82), after comminuted fractures near the top of the limbs; and by the latter as well as by Bouchut (contrary to the opinion of Nysten), in extremities affected with hemiplegia. Kussmaul found that, after injecting a solution of common salt into the abdominal aorta of a dog killed by a blow on the nape of the neck, the hind legs did not stiffen, and the fore legs but partially. Ligature of the arteries of the limbs during life (Stannius), and the injection into them of chloroform, induced rigidity, which again disappeared. On killing the animals subsequently, the limbs operated on passed directly into the putrid state.

Kussmaul, while confirming its general accuracy, has slightly modified the assertion of Nysten* as to the exact and regular order in which the rigidity makes its appearance and takes its departure from different parts of the human body. The stiffness almost invariably commences in the neck and lower jaw. From the neck its direction is simultaneously both upwards and downwards: upwards to the muscles of the face, and downwards to the remaining muscles of the trunk; and those of the upper, subsequently affecting lastly the muscles of the lower extremities. Even in these last it is said to follow the direction from above downwards, passing on from the shoulder to the elbow, from the hip to the knee. Its departure, as a general rule, follows the reverse order; disappearing, however, lastly from the face. The commencement, acme, decline, and departure, are all alike gradual and imperceptible. (p. 85.)

We are scarcely yet in a position to fix with precision the proper limits to the duration of cadaveric rigidity in the general case. The data adduced by writers prior to Sommers' time would determine the earliest occurrence of this phenomenon at periods ranging between twelve and forty-eight hours; while this writer, as the result of a series of carefully conducted observations on the bodies of two hundred individuals, fixes its first approach at from ten minutes to seven hours, and its earliest departure at twelve hours after death. As the nearest approach which we could obtain to these two periods, we noted the state of the bodies of 90 drowned persons, as they came under our observation on their removal from the water:

"In 54 (or 46·9 per cent.) of the observations, the joints were in a rigid state. The average period after death at which the general rigidity of the joints was observed, in 40 of the cases in which the time of death was ascertained, was thirty-nine hours 26·5 seconds, with a range of betwixt one and a quarter hour and ten days. In such of the cases as belonged to the six winter months, the periods after death averaged sixty-five hours nineteen minutes, with a range of between three hours and ten days; while in those of them which belonged to the six summer months, the periods after death averaged fourteen hours thirty-four minutes, with a range of between one and a quarter and thirty-five hours."†

In 3 out of 10 cases of suicidal hanging, we had an opportunity of noticing partial rigidity as early as three, five and a half, and six hours, and general rigidity in the remainder at periods of from seven and a half to thirty-five hours after death—the latter averaging nine hours. In 9 other instances of death by suspension, the rigidity had not come on at periods after death ranging from thirty minutes to nine hours, or an average of two hours forty-nine minutes.

* Op. cit. p. 386. † London Medical Gazette, vol. xlvii. p. 854.

For the earliest systematic attempt at the determination of the varying circumstances which may be presumed to influence the periods of the appearance and disappearance of cadaveric rigidity, we are indebted to Nysten.

One of the generalizations of this writer is, that the more early the development of the rigidity, the less marked its degree, and the more prompt its disappearance (p. 419). While assenting to this as the general rule, Kussmaul has adduced some exceptions—such as certain cases by Sommer of pneumonia, enteritis, and typhus, where the rigidity was both early and persistent; Brücke's experiments with strychnia in animals, and bodies immersed in cold water, where the same thing has been observed; as also his own experiments to the same effect, from the injection into the limbs of animals after death of alcohol, ether, chloroform, oil of mustard, &c.

Nysten's conclusion, to the effect that the degree and duration of the rigidity after death is in the direct ratio of the previous vigour and integrity of the muscles (p. 419), Kussmaul resolves into the four following propositions:—1. That, *cæteris paribus*, in new-born infants and in children, the cadaveric rigidity is less strongly developed, and continues for a shorter time, than in adults; 2. That the same thing holds good in the case of the aged as in that of the young; 3. That the more sudden the death of the individual, the more marked and persistent (other circumstances being alike) the rigidity, and the later it is of making its appearance; 4. That the more the previous illness has interfered with the nutrition of the muscles, the earlier the appearance, the less marked the kind, and the briefer the stay of the rigidity. (pp. 94–8, *pass.*)

The two first of these propositions of Kussmaul may be accepted *provisionally*, as not inconsistent with known facts, and as supported by such observations as have hitherto been made.

The general accuracy of Kussmaul's third proposition is borne out by experience in the cases of individuals who have died by decapitation, hanging, hæmorrhage, precipitation, fractures, &c. It cannot, however, be made to embrace those cases of sudden death in which the integrity of the muscular or nervous tissues, or the constitution of the circulating fluid, is seriously interfered with.

The same remark applies in substance to Kussmaul's remaining proposition, as witnessed in cachectic, scorbutic, dropsical, and rachitic subjects; as also after putrid, exanthematous, and miasmatic diseases, though even here some apparent exceptions have been pointed out.

The space at our disposal will not permit us to enter on the subject of cadaveric rigidity as it is found to affect the muscles of organic life, the heart, stomach, intestines, &c. This we the less regret, as, beyond a few observations on the bodies of animals, we are not yet in possession of data for the elucidation of this most important department of pathology.

For the same reason, we must reserve for future notice the materials which have now been accumulated for the correct settlement of the true cause of cadaveric rigidity, merely stating here that Kussmaul adopts the chemical theory which originated with the school of Liebig.

Review III.

1. *Die Geographischen Verhältnisse der Krankheiten oder Grundzüge der Noso-geographie.* Vols. I. and II. Von Dr. A. Mühry.—*Leipzig* and *Heidelberg*, 1856. 8vo, pp. 508.

The Geographical Relations of Disease; or, Outlines of Noso-geography. By Dr. A. Mühry.

2. *On the Geographical Distribution of Health and Disease.* The Article introductory to Plate 35 of, and at p. 117 of, 'The Physical Atlas of Natural Phenomena.' By Alexander Keith Johnston, F.R.S.E., F.R.G.S., &c., Geographer at Edinburgh in Ordinary to Her Majesty. A New and Enlarged Edition.—*Edinburgh* and *London*, 1856.

A certain concurrence of injurious conditions is requisite for the production of disease in mankind, individually or collectively. In our endeavours to analyse these, we meet with difficulties in discriminating which of the morbific agents, under some given concurrence of circumstances, may be singled out as the one specially productive of the diseased result. Extended sickness and mortality among military masses may be caused by peculiar predisposition, by dietetic errors, bad water, want of cleanliness or suitable clothing, defective ventilation in the sleeping berths, mental depression, and over-fatigue in marching or in the duty of the trenches. But all such causes of disease are greatly aggravated by the conditions of the soil and drainage of particular countries, climatic degrees of temperature, atmospheric humidity or density of the air, associated with the course of the seasons in various localities, and the different degrees of latitude and longitude. Malarious, intermittent, and remittent fevers, yellow fever, typhus, plague, dysentery, diarrhœa, and cholera, have a certain natural geographical order and distribution, depending on temperature and moisture, with other concurrent agencies of diet and topographical position. The facts of physical geography and of vital statistics, then, applied to investigate the laws which regulate the distribution of health and disease among the human family, constitute a new and most interesting branch of medical etiology, under the head of Medical or Noso-geography. This promising field of research, which may be said to be yet in its infancy, must be ever of interest and utility to all those destined to sojourn in foreign climates, or in our remote colonies; or who may be entrusted with the command of soldiers or sailors in these countries, or with the preservation of their health. It was truly said of old by Hippocrates, that the constitutions of men change with the seasons; but to which he might have added, *more particularly with climate and geographical position.* Just in proportion as the physiological conditions of plants and animals vary according to different degrees of latitude, or, speaking more specifically, with the different lines of equal temperature and moisture north and south of the Equator, so must the pathological character of disease differ. On this subject, Humboldt first developed the theory of botanical geography, or the nature of the more material causes which geographically regulate not only the distribution but the acclimatation of plants. Blumenbach subsequently generalized the physiological facts connected with the various races of man and animals in different quarters of the globe; proving that certain types of form and varieties of the species owe their geographical distribution to certain concurrent physical agencies, which regulate the diversities of animal and vegetable structure.

The distinguished authors of the two works at the head of this article are those more particularly deserving of notice on the subject of medical geography. Following the examples of Humboldt and Blumenbach in other departments of natural science, they have brought the ascertained facts of the meteorology and climatology of our globe to explain the geographical limits of particular diseases, and their regulated distribution, according to atmospheric temperature and moisture, the density and electricity of the air. While these meteorological causes determine the laws by which certain diseases are geographically distributed, the concurrent causes of topographical situation, geological nature and elevation of the soil, state of the vegetation, and habits of the people, stamp a special character on the diseases of certain countries. Dr. Mühry, in classifying diseases, arranges them under the head of zymotic and of dyscrasial disease—a distinction of some importance, as indicating diseases mainly under the influence of meteorological causes, and those that prevail independent of such influence. Just in proportion as we become capable of knowing that particular diseases are limited to certain portions of the earth, and can trace the meteorological laws of their geographical distribution and diffusion, we necessarily obtain clearer conceptions of the causation of disease, as well as more practical knowledge of the means of prevention and cure, by certain climatic changes and topographical conditions. So extensive and useful is this subject of medical geography and climatology, that it may be made applicable to the acclimatation of masses and individuals in various countries; the topographical position and construction of habitations, military barracks and hospitals; as well as the diet, clothing, and military exercises of troops. In the works of neither of the authors quoted have the principles of medical geography been yet made applicable to those varied subjects of practical hygiène.

Dr. Mühry's work is divided into two parts,—the first explains the principles of *medical geography*, or diseases in their relation to geographico-meteorological causes; and the second is devoted to the *climatology* of various quarters and districts of the globe. The principles are divided into ten chapters, embracing various considerations on the nature and origin of diseases, which may be conveniently reduced to the following summary:—First, the general condition of natural diseases; second, first lines of climatology, or the geographico-meteorological relations of diseases; third, the geographico-geological relation of diseases; fourth, classification and terminology of diseases; fifth, system of the geographical distribution of diseases on the earth; sixth, the nature of miasms, malarious fever, yellow fever, and cholera indica, considered geographically; seventh, the contagious diseases, particularly plague and typhus, considered geographically;

eighth, of the absence of typhus in the tropical zone, and from the entire southern portion of the earth; ninth, the geographical relations of influenza, ophthalmia, dysentery, and scrofula; tenth, results from these investigations for epidemiology and hygiène.

The work of Dr. Mühry is a worthy offering of philosophy and science to Alexander von Humboldt, to whom it is dedicated. Though less purely geographical and statistical than the article on the same subject by Mr. Keith Johnston, it is highly creditable to the medical acquirements and reputation of the author; still we cannot give unqualified assent to some of the arbitrary classifications and doctrines he has endeavoured to establish in regard to diseases which, as Mr. Keith Johnston remarks, present the greatest unity in their pathology, notwithstanding the utmost diversity of climate, soil, and other causes from whence they originate. In any system of classification that might distinguish diseases which are in a great measure independent of atmospheric agencies, and those mainly dependent on temperature and humidity, with other terrestrial and atmospheric agencies, Dr. Mühry seems perfectly aware how difficult it is to draw a line of distinction, when he says:

"If we would at large survey the aggregate of diseases, their constant, quiescent, natural distribution, and also their never-resting fluctuations, depending on the intercourse of mankind, on change of seasons, on meteorological agencies, vegetation, and spreading epidemic influences, it is then important to seek in their conditions a right and clear classification of the existing types, and to adhere to this. Such classification must certainly be quite simple. In this endeavour, it is useful to remember the natural law already mentioned, that, in spite of all variations that have in the course of time taken place, diseases have nevertheless continued, and still remain, of their primitive type—namely, that no specific new creations have produced any alteration of their primitive state. This view of the subject is quite in accordance with the facts of botany and physics; for when some new forms of epidemic disease appeared among plants, the former declared that oidium is not a newly-created species of fungus, but a new epidemic extension of an old one to the vine."

Notwithstanding the difficulty that here presents itself, Dr. Mühry in his fourth chapter attempts the classification and terminology of diseases. He divides them into specific, dyscrasial, and localized diseases, or those affecting particular anatomical tissues and organs. Under the head of Specific Diseases are arranged the zymotic, miasmatic, and contagious diseases; while dyscrasiæ are made to embrace all the diseases of depraved nutrition and constitutional origin. The imperfections of this classification become more apparent when, in the next chapter, the author comes to consider the system of their geographical distribution on the earth, distinguishing them according as they are *independent* and *dependent* of temperature and moisture. In this chapter he considers the geographical order of diseases, and arranges them as—1. *Ubiquitous diseases* prevailing everywhere, and not geographically limited by equal lines of temperature and moisture; 2. Diseases geographically distributed into zones, and limited by isothermal lines; 3. Particular endemic diseases; 4. Diseases unknown to certain areas of the earth. Under the head of Ubiquitous Diseases he includes both those of the specific and dyscrasial class, separating the febrile exanthemata and erysipelas from the gouty, rheumatic, scorbutic, and herpetic inflammations. Such a classification is arbitrary, and opposed to the facts of pathology. It is true, indeed, that erysipelas is not limited by isothermal lines to either the tropical or temperate zones, but the statistical ratio of its prevalence in the former is much smaller than in the other; and while it follows as a result of cold atmospheric humidity, it is not less a dyscrasial disease than gout, rheumatism, or herpes: an opinion of humoral pathology that dates back as far as Hippocrates and Galen.

Neither are other dyscrasial or constitutional diseases of depraved nutrition—as cachectic ulcers, rheumatism, scurvy, and consumption—independent of temperature and atmospheric humidity, whether we regard their organ or their ratio of geographical distribution. The great error of the statistical researches instituted to determine the causation of disease among the soldiers of the British army is, that atmospheric agencies more particularly, we might say exclusively, have been dealt with in the calculations. Hence the extreme deductions of unmodified and partly erroneous opinion, that consumption and rheumatism, in their origin, are quite independent of these agencies. Doubtless they are mainly, but not altogether so. Regarding consumption, Mr. Keith Johnston observes—

"It originates in all latitudes—from the equator, where the mean temperature is 80°, with slight variations, to the higher portion of the temperate zone, where the mean temperature is 40°, with sudden and violent changes. The opinion long entertained, that it is peculiar to cold and humid climates, is founded on error. Far from this being the case, the tables of mortality of the army and navy of this and other countries, as well as those of the civil population, warrant the conclusion that consumption is more prevalent in tropical than in temperate countries. Consumption is rare in the Arctic regions, in Siberia, Iceland, the Faroe Islands, the Orkneys, Shetlands, and Hebrides. And in confirmation of the opinion that it decreases with the decrease of temperature, Fuchs shows, from extensive data, that in Northern Europe it is most prevalent at the level of the sea, and that it decreases with increase of elevation to a certain point. At Marseilles, on the seaboard, the mortality from this cause is twenty-five per cent.; at Oldenburg, eighty feet above the sea, it is thirty per cent.; at Hamburg, forty-eight feet above the sea, it is twenty-three per cent.; while at Eschwege, four hundred and ninety-six feet above the sea, it is only twelve per cent.; and at Brotterode, eighteen hundred feet above the sea, 0·9 per cent. It is calculated that in the temperate zone, within which nearly all the civilized inhabitants of the globe are located, at least one-tenth of the population die of this malady. It is uniformly more fatal in cities than in the country. In England, the excess in cities is equal to twenty-five per cent. The greatest mortality occurs from the age of fifteen to thirty." (p. 121.)

Respecting consumption in the United States climate, Dr. Forry has also established, by numerical facts, that the number of consumptive cases which originate in summer are not less than those of winter; and that the frequency of the disease in the United States army, located in the warmer, moister, and more uniform climate of East Florida (as in our own army in Jamaica and the West Indies), is greater than in the more inclement northern regions of America or Canada. By the statistical reports of the British army, it appears that the proportion of attacks in Jamaica and the West Indies is 12·5 per 1000, but in Canada and the United Kingdom only 6·5. In the southern divisions of the American climate, the Lower Mississippi and East Florida, the average proportion attacked is nearly 10·5 per 1000; while in the most inclement regions of the north, the average is little more than 5·0 per 1000.

Dr. Mühry's Fifth Chapter, On Miasmatic and other Diseases, mainly Influenced by Temperature, and on the System of their Geographical Distribution, is an admirable exposition of facts on the subject of zonic diseases. They consist chiefly of miasmatic diseases, and those of a self-generated contagious type, requiring a certain fixed amount of temperature for maintenance and prevalence,—as remittents, yellow fever, plague, genuine typhus, and cholera. Their geographical distribution into zones, north and south of the Equator, would appear in a great measure regulated, in America, Africa, and Europe, by relative degrees of temperature and humidity in the several places where they prevail. Their causation, however, is not solely or exclusively atmospherical, whether we view the causation of diseased action either in its relation to the temperature and humidity of the air, or to its relatively daily and annual states of density and electricity. Diseases bounded by isothermal lines are associated no less with locality and the vegetation produced by atmospheric causes of heat and humidity, than they follow the physiological action of food and habits of the several animal organisms in different latitudes. Malarious fevers, yellow fever, plague, and typhus, have, like plants, particular climates or zones where each thrives best, and beyond the limits of which the disease is never produced, unless change of seasons gives rise to a state of climate analogous to that which is due to the latitude and position of particular localities.

When we review the history of the circumstances present at the seasons of the year, and in those countries where *intermittent* and *remittent* fevers become prevalent, it appears more than probable that a diminution of atmospheric oxygen and an augmented temperature, in those marshy situations where they appear, are the remote causes of the paroxysmal susceptibility generated in the nervous and circulating systems of the living body. If the febrile paroxysms are distinctly observable, they terminate in less than twenty-four hours; and when fevers are of more than one day's duration, they consist of repeated paroxysms, subject to the same diurnal periodicity of increase and diminution, indicated by the frequency of the pulse and the temperature of the skin. The symptoms of remittent fevers suffer diminution or remission during the lessened temperature of night or morning, and experience exacerbation during the sun's ascension, and the extreme heat after noon. The strength and frequency of the pulse varies throughout the day, and rises in proportion to the heat of the atmosphere. During the exacerbations, it may rise to 140° in the minute, and falls to 110° or 118° with the remission. Continued high temperature has a tendency to convert intermittent forms of fever into remittent, while cold or diminished temperature changes the latter type to the remittent. The following extract from Mr. Keith Johnston's remarks on the influence of topographical situation and degrees of temperature in producing intermittent fever, is well worthy of attention :—

"From its occurring constantly within the tropics, but ceasing far south of the polar circle, it appears that a high temperature is a condition necessary to its production, but this can only be considered as an exciting cause. It is found that a summer temperature of 60° is necessary to the production of the fever, and that it will not prevail as an epidemic where the temperature is below 65°. It therefore occurs in winter at places where the season has a mean temperature of 60° or upwards, as at Vera Cruz, Tampico, Havana, &c.; but at New Orleans, and generally under the thirtieth parallel, where the mean winter temperature is under 50°, the fever is suspended. At New Orleans, the necessary heat exists for nine months of the year—March to November; at St. Louis, five months—May to September; at Montreal, four, and Quebec, three months. A continuance of more than two months of a heat equal to 60° is necessary to its development; hence it prevails more in October than April, though their mean temperatures are nearly the same, and its greatest prevalence in every latitude is generally some weeks after the hottest months of the year. It is rarely directly fatal, but frequently results in liver disease and dropsy. The western area of the disease is limited in America on the east by the range of the Appalachian Mountains, into the very gorges of which it ascends, by the valleys which penetrate their flanks: while that of the seaboard extends inland to the eastern base of the same range. South of lat. 33°, where this barrier terminates, its eastern limit is the Atlantic Ocean. On the south-west its boundaries are the Cordilleras of Mexico and the southern Rocky Mountains. It is almost unknown three hundred miles beyond the western boundary of the States of Missouri and Iowa, and above lat. 37° north. On the north it ceases to prevail as an epidemic at lat. 44°, and it does not occur even sporadically at lat. 47°. In Western Europe, its limits include Scotland, and on the Continent it extends to the mouth of the Angermann River, at 62° 40′, in Sweden. Farther eastward it sinks to a lower latitude, and in Central Asia it appears not to extend beyond lat. 55° or 57° north, forming a curve nearly coinciding with the isotherm of 41°. To the south of this, from lat. 54° to 40°, at the level of the sea, on the coasts and river banks, it constitutes one of the most prevalent diseases. On the shores of the North Sea it causes a mortality of one in twenty, and even one in fourteen. On the northern boundary it appears only in its more simple form during summer and autumn. Between lat. 55° and 40° it occurs usually in spring as tertian, and in autumn as quartan ague. It is prevalent on the Lido shores and in the islands of the Gulf of Venice, but does not enter the city. It is periodical at Rome. Elevation above the level of the sea has a very marked influence on the occurrence of intermittent fever; thus while it ravages the *tierra caliente* of Mexico, near the level of the sea, it is almost unknown in and around the city of Mexico, 7450 feet above that level, although both places are in the same latitude. The inhabitants of the Appalachian Mountains, at an elevation of about 3000 feet, are almost exempt, while those who inhabit the valleys, under the same parallels, are affected. Farther north, at an elevation of 1500 feet, at the sources of the Alleghany and Genesee rivers, the disease is almost unknown, while on the low shores of Lake Ontario, directly north, it is prevalent. In lat. 41° it is prevalent at 900 feet above the level of the sea. It also prevails at lat. 41° 30′ north, at 1100 feet in elevation, all along the rivers and ponds in the Cuyahoga Basin. The constantly increasing elevation of the desert to the west of the Mississippi, and the increasing dryness of the plains, are probably the chief causes of the disappearance of the fever, under the same parallels in which it prevails on the banks of that river. In Europe, in lat. 52° north, at Cassel, it rises little more than 400 feet above the sea. One degree farther south it occurs every year at an elevation of 600 or 700 feet, near Berka on the Werra; but at 900 feet it comes only once in ten years in isolated cases. In lat. 47°, at Grätz, 1200 feet above the sea, it is endemic; it is sometimes epidemic at Stanz in Switzerland, 1700 feet high; and it is prevalent on the plateau of Castile, 2300 feet high. In Peru, ague is observed at an elevation of 10,000 or 12,000 feet above the sea; and according to Tschudi, it occurs there in dry and barren regions. In Iceland, no native is attacked by ague, and strangers suffering from it soon recover. It is unknown in Tasmania." (Johnston, p. 120.)

The isothermal lines, first employed by Humboldt to measure the heat and cold of the earth, and to connect places having the same mean temperature, differ sensibly from the lines of latitude. We need not now enter into details how the earth's annual rotation and oblique motion, in relation to the sun, the centre of the system, fixes the tropical limits of the sun's apparent declination south and north of the equator, and produces alternate winter and summer on either side of the line, as it will be evident that the mean annual temperature obtained at different latitudes must decrease from the equator to the poles. Had the whole surface of the earth been uniform, presenting the like relations to radiant heat, unaffected by the unequal action of disturbing causes, the mean temperature of every point would have been in proportion to the radius of the parallel of latitude. But the mean temperature of places, calculated according to Dr. Brewster's formula, from an equatorial mean of 81° 50′ Fahr., differs considerably from the mean obtained by observation. The mean temperature is usually higher at the same latitude in the Old World than in the New, and in north latitude than in south. Thus the isothermal line of 59° Fahr. traverses the latitude of 46° in Europe, but descends to latitude 36° in America. The general causes which disturb the symmetrical distribution of temperature, are the annual variations of the upper equatorial and lower polar currents of the atmosphere, the differences of its contained humidity, the unequal distribution of land and water in various countries, the peculiarity of the surface land, and its relative height above the level of the sea—all of which causes have more or less influence in determining the local temperature or climate of countries, and in fixing the isothermal lines that mark out the zones of disease.

The zones or belts of disease thus marked out on the globe by Dr. Mühry and Mr. Keith Johnston, are the tropical, temperate, and polar zones; which are distinguished on Mr. Johnston's map by being respectively coloured brown, green, and blue. The limit of the north tropical zone, and consequently the southern limit of the temperate zone of this hemisphere, is formed by the isothermal line of 77° Fahr., or 19° Reaumur. It traverses, in America, Cuba and Florida; skirts the Cape de Verde Islands to Africa, where, extending beyond the mathematical limits of the tropic, it passes the northern part of the Sahara below Algiers, runs through Egypt, Northern Arabia, and Persia, into China, where it sinks into the Pacific Ocean, and below the limits of the northern tropic. The limiting line of this zone ascends somewhat in summer, when the sun is north of the equator; and descends again in winter, when the sun is to the south of it.

This is the peculiar habitation of the worst forms of malarious, intermittent, remittent, continued, and yellow fevers, and of those diseases found in alliance with them—as dysentery, diarrhœa, cholera Indica, and affections of the liver. Its northern limit is the southern bar to the prevalence of epidemic contagious typhus, which is the proper and peculiar product of the northern temperate zone. The marsh malarious fevers of the tropical zone prevail most in flat, low-lying countries, possessing a damp argillaceous soil. They usually make their appearance soon after the setting in of the rainy season, or when overflowed grounds—as rice-fields, the oozy beds or mouths of rivers, and irrigated cane plantations—begin to dry up and leave portions of wet land exposed to the sun. So many and varied have been the observations made in respect of this, that they force upon us the inference that the general cause producing these fevers, in all their different forms, must be a miasm emanating from the soil, and acting as a poison on the blood. Some have been even bold enough to assert that the matters so emanating may be condensed and made susceptible of observation under the microscope; but we must honestly confess that as yet we are unacquainted with the specific nature of this miasm, and whether it differs in kind or not. This we know, however, that intermittents, and fevers of a remittent and continued type, appear in all countries under similarity of climate, season, and soil, when the mean temperature of places exceeds 2°—3° of Reaumur, or 41° of Fahr. The constant occurrence and similarity of these diseases (the type of which Mr. Johnston demonstrates the bilio-putrid type of summer and autumn), in various countries, and under similar conditions of temperature, soil, and moisture, concur to prove that they depend on these agencies, and are the consequences of a common cause. Dr. Mühry's theory of its nature assumes that it is a vegetable fungous organism; but to the validity of this we cannot assent. The mortality from the entire class within this zone, as Mr. Johnston tells us, amounts to seventy-five per cent., and decreases with the lowering of temperature in the seasons of their occurrence. In a series of dysentery epidemics narrated by Ozonam, thirty-six occurred at the end of summer, twelve in autumn, and only one in winter.

The space between the tropical and polar zones, known as the temperate zone, is inhabited by that part of the human race which is capable of manifesting the greatest amount of bodily and intellectual vigour. It is limited southwards by the isothermal line of 77° Fahr., and northwards by that of 41° of the same scale. This zone may be said to embrace the extreme climatic conditions of the two other zones, under the seasons of summer and winter. At these periods the prevailing disorders to which armies and military masses, moved from one country to another, must be more peculiarly subject, will partake alternately of the character of diseases prevalent within the tropical and polar zones. The results and experience of the Crimean expedition have afforded ample evidence of this fact. In the Crimea more particularly, and in the European countries of this zone also, both soldiers and seamen, but specially the former, will suffer greatly from diarrhœa and dysentery in summer; from intermittents, fevers of a remittent and continued type. The latter, under bad diet and imperfect ventilation of the soldiers' huts and hospitals, soon degenerate into genuine *contagious typhus*. This true typhus, once so prevalent, and now, under a better and more scientifically-administered sanitary system, but rarely seen, as well as the true glandular plague, have their special habitation in this zone, between the thirtieth and fortieth degrees of northern latitude; and though propagated occasionally beyond these limits, by a secondary and specific poison generated from human bodies, they have evidently a distinct primitive origin from local and atmospheric agencies.

Mr. Johnston's comments on the epidemic prevalence of *plague* and *typhus* present such a striking array of valuable statistical facts, that we cannot better familiarize our readers with the peculiar character of these diseases, than by quoting a portion of the author's remarks thereon:

"The *Plague* has its endemic seat on the eastern shores of the Mediterranean, where it has been known to exist since the middle of the sixth century. . . . . It may be considered as occupying permanently a portion of the Old World, extending between the parallels of latitude 29° and 42° north; and while it is thus permanent in some places, it appears more or less frequently in others. Its term of periodicity was reckoned to be—for Constantinople nine years, Egypt five, Aleppo ten, Antioch fifteen, and Cadiz forty-three years. In Sydenham's time it was said to ravage England every forty years. It has not appeared in Scotland since the reign of Charles II., although it remained a few years longer in England. It seldom extends to the southward beyond Siout in the valley of the Nile, or Jiddah on the Red Sea. In Asia it prevails chiefly on the coasts of Syria, and a portion of the shores of Asia Minor, where it sometimes ascends the river valleys. In Europe it is endemic only on a part of the eastern coast of Turkey. In 1816 it was very destructive in the Ottoman empire, and extended into Austria, Italy, and Sardinia; and it was at Moscow and Marseilles last century. In 1841 it raged in Syria and at Erzeroum with great violence. It has never yet appeared in the southern hemisphere, nor in America.

"Like the yellow fever, the plague appears to be limited to the lower portions of the earth's surface, the more elevated situations being usually exempt from its scourge. When it is ravaging the lower quarters of Constantinople, the inhabitants of the higher portions of the seven hills on which the city is built often escape altogether; and Brayer mentions a village situated on Mount Alem Dagh, at an elevation of about sixteen hundred feet above the sea, where it was never known to appear, and which was resorted to as a place of refuge for the citizens; and there is a place in Malta hitherto inaccessible to the disease, and on this account called *Safi* (pure). It is recorded by the French physicians, that during their occupation of Cairo, the plague never reached the citadel of that city; and Clot Bey states that it, as well as the village of Loumeldik, situated at a considerable elevation, was spared during the epidemic of 1835. The nature of the soil has much to do with the development of this disease. As an argillaceous soil is most favourable for the development of malarial fevers, so it is a characteristic of the localities where the plague is endemic. . . . .

"*Typhus.*—This form of fever, which occurs frequently as an epidemic, appears to belong exclusively to the north temperate zone, and even here it avoids extreme latitudes. It is scarcely ever mentioned by medical voyagers in hot countries. As yellow and intermittent fevers occur in low latitudes, near the level of the sea, so typhoid fevers have their base line in a high latitude, and at a greater elevation. Yellow and intermittent fevers decrease from south to north; but typhus, on the contrary, decreases from north to south. In America, typhoid fevers diminish in frequency beyond the parallel of 45° north. Typhus does not appear among the fur stations of the Hudson Bay Company between the parallels of 48° and 58° north; and no mention is made of its occurrence among the crews of the Arctic voyagers nor among the Esquimaux, who live in close, unventilated snow-huts; neither has it been observed by Ermann and Wrangell among the inhabitants of Siberia. Typhus has, therefore, a northern as well as a southern limit. In Western Europe it prevails between the parallels of 44° and 60° north, or between the isothermal curves of 48° and 52°; and in North America between the parallels of 32° and 48°. In places where the mean annual temperature rises above 62°, or falls below 40°, it prevails but little in either continent. The geographical and climatal limits of typhus in Europe and America will be found to correspond nearly with those of the glutinous cerealiæ and the potato. It decreases with elevation; and to this cause has been attributed its absence in the hospital of Madrid, 1995 feet above the sea. It occurs in every season, but is most prevalent in autumn and winter." (Johnston, p. 121.)

Yellow fever also, from the tropical regions, makes occasional incursions into this zone, when favoured by tropical identity of climate. Under high degrees of temperature it has been met with both at Gibraltar and Cadiz; and has extended, in America, southwards beyond its usual limits, under like favouring circumstances. But as soon as the temperature falls below 55° of Fahr., the importation of yellow fever into this zone becomes impossible, and supersedes the necessity of quarantine. Elevation of site above the level of the sea, by its association with decrease of temperature, exerts the same agency in limiting or extending the prevalence of yellow fever, as does increase of latitude. On this subject, and the perpendicular distribution of the disease, we again quote from Mr. Johnston:

"*Perpendicular Distribution.*—From a similar cause, decrease of heat, the yellow fever never appears beyond a certain elevation. At Xalapa, in Mexico, on the same parallel as Vera Cruz, but 4330 feet above the sea, it is unknown. Maroon Town, and the Phœnix Park, Jamaica, are noted for healthiness; and while the pestilence of yellow fever rages in the low grounds and along the coasts, cutting off thousands annually, these elevated regions enjoy a complete immunity from its effects; for that bane of European life has, according to Major Tulloch, never been known, in any climate, to extend beyond the height of 2500 feet. The inner Cabrite 430 feet, and the outer Cabrite 590 feet in elevation, are also remarkably healthy. In the island of Grenada, Mount Cardigan, 500 feet, and Richmond Heights, 730 feet, are not sickly. Mount Desmoulin, near Roseau, in the island of Dominica, 1500 feet above the sea, has invariably been free from yellow fever. The same immunity has been observed in San Domingo, in the mountainous parts of which, whatever be the nature of the soil, this disease does not prevail. In the United States the yellow fever is never known to prevail in very high situations, whatever be the condition of the localities; but at what point it ceases to appear or prevail, is still an unsettled question. The disease varies in intensity, and in the numbers attacked, according to latitude. M. Moreau de Jonnés shows, by elaborate statistics, that in the United States the mortality amounts to one-half of those attacked, while in Spain it is limited to a third or a fourth of the total number. This is accounted for from the difference of climate and soil between Europe and America, which in winter is so extreme, that in order to find in

Europe a cold as intense as that of the United States, it would be necessary to remove 12° or 14° farther to the north." (p. 121.)

Besides these diseases of the temperate zone, which in hot, close, humid summers afflict both soldiers and seamen, diarrhœa, dysentery, and cholera prove occasionally most destructive of life. During winter, again, soldiers and seamen within the temperate zone will suffer from *gelatio*, or frost-bite, erysipelas, scurvy, and influenza; as in the polar zone, when not provided with suitable diet and clothing, as preventive means against these diseases.

The polar zone, again, is directly opposite in climate to that of the tropical zone. Its southern limit is the northern boundary of the middle or temperate zone, being the isothermal line of 41° Fahr., or 2° or 3° of Reaumur; while its northern limit extends 8° or 12° below the zero of Reaumur. It commences on the western coasts of North America, above Sitka, sinks downwards till it comes to Canada and Newfoundland (52° N. L.), and running across to Europe, ascends till near the borders of Iceland (62° N. L.); from whence it sinks towards Norway and Sweden, and running above St. Petersburg and Moscow, crosses Siberia (55° N. L.). Here the diseases most prevalent are affections of the skin, digestive organs, and influenza, with dyscrasial types of disease, scurvy and erysipelas; while all classes of malarious fevers disappear, except on very rare occasions, in summer, when both cholera and dysentery may occasionally prevail.

The subject of Medical Geography is intimately connected with the improvement of epidemiology, and the acquisition of clear and scientific rules for acclimatizing soldiers and seamen in various quarters of the globe. But we must be satisfied with merely drawing attention to this important point; nor can we do more than allude to the peculiarities in the climatic conditions of the tropical and temperate zones of the south, which, in respect to salubrity, may be said to surpass all others.

We have endeavoured to give an exposition generally of the principles of Medical Geography, and their practical application to a better knowledge of diseases; and while we have attempted to offer something beyond a dry abstract of Dr. Mühry and Mr. Keith Johnston's valuable labours in this field, we gratefully acknowledge the vast amount of carefully analysed facts and statistical information they have brought to bear, and thus augmented our sources of true knowledge. We would only, in conclusion, express a hope that we may receive many like contributions from these authors.

Review IV.

Was the Roman Army provided with Medical Officers? By J. Y. Simpson, M.D., F.R.S. E., F.R.C.P., Professor of Medicine and Midwifery in the University of Edinburgh.—1856. pp. 29.

Nothing can be more obvious than the conception of a parallel between the pursuits of the physician and those of the antiquary. Both, to a certain extent, practise conjectural arts, where, from something that is revealed, the task is to divine something that is hid. The same sagacity is required for both. Each must examine thoroughly the remote, observe accurately and discriminatingly the immediately accessible, and collate carefully the adjuncts in either; and thus, all the necessary conditions being assembled and weighed, he may proceed to infer prudently and safely. So conducted, no effort of the mind can be more interesting than the process of their research; and the results, of paramount importance in the physician, are not valueless, or merely curious, in the antiquary. The mental attributes being so evidently congruous, it is no wonder that not a few good physicians have been also zealous antiquaries; among whom we may notice our own Mead and William Hunter. But, as both descriptions of researches have their manifest difficulties, they have alike their manifest dangers; and the mind whose temperament is not at once acute and patient is tempted in both to leap to conclusions, which, in the one case, lead to a fault, and in the other, to a folly.

A subject for archæological discussion, especially where it connects itself with a medical question, cannot be valued so much intrinsically, as because it presents merely a kind of theorem, in the proving of which many resources of the intellect and of the memory, and many appliances of ingenious yet exact scholarship, are to be brought into exercise; and the more copious, the more varied, the more pure, the more direct, and the more precise these are, of course with the more certain results. If the tenor of our remarks, then, should sometimes show, that to enter the realm of the antiquary is to tread upon arduous ground, and among paths whose voids as well as whose intricacies require the most vigilant circumspection, it will certainly be in no critical spirit towards Dr. Simpson, whose ability and ingenuity we acknowledge and admire; and whom we shall not for an instant pretend to class with those, and they are many, whose shallow inanities make too often antiquarianism a by-word of reproach. We merely desire to contribute our own humble elucidations towards the interesting topic he has selected, as these may chance to present themselves in the course of our perusal of his memoir. Cursory, therefore, in point of method, and casual as to previous opportunities of preparation, but not foreign to at least our more juvenile tastes, they pretend to enough if they aspire to the merit of being accurate in as far as they extend, and if they can attract or renew for a few minutes the attention of our readers to the remarkable pamphlet before us.

Dr. Simpson opens his interesting disquisition by the remark, that "little or nothing has hitherto been written by archæologists regarding the medical staff of the Roman army." He is thus not aware of the previous discussion of the subject by Kühn,* to which we have ourselves had no opportunity of referring; nor, apparently, of the more general, yet important, notices embraced in the medical histories of Hecker† and Haeser.‡ The author then proceeds to tell us, that he

* De Medicinæ Militaris apud veteres Græcos Romanosque conditione. Lips., 1824–7.

† Geschichte der Heilkunde, Band ii. p. 270. The remarks by Hecker extend to twenty pages, and show the usual learning and acuteness of this eminent historian. He expresses himself as largely indebted to the previous labours of Kühn.

‡ Lehrbuch der Geschichte der Medicin, Zweite Auflage, p. 210.

has searched in vain among the Roman medical writings, and among those of the Greek physicians who practised in Rome, as well as in the Roman classics generally, for any direct information relative to the medical or surgical care of the numerous armies employed by Rome in the different quarters of the world. What he has gleaned there has been merely a few incidental observations; and these, necessarily scanty in point of number, as they are defective in precision, yet valuable from their source, he presents in detail. Dr. Simpson next refers to the histories of other more ancient governments than that of Rome, and shows that these are not without allusion to the office of army physicians; while he gathers especially some scattered notices of military surgical services from amid the classical literature of Greece. Perhaps his most conclusively apposite reference here is to the practice prevalent in Egypt, which he cites on the authority of Diodorus Siculus; who mentions that the soldiers, in the military expeditions of that country, were cured without fees, for the physicians of the army received a salary from the State. We must recollect, however, that at the time when Diodorus wrote, Egypt had been for nearly three centuries under a Greek dynasty, and was beginning to be largely influenced by the Roman policy. It is not till near the close of his essay, that Dr. Simpson adverts to the relatively significant organization of the valetudinarium in the Roman camp, as described by Hyginus Gromaticus, towards the commencement of the second century of the Christian era. Still nearer the termination, he refers to the various instances in which medical men are recorded to have been in professional attendance, not upon the troops generally, but upon the Roman dignitaries personally, during the course of their military campaigns; a custom, we may add, which prevailed long after, with reference to the leaders of the armies of the Middle Ages.

We shall not make any close attempt to follow the able author through the wide and diversified field of research which he has evidently traversed in the earlier part of his inquiry, and still less shall we endeavour to add to the number of the authorities he has cited, even were this to entail upon us little more trouble than a dexterous manipulation of indices; but we could have wished at least that a few of these authorities had been more accurately interpreted. Without pausing to cavil at such points as mistaking the *restitutio ad integrum* of the Roman law, exercised in favour of the military surgeon, for merely an exemption (p. 6) from burdens and taxes, we must really take exception to such renderings as, in a passage of Vegetius, *ægri contubernales,* into not only *sick comrades* simply, but the elevating of these into the comrades of the "generals, tribunes, and their assistants, as wielding the highest authority;" while we are especially surprised at the purport here assigned to the concluding words of the original: "*ipsius comitis,* qui majorem sustinet potestatem." Notwithstanding Dr. Simpson's remark in a subsequent passage (p. 23), that Vegetius says nothing to confirm the notion of the provision of a separate valetudinarium, or camp hospital, as described at an earlier period by Hyginus, it seems clear that the persistence of this arrangement is here distinctly implied in the use of the term *contubernales;* a word manifestly signifying, not comrades generally, but strictly, those occupying the same tent or *contubernium*. On the other hand, it is a sad degradation of the *ipse comes*, the chief officer of a province under the later Emperors, to reduce him, apparently split into a plurality of entities, to the assistantship of the tribunes; as it is inopportune to overlook here the fine remnant of Roman discipline, which marks out expressly the sick soldier as still the care, not only of high, but of the very highest authorities. In a reader of Homer, again, even through the often lax translation of Pope, we might have wondered, but in one who, like Dr. Simpson, is able to revert to the Greek original, we wonder still more, that he could metamorphose the gigantic refreshment prepared by the slave Hecamede for Nestor and Machaon, into an example of an "internal medical treatment;" considering the nature of the ingredients, its participation by both the sage and the physician, the whole and the hurt, to the relief of their ardent thirst and the excitement of their garrulousness—

Τὼ δ' ἐπεὶ οὖν πίνοντ' ἀφέτην πολυκαγκέα δίψαν,
*Μύθοισιν τέρποντο πρὸς ἀλλήλους ἐνέποντες,**

and its administration, too, in a cup so huge, and so well replenished, that any other old man than Nestor could not have lifted it from the board.

Dr. Simpson has been at some pains to show, that in no ancient medical writings, in as far as he has observed, is there "any allusion to the circumstance of surgeons or physicians being regularly appointed as army medical officers in the Roman army." This, perhaps, is said somewhat too absolutely, as he himself adduces a passage from Galen which has reference to the physicians employed in the German wars; and certainly not, we have a right to presume, employed in them only. We might add, that the same eminent writer, in a notice† which appears to have escaped Dr. Simpson's observation, distinctly designates Antigonus as a military physician of celebrity. But relinquishing this description of evidence as incomplete and indefinite, he presents the main foundation for his inferences in the production of several ancient Roman inscriptions, discovered in this country or elsewhere; and regards the terms of these as furnishing conclusive demonstration that such a provision of medical officers was made, at all events in the time of the Empire. Of these inscriptions, the greater part have long been known; a portion of them being extracted from the work of Gruter, published two hundred and fifty years ago, and not one of them appearing for the first time. The evidence they supply we are willing to accept as decisive, and yet it scarcely justifies more than the same kind of inference which appeared deducible from the passage of Galen, and merely leads us to conclude that, as certain cohorts and legions possessed medical attendants, they were not wanting in the whole of the others also. Still, looking at all the probabilities, at the collateral facts which are accessible, at the character of the Roman civilization and discipline, and, above all, at the recognised position proved to be assigned to the

* Homeri Ilias, lib. xi. ver. 642.
† De Compositione Medicamentorum secundum Locos, lib. II. cap. 1 et 2.

military physician in various legislative enactments, it would be supererogatory to demand anything more explicit; and we have to thank the learned writer for having so grouped the materials he has collected as to give an answer which ought to be entirely satisfactory to the question he proposes.

Yet, if we were to concede that these inscriptions had been presented to us in a style of accuracy, whether of transcription or of interpretation, fitted to give content to the medical archæologist, we should be departing farther from the line of duty we have marked out for ourselves, even when discussing the literature of our art, and should be approaching nearer the gaping adulation of the newspaper criticasters, than would be consistent with the proper functions we have assumed, and which, whether in matters scientific or merely æsthetic, we are ever anxious to discharge efficiently. Of the six inscriptions cited by the author, three are inaccurately rendered, not one is scrupulously in form, and two only are interpreted to our satisfaction. He informs us, and in so far truly, that "these old Roman inscriptions" abound in errors of orthography and grammar; yet on this very occasion (note, p. 23) he himself adds to their number, by supplying an ellipsis (here a classical elegancy rather than an error) by means of the word *uxoris* instead of *uxori*, the latter of which was alone consistent with even his own interpretation. But we recognise the greater necessity for avoiding such faults as do not appear in the originals: as *commilitionum* for *commilitonum* (p. 20), *libertatibus* for *libertabusq.*, with the omission, moreover, of a whole line of the context (p. 21), which either deface the relic, or materially affect its sense. It is not mere fastidiousness to notice such defects. If it were so, we should have passed them in silence. But that art by which the antiquary clothes the naked relics of time in a fresh garb of vitality, and elicits the picture of an extinct civilization from the apparently barren record of a few mutilated words, depends wholly for its success upon his faithful observation and employment of all the materials within his reach; and he must adopt these in their strict purity and authenticity of descent.

Let us take, for example, the inscription from Hagenbuch (not *Hugenbach*) and Orelli, given (p. 23) by our author, with the interpretation, that it was dedicated by "Atticus Patronus" to "Titus Claudius Hymnus, physician to the twenty-first legion, and to Claudia Quieta his wife." The original is thus transcribed:

TI CLAVDIO HYMNO
MEDICO LEG XXI.
CLAVDIÆ QUIETÆ EIUS
ATTICUS PATRONUS.

Now, the significance of this inscription, and it reveals much, is greatly impaired by the primary mistake of accepting its two first letters as denoting the name of Titus. In all abreviations of Roman names, T alone stands for Titus, and TI for Tiberius. Adopting, then, justly, the name of Tiberius, and noticing its immediate connexion, we turn to that of Atticus, when it instantly occurs to us that the addition of Patronus is here no cognomen, but expresses the position of him who has erected the tablet, with reference to the persons commemorated;

because, if he do not stand in the capacity of patron, it is difficult to divine in what relation he appears. Atticus was, then, the patron of Tiberius Claudius Hymnus, and Claudia Quieta, his freedman and freedwoman, and formerly his slaves; nothing having been more usual in ancient Rome, than to find the physician, the *grammaticus*, and, alas! even the *antiquarius*, in the servile, or newly-emancipated, condition. But Atticus is itself a cognomen, and we know that already, about the first age of the Empire, the cognomen singly was in habitual use as the ordinary proper name. This patron may have been Ti. Claudius Atticus Herodes; but we prefer to believe, on various grounds, that he was more probably T. Pomponius Atticus, the distinguished friend of Cicero. If so, he was also the contemporary of Tiberius Claudius Nero the elder, and doubtless an intimate friend; the son of the latter, the future Emperor Tiberius, having married his granddaughter Vipsania. It was the custom for the proprietor of a slave, on granting him manumission, to confer upon him usually his own name, but often that of an honoured friend, in addition to the single name, now retained as a cognomen, which the slave had formerly borne. What, then, was more likely than that Atticus should have so employed that of Tiberius Claudius? We have thus a wide domestic and social history opening up in this brief epigraph, the whole of which would have escaped us by erroneously accepting the prænomen of Titus for that of Tiberius; yet it elicits, easily and naturally, many points of the greatest interest, as well with reference to the individuals named, as to the special condition of our profession in Rome, as determined from other sources.

But, beyond this, we have here a matter closely germane to our subject, because revealing a strong probability that the inscription in question dates at least from thirty to forty years before the Christian era: and causing it to establish, therefore, the existence of medical officers in the Roman army upwards of a century before the period of Domitian, noted by Dr. Simpson; and about two centuries before the earliest written authority, that of Galen with reference to the German war, adduced by him, yet scarcely insisted on; or three centuries before the brief and still vague allusion extracted from the letter of Aurelian, as cited by Vopiscus. But singularly enough, in a preceding sepulchral inscription presented by the author (p. 21), we find the name of another physician, evidently also a freedman, and now apparently a freedman of the elder Tiberius Claudius himself, although here again we find incorrectly introduced the name of Titus. This inscription is given on the authority of Reinesius and of Dr. Middleton, but the correctness of the transcript is sustained by a reference to neither. Accurately transcribed, its tenor is as follows:

D. M.
TI. CLAVDIVS. IVLIANVS
MEDICVS. CLINICVS. COH. IIII
PR. FECIT. VIVOS. SIBI. ET
TVLLIE. EPIGONE. CONIVGI
LIBERTIS. LIBERTABVSQ
CLAVDIIS. POSTERISQVE
EORVM
H. M. H. N. S.

We judge that Julianus was a freedman of Tiberius Claudius, not only because he bears so remarkable a name, but more especially because he dedicates his tomb, not to his own freedmen and freedwomen, but to those of the Claudian family, still evidently not imperial, and their descendants, in terms of the line omitted by Dr. Simpson; a destination which never would have occurred in the monument of one free-born: while in the letters at the close, disregarded by the author, we have the known abbreviature excluding even his heirs from its use: *hoc monumentum heredes non sequetur*. We have, besides, some authority, in the adoption of the O for the V in VIVOS, that the inscription belongs to the era in which Tiberius Claudius Nero lived, for then this style of orthography was customary. Had the vowel-sign V been really given throughout as U, as Dr. Simpson transcribes it, we should, on the other hand, have been compelled to descend to a later period, and to assign the tomb to a Christian, with whom only this form of letter appears to have been thus in use. Hence, considering the exact inscription, and depending upon its intrinsic evidences, we again fix upon the age of Augustus for the date of this monument, and for another proof of the comparatively early existence of a Roman medical staff. It is on scantier grounds, and merely from the scope of the designation, that we are inclined to consider here the medicus *clinicus* as having probably had the peculiar charge of the hospital tent, or *contubernium*, in the stationary camp; while on the medicus *ordinarius*, named elsewhere, devolved the duty of more casual or detached services. Nothing was more remarkable in Rome generally, than the multifarious subdivision of the different departments of medical and surgical practice.

There is another evidence, in an inscription given last by the author, of what was ordinarily the aboriginal position in life of the medical officer in the Roman service. The version appended, which is given in somewhat hesitating terms, and is indeed such as the construction evidently does not admit without constraint, bears that "M. Satrius Longinus, physician to the three-banked ship or trirem, the Cupid, and those, or the heirs of those freed by Julia Venerias, his wife, erected the tablet to the manes of this deserving lady." The original is given as follows:

D. M.
IVLIÆ VENERIÆ.
M. SATRIVS LONGIN
MEDIC. DVPL. III. CVPID
ET. IVLIA VENERIA LIBER
HER. BEN. MER
FECER

Our translation will interpret it somewhat differently, and, we venture to assert, more correctly. The double emolument of the *Duplarius* is, however, noticed in the essay:—"To the shades of Julia Veneria. M. Satrius Longinus, double-salaried physician of the trireme the Cupid, and Julia Veneria, the freedwoman, to a well-deserving mistress." (*Heræ bene merenti.*) Possibly, and indeed not improbably, from the conjunct action of the parties, the *liber*, which we have here interpreted only as *liberta*, may apply to Longinus, as much as to the freedwoman using *simpliciter* the name of her mistress. At all events, the way in which the names are interlinked gives a reasonable indication of the social position of the physician also.

There are two physicians mentioned with the *cognomen* of Ingenuus, whom Dr. Simpson unaccountably introduces as bearing the same *nomen gentilicium;* though of this description of name that in question possesses none of the characteristics. With regard to these, we are inclined to suspect that they were also but a short space removed from the servile condition. We may presume that the first possessor of the cognomen of Ingenuus, or free-born, must have been the son of a liberated slave, otherwise the distinction of the appellation was one not likely to have been adopted. With one of its bearers here, it might be considered a mark the more necessary, as he was physician to a cohort of the watch, a body nearly wholly composed of manumitted slaves. Attached to the name of this individual, there is in Gruter* a monogram denoting that he was a beneficiary of the prætor: a circumstance which we remark as illustrative, with regard to the military service, of the analogous exemptions conceded in civil matters, and alluded to in the latter part of a clause quoted by Dr. Simpson from the *Corpus Juris Civilis:*—"Cum te medicum legionis secundæ adjutricis esse dicas, munera civilia quamdiu Reipublicæ causa abfueris, suscipere non cogeris. Cum autem abesse desieris, si in eorum numero es, qui ad beneficia medicis concessa pertinent, ea immunitate uteris."† But we are able to adduce also from Gruter‡ a notice of another physician of a cohort, and of one who was not even a freedman, but apparently remained a slave. T. MEDICUS COH. participates in a votive inscription to the Genius of the Centuria; and, having only a single name, was doubtless still in the unemancipated condition. To this, analogous instances are easily encountered elsewhere. In truth, however humiliating the circumstance, and however much it has been controverted, everything unites to show that nearly all the physicians of this nation of soldiers were for long either slaves or freedmen, very rarely Quirites, and received the Roman citizenship only by concession. It appears even to have been only upon free physicians settling from a distance in Rome, that Cæsar proceeded at last to confer the right of citizenship; and such also were those whom Augustus relieved from the public burdens. As to those who remained slaves, they retained their money price in the time of Justinian, in whose code§ they are rated at sixty solidi.

We have seen that Dr. Simpson has cited proofs of the appointments of the ordinary and clinical physicians of the cohort, of the physician of the legion and of the trireme; and that he has not neglected to note the *emeritus* dignity of the *medicus duplarius.* With these he seems to consider that he has exhausted the whole readily accessible sources of information regarding the grades and varieties of medical rank in the Roman service; while he has elicited no reason to conclude that more than two medical officers were attached to the cohort. But,

* Corpus Inscriptionum, p. 269, No. 3.

† Codex, lib. x. tit. LII. lex 1.

‡ Corpus Inscriptionum, p. 108, No. 4.

§ De Comm. Serv. Manumiss., vii. 7, c. 1.

in an inscription discovered at Lyons, we find Bononius Gordus, medicus *Castrensis*, commemorated as joining certain co-heirs in erecting a monument;* while on a votive tablet† to the godhead and majesty of Marcus Aurelius, we remark, along with the names of other officers, those of C. Runnius Hilaris, C. Julius Hermes, Q. Fabius Pollux, and S. Lutatius Ecarpus, as "medici" of the fifth cohort of the watch. We are thus enabled to offer two additional facts: the existence of a functionary designated as camp physician, and the apportionment of at least four medical officers to a single cohort. Thus the Roman medical staff was more extensive and complicated than Dr. Simpson's researches, or indeed those of the other medical archæologists to whom we have referred, have succeeded in depicting. We have also shown, by an effort to assign a more precise value to the intrinsic evidence afforded by certain of the inscriptions, which all had resorted to, that its organization is of a higher antiquity; and we have no doubt that an inquiry less fortuitous than ours, and more rich in opportunities, might still easily add many material contributions. There is a suggestive passage in the account by Velleius Paterculus of the expedition into Germany under Tiberius, and therefore relating to a time close to the dawn of the Empire, where he describes‡ the provision of physicians, and of other requisites for the health of the army, as in such profusion, that only home and domestics were wanting.

We had marked a few other points in which it appeared to us that the author had approached his conclusions without complete success, or without a due amount of caution. Our object, however, is not hypercriticism, but to contribute cognate or essential facts. With this view, before finally leaving the question of the completeness of the supply of a medical staff in the Roman armies, or that of the position of the officials which we have sought to blend with it, we may advert to a kind of collateral evidence to be obtained in the analogous provision of physicians for the civil community (*archiatri populares*), appointed in Rome for each of its divisions,§ and evidently chiefly for the service of the poor. By a rescript of Antoninus Pius, and therefore in the prior half of the second century, which we find included in the Digest‖ on the authority of Modestinus, it appears that it was ordained that each of the minor cities also should be entitled to have five, the larger cities seven, and those of the principal class ten physicians, with certain privileges and immunities. We may reasonably infer, therefore, that the arrangements were equally complete in both departments.

It would have gratified us to have followed our author more closely into more than one of the several little digressions incidental to his essay, all of which are full of interest and suggestion. We may, for instance, diverge to express our surprise at his statement (note, p. 23), that of the almost innumerable Roman monumental inscriptions that have now been copied and published, not one, in as far as he is aware, ventures to refer to the hope of a future life; and that there is, as he

* Orellius et Henzen, Inscript. Latin. Select. Collectio. Turici, 1828-56, No. 6808.
† Ibid., No. 6791. ‡ C. Velleius Paterculus, Histor. Roman., lib. II. c. 114.
§ Codex Theodosianus, lib. XIII. tit. III. lex 8.
‖ Justiniani Pandectæ, lib. XXVII. tit. I. lex. 7, § 2.

has previously somewhat qualified the expression, something strange and interesting in their total, or almost total, silence as to a future state, and the possibility of meeting beyond the grave. If this be said with any reference to such hopes as those which may be justly founded on the positive promises of the Christian revelation, we can only remark that the relative conditions do not admit of any comparison. But if it be said, as it is really said, at least in part, of a period when the great and precious doctrine of the immortality of the soul was not even propounded in the Mosaic writings; and when the Sadducees, a powerful, if not sometimes a predominating, sect among the ancient Jews, denied its truth while professing to be guided by their authority, we think the wonder is rather that the Romans, with the Greeks, should have believed so much. Some of the best of the philosophers inculcated the doctrine of an immortal existence, and of a future state of rewards and punishments; and with the mass of the people, as with the poets, the faith was all but universal. Surely the DIS MANIBVS, as a part of every sepulchral inscription, was itself a sufficient proof of the prevailing belief; while instances of its more direct acknowledgment are by no means so rare as Dr. Simpson has imagined. *Ut et ego possim dulcius et celerius apud eum pervenire* is a portion of an inscription given by Orellius: *præcedere voluisti sanctissima conjux*, in another from Fabretti, now before us, surely implies a hope that the mourner was to follow to where his wife had preceded him: *te jam jamque videbo cum vita functus jungar tuis umbra figuris* is an exclamation of an aged parent over an adopted child, again from Orellius: and often the person interred is feigned to speak also in the epitaph, in terms which infer the notion of an enduring state of consciousness and susceptibility to affection, and which, for their elegance and tenderness, might be advantageously offered for imitation in many of our cemeteries. What, for instance, can be more gracefully simple than the following, recorded by Maffei?

QVANDOCVMQVE LEVIS TELLVS MEA CONTEGET OSSA
INCISVM ET DVRO NOMEN ERIT LAPIDE
SI QVA TIBI FVERIT FATORVM CVRA MEORVM
NE GRAVE SIT TVMVLVM VISERE SAEPE MEVM
ET QVICVMQVE TVIS HVMOR LABETVR OCELLIS
PROTINVS INDE MEOS DEFLVAT IN CINERES.*

A proper conclusion to these observations, and one which we shall ourselves supply, would be an indication of the period at which, after the darkness of the Middle Ages, the medical arrangements of an European army began again to assume a regular form, and to advance towards that comparatively perfect system under which they now subsist. Hyginus represents the Valetudinaria of the Roman legion as consisting of tents regularly disposed in the centre of the camp, and describes them as

* Though long unused to verse, we are tempted to offer a translation, making no other pretensions than to fidelity:

When the light earth is o'er me thrown,
And my name marks the sullen stone,
If dear to thee my life or doom,
Spare not to visit oft my tomb,
And every tear-drop from thine eye
Shall glide to where my ashes lie.

extending to the length of seventy Roman feet. We have seen that long afterwards, in the time of Vegetius, or towards the close of the fourth century, their customary employment, as was to have been anticipated, is still obviously indicated. With the utter ruin of the Roman power and discipline, however, an institution which, with its associated functionaries, depended on the vigour of a comparatively ripe civilization, would naturally perish, and for a lapse of ages we lose its traces. Its earliest revival is claimed by the Spanish historians for the energetic reign of Ferdinand and Isabella. Thus we learn from the contemporary authority of Hernando del Pulgar, the able secretary and chronicler of the Catholic sovereigns, that a field-hospital, with its appropriate staff, was first provided by the Queen herself; probably prior to the year 1484, but, at all events, at the siege of Alora in that year. "For the cure of the sick and wounded," says Pulgar, "the Queen sent always to the camp six large tents, with bedding, and the requisite furnishings, and provided physicians and surgeons, and a supply of medicines, with attendants; and commanded that they should make no demand for their services, as she would sustain the entire charges. These tents, with their whole appointments, were called in the camp the Queen's Hospital."* We have a somewhat later testimony to the same effect, in a letter from Pietro Martyr to the Cardinal Archbishop of Milan, dated at the camp before Bacia (Baça or Baza, in Granada), in 1489.† The Italian here speaks of the arrangement as a "providum Reginæ pietatis inventum;" and describes the number, order, and diligence of the officials, and the completeness of the equipments, as highly admirable, and not to be surpassed in the best hospitals of Milan. "The royal liberality," he adds, "provides a remedy for whatever sickness or casualty arises, unless where the day appointed for all is at hand."

The son of the Emperor Maximilian I. of Germany having married the daughter of Ferdinand and Isabella, it is easy to see how this precious offshoot of the Spanish discipline became an object of imitation in the German armies; and that there is injustice in the pretensions of those native writers who claim for Germany the merit of first reproducing the conception, though it appears to have been there materially modified and extended. It was in the beginning of the sixteenth century that Georg von Frundsberg, accounted the bravest soldier, as well as one of the most distinguished leaders, of the Imperial army, in organizing his celebrated lansquenets, required a surgeon and his assistant to be appointed for every 400 men; not as a discretional act of grace, as it appears had frequently happened in portions of the troops before, but as a fixed and peremptory arrangement. The instructions, in their antiquated German, hint at what had been the fate of many a gallant soldier formerly, from want of a like benevolent provision:—"Dieweil man unter eines jeden Fendlin eines Feldscherers und Wundarztes nottürftig ist, so soll ein jeder Hauptmann sehn, dass er ihme einen rechtschaffnen, kunstreichen, erfahrnen und

* Crónica de los Señores Reyes Católicos Don Fernando y Doña Isabel de Castilla y de Aragon, P. III., c. xxxiii.: *Como el Rey tomó la villa de Alora.*

† Petri Martyris Anglerii, Opus Epistolarum, lib. II., Ep. 73.

wohlgeübten Mann zu einem Feldscherer und Baderknecht bestelle; wie umb gunst's willen zum öfftern mal geschicht; dann wahrlich ein gross hieran gelegen; dann mancher ehrlicher Gesell etwan sterben oder erlamen muss, hette er ein rechtschaffnen, erfahrnen und geübten Meister ob ihme, er bliebe bei leben und gerad."*

The German military surgeon was thus the barber-surgeon of the period, for no other was attainable; and we learn that his pay was exceedingly humble, amounting apparently to only four guldens a month above that of the common soldier. While they were required, however, for this amount of remuneration, to be constantly in attendance on the troops, with the requisite supply of medicines and instruments placed at their disposal, and to administer to every sickness and casualty actually the product of the service, they were entitled to demand additional recompence for all beyond this, and had besides a few perquisites. The plan devised by Frundsberg for his lansquenets was extended to the rest of the troops. A body of a more highly-educated class of medical officers or of physicians was appointed, to whom was confided the superintendence of the surgeons, the general sanitary arrangements for the camp and army, and the direction of the more difficult operations in surgery or more serious emergencies of disease. At the head of the entire staff was a physician-general, with a monthly allowance of forty guldens, subordinate only to the commander-in-chief. We have thus here a very extensive organization, evidently differing more, as to that which subsists now, in the quality of its parts, than in the comprehensiveness of its design. Possibly, Von Frundsberg, its meritorious institutor, is better known to many of our readers from his address to Luther when before Charles V. at the Diet of Worms. Striking the Reformer on the shoulder, he said to him: "Good monk, good monk! thou art now on a course such as I and many a commander have never followed in the toughest battle. But if thou be honest in thy intentions, and sure of thy doctrine, go on, in God's name, and take comfort: God will not forsake thee!"

When we arrive at the time of even a still greater tactician and disciplinarian than those who formed the Spanish infantry or the German lansquenets, or at what may be termed the third great epoch of the science of modern warfare, we find proofs that the new arrangements had diffused themselves with exceeding tardiness through the armies of different states. It was only under Gustavus Adolphus, or after 1611, that a complete medical staff was first established in the army of Sweden, for long thenceforward to be the model army of Europe. Under this monarch, a regiment of foot, of about 1200 men, was appointed to have four permanent surgeons, of whom one was designated as chief or regimental surgeon, and the three others were named company surgeons. In the cavalry, each company, usually of about 125 men, had its surgeon. The whole of these were of the class of barber-surgeons; but they were superintended, throughout the forces, by a number of field-physicians, as in the German army. It is evident that Gustavus provided also military hospitals, because, in an ordinance

* Zimmermann, Hecker's Annalen, Nov. and Dec. 1834: Medicinische Unterhaltungs-Bibliothek, Band iv. p. 189.

issued by him in 1621, he directed that a tenth of the booty of the troops should be reserved in future for behoof of the hospital for sick soldiers.*

We trust, in conclusion, that this may not be the last of its description of erudite disquisition, for which we are to be indebted to the pen of Dr. Simpson. There are still many among the physicians of this country, though more, we fear, among those of the Continent, who, like ourselves, will gladly follow him through his researches, as interested and heedful commentators, and will sympathize with any attempt to show how gracefully, and with how delicate a solace, the pleasures of literature may be joined to the austerities of science. We must stipulate, however, for a union; and not for a surrender of even a tittle of the solid qualities of the one to the easy fascinations of the other.

Review V.

Medico-Chirurgical Transactions. Published by the Royal Medical and Chirurgical Society of London. Vol. XXXIX. 1856. 8vo, pp. 360.

This volume is got up in the usual good style of the Society's Transactions, and is copiously illustrated by excellent lithographs. It contains twenty-one papers, some of which consist of the detailed histories of isolated cases remarkable for their great interest or singularity, while others are contributions of no small merit to the science and practice of medicine. We proceed at once to give a brief analysis of some of the more important of these papers. The first is entitled—

I. *On the Action of Digitalis upon the Uterus.* By W. Howship Dickinson.—The object of this memoir is to show that digitalis administered internally excites muscular contractions of the uterine walls, which are quite independent of any change in the heart's action or general circulation. This conclusion is arrived at from observing the effects of the drug in cases of menorrhagia and on parturition. In every case of menorrhagia admitted into the Burton Ward of St. George's Hospital during the period of one year, and requiring active remedies, the administration of digitalis is stated to have constituted the sole treatment, and in every case the result was "a speedy cessation of the discharge,"—in one case, after the failure of all the ordinary means. The mode of administration was from one half to one and a half ounce of the infusion three times a day. In several cases, each dose was found to be followed by uterine pains resembling those of labour, the expulsion of a clot, and temporary cessation of the discharge, these periods of cessation increasing with each dose. Again, several cases are recorded in which severe "after-pains" appear to have been induced by the administration of the drug soon after delivery, and one, in which labour was brought on by it. The author's observations are of much interest, and the subject is one which deserves further investigation. Thus, experiments might be instituted to test

* A. H. Wistrand, Kort framställning af Stats-Medicinens uppkomst och utveckling inom Sverige: Hygiea, Band xii. p. 389.

the value of digitalis as a substitute for ergot of rye, Indian hemp, &c., in cases of prolonged labour, depending upon atony of the uterine walls. At the same time, we are not a little surprised, that the author should have observed so little effect upon the pulse or heart's action, from the use of the medicine.

II. *History of a Case in which a Cedar Pencil was lodged in the Cavity of the Abdomen for Eight Months.* By JOHN ERICHSEN.—The pencil, which was five and a half inches in length, had been employed by the patient to relieve a difficulty in micturition, but from some interruption, had slipped out of her hand, and upon her sitting down, had been forced through the posterior and upper part of the vagina, and become lodged in the abdominal cavity, perforating two coils of the small intestine. In this position it remained for eight months, producing intense pain and repeated attacks of peritonitis. The point of the pencil could be felt, midway between the umbilicus and the right Poupart's ligament. An incision was made down upon this point, and the body removed; but the patient died on the fourth day of peritonitis. This case resembles in many respects one recorded in the thirty-first volume of the Society's Transactions (page 315), by Dr. C. W. Lever and Mr. J. Hilton.

III. Mr. NATHANIEL WARD records a case of suicidal incised *Wound of the Abdominal Parietes*, through which there was protrusion of the omentum and transverse colon, and division of the latter to the extent of four-fifths of its circumference. The edges of the wounded bowel were brought into accurate approximation by an uninterrupted suture, and the operation was followed by the administration of opium in large and repeated doses. Perfect recovery took place, the abdominal wound having healed on the thirty-second day after its infliction. The patient was a female lunatic, aged fifty-one.

IV. Mr. T. P. TEALE, of Leeds, reports a *Case of Detached Piece of Articular Cartilage, existing as a loose Substance in the Knee-Joint.*—The patient was a man, aged thirty-seven, in whom, from an injury of the knee, a portion of the articular cartilage from the lower end of the femur, about an inch in diameter, became detached as a loose body. Fourteen months after the accident, this loose body was removed by direct incision. The operation proved fatal, and there was found after death, on the under surface of the inner condyle, a depression, exactly corresponding to the body which had been removed.

V. *Hourly Pulsation and Respiration in Health.* By EDWARD SMITH, M.D.—In this paper, Dr. Smith gives the result of an elaborate series of experiments, made upon himself, aged thirty-six, and upon four female members of his own family, of the respective ages of thirty-nine, thirty-three and three quarters, eight and a half, and six, in order to ascertain the absolute and relative frequency of the pulse and respiration at different hours of the day and night, and the influence exerted upon them by food, fasting, &c. The investigation was undertaken with the object of obtaining a basis of comparison, for a similar inquiry in reference to phthisis pulmonalis. The results of the author's observations are exhibited in eleven tables, and in two diagrams remarkable for their ingenuity. The principal results were as follow:—Both respirations and pulsations, but especially the latter, were found to be higher in the day than in the night time; but the two functions did not correspond with each other in reference to their range. "That of pulsation increased through the 'night' (one to five A.M.), 'evening' (nine P.M. to one A.M.), 'morning' (five to ten A.M.), and 'day' (nine A.M. to nine P.M.), in their order; whilst the order of increase in reference to respiration was 'night,' 'day,' 'evening,' and 'morning.'" There were also elevations and depressions dependent upon the meals. Thus, after each meal there was found to be an increase of the pulsations for a period of from one to three hours, this increase varying with each meal; the average after breakfast being 15, after dinner 12, and after tea 6. The same remark applied to the respirations; the average increase after breakfast being 4·4, and after dinner and tea 2·1. The most *enduring* influence, however, was exerted by dinner. Prolonged fasting was found to lower the pulsations and also the respirations, but the latter to a less degree. The ratio of the respiration to the pulsation was found to be very variable, the lowest being observed in the youngest, and during sleep (1 to 5·7), and the highest in the oldest when awake (1 to 2·9). The author draws the inference from his observations, that although "neither function is necessarily dependent for its rate upon the other, it is evident that the circulation is greatly controlled by the respiration."

There is one source of fallacy in the observations upon the respirations, which appears to have been lost sight of—viz., that the mere direction of the attention to it will increase its frequency and amount.*

VI. *On Atrophy of the Brain, with Cases in which there were remarkable Inequalities of the Cerebral Hemispheres, attended with Hemiplegia and Contraction of the Limbs, on the side opposite the Atrophied Hemispheres.* By P. BOYD, M.D., Physician to the Somerset County Lunatic Asylum.—In atrophy of the brain from any cause, the atrophy is often unequal on the two sides, one hemisphere being much smaller than the other. According to Dr. Boyd's experience, this unequal atrophy "is at least twice as frequent in the insane, and males of this class are very much more subject to it than females; and it is also very common in epileptics." In one of the cases recorded, the difference in weight between the two hemispheres amounted to six ounces. In this, and several other cases, there had been epilepsy, and hemiplegia on the side opposite to the greatest atrophy, with shrivelling and rigidity of the paralysed muscles. It would have been interesting to have known the period at which this rigidity commenced.

VII. *Observations on Congenital Deficiency of the Palate, and the Means to be used for its Relief.* By GEORGE POLLOCK, &c.—The object of this paper is to show that the operation first practised by

* See Carpenter's Principles of Human Physiology, fifth edition, p. 270.

Dr. Warren of Boston, for the closure of the fissure in the hard palate—viz., by dissecting the soft tissues from off the vault of the mouth on either side, and bringing together their raw edges, "may be undertaken with confidence, and that we may occasionally expect a large share of success to reward our efforts." Three successful cases of the operation are recorded, and numerous judicious instructions are given for its performance.

VIII. *Case of Destruction of the entire Palate, successfully relieved by Mechanical Means.* By Edwin Sercombe, M.R.C.S.

IX. *Cases illustrative of the Pathology of the Ear.* By James Hinton, M.R.C.S.—Of 56 ears taken indiscriminately from persons of all ages, 12 only, or 23 per cent., were healthy; and of the remaining 44, 7 belonged to persons known to be deaf, 9 to persons known not to be deaf, while in 28 the history was unknown. Notes are given of a few of the more interesting cases, and the morbid lesions observed in all are arranged in a tabular form.

X. *On Myeloid and Myelo-Cystic Tumours of Bone; their Structure, Pathology, and mode of Diagnosis.* By Henry Gray, F.R.S.—This is a very valuable contribution to our knowledge of a class of tumours which have but recently received attention. These tumours were first distinguished by Lebert, and by him named *fibro-plastic*, and more lately they have been described by Mr. Paget under the title of *myeloid*, from the resemblance of their microscopic constituents to those of the marrow and diploe of the bones, especially in the fœtus and in early life.

Mr. Gray publishes the details of nine cases of this disease, and gives the following summary result of his observations :—

"1st. That the essential element consists, in all cases, of forms precisely similar to what is found in the marrow and other elements of bone, in the fœtus, and at an early period of life; hence the name 'myeloid' tumours. In some instances, however, their structure is so intermixed with cysts, that I would propose the term 'myelo-cystic' tumours to be given to them in such cases.

"2ndly. That these tumours are, for the most part, limited in their development and growth to the osseous tissue, or its investing membranes, the periosteum, and dura mater.

"3rdly. That they may probably occur in any bone.

"4thly. That they occur in all the cases at present recorded at an early period of life, and that their growth is generally much less rapid than that of malignant disease.

"5thly. That these tumours are not malignant, and when entirely removed, never return.

"6thly. That they present a near relation with fibrous and fibro-cystic tumours, and cartilaginous and osseous." (pp. 124-5.)

Perhaps the most important among these considerations is the question of the malignancy of such tumours. Mr. Gray grounds his belief of their non-malignancy upon their peculiar structure, distinct from cancer, their slow growth, the complete absence of any affection of the lymphatic glands, and of the malignant cachexia; and upon the fact that the removal of the limb, although in many of these cases *through* the bone in which the disease had originated, was not followed by a return of the disease after many years. It is but right to state, however, that on this matter Mr. Gray differs somewhat from Mr. Paget. The latter, although he admitted that these tumours are "generally of innocent nature," yet believed that in different persons they might pursue "very different courses, appearing in some as an innocent, in others as a malignant disease."* Two cases are recorded by Mr. Paget, presenting the myeloid structure (and neither of which, by the way, originated in bones), in one of which the tumour suppurated, the glands became affected, and the disease returned six months after removal; while in the other, a myeloid tumour of the neck, similar deposits were found after death, in one of the cervical glands and in the lungs.

XI. *Rate of Hourly Pulsation and Respiration in Phthisis, and its Relations to Sleep, Food, Sunlight, &c.* By EDWARD SMITH, M.D. —This is the sequel to Dr. Smith's former paper, to which we have already alluded (No. V.). We have recently (April, 1856) devoted a considerable space to an original communication by Dr. Smith, "On the Rate of Pulsation and Respiration in Phthisis, and its Relation to the Period of the Day, Posture, Temperature, &c.," and we regret that our limits will not allow us to enter into detail respecting the points in the paper now before us. We would merely mention that one of the greatest deviations from the normal condition of these functions, the existence of which in phthisis has been ascertained by Dr. Smith's investigations, consists in the greater frequency of the respiration during the night than during the day, the rate of pulsation, however, being, as in health, the greatest in the day-time. This paper is also illustrated by a number of ingenious diagrams.

XII. *Cases of Paraplegia associated with Gonorrhœa and Stricture of the Urethra.* By WILLIAM GULL, M.D.—Three of the four cases recorded proved fatal, and there was found to be softening of the cord; and hence the author is led to throw doubt upon the views formerly expressed by Mr. Stanley, that in such cases, paraplegia may result without organic lesion of the cord, from a morbid impression being conveyed to it from the kidneys, and reflected outwards to the extremities.

XIII. *Case of a Young Woman, in whom the main Arteries of both Upper Extremities, and of the Left Side of the Neck, were throughout completely obliterated.* By WILLIAM S. SAVORY, F.R.C.S.—This case is of extreme interest, and is very carefully reported. The patient was a female, aged twenty-two, who had been out of health from her sixth year. She had suffered from vague flying pains, chlorosis, and chorea; and when admitted as an out-patient of St. Bartholomew's, "no pulse could be distinguished in any of the vessels of the head, neck, or upper

* Lectures on Surgical Pathology, vol. ii. pp. 228.

extremities. The femorals and the vessels of the lower extremities pulsated, but not strongly. A bruit could be detected in the right common carotid. A shrill bruit, almost amounting to a soft whistle, could be heard at the top of the sternum: beyond this, no morbid sound was detected." At the post-mortem it was found that "about an inch or less from their origin, the right subclavian, the left carotid, and the left subclavian arteries became suddenly contracted to one-fourth or one-fifth of their natural size. This change extended throughout these vessels, through the axillary, brachial, radial, and ulnar arteries on both sides, and the left external carotid artery. The contracted canal in their interior was completely blocked up and obliterated by a fibrous cord, which extended, with scarcely any interruption, throughout their entire length." Among other important points may be mentioned the ulceration on the left side of the head, involving successively integuments, bone, and brain, ulceration of the left cornea, &c., in connexion with the obliteration of the left carotid artery.

XIV. *Analysis of Cases of Amputation of the Limbs in the Radcliffe Infirmary, Oxford.* By E. L. Hussey.—This paper gives in a tabulated form the results of all amputations of the limbs at the Radcliffe Infirmary, from the year 1838 down to the present time, and also of all the cases in which primary amputation has been performed since 1810. The paper contains many facts of considerable value to those interested in surgical statistics. The author insists, and with justice, that in arranging cases of amputation for purposes of comparison, due regard must be had to the cause of amputation. A striking illustration of the propriety of this injunction occurs in the paper, in the case of amputation of the thigh. Of 57 cases in which this operation was performed for disease of the knee, there were only 6 deaths (or 1 in $9\frac{1}{2}$) from the immediate effects of the operation; whereas of 6 cases of primary amputation of the thigh on account of injury, 5 proved fatal within little more than three weeks.

XV. *On the Structure and Nature of the so-called Colloid Cancer.* By Septimus W. Sibley.—There is much that is interesting in Mr. Sibley's memoir, yet we regret that on some points we are compelled to differ from him.

Mr. Sibley makes the statement, upon which he lays great stress, that the stroma of colloid, in its most characteristic form, assumes the shape of a convoluted membrane, and that the fibrous appearance which may sometimes be observed, is due either to a folding and stretching of the membrane, or to the membrane having split up into fibres in a direction corresponding to the folds. In a strictly histological sense, a *membrane* is perfectly homogeneous and structureless, and the term is limited to the wall of a primary organic cell, or to an aggregation of the walls of such cells. Do we find a membrane conforming to such a definition in the stroma of colloid? Our own observations negative such a belief, and even Mr. Sibley's figures and description are at variance with it. The occasional presence of nuclei scattered throughout these fibres, as pointed out by Mr. Paget (although denied by Mr. Sibley), would seem to contra-indicate the opinion expressed by the latter as to the development of the stroma from the original colloid cells.

The "colloid bodies" which fill up the loculi in the stroma are described as consisting of two parts—a central portion, or "kernel," composed of one or more nucleated cells, and a surrounding gelatinous substance, apparently deposited in concentric layers.

The author records 9 cases of the disease which he has observed, and draws the conclusion that "colloid is a disease perfectly *sui generis*," distinct from cancer, and having nothing malignant in its results, beyond that induced by the presence of a serious local disease. These conclusions, it is almost needless to observe, are opposed to those arrived at by Lebert, Paget, and others—viz., that there are many cases of colloid which, in their clinical history, exhibit "all the distinctive features" of true cancer. The number of well recorded cases, however, is yet but small, and those of Mr. Sibley constitute a valuable contribution towards the clearing up of this important subject.

XVI. *Case of Fracture of Four Cervical Vertebræ, with Dislocation, produced by slight and unusual Cause, and resulting in immediate Death; with Notes of a Case of Fracture of the Os Calcis.* By GEORGE GREEN GASCOYEN, M.R.C.S.—The fracture in the first case is particularly worthy of notice, from the peculiar mode of its production—viz., by a man seizing the deceased's hat by the brim, and forcibly twisting his head from side to side several times, and then pushing him back upon a form in the sitting posture. "His head then dropped forwards," he slipped off the seat, and in a few minutes exhibited no signs of life.

XVII. *Two Cases of Extensive Absorption of the Bones of the Head, followed in one of them by Hernia Cerebri.* By CÆSAR H. HAWKINS, President of the Society.

XVIII. *Two Cases of Malformation of the Heart.* By H. HANNOTTE VERNON, M.D.—In one of these interesting cases, the heart was obtained from the body of a child, who died cyanotic four hours and a half after birth. The foramen ovale was closed, but the tricuspid valve was incompetent, and the septum ventriculorum imperfect, as a consequence of which the aorta had a common origin from both ventricles. The pulmonary artery arose from the right ventricle, and had no communication nor connexion with the aorta.

The second case was that of an infant, who died suddenly with dyspnœa, on the eighth day after birth, and who had appeared perfectly well until within sixteen hours of death. The auricular septum was found to be incomplete, with not the slightest vestige of a septum between the ventricles. From the common ventricle a single vessel arose, which gave off, first a right and left pulmonary artery, and then a third vessel, which divided into innominata, left carotid, and subclavian arteries. A common coronary artery passed down from the innominata.

XIX. *On Encephalocele; being the History of a Case, with a Tabular Analysis of Seventy-five Cases.* By JOHN Z. LAWRENCE, F.R.C.S.—The case recorded was that of a female child, who survived her birth five months, with a congenital tumour on the back of her neck, almost as large as her head. After death, this tumour was found to contain the whole of the cerebellum and part of the cerebrum, which was continuous with the intra-cranial portion of the encephalon. Seventy-five similar cases have been collected by the great industry of the author. Of these 75 cases, 53 were in the occipital region, and in 16 there was also spina bifida. In 5 only of 44 cases was the brain healthy, there being in the majority, atrophy, softening, or ventricular effusion. The great majority of cases were either born dead, or did not long survive birth. Only 6 cases on record have lived for some years.

XX. *An Account of the Arrangement of the Muscular Substance in the Urinary and certain of the Generative Organs of the Human Body.* By GEORGE VINER ELLIS.—This paper is one of much value, containing the author's careful description of his own dissections and observations. Our space will not permit us to do more than make a passing allusion to some of the facts mentioned in it.

The prostate is described as consisting for the most part of circular muscular fibres, continuous with those of the bladder, and hence "the propriety of calling that body a gland is rendered doubtful." A submucous stratum of longitudinal muscular fibres (but no circular ones) is described as surrounding the urethra.

The vesiculæ seminales are enveloped in a longitudinal and transverse layer of muscular fibres, which, from their office, might be named "*compressor vesiculæ et ductûs seminis.*"

The corpora cavernosa of the penis have a double sheath, composed of longitudinal and circular fibres; the corpus spongiosum, a single sheath, composed of circular fibres only.

XXI. *On Mercurial Fumigation in the Treatment of Syphilis.* By HENRY LEE.—The object of this communication is to record a plan of fumigation adopted by the author, which he has found very effectual in bringing the system under the influence of mercury, without any disagreeable effects. The peculiarity of the plan consists in the use of calomel as a substitute for the mercurial preparations more ordinarily employed, and in combining the fumes with the vapour of water. We can have little doubt as to the efficacy of such a plan in inducing mercurial action, but whether we are warranted in adopting such a procedure in all cases of secondary and tertiary syphilis, as recommended by Mr. Lee, we think very questionable.

Review VI.

Lehrbuch der Physiologie des Menschen. Von C. Ludwig, Professor an der Josephs-Akademie in Wien. Vol. i. pp. 458, vol. ii. pp. 501. —*Leipzig* und *Heidelberg.* 1856.
Text-book of Human Physiology. By Professor Ludwig. 1856.

The diversity of titles under which the works on physiology at the present day are brought before the scientific reader, appears not a little remarkable. We have books purporting to treat of Human Physiology, Comparative Physiology, Vegetable Physiology, Chemical Physiology, Experimental Physiology, in fact, of every imaginable kind of Physiology; and if we analyse these various works, what do we find? Simply this, that each author in his particular department has attempted to explain the fundamental laws governing organic life.

The term Physiology had formerly a far wider application than we moderns are in the habit of giving to it. It was at one time considered to include Natural Philosophy, and to comprehend within its range every discovery which in any way elucidated the nature and properties of animate and inanimate existence. It was the science which explained cause and effect, phenomena and laws inherent in every material object. A treatise on the nature and functions of the different parts of which animals and plants are composed, and, at the same time, a commentary on the philosophy of the complex affections and powers of the mind; it may, in fact, be not inaptly defined as a union of the two sciences which we are now accustomed to regard separately under the respective titles of Physic and Physiology. In later times, the science of Physics has been understood to signify more especially a knowledge of the laws which govern the inorganic world, while that of Physiology has been restricted to the study of the functions of the different parts and organs composing each individual animal and vegetable—in a word, the phenomena of life.

The science of nature is thus seen to have passed through two great eras, forming during the first a single object of study, but divided during the second into distinct and separate branches. A third great era seems even now to be casting its shadow before, as it is the custom of coming events to do. It appears not improbable that in the advent of this new era, physiology and physics, reunited into one science, will once more form a single object of study to the philosopher. In proportion as our knowledge advances, the more visibly can we trace the intimate relation existing between the animal and vegetable kingdoms. We may even add, that day by day science is leading us gradually on to the discovery that the organic and inorganic form but one world, and that the laws governing the one, are equally effectual in controlling the other. To such men as Faraday, in his 'Experimental Physics;' Helmholtz, in his 'Researches on Electricity,' &c.; Grove, in his 'Correlation of the Physical Forces;' Carpenter, in his able Essay 'On the Mutual Relations of the Vital and Physical Forces;' and, lastly, "Nomos," in his search after a central law in nature, are we at present chiefly indebted for the lenses with which we peer into the darkness in which the workings of nature are shrouded.

The views of Professor Carpenter on this subject, as expressed in his paper 'On the Mutual Relations of the Vital and Physical Forces,' may not be inappropriately quoted here.

> "There can be no doubt" (he says) "that the present tendency of scientific investigation is to show a much more intimate relation than has been commonly supposed to exist between the *vital* and *physical* agencies; and to prove that, whilst the former are of a nature altogether peculiar, they are yet dependent on conditions supplied by the latter. And the more closely these phenomena are investigated, the more intimate and uniform does that dependence appear; so that we seem to have the general conclusion almost forced upon us, that the vital forces of various kinds have the same relation to the several *physical* forces of the inorganic world that they have to each other."

Many physiologists of high standing are even inclined to go a step further than the author just quoted has done. The discoveries made in physiology, and the other experimental sciences, during the last few years, have already, they think, gone far to prove that the phenomena of life both originate in, and are dependent on, the laws which regulate the inorganic universe. Day by day it would seem that the theory which attributes every phenomenon of life to a purely vital cause, ignoring entirely the influence, and even the existence, of physical force in the manifestations of animal life, is becoming less and less secure. In the present state of science, the existence of a vital principle ought not to be dogmatically denied; for unless we admit the presence of an inherent power in animals, different from all the physical forces with which we are as yet acquainted, it becomes perfectly impossible to explain how the primary cell of one animal which to appearance has a perfect identity with that of another animal, in the one case develops itself into a being with all the attributes of a man; while, in the other case, notwithstanding that it has received as nourishment the same inorganic elements and organic compounds, its development stops short at that of a lower animal. How is it, we say, that in the same species one cell is ultimately developed into a male, and another cell, placed, as in the case of twins, in the very same conditions in respect to time, place, and nourishment, becomes a female; the former possessing all the physical and mental characteristics of the father; the latter the various hereditary endowments of the maternal parent? This cannot be explained as yet on any other supposition than that there exists a force in nature which we have failed to define. But while asserting this, we must not attempt to ignore the fact, that the territory held by the so-called vitalists is gradually narrowing in extent, while the domain of those who have been censured for supporting the chemico-physical doctrine of life, proportionally widens on every side. The book which we are about to introduce to our readers contains ample evidence of the correctness of this statement, and no one can rise from an attentive perusal of Ludwig's 'Textbook of Human Physiology,' without being forced to acknowledge that it contains many facts tending to show that physiology, without impropriety, may be called the chemistry and physics of life. The author has indeed limited himself almost entirely to the consideration of the subject in a purely physical point of view; yet throughout the work there is sufficient to prove to every reasoning mind, that chemical agencies are of equal, if not of superior, importance to physical forces, for the development of every animal function.

In the preface, the author remarks that pathologists are often complaining of the unpractical direction of physical physiology, forgetting altogether that physiology can never be of much service to pathology, until it has penetrated those mysterious primary conditions from which healthy life emanates. When this important desideratum has been obtained, the medical practitioner will be able to recognise the boundaries of health and disease. And when he has analysed the diseased process, as well as the physiology of the healthy one, he will understand what conditions require to be changed in order to reproduce in a diseased part a normal healthy action. The complaints of the pathologist are as groundless as those made by the practical mechanic and chemist against what they considered the unpractical direction of the researches of the theoretical physicist and chemist. The pathologist, too, we think, cannot fail to see that neither his researches in the deadhouse, nor his microscopic investigation of diseased tissue, can alone lead to a true elucidation of the nature of diseased action. For such information he must search in the laboratory of the physiologist. There, and there only, must the fundamental laws of diseased, as well as of healthy action, be studied. Already is the working of these laws to some extent manifest, and even the laws themselves are to a certain measure appreciated; but a long time, we fear, may yet elapse before these laws will be completely understood. Physiology, although a special science, has yet its branches ramifying into all the inductive sciences,—its advance, consequently, keeps pace with that of those sciences, proceeding as they do, sometimes at a quicker, sometimes at a slower rate. It can never outstrip its companions, and it is still less reasonable to expect pathology to place itself in the van; the progress of pathology being even more dependent than that of physiology on the march of the collateral sciences, its speed would perhaps increase were its votaries more minutely to copy the methods of research adopted by philosophers engaged in the sister sciences.

Throughout the work under review there is a want of minute anatomical detail, — a want which we are unaccustomed to find in English text-books on physiology. But we can neither be surprised nor disappointed at this apparent omission when we consider the wide field the author has had to traverse, and the impossibility of giving in two volumes a complete description of general anatomy in addition to a treatise on physiology. Indeed, such a combination is quite unnecessary, and when it does occur, it is censured as superfluous by Continental physiologists. They consider, and we believe justly, that there exists as great a distinction between general anatomy and physiology as there is between the latter and chemistry. Although, as our author remarks, human physiology can in no case dispense with anatomy, yet still less is it possible for physiology to make use of any anatomical description, no matter how exact it may be. A text-book at least can only make use of those anatomical descriptions which are specially devised with regard to the wants of physiology, and that are so characteristic, that an idea of the functions can be formed from the description of the construction of the parts; or in other words, when a knowledge of the structure is sufficient to convey an idea of the duties to be performed. A description of this kind, however, is impracticable until science has so far advanced as to enable us to possess a definite idea of the function belonging to each peculiar form. The correctness of this statement will be denied by no one who knows the history of syndesmography and osteography, and the progressive descriptions which have brought these studies to their present physiological position. Although they had been cultivated for centuries, they were not made serviceable to physiology until the recent labours of the Brothers Weber. If, on the other hand, we look at the descriptions of the muscles and vessels as they are given in most of the text-books of anatomy, we shall at once see their utter uselessness, even in the hands of the most accomplished masters in mechanics and hydraulics. The former would be as much at a loss to determine how a known amount of power developed by the described muscles was employed in moving the limbs, as the latter would be puzzled to demonstrate by calculation based on the known volume and velocity of a stream at the entrance of the aorta, its action in its passage through the remote vessels. By an intimate comparison of the results already achieved by descriptive anatomy, with the amount of assistance it is expected to render to physiology, we at once see how limited is the parts of anatomical books really available to the physiologist.

The following is the order in which Professor Ludwig has arranged his subject. The first volume is devoted to a consideration of the elements and of the organic compounds composing the human body; then, of the nerves and muscles; and lastly, of the physiology of the senses. The second volume is a treatise on the development and decay of the tissues and fluids of the body (nutrition); the last chapters being devoted to the consideration of animal heat. As all text-books are to a certain extent compilations, it is sometimes no easy task for a reviewer to select for criticism the parts of the work which may appear most interesting to his readers; and the difficulties of the task are certainly not diminished when, as in the present case, the selection requires to be made from a mass of matter as overwhelming as the sources from which it is drawn are various. But as the value of a systematic treatise depends chiefly on the amount of care and discretion with which the author has extracted his materials from the journals and monographs on the subject treated of, and still more on the general conclusions drawn by him from the facts he has collected, we consider it more advisable to examine the deductions of the author, than to follow him out and criticise the sources from which he has culled his information. We are well aware of the difficulties with which authors of Text-books on Physiology have to contend, in consequence of the rapid progress of the science, and the immense quantity of ephemeral literature which burdens the mind with transient theories tending not unfrequently to obscure the very facts upon which they are based. We should therefore, in differing from our author, be cautious how we oppose our opinion to his; for it is safer to pass by in silence a doubtful theory, than rashly to raise a perhaps untenable objection. Hasty counter-generalization is at all times to be condemned; and never more so than in the consideration of physiological science, where every new experiment is apt to modify, if not to overturn, the usually received doctrines.

Before proceeding to discuss that portion of Ludwig's work devoted to the consideration of atoms, we think it advisable to mention what he says in his Introduction regarding the elementary conditions of the body, as it is strongly indicative of the physical tendency of his work.

"As often," he says, "as we analyse the organs of the animal body, we are forced to the conclusion that they are ultimately composed of a limited number of chemical atoms, associated with light, heat, and electricity. From this fact we draw the conclusion that all animal phenomena may be considered as the result of the simple attraction and repulsion which is observed in every elementary being. This theory will be undeniable if it is possible to show with mathematical exactitude that the elements in the animal body are so arranged as to time, place, and quantity, as to render it possible to deduce from their reaction all the functions of the living and dead organism."

This is, as our author himself remarks, not the view most commonly received on the subject; nevertheless, it is the doctrine held by those physiologists who are ranked as belonging to the new school, in contradistinction to those who still hold the old theories regarding the "living ether" of nerves, &c. This leads us to speak of what the author calls the Atomic Arrangement of the Animal Organism. To give the reader a clear idea of his views upon this point, it may be as well to translate a couple of the paragraphs occurring in the first chapter, as they may be considered to a certain extent a key to the remainder. The chapter begins with the statement, that after organic chemistry has shown the ponderable mass of the animal body to be a combination of individual atoms, it becomes the province of Physiology to discover what are the functions fulfilled by each of these more or less complex atoms in the living organism. This problem will be regarded as solved when we have fully determined the arrangement of the elements in each compound, the amount of latent heat, and the degree of affinity which every individual atom manifests towards all the rest in the animal body, under the there existing circumstances:

"*Rational Formula of the Arrangement of Atoms.*—It is known that by the action of heat, light, electricity, and other chemical agents, the chemical compounds in the body can be decomposed. From this decomposition, not elements themselves, but the 'immediate principles,' which contain a greater or lesser number of atoms, are generally obtained. From this circumstance, chemists conclude it to be possible that the complex organic combinations do not originate in the direct union of simple elements, but in the union of compounds, these compounds themselves being an aggregate of simple atoms. Mechanically explained, this means, that in each complex organic combination the simple elements attract each other with unequal force, and that a certain number of them having a greater attraction for each other, form themselves into compact groups, which groups again, by adhering together, form the complex compound. Each one of these individual groups of atoms would attract another similar group, as if it were an indivisible whole. We might add, as a supplementary remark, that as the compound is more easily broken up into the individual groups than these groups are separated into their ultimate elements, the attraction of the elements in each group must be stronger than the attraction of one group for another.

"This theory can be further explained by a diagram, wherein we represent the elements by points, and indicate the degrees of their reciprocal attraction by the space between each point, as is seen in the accompanying figure,

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science is far from being able accurately to define the exact conditions on which the facts comprised under the term 'atom arrangement' depend, it must nevertheless be accepted as a fact, that in every complex organic compound a certain number of atoms possess a far greater attraction for each other than for any of the others with which they are associated.

"The facts mentioned now further lead to the adoption of the idea, that the arrangement of the atoms in a complex organic substance cannot be absolute, but are liable to change according to circumstances; for the same substance under different influences yields various products of decomposition. Based as it is on facts, this extension of the above explained idea of atomic arrangement is found, when closely considered, to be dependent on a theoretical necessity. For as we look upon the existence of an organic compound as the consequence of the attraction of affinity inherent in it, we must see that when a new active influence is brought to bear upon it, in addition to those already at work, a change must take place in the amount of attraction which previously existed. To explain this, we shall once more make use of the foregoing diagram. We shall suppose, for example, that by the presence of some decomposing influence

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caused by the presence of an additional chemical agent possessing a special affinity to two of the groups of atoms—the middle one, and that to the right hand. The result of this change in position would manifestly be that the remaining three groups of atoms—the left, upper, and lower ones—being freed from the attraction exerted by the other two, could form new and independent combinations. The uncertainty which still obscures the theoretical statements which have hitherto been made, might, if we consider the matter strictly in a scientific light, render questionable the value of a statement of this kind; but while we do not deny this, we must all the more earnestly direct attention to the practical importance of these facts. It follows from them, that all the results that can possibly arise from the presence of a compound, depend neither upon the absolute number and quality of the elements which enter into combination, nor upon the amount of decomposition which a complex substance may undergo, from one circumstance or another; but that in order completely to characterize these results, it must be shown what decomposition a complex compound undergoes under accurately determined conditions. In chemical language, it is necessary to demonstrate to the physiologist, the disposition of the rational formula of the 'immediate principles' during the time of their sojourn in the animal body." (vol. i. p. 15.)

From this quotation the reader may form some idea of the philosophic manner in which our author treats his subject. His reasoning is far removed from the common style of mediocrity. He enters upon his task fully aware of the difficulties he has to encounter, and imbued with that confidence which necessarily accrues from an extensive acquaintance with the scientific literature of the time. He neither fears to grapple with the *vexatæ quæstiones* as they occur, nor shrinks from a candid avowal of the inadequacy of our present knowledge to unravel the Gordian knot by which many of the fundamental laws ruling the workings of animate matter are bound. A single paragraph, although it includes the consideration of an intricate point, is hardly a sufficient criterion of the writer's mode of generalization; but the limited space at our command would not permit of our giving numerous extracts.

A solution to the problem of the physiological action of atoms has not yet been obtained: nor will it be obtained until we have discovered all the immediate principles which compose the various parts and organs of the animal body. Much remains to be done in this direction; for even at present we know little more than the reactions of many of the immediate principles discovered some years ago, their atomic equivalents having not yet been ascertained.

The first element which Ludwig devotes space to, is Oxygen. This gas, as is well known, is found in every part of the animal body, in the fluids as well as in the solids, both in a free and in a combined state; but it was not supposed until very recently that it there occurred in the form of ozone. That such is in reality the case, however, is now, since the publication of Schönbein's experiments, the commonly received opinion. Other chemists have followed up Schönbein's researches, and have obtained results for the most part in accordance with his views. M. De Luca recently communicated to the French Academy of Sciences some very important and interesting results, which it will not be superfluous to mention. M. De Luca's researches were made with the view of ascertaining if the oxygen exhaled by the leaves of plants in the sun's light, presented the properties of ozone, Schönbein having discovered that the juice of certain mushrooms possessed the power of transforming oxygen into that substance. To prevent misconception, we must here remark, however, that these gentlemen do not look upon ozone as a peroxide of hydrogen (the old view), but have adopted the new opinion, which pronounces ozone to be nothing more than modified oxygen—an allotropic condition; and consider that it may be without difficulty analysed.

M. De Luca's experiments were conducted on a large scale, and prosecuted with every attention which could ensure exactitude. They chiefly consisted in causing a large quantity of air (twenty thousand litres) taken from the immediate vicinity of plants, to pass slowly during the day through an apparatus containing, in separate compartments, sulphuric acid, potassium, and a dilute solution of pure potash. The experiment lasted six months, and at the end of that time, the sulphuric acid was found to contain ammonia, the potassium had been partly transformed into nitrate of potash, while the alkaline solution contained nitrates. From this M. De Luca concludes, that the oxygen exhaled from the leaves of plants by the action of light, contains ozone, and that this ozone produces the oxidation of the nitrogen of the air, thereby forming nitric acid, in the same way as ozone, artificially prepared, produces nitrates in the presence of air and alkalies. If this be a true explanation of the results, we would no longer have to believe that plants absorb pure nitrogen, but that they assimilate the nitrogen in the form of nitrate and carbonate of ammonia, the former originating under the influence of vegetation, the latter being produced in the atmosphere.

In 1852, when Ludwig published his first volume, the experiments of Schönbein had already been made public, and our author, recognising at once, with true philosophical acumen, the great value of the discovery, did not hesitate to embody in his work the new ideas to which this discovery gave rise. The following are his own words:—

"In consequence of the fact, that our food consists of substances poor in oxygen, while our excreta are rich in that material, the chemical processes occurring in the animal body have been compared to combustion. This mode of expression is perfectly correct, if we regard that combustion as being of an altogether peculiar description; the peculiarity consisting in the fact that it requires no high temperature for its development. The enigmatical phenomenon of the transformation in the animal body of the almost incombustible materials into carbonic acid, water, &c., has been brought nearer to a solution by the important discovery of Schönbein; from which it appears that oxygen exists in two allotropic conditions. One of these conditions, named by Schönbein, the excited oxygen (*erregter Sauerstoff*), has such energetic affinities, that it oxidizes substances at any temperature. If, as we dare to suppose, this allotropic modification of oxygen exists in the animal body, we can easily comprehend how substances are therein consumed without a great increase of temperature. Animal combustion is peculiar not only on account of its requiring no great increase of temperature for its production, but also because the products are different from those of ordinary combustion. It is well known that albumen, fat, &c., in the presence of oxygen at a high temperature, are not at once transformed into CO_2, HO, NH_3, &c., but into other compounds, which can afterwards be completely consumed. The *ultimate* products of combustion occurring out of the body, are identical with those of combustion within it; but the intermediate results are different; at least, we do not find the same intermediate products in the animal organism.

"The close affinity between the two modes of combustion is made still further manifest by the fact that they both develope the same amount of heat. This conclusion may be adopted with safety, since it is known that the products of animal combustion contain the same amount of latent heat as those of the flame. That the heat transformed from the latent into the free state in the animal frame, is employed in the production of motion (*Bewegungserzeugend*), is made apparent by the fact that both nerves and muscles require oxygen for the development of their functions." (vol. i. p. 17.)

These conclusions bear a great analogy to those at which M. de Luca* arrived but a short time since; M. de Luca thinks it not improbable that the respired oxygen exists in the blood partly in the form of ozone, like the oxygen dissolved in turpentine, and most probably produces similar phenomena of oxidation. These views are strengthened by the important and interesting experiments of Béchamp and Picard,† who found that by slow combustion, effected by means of a solution of permanganate of potash at a temperature of 176° Fahr., albumen can be directly resolved into urea. Urea can also be obtained out of

* Comptes Rendus, Nov. 3rd, 1856, p. 865. † Ibid, Sept. 8th, 1856.

the body from other azotised animal products; and it is not unreasonable to suppose that this crystalline substance, which we daily excrete, may be to some extent a direct product of the respiration, formed like CO_2, in the blood, by the process of slow oxidation by the oxygen of the respired air of the excess of nutritive material; the other portion of urea is of course derived from the effete tissues. We will not encroach further upon the subject of the chemical action of the respired oxygen in the blood, but refer our readers to our experiments on the chemistry of respiration, detailed in the review of Magnus's doctrine, in the number of this Journal for October, 1856.

The large amount of interesting matter in the work before us obliges us to pass somewhat abruptly to our author's third division of his subject—The Physiology of the Nervous System. The first few paragraphs are confined to a description of the minute structure of nerves; they contain nothing new, and we need not therefore stay to discuss them. At page 77, our author remarks that the leg of a frog is superior to Du Bois's multiplicator as a test of nerve electricity—firstly, on account of no apparatus being required; and secondly, because the leg will twitch even in cases where the magnetic needle remains perfectly motionless; as, for example, when two streams of electricity, proceeding in opposite directions, exactly balance each other. The direction of the electric current in a frog's nerve can also, without the aid of the electro multiplicator, be shown to be from the surface to the transverse section. For a description of the manner in which this is done, we must refer our readers to page 79 of Ludwig's book, where they will find it fully explained. The electric currents in dead nerves proceed either in a diametrically opposite direction from those in living nerves, or at most, *very* slightly in the same direction. The positive point becomes in this case the negative, and *vice versâ*.

The rapidity with which the currents traverse nerves, has been minutely investigated by Helmholtz, and the rate of their progress found to be, in sensory as well as motory nerves, 61·5 metres in a second. A metre is 39 inches—the stimulus therefore travels at the enormous rate of 66·62 yards in a second. This however is not always the case, for the rapidity of the current depends upon the state of the nerve, as well as upon the kind of animal operated upon. In man it travels three times quicker than in the frog. The temperature of the nerve also exerts an important influence upon the rapidity of the current, a nerve of a frog at 0° of temperature, having proved a much worse conductor than at the normal temperature (10°–12° Cent.).

The method by which the rapidity of the stimulus travelling in nerves can be estimated, is based on the principle first pointed out by Pouillet, of measuring short spaces of time by means of an electric current; the deflection of the magnetic needle indicating the intensity of the current, as well as its duration. This is very easily understood if we regard the action of the electric current upon the needle as a series of uninterrupted shocks. The intensity of the stream would be indicated by the strength of the blows, and its duration by their number. The mechanical action of a current upon a magnetic needle would consequently be proportional to its duration. If we permit the magnetic needle to be but very slightly deflected, the deflection produced by equally strong, but unequally enduring currents, would be in direct proportion to their duration. Upon this principle, we can measure the length of time between the application of a stimulus to a nerve, and the contraction of the muscle to which the nerve is attached, by joining a current of known intensity exactly at the moment the stimulus is applied to the nerve, and opening the current at the precise moment when the excitation has reached the muscle.

This Helmholtz has accomplished by means of an ingeniously constructed apparatus, a brief but lucid description of which is to be found at p. 115 of Ludwig's book.

On the subject of the physiological action of nerves, our author remarks that the excitors of nervous action are, as a general rule, mechanical: heat, light, electricity, and a number of chemical elements which possess a certain affinity for nerve-substance. Experience however has shown that these agents do not excite all the nerves in a uniform manner. The difference of their actions is observable in three respects:—It is seen, firstly, in the fact that the same substance does not act as a stimulus to every nerve; secondly, it has been found that an agent which can excite several nerves, produces in some special kinds of excitement (various sensations); thirdly and lastly, it is known that a certain stimulant applied to different parts of the same nerve, gives rise to various results.

In order more fully to explain this statement, we may add that, as the retina can only be excited by "waves of light," electricity, and pressure; the acoustic nerves by "sound-waves" and electricity; the olfactory nerves by volatile substances; and the gustatory nerves by soluble substances (but not by all)—so the sensory nerves are to be excited only by pressure, heat, and chemical and electrical action; the motory nerves by pressure, heat, electricity, and a limited number of chemical substances; and lastly, the nerves supplying the glands by chemical and electrical stimuli. On the other hand, the same means which, under similar circumstances, excites various nerves, produces in each of them, apparently or really, different kinds of sensation. Thus, for example, when applied to a motory nerve, the pressure produces motion, to a cutaneous nerve, pain, and to the retina, the sensation of light; while, again, the same agent applied to the periphery, produces different results than when its power is exerted on the course of the nerve. Here also is to be found the reason why the sensation of light follows the illumination of the distribution of the optic nerve in the retina, and not that of the optic trunk (Helmholtz), why warmth is experienced when heat is applied to the skin, and why we feel pain on the application of the same agent to the trunk of the nerve (E. H. Weber).

The various and manifold results just enumerated may cause us to question whether we are justified in regarding the constitution of the nerves as uniformly identical; and may make us inquire if we should not rather divide the aggregate of the nerves into special acting groups. We can see no necessity for adopting the latter plan until we discover that the various physiological effects produced by the excitation of different nerves are dependent upon a variety of constitution in the nerves themselves, and not upon the peculiar structure of the organs with which they are united, either at their periphery, in their course, or at their central end. This point might be determined in three ways. The first and most direct way would be to try the possibility of uniting the divided ends of nerves supplying different organs, which nerves are hypothetically supposed to have different functions. If after this welding together of different nerves, a change was produced in the results normally following excitation of the nerve—for instance, if the stimulation of a sensory nerve that had been welded to a motory gave rise to motion—we might acknowledge that the cause of the various functions does not exist in the nerve, but is to be sought for elsewhere. Bidder has very ingeniously worked out this idea, inasmuch as he tried to unite the cut ends of the sensory lingual branch of the fifth to the motory hypoglossus. Unfortunately, these interesting experiments have as yet given no satisfactory result.

A second manner of solving the problem would be, to deduce the dissimilar results obtained by exciting the nerves from the peculiarities of the organs with which they are united. It is quite certain that many of the different actions of a nerve depend upon its peripheral distribution. The excitation of a cutaneous nerve or of the retina can, for example, induce directly no muscular contraction, the nerves not being attached to any muscle. Light, moreover, acts upon a cutaneous nerve, not as light, but as heat. The specific properties of the organs, as well as the part that the nerve plays, are more readily understood when we reflect that a cutaneous nerve is sensible to temperature only so long as it still ends in a tactile papilla, and that the optic nerve is excited by waves of light at no other point than at its ultimate termination. These facts, however, must not lead us to suppose that the peculiar actions of nerves are entirely dependent upon the organs to which they are supplied. Other facts point out that there are a variety of nervous actions which are either dependent upon the nerves themselves, or upon the part of the central organ in which the nerves originate. For we find different results following the application of a stimulus to that portion of a cut nerve which is still attached to the brain or spinal cord. Pressure to the central end of a divided motory nerve never produces pain; from a cutaneous nerve it elicits only pain; and from the optic nerve only the sensation of light. These various results, however, can still be attributed to a peculiar kind of termination of the nerves in the brain, as well as to a peculiarity of structure of the so-called sensory organs, or those of the will. In reality, the nerves enter the brain and spinal cord variously, according as they are sensory or motory; and the sensory nerves are even attached to the brain, both locally and histologically, in different ways. This series of considerations offers no obstacle to the belief that the nerves themselves are everywhere alike—indeed, it seems rather to assist in establishing the fact.

With one short quotation upon the origin of nerve-force, we shall leave the department of the nerves, which is full of interesting and instructive remark:

"The source of nerve-force is most probably to be found in the chemical decomposition of the substances of which the nerves are composed. In proof of this, it may be mentioned that nerves only retain their irritability so long as they possess a definite chemical composition; and also that living nerves, in an excited as well as in a non-excited condition, gradually lose their normal composition." (p. 119.)

Although to the present moment not demonstrated by chemical analysis, this is yet a very probable view, when we remember that all chemical and physical agents which alter their composition destroy their irritability. In this case, widely different influences produce similar results. Heat, which evaporates the water from the nerves, destroys their irritability just as much as an excess of water, which removes their salts. The same effect is produced by substances which coagulate their albumen or attack their fats.

Next comes the question, What is nerve power? This is a subject which, since the days of the first controversies of Galvani and Volta, has more or less occupied the minds of our greatest experimental physiologists; and although it is nearly a century since electricity was discovered to be inherent in the nerves and muscles, it was not until the last few years that the wonderful phenomena here alluded to could be considered as reduced to definite laws. Du Bois-Reymond, by his valuable researches in the field of animal electricity, has opened up to us a mine of untold wealth. He has shown us the connexion of facts which were before heaped together in the utmost disorder; he has made us acquainted with electro-physiology in all its branches; and, by a wonderful power of generalization, he has defined the various forms of animal electricity, and their intimate connexion with one another. While we give all due honour to the German philosopher, let us not withhold due praise from his great Italian compeer, Matteucci, who, with an equal energy, has pursued the subtle spark. He, too, with laudable diligence, has followed in the steps of his compatriots; and if he has been less successful in his results, has nevertheless done equal service to science by pointing out facts which might otherwise have long lain hid.

Electro-physiology may now be said to have passed through one of the most critical parts of its history. At one time—the period, namely, between the publication of Aldini's last work in 1804, and the appearance of Nobili's book in 1827—it was almost entirely forgotten. Since then, it has been kept more or less before the public by a variety of memoirs on the subject; but it was not until the publication of Du Bois-Reymond's work in 1848, that it was placed upon what appears now to be an uncontrovertible scientific basis. No man possessed of the information at present attainable on the subject, will venture to deny the existence of an inherent electricity in the nerves and muscles, or doubt that the electric phenomena of motive and sensitive nerves are identical, and still further, that every muscular contraction is accompanied by a discharge of electricity.

We might here call to the recollection of our readers the results of the experiments of Humboldt, who found that by bending the thigh of an animal upon its sciatic nerve, the muscles were induced to contract. On simultaneously touching even the crural nerve and the muscles of the limb by a detached portion of nerve, muscular contractions were immediately observed.

Professor Matteucci has lately communicated to the Royal Society* some fresh experiments corroborative of the phenomenon which he terms *induced contraction*, when the nerve of a galvanoscopic limb of a frog, laid on a muscle of a living animal, receives, by the contraction of that muscle, a stimulus sufficient to cause a corresponding contraction of the muscles in the galvanoscopic limb. He endeavours at the same time to show that this phenomenon is due to an actual electric discharge which takes place in a muscle at the moment of its contraction. This is no new theory; but it has found so many opponents in the so-called vital school, or more properly the HYPOTHETICAL SCHOOL, that every new fact that can be brought forward in its support becomes invested with a peculiar value. Ludwig and his companions—who deservedly stand at the head of their profession—have been forced to acknowledge their inability to explain nerve-force upon any other ground than that of electric agency.

Many medical men object to the view of nerve-force being identical with electricity, on the sole ground that different nerves call into play different actions,—the excitation of one producing motion; of another, sensation; of a third, secretion; and so on; forgetting altogether the absence of proof that the property of calling a specific function into play exists in the nerves. On the contrary, the specific property seems rather to be inherent in the organs to which the nerves are attached. The nerves are only the channels through which the stimulus passes, and therefore the agent by which they excite may perfectly well be one and the same, although the effects they produce are very various; just as steam—that important agent of motive power—calls forth effects entirely depending upon the machinery to which it is applied: in one case—as in the railway engine—producing locomotion; in another—as in the steam-whistle—sound. The various organs of the animal body may be regarded as so many different machines, each constructed for the performance of a special and peculiar office, possessing within itself its specific property, altogether independent of nervous agency. Muscles, for example, are now known to contract when nervous agency is completely in abeyance, as Bernard has shown by his experiment with wourali poison; and if we could equally well separate the nerve agency from the internal organs, we doubt not but that we should be alike successful in calling their functions into action by the direct application of electricity to the tissue of the organs themselves. Viewed in this light, it at once becomes apparent how one and the same agent may produce such multifarious results. The liver is constructed for the secretion of bile, sugar, &c., and it is probable that it would continue to secrete its normal products, if, in place of the ordinary nerve stimulus, an artificial stimulus were directly supplied to it. We already know, from the results of our own

* Proceedings of the Royal Society, vol. viii. No. 22.

experiments,* that when stimulants are directly injected into the liver, its normal function of secreting sugar is greatly increased; and who can say whether or not this increase is not dependent on the direct stimulation of the tissue of the organ? The kidneys might, on this supposition, excrete urine, even were the place of their nerves supplied by the nerves normally going to the liver, the reason being found in the fact that the nerves only call into action a power inherent in the organ itself.

From general physiology, our author passes on to the special physiology of the nervous system, and treats in succession of the spinal cord, the brain and its nerves, and the sympathetic nerve. He next investigates the different senses, beginning with that of vision. In the chapter devoted to this branch we find the following interesting remarks regarding the mechanism of focussing the eyes. Ludwig attributes the change which occurs in the interior of the eye when we pass from the contemplation of a distant to that of a nearer object, in a great measure to the contraction of the voluntary muscles, and he agrees with Hueck† in considering it probable that the lens is carried forward by this muscular contraction. He adopts this view the more willingly, he says, from the consideration that we possess the power of adapting our eyes to different distances; and further, that a certain time is necessary to enable us to exert this faculty. According to Volkmann, the time thus required very much resembles that employed in small muscular contractions.

The other view which he dilates upon, attributes the focussing of the eyes to the change produced in its axis by the pressure of the contracting muscles. The theory which recognises the altering of the focal point to be dependent upon the change of position of the lens, produced by the action of the tensor-choroideæ muscles, receives, we believe, the most favour. When belladonna is taken, the power of accommodation of the eye is lessened, Ludwig thinks in consequence of the paralysis of the tensor-choroideæ muscle. The eye becomes farsighted. Several observers, among whom we may mention Hueck and Listing, have seen the advance of the lens when the eye is accommodating itself to view an object brought near to it. The idea that the accommodation of the eye depended upon the change in size of the pupil, has been so completely disproved as not to require further consideration. The action of the iris in focal adjustment has been ably treated of in an interesting paper by Professor Allen Thomson, in the January number of the 'Glasgow Medical Journal.'

We have not space to make any remarks upon what our author says regarding the other senses, but must rapidly pass to the consideration of the muscular system. In perusing the portion of the work devoted to this subject, we found the following interesting remarks upon rigor mortis. Ludwig divides the rigor of muscles into two kinds—firstly, the artificial rigor produced by an increase of tempe-

* Recherches sur la Physiologie du Diabète Sucré; Nouvelle Méthode pour produire Artificiellement le Diabète chez les Animaux: Gazette Médicale de Paris, p. 506. Août, 1853.

† Valentin's Lehrbuch der Physiologie, Band ii. Abth. 3, p. 250.

rature; and secondly, the rigor of death. The consideration of the peculiar appearances observed in these two kinds of muscular rigor need not detain us long. The first kind, which we may term rigor caloris, is produced if the muscles of a frog's leg are kept for twenty-five seconds in water at a temperature of 65° Reaumur, or for some minutes at a temperature of 30° Reaumur. The muscles contract and become stiff. According to Pickford,* the rigor caloris disappears in a few minutes.† When in this state, they cannot be excited to contraction by the ordinary stimulants, and they deflect the magnetic needle so as to show that their transverse cut is positive, and their longitudinal cut negative. This last observation was made by Du Bois, who also discovered that when nerves are exposed to the heat of a red-hot body, the electric current changes its direction in a similar manner as it does in muscles. If the nerve, while it is in this state, be excited to action, the current in the abnormal direction increases; and if the nerve still imbedded in muscle be left quiescent for a short time, the current regains its normal direction.

The second kind of muscular rigor, which generally occurs in the dead body, may be said to present the following characters. The optical appearances are not much changed; the muscle is perhaps somewhat more opaque than during life, and the transverse striæ are better marked. Besides this, the fibrillæ are shortened and extended laterally, as happens during the contraction of living muscles.

The state of rigor mortis, however, cannot be said to be analogous to the contraction of living muscles; for, as Ludwig remarks, there are most important differences between the two. He says, that during contraction muscles become softer, and during rigor mortis harder. We perfectly coincide in the remark, that muscles in a state of rigor after death are hard; but that living muscles are softer during contraction than relaxation, we cannot believe. Our author, we suspect, has fallen into a slight mistake in making such a statement. The reader can easily convince himself that the muscles harden with the degree of contraction to which they are subjected, by applying one hand to the biceps of the opposite arm. In contraction, there is a "negative fluctuation" (*Schwankung*) of the electric current; in rigor mortis, the current is destroyed. Contraction produces heat; rigor mortis does not, &c. &c.

Since the publication of the book under review, an interesting series of experiments by Dr. Kussmaul has been published, in which he shows how the injection of stimulants, such as ether, chloroform, &c., into the vessels of a limb, produces rigor.

As everything connected with the chemistry of the animal body has at the present moment a peculiar interest to the physiologist, we

* This statement of Dr. Pickford only holds good for experiments made at the above temperatures; if, as we have found from personal experiment, the hind limbs of a recently killed frog be kept in water, at a temperature of seventy degrees Cent. only, during fifteen seconds, or at a temperature of eighty degrees for ten seconds, the rigor caloris is so strongly induced that the limbs remain in a rigid condition during several *hours*, sometimes upwards of twenty-four hours. We have found this result to follow on frogs operated upon in summer as well as in winter.

† Zeitschrift für rat. Medicine, Neue Folge i. p. 110.

may venture to condense a few of Ludwig's remarks upon the chemical properties of muscle.

At every period of their existence, muscles develop different kinds of force—chemical, electrical, thermical, and mechanical. The theory of these various forces has first to demonstrate the problem of all muscular force being the result of the elementary constitution of muscle; and then to show the intimate relation—or, more properly, correlation—existing among the forces themselves; whether, for instance, a portion of the mechanical action may or may not be regarded as the result of the thermical or electrical properties inherent in the muscle. Such a demonstration, however, is attended, in the present state of our scientific acquirements, with great difficulties.

The idea of the development of heat, and of the electric current in muscles having a common origin in the chemical transformation of their substance, may be hazarded with considerable confidence—firstly, because these effects are frequently the result of chemical action; secondly, because the development of these agencies in muscles proceeds in a parallel ratio with the transformation at least of a portion of them, and the product of the transformation, carbonic acid, is always accompanied by the development of heat. Future researches must, however, point out the chemical process by which the metamorphosis of the muscular substance produces electricity, and especially the varieties of the direction and strength of the current which is at different times observable.

Concerning the mode of connexion among the different forces already alluded to, we may throw out the conjecture, that it is probable, as Ludwig says, that the change in the arrangement of the molecules which induces the state of contraction, is dependent upon the electrical tension of the muscle. This idea being founded on the observation made by Du Bois, that the strength of the muscle is proportional to the formation of the electrical power, and moreover that muscular contraction is accompanied by an appreciable change in the arrangement of the electric motor elements. As electricity may avowedly be used as mechanical power, it would be unreasonable to deny the probability of the correctness of the foregoing conjecture.

There are some chemical points of similarity between the muscles and nerves that we may here take a short notice of. It is worthy of remark, for example, that both muscles and nerves manifest their living properties only so long as they retain a certain chemical composition; that with the development of their physiological powers, in a state of rest as well as of activity, certain chemical changes with which oxygen is connected occur in their substance; they possess a very similar, if not identical, electrical constitution; and lastly, that the same stimulants produce a change in the molecular properties of each. These appearances, however, are far from making us regard the two apparatuses as identical, for their physical constitution is perfectly different, and the immediate principles of which they are composed are very dissimilar.

The second volume contains, besides some very interesting chapters on the motion and tension of the blood in the arteries and veins, many valuable remarks upon the results obtained from the employment of the kymographion. We would most certainly have given a considerable portion of our space to the consideration of this important subject, had it not been for the fact that a very full review of Hæmodynamics appeared in a former number of this Journal (vol. viii. p. 98), to which we would beg to refer our readers.

We have already devoted so much space to the revision of the contents of the first volume, that we have left ourselves little room to discuss the second; but the subjects which Ludwig treats in the last half of his book have in general been fully handled in recent numbers of this Journal: for instance, the glycogenic function of the liver, digestion, the mechanism of respiration, and of animal heat. Upon the last-mentioned subject he justly remarks, that all parts of the body have not an equal temperature at a given time. The recent experiments of Bischoff, Liebig, Bernard, and Walferdin have proved that the blood in the cutaneous veins of the head and extremities is colder than that in the arteries; while the blood in the great veins in the neck and extremities is, again, colder than that in the corresponding arterial trunks. On the other hand, the blood coming from the liver and kidneys is hotter than that carried to these organs. From this it would appear that the liver and kidneys are heat-generating organs. The warmest blood in the body is that of the hepatic veins. The blood on the right side of the heart is warmer than that on the left side. In contradiction, therefore, to the old theory of Davy and others, the lungs must be regarded as cooling organs. The difference in temperature of the two sides of the heart is not very considerable. It varies from 0·05° to 0·19° Cent.

The temperature of the body varies at different times. It in some measure depends upon the age, diet, health, and occupation of the person on whom the observation is made, the time of the day at which the experiment is performed, and the temperature of the surrounding atmosphere.

Dr. J. Davy* distinctly showed the influence of the latter by taking the temperature of the mouth at different times. He found, for example—

Temperature of the air.	Temperature of the mouth.
33·3° Cent.	38·00° Cent.
18·2° „	37·09° „
5·6° „	36·10° „
4·4° „	36·20° „
0·6° „	35·90° „
0·0° „	34·90° „

Chossat found that, by preventing animals from moving and giving them insufficient food, the animal temperature was diminished very rapidly in a cold atmosphere. By an external temperature of 12° to 18° Cent., the heat of the animal fell to 25°, a temperature fatal to life.

As heat is not a substance, but only a sign of action—or, as Ludwig

* Physiological and Anatomical Researches, vol. i. ch. 8.

calls it, a peculiar kind of motion—which may be developed by an infinity of causes, it becomes a very difficult matter to define it and point out how it originates in animals. The familiar doctrine which ascribes the heat in the animal body to the slow combustion of its organic materials, has never been proved by direct experiment; yet we are forced to accept of this explanation in absence of a better. Animal heat cannot be said to proceed from muscle or nerve force, for these forces are themselves dependent upon the transformation of the organic substance for their development. It follows, therefore, that the friction produced by the motions of the muscles, tendons, joints, and blood upon the walls of the vessels, originally comes from the same source. The heat produced by the various parts of the body is therefore always traced back to the transformation—or, as Liebig first called it, the slow combustion—of the albumen, fat, oxygen, &c. The transformation of albumen and fat into carbonic acid, water, and urea, is attended by the setting free of a definite amount of heat. Inasmuch as it is no more possible that any force in nature should be lost, than it is that any element should be destroyed; and as the amount of force in the shape of heat, electricity, motion, &c., is developed in every instance of the transformation of substance, the appreciation of the source of animal heat becomes a matter of little difficulty. The only difficulty is to point out the individual substances from which it emanates, the amount that is developed, and the purposes it serves.

The amount of heat set free during the combustion of a substance is always the same, no matter whether it be directly changed into carbonic acid and water, or transformed during the combustion into a series of other compounds, before its ultimate change into carbonic acid and water. For example, one grain of stearic acid, burned in the presence of oxygen into carbonic acid and water, gives off the same amount of heat as when the combustion has been so gradual as to permit of the formation of the lower forms of fatty acids before the complete transformation into carbonic acid and water. On the other hand, however, the quantity of heat which an element or an organic compound developes during combustion is dependent upon the form in which it is found. One gramme of carbon, for example, in the different allotropic modifications of diamond, graphite, and charcoal, gives out different quantities of heat. Then, again, an element combined with other elements gives off an amount of heat very different from the quantity it yields in an uncombined state.

The more rapid the transformation of tissue, the greater should we theoretically expect the amount of heat to be. This point has been established by direct experiment. Becquerel and Brechet found, by a thermo-electric apparatus, that the contracted muscle is from a half to a whole degree Centigrade hotter than the muscle in a state of rest. The stomach is hotter while engaged in digestion than when fasting, notwithstanding that in the former case it has been cooled by the introduction of food. It would be superfluous to multiply examples of the fact—for who does not know the effect of a brisk run in a cold day? In whatever part of the frame the circulation is increased, we find an increase in temperature; and the cause of this, as we before said, is to be found in the slow oxidation of the organic substances, in the blood, and different organs of the body.

With a strong recommendation of the work to the attention of our readers, we close our remarks on Professor Ludwig's 'Text-book of Human Physiology.'

Review VII.

Life: its Nature, Varieties, and Phenomena; also, Times and Seasons. By Leo. H. Grindon, Lecturer on Botany at the Royal School of Medicine, Manchester, &c.—*London.* pp. 328.

Among the many modes in which "life: its nature, varieties, and phenomena" may be considered, there are two which are essentially different and opposed, and there is a third which presents a combination of these two. The first takes for its starting-point material organization; the second proceeds from human consciousness; the third advances from them both: and thus, while those who adopt either the first or the second method regard their respective antagonists as either hopelessly ignorant or wilfully perverse, and in either case utterly wrong, those who proceed in accordance with the third method, recognise some truth in each of the former two, appropriate this truth, and reject those portions of each system which are worthless, because they are untrue.

The first method consists in such direction of effort, that, proceeding from the simplest facts or phenomena presented for material examination, and advancing step by step, general statements may at length be framed with regard to the more complicated. The sciences of number, of physics, and of chemistry, are all brought to bear upon the interpretation of life; and vital phenomena are expressed in numerical, physical, and chemical terms. The object of such method is quantitative as well as qualitative analysis. Its perfection consists in such accurate appreciation and measurement of the physical changes accompanying vital processes, that we may foretell the results of various modifying circumstances, and calculate causes from effects, as well as effects from causes. Life is considered in the race, and not in the individual. Laws of mortality, tables of insurance offices, general sanitary directions, and the political histories of different nations, are not only the ends to be attained, but the means of progress. Human consciousness in the individual is thrust aside; the man is recognised through the nation, not the nation through the man; and all the fine thoughts and finer feelings of individuals are, unless of some immediately practical bearing upon the many, regarded as so many interesting but aberrant phenomena, which will eventually produce their legitimate result in the history of the people to whom these individuals may belong. There is a general ignoring, or at least underrating, and distrust of everything which cannot be weighed in the chemist's scales, or which cannot have immediately assigned to it its due value in those tables of probabilities which may guide the speculative in their monetary transactions; and so prevalent in the scientific world is this method of procedure at the present day, that many a man who, true to that within him, which is as real and far more real than all beside, could utter some hypotheses, or throw out into the darkness beyond the limits of positive science, hints of a truth which his mind has seen, is hushed by the presence and the sneers of a rigidly scientific companion, who despises every proposition possessed of less certainty than that two and two make four.

The method in direct opposition to this is that adopted by the author of the work the title of which is placed at the commencement of this article. Such method finds in the human consciousness of the individual the starting-point of inquiry, the means of investigation, and the ultimate court of appeal as to the correctness of its results. The reduction of all phenomena to some expressions which convey, in abstract terms, ideas, or descriptions of mental processes, is the end of scientific analysis. Phenomena are passed over as explained when some analogy is discovered between them and certain conditions of the mind. The physical changes of the organized body are regarded as merely types of, or as identical with, those metaphysical changes of which we are conscious, and which may have been from time immemorial expressed by terms derived from the region of physical existence. The beating of the heart is by this means made identical with the palpitation of an inner life: the drawing in of air by the lungs is regarded as divine inspiration: the union of the sexes as the combination of wisdom and goodness, leading to a new incarnation of life: the processes of excretion as so much exorcism of the devil, and so on. Accuracy of detail and consistency of plan are sacrificed, or disregarded, in the overweening desire to evolve harmonies where there are no mutual relations, and identities where there are only analogies: one half of the truths of science are completely lost sight of, and the other half so frequently distorted that their value is reduced to a minimum.

In our opinion, these two methods are equally erroneous. Recognising life in ourselves, we find it partly in material phenomena, such as growth, secretion, motion, and the like; and partly in immaterial phenomena, such as thought, emotion, judgment, will, and so forth; and no matter how intimate soever may be the relations subsisting between these two classes of phenomena, we cannot accurately express either in terms of the other. Changes in either group may, and do, produce changes in the other; and these results may be expressed in terms which are appropriate; but the results discovered in either group, as the coincidents of phenomena or processes in the other, do not represent the whole of the change. Certain vital processes produce, or are accompanied by, physical modifications of the organs which perform them. We may enumerate the latter in chemical, thermal, electrical, and other terms; but we have not then made an exhaustive statement; we have simply described results, and have left out of the enumeration that which it was our object to observe. In the processes of thought, volition, and emotion, we are conscious of much that cannot be expressed in any but their own appropriate language, although we may measure some phenomena of these processes in the beatings of the heart, the colour of the cheek, the gestures of the frame, and in the products of nervous disintegration. On the other hand, there are simple physical phenomena occurring every moment in the organized body which transcendental expressions only obscure, and of which they can convey no definite account. It is as absurd to consider a statement of the results produced by thought, feeling, and will, in a number of individuals collected together as a people, as a full expression of the life-processes of each, as it is to express all the physical processes of the body in terms which apply accurately only to the mind. The former error is that committed by the first school alluded to; the latter is that fallen into by the second.

In order to add fractions together, we must reduce them to a common denomination. In the science of life we have as yet but fractions of the truth, and many of them are insusceptible of this reduction. Physiology proper and psychology proper cannot yet be thrown together. Certain facts and statements, although they may be hereafter decomposed, must be treated as ultimate elements now. We must proceed from the material, and from the immaterial; and the appeal as to accuracy must be, in the one case, to facts which can be appreciated objectively; and, in the other, to consciousness which is appreciated subjectively. Until the deeper and wider truth which underlies them both is legitimately perceived and recognised as their true bond of union, we must consider them as distinct, treat them as separate sciences, and above all things, avoid the assumption of unproved and unsatisfactory relations between them, which have this twofold danger, that they are erroneous in themselves, and set a limit to the earnest inquiry after that which is true. They may be as beautiful as the clouds of sunset; but they are as insubstantial and evanescent, or they serve only to deepen the gloom of approaching, "all-involving night."

As gathered from the preface, the object of Mr. Grindon's work is twofold:—

"First, it is proposed to give a popular and succinct account of the phenomena which indicate the presence of that mysterious sustaining force we denominate life; secondly will be considered those spiritual or emotional and intellectual states which collectively constitute the essential history of our temporal lives, or enduring existence, either pleasurable or painful."

With these objects in view, the author has passed in rapid survey many of the phenomena and laws of life, presenting little that professes to be original, but much that is extremely beautiful; and which, having "brought inexpressible happiness to the writer," cannot fail in giving great pleasure to the reader. But the latter will not find any "succinct account" of the phenomena of life; he will, on the contrary, find a certain mode of viewing many of them which is interesting in itself, but which is, as we think, erroneous.

What Mr. Grindon understands by life may be at once perceived by the following definition:—

"Life, in its proper generic sense, is the essence of the sustaining principle by which everything out of the Creator subsists—whether worlds, metals, minerals, trees, animals, mankind, angels, or devils—together with all thought and feeling. Nothing is absolutely lifeless, though many things are relatively so; and it is simply a conventional restriction of the term which makes life signify no more than the vital energy of an organised material body."

Almost all words have their "meaning limited by conventional restriction," and the word Life shares only in this common lot. It has a restricted meaning, and should continue to be restricted to a group of certain phenomena which are at present incapable of any wider, and at the same time satisfactory, generalization. As soon as the commonly so-called vital processes are shown to be identical with others at present placed in a distinct category, it will be the time to destroy the barrier between them; but to destroy the barrier before discovering their true mutual relations, is to reproduce confusion out of order, and to retard the progress of science. No doubt that there are many things in common possessed by all the forms of existence, which Mr. Grindon has included in his definition of life; but if the term life is applied in the manner there described, we need some other word which shall denote that collocation of properties presented by organic bodies, and hitherto termed "vital."

Mr. Grindon continues:—

"When it is popularly said that one thing is animate and another inanimate, that life is present *here*, but absent *there*, all that is essentially involved in the words is that a particular *manifestation* of life is absent or present. Such phrases come of confounding expression, which is variable, with principle, which is uniform."

We believe that much more is "essentially involved" in the application of the term life, or animate, than a difference of degree from the dead or inanimate. It is a mere assumption that the principle underlying all these different manifestations is the same, if we mean anything less general by this principle than the life and will of Him who "giveth to all life and breath and all things," and in whom "we live and move and have our being." The object of scientific inquiry and of philosophical speculation is, passing from the starting-point of human consciousness and perception, to arrive at the central truth of all things, and to know Him who is the Author of them all; but we must not assume the conclusion towards which we tend, and placing that conclusion among our scientific dogmata, use it as the means of progress. We must not, in our eagerness to reach the goal, leap over any single hiatus in the way; we must not confound together, and describe by one name, phenomena which are at present outside our harmonies; although, proceeding from another starting-point, we may be assured of the existence of a harmony which will be eventually revealed. Mr. Grindon does not by any means conceive of life as the correlate of physical forces, but his views upon this question, and also upon vitality more generally, may be gathered clearly from the following:—

"Vital force is no sort of physical entity, as correlation would make it. (?) It is but a technical name for that expression of the Divine life which we witness in organized beings; and the relation which the vital stimuli have to it, is simply that of *pathway*."

We confess to some obscurity with regard to the meaning of the last word in the above quotation; but it is evident that Mr. Grindon, rejecting the light which a study of the correlation of forces has thrown upon vital phenomena, rests satisfied with a general expression, which may be perfectly true, but which cannot be said to advance our knowledge of "life: its nature, varieties, or phenomena."

We have made these remarks without any wish to disparage the work of Mr. Grindon, but simply from the desire to place in what we conceive its true position, the method that he has adopted. For the interesting and elegant form in which he has applied that method, we sincerely thank him. In these eminently practical days, when we weigh everything in the balances of a rigid system of observation, and make neat scientific parcels of all the phenomena of life, we are in danger of losing sight of the importance of individual convictions—of disregarding much which we are bound to observe—of resting satisfied with a scientific routine which embraces only a portion of the truth, while it professes to include the whole—and of making up, by a scientific dogmatism with regard to what is known, for the vast extent of ignorance which yet remains. With these tendencies at work, it is of service to have thrown out before us, as a protest against such evils, the admirably-expressed and strong convictions of a man intimately acquainted with the sciences of which he treats, but who is so powerfully impressed with the value of much that is yet so dimly perceived that it finds no place in our scientific categories, that he fears not to venture into the higher regions of speculative thought; and who, by pointing out harmonies between the material and immaterial phases of our life, may, without solving the mysteries which surround them, at least "render the strange mystery of life less perplexing," and confer upon his readers some portion of the happiness which such considerations have given to himself.

Review VIII.

1. *The Progress of Preventive Medicine and Sanitary Measures: being the Thruston Speech on the Wendy Commemoration at Caius College.* Delivered on May 10th, 1856, by A. W. Barclay, M.D., Cantab. and Edin., F.R.C.P., Physician to the Chelsea Dispensary, Medical Officer of Health for the Parish of St. Luke, Chelsea.—*Cambridge,* 1856. pp. 35.

2. *Report on the Sanitary Condition of the City of London for the Year* 1855-56. By Henry Letheby, M.B., Medical Officer of Health for the City of London, &c. &c.—*London,* 1856. pp. 23.

3. *The First Quarterly Report of the Medical Officer of Health for Islington.* By Edward Ballard, M.D., L.R.C.P., &c., Medical Officer of Health.—*London,* 1856. pp. 8.

4. *Report on the Past and Present Sanitary Condition of Hackney District.* By John William Tripe, M.D., &c., Medical Officer of Health for the Hackney District.—*London,* 1856. pp. 28.

5. *The Medical Report on the Sickness and Mortality of the Borough of Swansea, for the Year* 1854. By W. H. MICHAEL, late Medical Officer.—*Swansea*, 1856. pp. 28.
6. *Annual Report to the Local Board of Health for the Borough of Durham, on the Sanitary Condition, Disease, and Mortality, for the Year ending March* 31*st*, 1854. By NICHOLAS OLIVER, Esq., Officer of Health.—*Durham*, 1854. pp. 37.
7. *Annual Report to the Local Board of Health on the Sanitary Condition of the Town of Darlington, for the Year* 1853. By STEPHEN EDWARD PIPER, F.R.C.S.—*Darlington*, 1854. pp. 12.
8. *A Report on the Sanitary Condition of Leicester in* 1853. By JOHN MOORE, Surgeon, Officer of Health.—*Leicester*. pp. 11.
9. *Report on the Accommodation in St. Pancras Workhouse.* By HENRY BENCE JONES, M.D., F.R.S., Physician to St. George's Hospital. Presented to both Houses of Parliament by command of her Majesty.—*London*, 1856. pp. 18.
10. *Summary of Experience on Disease, and Comparative Rates of Mortality.* By WILLIAM LEE, Esq., Superintending Inspector. —*London*, 1851. pp. 77.
11. *Report to the General Board of Health on a Preliminary Inquiry into the Sewerage, Drainage, and Supply of Water; and the Sanitary Condition of the Inhabitants of the Town of Darlington.* By WILLIAM RANGER, Esq., Superintending Inspector.—*London*, 1850. pp. 74.*

THE firstfruits of the recently inaugurated organization for the local sanitary management of the metropolis lie before us; and although, from the brief period during which the Medical Officers of Health have held office, it would be premature to pass any opinion upon the results of their labours, we propose making the several Reports that have reached us, and some recently published documents bearing upon public health, the text of a few remarks upon the desiderata of sanitary science, and upon the kind of inquiry most likely to supply the want. Before proceeding with our immediate object, however, we should be doing violence to our own feelings, and injustice to the Medical Officers of Health, if we did not express our strong sense of the earnest desire they have manifested to fulfil faithfully the important duties that they have undertaken. We would allude more particularly to their having associated themselves into a Society for prosecuting inquiries into the causes which affect the public health, and to the organization of committees for carrying out special branches of investigation. The committees that have been appointed are, one upon unwholesome meat—which has already reported—a committee for general purposes, a trades-nuisances committee, an adulteration committee, an ætiological committee, and a meteorological committee.

* To these reports we have to add those published by the local boards of Whitechapel and of the Belgrave sub-district of St. George's, written respectively by Mr. Liddle and Dr. Aldis. We regret that they have reached us too late to permit of our paying them that attention in the present article which we should have desired. The same remark applies to Dr. Sanderson's Report for Paddington.—ED.

Two documents have already emanated from the Society, exclusive of the reports of members in their individual capacity—the Report on Unwholesome Meat just referred to, and a schedule of Notes for Ætiology of Cases of Continued Fever. Of the latter, we can but express our high approval. It is most ably drawn up, and constructed so as to include a record of all the most important points in the history of fever. If the investigation be fairly carried out, as we presume is intended, by all the parochial medical officers of London, it cannot fail to be the means of solving many important problems in the causation and history of fever. We venture to suggest attention to one point which appears to have been overlooked: it is, that the result should be recorded in each case—that is to say, the actual result as regards the individual practitioner—viz., whether the patient died, recovered, went to hospital, or in any other manner passed from his observation. Thus, materials would gradually be gathered together by the Association from which both the comparative prevalence and intensity of the disease at different periods, in different localities, and under different local circumstances, might be estimated. This question of the variations in the prevalence and intensity of disease under diversity of conditions has not received that share of attention from inquirers into the public health, which its importance demands—perhaps mainly because no means have heretofore existed for its satisfactory elucidation. From the Registrar-General's returns, the death-rates of individual diseases for the metropolis may be computed, but no means exist for calculating the district death-rates of special diseases, still less of estimating the comparative intensity of disease in different localities, as indicated by the proportion of deaths to cases. This want would be at least partially supplied in regard to fever, if our suggestions were adopted. The entire value of the proposed returns will depend on the manner in which the schedules are filled up, and particularly upon the returns being made after an uniform plan. We would direct attention especially to the columns on destitution and over-crowding, as demanding the utmost possible uniformity and accuracy, and to the importance of noting in the column for remarks the existence of any open ditch, foul stream, or other source of malarious emanations, in such propinquity to the patient's dwelling as to vitiate the internal atmosphere, or otherwise to exert any probable influence over health.

The Report on Unwholesome Meat is, we conclude, only of a preliminary character, and has doubtless been hastily prepared for the purpose of making the Inspectors of Nuisances acquainted with the signs of unwholesome and diseased meat. Probably on this account it is that the ill effects arising from the use of unwholesome meat are touched on very briefly, and without the citation of a single fact in proof of the general assertion, that several members of the Committee have met with instances in which symptoms of poisoning have been caused by the use of partially cooked or unsound meat, and, according to Dr. Druitt, even of over-fat meat. We could have wished the Report had contained some clear and explicit account of the circumstances under which the symptoms of poisoning referred to occurred, as well as of the nature of the indisposition. It is indeed true that the Committee mention debility, cachexia, poverty of blood, diarrhœa, and other intractable maladies, as among the evils which, they have no doubt, are caused by the use of unwholesome meat, although, they add, it may be difficult to prove the correctness of the opinion by actual cases. We trust that no such statement would have been admitted into the Report unless the Committee had been prepared to furnish some specific evidence in support of it in their next publication.

The important subject of parasites is summed up in the assertion that "the eating of measly pork, and of ill-cooked animal food in general, is notoriously a cause of tapeworm and of various forms of hydatid that infest the human subject." Surely it cannot be meant that the eating of ill-cooked meat can, under any circumstances, produce parasites in the human body, unless the germs already existed in the animal used for food! We fancied that the idea of the spontaneous generation of parasites had been long since exploded. No reference is made either to the diffuse cellular inflammation caused by the application of the fluids of over-driven animals to scratches, or to the asserted occasional production of gangrenous boils, *pustules malignes*, by the application of the same fluids, or of the fluids of animals suffering from carbuncular disease, to the unbroken skin. Neither is any reference made to the analogous disorder which is believed to be sometimes produced in Germany in persons who eat the flesh, as well as in those that handle the skin and other parts of animals suffering from an epidemic called *Milzbrand.* These subjects merited at least a passing notice in this preliminary Report, if only for the purpose of directing inquiry towards their confirmation or disproval. In reference to the production of carbuncles and boils by eating the flesh of diseased animals, it is not unworthy of note, that carbuncles and furuncles have been unusually prevalent in this country of late years, since there have been considerable importations of cattle from the Continent. Let it not, however, be inferred that we connect the two circumstances as cause and effect; we only point out their coincidence as suggestive of investigation. As the production of carbuncular disease in man is attributed to handling the carcase as well as to eating the flesh of diseased animals, it would form an interesting subject of inquiry whether butchers engaged in the slaughter of foreign cattle have especially suffered from boils, carbuncles, or the other disorders attributed by the German writers to this cause.

No one can have studied our literature on Hygeiology—a term first suggested by Dr. Farr, and more in keeping with our scientific nomenclature than the French word Hygiène—without having been impressed with its oftentimes loose, inaccurate, and unscientific character. This has probably, in some measure, arisen from the comparative freshness of the subject, but much more from official sanitary inquiries having been for the most part conducted by persons who lacked that special professional training and experience of disease, essential to the right appreciation and due sifting of medical evidence upon the causes of ill-health and aggravated mortality. Two great mistakes have arisen—at least in part—from this cause: the almost exclusive reference of aggravated mortality to what are termed structural defects, and the assertion that zymotic diseases are the chief causes of this mortality. It has thus become common, even among medical men, to speak of zymotic disease and preventible disease as synonymous terms. The most cursory consideration shows that several different kinds of disease are classed under the term zymotic—diseases, namely, which arise from an external and removable cause, as well as diseases which, so far as we know, depend upon personal contact either with an already infected person, with the atmosphere he has breathed, or with some article which has become charged with his diseased excretions. Probably, also, some of the so-called zymotic diseases are chiefly dependent on atmospheric causes. Ague affords an apt illustration of the first of these classes, scarlatina and small-pox of the second, croup and influenza of the last.

As regards the pernicious influence exerted over health by the emanations from putrefying organic matter, by the breathing of an atmosphere already partially saturated with the products of respiration, and of imbibing unwholesome water, there can be no doubt; but to point to "localized filth accompanied by moisture, as the great cause of disease and death;" or to assert "that the causes of excessive disease and death are similar in all towns, and that the diseases which create the excessive mortality are the same in all towns," as Mr. Lee, one of the non-medical inspectors of the former Board of Health, asserts, is utterly opposed to facts, and calculated only to mislead. Entertaining such views, it is no wonder that the execution of comprehensive works, as they are termed, has been held out by this school of sanitary reformers as the alpha and omega of preventive medicine. It is not surprising that such extravagant assertions should turn out unfounded, and that disappointment should have been expressed in towns where considerable expense has been incurred for drainage and water-works, at the subsequent recurrence of some of these so-called preventible diseases—for the public is ever more ready to mark an apparent failure than to acknowledge a silent but unquestionable benefit. We are induced to dwell thus at length upon these current dogmas of sanitary science, because they appear to be adopted even by the Officers of Health themselves. Thus Dr. Tripe, in his Report on the Sanitary Condition of the Hackney District, says: "There must be many preventible causes of disease in operation to account for the comparatively large mortality from ordinary epidemic diseases;" and Mr. Moore congratulates the Local Board of Health on the improved state of the public health of Leicester in 1853, more particularly because he finds, "on examination of the returns, that a diminution more than equal to the decrease in the mortality, exists in the zymotic class alone—a class which comprises fever, small-pox, diarrhœa, measles, &c., or what are usually termed preventible diseases." Although, on analysing the causes of death in Leicester, for the years 1852 and 1853, we find that the diminution in the aggregate mortality from small-pox, scarlatina, and hooping-cough—which amounted to 188—exceeded the diminution in the gross mortality, as corrected for the increase of population, by 67, we should be far from attributing this happy change to any preventive measures adopted within that brief period. There had, on the other hand, been no diminution in the mortality from diarrhœa and continued fever—diseases which are probably the most immediately influenced by general sanitary improvements—or from convulsions and teething, which perhaps follow next in the series of mitigable or removable diseases.

Setting aside fever and cholera—including under the latter the annual choleraic diarrhœa, so justly said by Dr. Farr to be evidently a variety of cholera*—which, if not absolutely caused thereby, yet evidently find the conditions of their development in certain well-marked local circumstances peculiar to each, the several diseases of strumous origin are probably those, the production and aggravation of which are most dependent upon the causes of ill health to which the dwellers in the unhealthy parts of towns are exposed. The correctness of this opinion derives support from the circumstance that the annual number of deaths from strumous diseases has diminished *pari passu* with the diminished mortality of London, and in much larger proportion than that of any other kind of disease. Exclusive of phthisis, the correct diagnosis of which anterior to the use of auscultation must be questionable, the annual death-rate from the principal forms of strumous disease in London, between 1681 and 1690 inclusive, amounted to 80 in each 10,000 of the population. The death-rate of analogous diseases at the present time, also calculated over an average of ten years, is only 20 in each 10,000—a diminution so remarkable as to prove that these diseases vary greatly with the external circumstances of the people. There is, moreover, no lack of proof that even phthisis is to a considerable extent caused, or at least developed, by some definite and probably removable local conditions. Here, again, since we find a large mortality from phthisis, and a small mortality from diseases of the organs of respiration, returned from some districts, and *vice versâ*, we must, for the purpose of comparison, throw the two into one class, in order to obviate errors of diagnosis. Taking, then, the mortality of 1841, when the causes of death in each registration district were published by the Registrar-General, whilst the census also taken in that year enables us to calculate the death-rates of diseases approximatively, we find that whilst diseases of the organs of respiration and phthisis collectively produced 118 deaths out of each 10,000 males residing in Liverpool, 90 in Manchester, 80 in Hull, and 70 in Leeds and Hunslet, only 55 deaths occurred from the same causes out of each 10,000 males living in York and Taunton, 53 in Spalding, Holbeach, and Boston, and 39 in Bridgewater. There can, we imagine, be no doubt that this very wide diversity of death-rates must be attributable to some definite local causes, whether of permanent action or varying with place and circumstance, that exist in a more aggravated degree in the one class of towns than in the other.

Here, then—in the investigation of the ætiology of strumous diseases, including phthisis, of fever and cholera, of diarrhœa, and, we may add, of bronchitis—lies the rich mine from the working of which, without disregarding other diseases, the metropolitan and provincial Officers of

* Seventeenth Annual Report of the Registrar-General, Appendix, p. 75.

Health may hope to reap the best reward of their exertions; in which they may best subserve their specific function of preventing unnecessary disease and premature death, and may win golden opinions as to the value of their labours. To insure such results, it is furthermore necessary that they should work in unison, that there should be a strict uniformity in their mode of collecting and recording facts, and in the main branches of investigation to be followed out. It is no less essential to success, that the parochial medical officers, and other officers of unions, should afford all the assistance in their power in furtherance of such investigations, and even to a certain extent be placed under the direction of the district medical Officer of Health. If, for example, the parochial medical officers were required periodically to furnish an accurate and uniform register of all cases attended by them, a large amount of valuable information would gradually be accumulated. Of course, to obviate professional jealousy, and to insure the satisfactory assistance of the parochial officers, they ought to be remunerated for this extra work. Already very indifferently paid for their labours, it would be manifestly unfair to impose additional and onerous duties upon them without an adequate increase of stipend. If we are correctly informed, an arrangement of this nature has already been made in St. Pancras.

That the Medical Officers of Health are anxious to fulfil their duties faithfully, and to work in conjunction for the public good, is patent from the proceedings we have already detailed. Whilst, therefore, we would desire, if possible, to strengthen their position, and to confirm their present most excellent intentions, we would venture to urge upon them the paramount importance of accuracy in observing, and precision in recording, all facts bearing upon the causation of disease. The first step towards the attainment of these objects would consist in the adoption of some definite and simple classification and nomenclature of disease, so that the same term might under all circumstances convey the same meaning. This would apply not only to different, but likewise to the same disease, the different forms and stages of which should, where necessary, be clearly defined. Thus, for example, in reporting cases or deaths of scarlatina, not only should the variety be correctly designated, but it should be thoroughly understood what definite meaning is attached to such terms as scarlatina simplex, anginosa, or maligna. Again, when fatal, the period of the disease at which it was fatal should be correctly noted; and if the death be occasioned by dropsy, this should be recorded as scarlatinal dropsy, and not simply as scarlatina or dropsy. As we have already intimated in reference to fever, the result of each case should be invariably recorded; and to obviate the confusion alleged by Dr. Ballard to arise from the capriciousness of patients, it would be sufficient simply to note the fact of a patient passing from the observation of the medical officer prior to the final result in death or recovery.

. The value of such information would be greatly enhanced if the population of each street and court in the district were likewise ascertained; and this, although attended by considerable labour, might, together with several other useful facts relative to social position, over-crowding, ventilation, &c., be readily procured by the inspectors of nuisances at their periodical inspections. There are columns for noting several of the circumstances we have named, in the forms for the weekly returns of sanitary inspection, employed by Mr. Simon whilst Medical Officer of Health to the City of London.* There is even a vacancy for the number of present inmates of any holding in which there may have been recent death or illness: our suggestion, therefore, does but require an extension of the plan so successfully worked under his guidance. Such inquiries might in the first place be limited to certain well-defined parts of a district, the population of each street, court, house, and room in which being obtained, the general death-rate, the death-rates of particular diseases, and, with the aid of the parochial medical officers' reports, the intensity of disease within the area, might be estimated. We might thus, after a time, be able to appreciate the effect of well-marked local and social conditions, both upon the public health and mortality, and upon the causation and intensity of special diseases. Mr. Michael, Medical Officer of Health for Swansea, has already attempted an inquiry of the kind here suggested. In his Report for 1854 are tables intended to show the per-centage of deaths in the several streets of that borough. The intelligibility and value of these tables would have been much enhanced if they had contained columns for the population and number of deaths in each street, and been accompanied by an explanation of the term per-centage, and some account of the condition of the localities and their inhabitants. In their present form they are practically useless to the distant inquirer. Notwithstanding this defect, which we trust to see remedied hereafter, Mr. Michael's Report is one of the ablest of the provincial reports we have perused.

A much more complete and, at first sight, more valuable table, professing to supply the same kind of information, but accompanied by the details referred to as *desiderata* in the Swansea Report, is given in Mr. Piper's Report to the Darlington Local Board of Health for 1853. On turning, however, to Mr. Inspector Ranger's Report to the General Board of Health, dated October, 1849, we find a similar table, professing to furnish the death-rates of the same streets for the seven preceding years, and observe that the population of each street in 1853, exactly corresponds with that given in the table compiled four years previously. This discovery throws entire discredit on both tables, and removes all surprise at the great variations apparent in the death-rates of the same streets at the two periods. In the Report of Mr. Oliver, medical officer of health for the City of Durham, for 1854, is a table, showing the number of deaths in each street from each cause. It is to be regretted that this table also loses its chief value from the absence of information relative to the population and condition of the several streets. It is, however, sufficiently evident that all these gentlemen are anxious to perform their duty conscientiously and efficiently, and that the mistakes we have pointed out have not arisen from the want either of zeal or industry.

It is obvious that any deductions from the death-rate of small districts

* Report on the Sanitary Condition of the City of London for the year 1854–5.

would only be reliable if the observations upon which they were based extended over a period sufficiently long to obviate the fallacies that arise from variations in the prevalence of disease, such variations of course being more likely to vitiate the calculations in small than in large districts. Moreover, their value in regard to the causation of disease would entirely depend upon the precision with which they were compared with the local conditions of the district and its inhabitants. Not only should the state of houses as to ventilation, overcrowding, and impure atmosphere, be accurately recorded after the plan employed by Mr. Simon in the City of London, and adopted in the schedule for taking notes to illustrate the ætiology of fever; but other circumstances, as the influence of etiolation and diet, would form important elements of the inquiry. For example, in cases of infantile disease and mortality, the question whether the child had been naturally or artificially nursed, would be very important; for, without under-rating the injurious effects of insalubrious residence, we are well assured that defective nutrition, whether this arise from the use of a food unsuited to the physiological requirements of the organism, and therefore incapable of perfect digestion and healthy assimilation, or from a positive deficiency of food, is a very frequent and influential cause of illness and premature death among the children of towns. The influence on health of residing in cellars, half dark as they always are, damp and ill-ventilated as they usually are; and, of living over stables, where the atmosphere is often highly charged with ammoniacal vapours, would form other important subjects of special inquiry.

Furthermore, any inquiry into the health of districts and the causes of disease, would be most incomplete which did not extend to the occupations of their inhabitants and to the effect which these exert over health and mortality. From a table appended to the Fourteenth Annual Report of the Registrar-General, we learn that whilst 18 out of 1000 males, of all classes, living between the ages of forty-five and fifty-five, die annually, the death-rate per thousand of miners belonging to the same sex and period of life is 20; of bakers, 21; of butchers, 23; and of inn and beer-shop keepers, 28, respectively. On the other hand, only 12 farmers, 15 shoemakers, 15 weavers, 16 grocers, and 17 blacksmiths, carpenters, tailors, or labourers, annually die out of each 1000 males living of these occupations respectively, between the forty-fifth and fifty-fifth years of life. Here, then, is a very important subject of investigation in connexion with the public health of districts, the neglect of which must impair the value of conclusions drawn from other data.

We find from Dr. Letheby's Report on the Sanitary Condition of the City of London for 1855–56, that this subject has engaged his attention, although, not having the means of calculating the death-rates of each class precisely, he has been unable to arrive at any reliable results. The table into which Dr. Letheby has cast his data is nevertheless very interesting, and would become still more so if additional columns for the chief causes of death were added, so as to include diseases of the organs of respiration, diarrhœa, &c. It is interesting to observe that Dr. Letheby's investigations confirm the opinion long since expressed by Thackrah,* that butchers are less liable to phthisis than the rest of the community. Considering the small mortality from phthisis among butchers, it would be interesting to learn what other diseases serve to supply the hiatus thereby left—further increased as this is by the large excess of mortality from all causes in butchers over the average of the general population. The value of Dr. Letheby's future inquiries would be much enhanced if he could give the occupational death-rates of individual diseases—an inquiry more likely to be correct if made in the City of London than anywhere, from the smaller fluctuations in its population.

The most cursory consideration of the several subjects at which we have glanced, will serve to show how wide is the field of sanitary investigation yet almost unbroken, and how rich and important the harvest to be reaped from its cultivation. But in truth we have very far from exhausted the subjects of inquiry. We have as yet only entered on the threshold of preventive medicine, as applied to the maintenance and improvement of the public health; and the subject is one that daily grows in width and importance from the daily-increasing growth of our unhealthier town population in excess of the healthier rural community. It is this—the larger portion of the nation that now resides in towns—that has rendered the general death-rate retrogressive of late years. It is this increasing death-rate for the community at large which renders the prompt and careful prosecution of such investigations as we have suggested so imperatively urgent.

Since, then, the welfare of the general community is so intimately allied with sanitary investigations, the question arises, whether it be possible for single inquirers to cultivate so extensive a field; whether investigations so necessary for the common weal ought not rather to be undertaken by the State; and whether, whilst the details of local inquiry may be best worked out by local authorities and their officers, the philosophical generalization of the gross materials thereby collected would not be better done under the direction of some superior superintending authority. We incline to this opinion, and think that just as the registration of births, marriages, and deaths; the superintendence of local pauper relief and expenditure; the inspection of factories and the custody of lunatics; are placed under the administration of public boards, so ought all inquiries into the influence of locality, circumstance, and occupation over the health and mortality of the public, to be directed and superintended by the General Board of Health. The metropolitan officers of health are at present actually, and their provincial brethren virtually, independent of the Board of Health. Hence there is no necessary uniformity of action between separate districts. If the Board of Health possessed the power of investigating the effect of supposed injurious agents upon health; of inquiring into the state of the public health in towns where improvements have been effected; of examining into the facts upon which the reports of officers of health are founded; and if, whilst leaving much of the detail to be arranged, as at present, by the local authorities, it was

* The Effects of Arts, Trades, and Professions, &c., as affecting Health and Longevity. By C. Turner Thackrah, Esq. Second edition. London 1832, pp. 10–12.

empowered to require certain periodical returns prepared after an uniform plan, sanitary science would gradually, and at no distant day, be redeemed from its present crude and uncertain state. The expense of the necessary staff ought to form no obstacle to inquiries of so important a character, and would neither be objected to by the House of Commons nor the public, if the case were fairly represented.

Fully to realize this suggestion, it would be necessary to place the sanitary inspection of workhouses, factories, prisons, and other analogous establishments, under the surveillance of the Board of Health, even though it should be deemed right to refer ali improvements and recommendations to the several boards whose special function it is to regulate these institutions. Thus the same Medical Inspector of the Board of Health who assisted the local authority in its sanitary labours, might report upon the sanitary condition of the prisons, workhouses, factories, and other public establishments of the district. His wider field of observation and his acquaintance with the public health and condition of other districts, would enable him to generalize the facts placed before him with greater accuracy and precision than the local officer of health, who would usually be glad to avail himself of such aid. It would be easy, did our space admit, to refer to special cases—as, for instance, the St. Pancras Workhouse—in which such a system of superintendence might have averted evils which have already occurred. We are persuaded that in many cases, if the existence of evils prejudicial to health were pointed out by a properly skilled officer, the best practicable means would be at once adopted to remedy them. Such, at least, we feel sure would be the case in factories and other private establishments.

Review IX.

'Αρεταίου Καππαδόκου τὰ Σωζόμενα. *The extant Works of Aretæus the Cappadocian.* Edited and translated by Francis Adams, LL.D. Printed for the Sydenham Society.—*London*, 1856. 8vo, pp. xx. 510.

"The Sydenham Society" (as we are told in a paper issued in 1855) "was instituted in 1843, with the view of supplying its members with Standard Medical Works;" or, as it is somewhat more periphrastically expressed a few lines below, "for the purpose of meeting certain acknowledged deficiencies in existing means for diffusing medical literature, which are not likely to be supplied by the efforts of individuals." The history of the Society up to the present time is contained in fourteen Annual Reports, from which we learn that upwards of 23,000*l.* have passed through the hands of the Treasurer. Of this large sum, about 5100*l.* have been expended in the purchase of paper; about 5800*l.* in printing; about 3200*l.* in bookbinding; about 3400*l.* in editorial expenses; and about 4400*l.* in the salaries of the secretary, clerk, and collector, in rent, office expences, and delivery of books. The smaller items in the statements of accounts we have not been at the pains to add together, and the above abstract is sufficient for our purpose; but a more minute analysis of the receipts and expenditure of the Society might not be uninteresting to the members generally, and especially to the Council in preparing for the next Annual Meeting on the 1st of May.

The result of the operations of the Society has been the production of nearly forty volumes; and though there are few competent persons that would approve of the selection and execution of *all* these works, there are probably still fewer who would be disposed to deny their literary and scientific merits *as a whole*, or to assert that they are (as a collection) unworthy of the character of the Society under whose auspices they have been given to the world. And again—to take another view of the proceedings of the Society—though it is quite possible that part of this large sum of money may turn out to have been wasted (though unintentionally at the time), and though it is also possible that some retrenchments may be effected for the future; still, we think that no one acquainted with the practical working of such a society can examine the annual statements of accounts that have been published, without being convinced that the sums entrusted to the Council from year to year have been, upon the whole, fairly, carefully, and judiciously expended.

We are the more willing to state thus distinctly the result of our convictions, after being subscribers from the beginning, because we think the Society has been exposed to unfair attacks and animadversions, and has not latterly received from the profession the degree of support to which it seems to us to be entitled. We were glad to see in the last Annual Report a considerable increase in the income of 1855 over that of the preceding year, and hope the statement of accounts next May will be still more satisfactory. In the meantime, one of the best proofs of the estimation in which the works issued by the Society are held by the public, is the high price which they bear in second-hand catalogues, there being few of the volumes that are to be procured much under the cost price, while some are actually sold above it.

The last work issued by the Society, Dr. Adams's 'Aretæus,' is the most notable in this respect—viz., that it is the only ancient medical author published in the original tongue. The reprinting the original texts of these writers was, from the beginning, one of the objects of the Society, and the publication of the present volume shows that it has not been entirely lost sight of; still, we cannot but think, that, if this had been done somewhat more frequently, it would have raised the reputation of the Society abroad, without materially diminishing its popularity at home. There are several other Greek and Latin works which we still hope to receive from the Sydenham Society, as they are hardly likely to be undertaken (at least in this country) by individuals. The selection of Aretæus for publication by the Council was quite unexceptionable. The work is one of the most interesting and valuable of the old medical classics; it is also one of the most celebrated—ranking, in this respect, nearly on a par with Hippocrates, Celsus, and Galen. Then, again, Aretæus is an *original* writer, and therefore widely different from Paulus Ægineta, Aëtius, or Oribasius; he is also an elegant and sensible writer, and his descriptions of disease may (in many cases) be read with pleasure and profit by practitioners even in the present day. Lastly, the Greek editions of his works are somewhat scarce and expensive; and the two previous English translations are incomplete. In every respect, therefore, we think Aretæus one of the most fitting of the old medical writers to be reprinted in Greek and English by the Sydenham Society; and if the execution of the volume is not altogether such as to eclipse the editions of Freind and Ermerins, or to supersede the necessity of the one which we may expect in due time from Dr. Daremberg, in Paris, still, the work is highly creditable to the translator and editor, and certainly very acceptable to the more learned portion of the medical profession in this country.

The volume contains, first, a short Advertisement by Dr. Adams, and then his Preface, in which he treats (§ 1) of the age of Aretæus, his doctrines and character as a medical author; and (§ 2) of the editions of his works. The former part is somewhat meagre, but at the same time might have been compressed without disadvantage. The bibliography ought to have been at least as complete as is to be found elsewhere; but Choulant (in his 'Handbuch der Bücherkunde für die Aeltere Medicin') gives us more matter in fewer words.

After the preface follows the Greek text, with English notes (chiefly philological) at the foot of the page. Dr. Adams has not adopted the text of any preceding editor, but has formed one for himself, after "diligently examining the text in all the existing editions, and collating with some care all the MSS. to be found in Great Britain—namely, the three following:" one in the British Museum; one in the library of the Medical Society of London; and one* in the library of Sir Thomas Phillipps, of Middle-Hill, Worcestershire. This statement is not sufficiently definite; nor do we know to what extent Dr. Adams has made use of these MSS., nor in what degree of estimation he holds them. They are not often referred to in the notes, so that altogether we doubt whether any future editor of Aretæus will hold himself excused from the labour of collating them anew—at least, those which have been used for the first time for this edition.

The text of Aretæus offers peculiar difficulties to an editor, partly from the imperfect state in which the MSS. have come down to us, and partly from the author having, for some reason or other (possibly in imitation of Hippocrates), chosen to compose his work in the Ionic dialect. In some instances, Dr. Adams seems to us to have improved the text, in others to have left it worse than he found it. But this is no more than what we should say of Dr. Ermerins, and we say it of both these learned men, with a full consciousness that in any given instance in which we may be inclined to think either of them wrong, they are quite as likely to be right as ourselves. The corrections (or rather conjectural emendations) of Dr. Ermerins certainly appear to us to be far too sweeping, and in many cases unnecessary; and, accordingly, Dr. Adams has acted judiciously in rejecting a great number. On the other hand, his own emendations are not always

* It appears that there are *two* MSS. of Aretæus at Middle-Hill, thus making *four* altogether in England. (See Dr. Daremberg's *Manuscrits Médicaux Grecs d'Angleterre*, pp. 142, 147.) Dr. Adams does not specify which of the Middle-Hill MSS. he has collated;—but perhaps this is not of much consequence.

such as we can approve, though some are excellent. We cannot, of course, in this place examine his text as fully as it deserves, but we will give two or three instances as illustrations of what we have said.

"The common reading in the MSS. is εἴδεος, which is evidently inadmissible. Petit suggests that the true reading is δέος ᾖ. Wigan also prefers πλησίον εἴη δέος. Ermerins accordingly reads πλησίον ἔῃ δέος. None of these editors, however, refers to any authority for this expression, which appears to me quaint and unnatural—*but if the dread of a paroxysm be at hand.* I prefer ἤδη on the authority of many parallel passages," &c. (p. 4).

Accordingly, Dr. Adams translates the passage, "if it be near the accession of the paroxysm." The emendation is ingenious and plausible, if not absolutely necessary.

"The reading in all the MSS. is ὦ κύφων οὖ—words which evidently have no meaning. . . . . The emendation suggested by Fabricius and Maittaire, and adopted by Wigan (viz., ὠκυφόνου), is so plausible, and requires so little change of the characters, that I should have had no hesitation in adopting it, provided there were any authority for it; but this, I fear, is not the case. I have therefore ventured to substitute ὠκυτάτου in place of it; and I am persuaded that, upon mature consideration, it will be approved of by every competent judge." (p. 29.)

At the risk even of having the "competency" of our critical "judgment" called in question, we must confess that we are quite unable to see the superiority of Dr. Adams's emendation over that of Fabricius and Maittaire; for even if the word ὠκύφονος has not hitherto been known by lexicographers to exist, and therefore does not (as far as we are aware) appear in any Greek lexicon, yet analogy is by no means against it, either as to form or meaning.

"I flatter myself that every person who is familiar with the metaphysics of the ancients, will admit that I have improved this sentence by changing ἤδε into ἡδέ." (p. 58.)

It is very possible; but it does not appear that Dr. Adams is the original author of the emendation, as certainly his mode of expressing himself in the above note would *seem* to imply, though probably he himself had no such intention.

"I at first changed, &c. . . . . But, upon second thoughts, by a slight change of the punctuation, I flatter myself that I have brought the text to a passable state." (p. 78.)

We are inclined to doubt whether future editors will be of this opinion, especially when they find that Dr. Adams is unable to construe grammatically his own amended text.

"The following most extraordinary reading occurs in all the MSS. :—καὶ ξυμπάντων τε ὁ βοῦς, ὄνος. . . . . What can an *ox* or an *ass* have to do with an attack of schiatica? Fortunately the Askew MS. belonging to the Medical Society of London, has guided me to what I do not hesitate to pronounce the true reading. . . . . In a word, the substitution of τοῦ βουβῶνος for τε ὁ βοῦς, ὄνος, is so natural, that I feel persuaded no reasonable critic will demur to adopt it. Some other slight changes, however, are requisite in this passage, which might be effected in two or three different ways. I subjoin a few of my own attempts," &c., &c. (p. 120.)

We fear Dr. Adams will think us incorrigibly dull or perverse, but here also we are inclined to prefer Wigan's emendation of ὁμοῦ πόνος (which is adopted by Ermerins), to the conjecture of the unknown amanuensis of the sixteenth century, especially as it seems to us to be very doubtful whether Dr. Adams's text can be construed grammatically.

In p. 339, in the chapter On Diabetes, occurs a passage which is thus translated by Dr. Adams:

> "Hence, the disease appears to me to have got the name of *diabetes*, as if from the Greek word διαβήτης (*which signifies a siphon*), because the fluid does not remain in the body, but uses the man's body as a ladder (διαβάθρη) whereby to leave it."

The note on this passage is as follows:

> "Altogether, this interpretation is so unsatisfactory, that I was almost tempted to alter the text quite differently from Wigan and Ermerins. . . . At all events, the reading of Wigan and Ermerins seems inadmissible; for how can the two comparisons, to a *siphon*, and to a *ladder*, be admitted together? It is possible, however, that διαβάθρῃ is faulty, and that we ought to read διαβήτῃ."

This emendation, which Dr. Adams does not venture to introduce into the text, and of which he speaks so doubtfully and hesitatingly in his note, appears to us to be one of the best in the book—indeed, so necessary, and at the same time so obvious, that it is strange that no previous editor or commentator should have hit upon it. The present text makes Aretæus say that the name *Diabetes* is derived from the Greek word for a *siphon*, because the fluid makes use of the patient's body as a *ladder* (or *bridge*, as Reynolds has it in his translation), whereby to escape. The inconclusiveness of the reason is manifest, to say nothing of the absurdity of water escaping either by a *bridge* or a *ladder*. By adopting Dr. Adams's emendation (as no doubt all future editors will do), the proper sense is at once restored—viz., that the fluid uses the man's body as a *siphon* whereby to leave it.

In p. 207, Dr. Adams has, in our opinion, given a better text than any former editor, but his note seems to require some such additional words as those which we have inserted within brackets:

> "The common reading, instead of μέγα, is μετά; in Kühn's edition [by a typographical error], κατά. [Wigan reads μέτα—i.e., μέτεστι; as does also Ermerins, who changes τὸ ἄκος into τοῦ ἄκεος. Petit's correction, which is] the reading I have introduced, seems to me self-evident."

It is always better to state distinctly whether a successful emendation of the text is original or borrowed.

Upon the whole, Dr. Adams's text is probably superior to that of any preceding editor, as his translation is undoubtedly better than either of the former ones. The previous quotations will give some idea of the general style of his critical notes, which are not unfrequently somewhat diffuse in form, and unwarrantably confident in tone. The following chapters (selected in a great measure on account of their brevity) will enable the reader to judge of the merits of his translation:

> "*On the Causes and Symptoms of Chronic Diseases. The Procœmium.*—Of chronic diseases the pain is great, the period of wasting long, and the recovery uncertain; for either they are not dispelled at all, or the diseases relapse upon any slight error; for neither have the patients resolution to persevere to the end; or, if they do persevere, they commit blunders in a prolonged regimen.

And if there also be the suffering from a painful system of cure—of thirst, of hunger, of bitter and harsh medicines, of cutting or burning—of all which there is sometimes need in protracted diseases, the patients resile [?], as truly preferring even death itself. Hence, indeed, is developed the talent of the medical man, his perseverance, his skill in diversifying the treatment, and conceding such pleasant things as will do no harm, and in giving encouragement. But the patient also ought to be courageous, and co-operate with the physician against the disease. For, taking a firm grasp of the body, the disease not only wastes and corrodes it quickly, but frequently disorders the senses—nay, even deranges the soul by the intemperament of the body. Such we know mania and melancholy to be, of which I will treat afterwards. At the present time I shall give an account of Cephalæa." (p. 293.)

"*On the Cure of Chronic Diseases. The Proœmium.*—In chronic diseases, the postponement of medical treatment is a bad thing; for, by procrastination, they pass into incurable affections, being of such a nature that they do not readily go off if they once attack; and if protracted by time, they will become strong, and end only in death. Small diseases also are succeeded by greater, so that, although devoid of danger at first, their progeny proves deadly. Wherefore neither should the patient conceal his complaint, from the shame of exposure, nor shrink from fear of the treatment; nor should the physician be inactive, for thus both would conspire to render the disease incurable. Some patients, from ignorance of the present and what will come at last, are content to live on with the disease. For since in most cases they do not die, so neither do they fear death, nor, for this reason, do they entrust themselves to the physician. Cephalæa, of which I am about to treat in the first place, is a proof of these statements." (p. 457.)

The translation is entirely separated from the Greek text, a plan which has its advantages, though upon the whole we are inclined to prefer having it on the same or the opposite page. But the reader ought, at any rate, to have been enabled to refer from the text to the translation (or *vice versâ*) with tolerable facility, either by the chapters being noted at the top of the pages, or by the pages being noted in the margin, or even by the help of a table of contents at the beginning of the volume. This the reader cannot do; nor has he any means of finding the corresponding passage of the Greek text in any of the former editions, a convenience which is so much valued in the volumes of Kühn's collection. The absence of these conveniences renders Dr. Adams's volume one of the most troublesome to use that we have ever met with—always excepting that of Dr. Ermerins, which is, in this respect, one degree worse.

There are sundry other blemishes and omissions which render the edition less complete and valuable than it might (and indeed *should*) have been made. The punctuation seems to us to be inferior to that of Ermerins's edition, and the sentences in the Greek text and in the English translation do not always correspond. For facility of reference the chapters should have been divided into sections of convenient length, and these should have been numbered, as is done in Dr. Daremberg's series now publishing in Paris, and in most of the best editions of classical authors. Dr. Daremberg also gives the most important *various readings*, as does also Dr. Ermerins; but we find a very scanty supply in this volume. The editor, indeed, excuses this omission by saying that if he had given *all*, it would have required more than sixty pages (p. xix.). Probably no one would have wished him to reprint them *all*, but a judicious selection would have given satisfaction to many of his readers, and have added to the critical value of his book.

Another defect is the want of proper indices, in which respect this edition is inferior to those of Wigan, Kühn, and Ermerins. It contains only an index applicable to the translation, "constructed very much upon the plan of Wigan's," but (in several instances in which we have compared the two) less complete. "An index to the text," says Dr. Adams, "would have been valued by so few of my readers, that I did not think of undertaking so very formidable a task." But this "formidable task" had already been undertaken and executed by Maittaire and Ermerins; and their labours might, with comparatively little trouble, have been adapted by Dr. Adams to his own edition. Even if the editor's courage failed him at the thoughts of undertaking an *index verborum*, he might have given us the short index of drugs and pharmaceutical compounds, which lay ready prepared by Dr. Ermerins for his use. An index of this sort would have been the shortest mode of explaining these articles, which at present are wholly unintelligible to most readers. Surely the experience gained from Paulus Ægineta might have convinced Dr. Adams that when people meet with such strange names as "that from the two peppers, that of Symphon, that of Philo, the liquid medicine from the wild creature the skink, that of Vestinus, that from the reptiles the vipers" (p. 489), they wish (and not unreasonably) to be told what they are reading about, or else at any rate to be assured that no satisfactory explanation can be given. There are several other words, names, and allusions in the text, which certainly ought to have been explained and illustrated in the notes, but which the editor has passed over in silence.

We have felt especially called upon to make these remarks on the defects of the work, because the appearance of a new edition of a Greek medical author in this country is an event in the literary world; and it might be imagined on the Continent that a volume brought out under the auspices of the Sydenham Society, and edited by such a man as Dr. Adams, approaches as nearly as possible to our British ideas of editorial perfection. And we may add that we have had the less scruple in saying what we have, because no one entertains a higher idea of the value of Dr. Adams's labours in the cause of ancient medical literature, than ourselves; and when at its first appearance his Paulus Ægineta was received with a storm of ignorant ridicule, we ventured to say a few words in its favour, which (we have reason to know) were not entirely without effect. We hope that Dr. Adams does not look upon himself as *donatus jam rude;* and that he may not think there remains no appropriate work for him to do, we would venture to suggest to him the completion of his translation of Hippocrates, by the addition of a third volume, containing the doubtful and spurious pieces that go under his name.

We have left ourselves no room to say more than a very few words about Aretæus himself, but this is of the less consequence, as there are few of the old medical writers about whom more has been written, and whose merits are more generally acknowledged. His name is tolerably familiar to the non-professional public in this country, from having been the subject of one of the late Sir Henry Halford's Essays read before "a mixed audience" at the College of Physicians; while his physiological and pathological merits were examined in a paper by Dr. David Badham in the *London Medical Gazette*, vol. xvi. 1835. The rather numerous papers and monographs published by foreign scholars on the subject of Aretæus we need not mention. We would therefore only warn those members of the Sydenham Society into whose hands these pages may fall, that in reading the chapters of Aretæus, they must bear in mind the state of philosophy and medical science at the time he wrote, and not judge him by the standard of the present day. There are, no doubt, very many expressions and sentences in his works which appear to us both ignorant and absurd, but we doubt whether there are many that appeared in the same light to his contemporaries; and therefore it is that we have a right to claim for Aretæus the same indulgence that must be extended to the very greatest names in philosophy and science—even to Bacon and Newton themselves.

REVIEW X.

An Exposition of the Signs and Symptoms of Pregnancy, with some other Papers on Subjects connected with Midwifery. By W. F. MONTGOMERY, A.M., M.D., M.R.I.A., &c. Second Edition.—*London*, 1856. pp. 706.

THE popularity attained by the first edition of this work, the length of time which has elapsed since its appearance, and the many additions which have been made to the present edition, claim for it a more lengthened and systematic notice than that which we usually accord to the second editions of works, however great their intrinsic merit; and yet we could scarcely hope to interest our readers by a mere fragmentary notice of the several emendations or additions which distinguish the present volume. We propose, therefore, to lay before them a concise account of the present state of our knowledge in regard to the signs and symptoms of pregnancy, founded upon the able exposition of the subject as embodied in the pages of this work.

Under ordinary circumstances, and in ordinary cases, nothing is perhaps easier than to diagnosticate the existence of pregnancy. When, for instance, a married woman experiences a cessation of menstruation, has morning sickness, an enlargement of, and shooting pains in, the breasts, with darkness of the areola,—followed in three months by enlargement of the abdomen, and subsequently feels the movements of the child, no doubt whatever can ordinarily exist upon the subject; but, unfortunately for diagnosis, these symptoms do not always accompany pregnancy—they may be absent in those who are pregnant, and present in those who are not. One person is pregnant who obstinately denies it; another is unwilling to believe it; whilst a third, who is not so, cannot give up the idea that she is. Under these circumstances, the investigation of the subject is beset with much difficulty and embarrassment, and the character of the practitioner is liable to be seriously injured by any error of judgment. To assist the one and protect the other, it is necessary to bear in mind not only the ordinary phenomena of pregnancy, but their chief aberrations also, and these we will endeavour very briefly and successively to indicate.

During the first three months the signs of pregnancy are of a somewhat equivocal character, and such as necessitate the greatest caution in the expression of an opinion. The first symptom is an omission of menstruation, which is soon followed by sickness, and this is most commonly felt on rising in the morning—diminishing, and at length ceasing, as the day advances. This is soon followed by an enlargement and increased sensibility of the breasts, development of the areola, and certain other mammary appearances to be hereafter described. In *rare* cases quickening may take place before the end of the third month, the dusky hue of the vagina may be perceptible, the dark abdominal line present, the placental souffle audible, as also the fœtal heart-beat, and the sounds produced by the movements of the child.

When three months have elapsed without menstruation, the abdomen begins to enlarge, and this enlargement gradually increases, so that during the fourth month a prominence can be perceived externally. The uterine tumour is now also in general well defined, and can be felt overtopping the anterior wall of the pelvis; and now also the umbilical depression begins to diminish. During the fifth month the fœtal movements are usually first felt by the mother, and may also be recognised by the hand externally applied. The uterine souffle and fœtal heart-beat may now also be heard; ballottement is available, and in most cases the dusky colour of the vagina and the dark abdominal line will have become distinctly perceptible. With these signs, the cervix uteri will be found to be fuller, rounder, softer, and more elastic when pressed by the finger; the areola of the breasts has now become more developed, and in some instances a peculiar mottled appearance, or secondary areola, to be hereafter described, is perceptible. In the sixth and subsequent months the development of the abdomen and enlargement of the uterine tumour go on progressively, the umbilicus becomes raised, the os uteri more patulous, the cervix uteri shortened; whilst above it the bulging body of the uterus may be felt, and within it the head of the child lying upon its anterior wall. Such are the progressive signs and characters of pregnancy in ordinary cases, but the aberrations in regard to them are so numerous in particular instances, that it will be necessary to enter briefly into an examination of each, in order to appreciate rightly the difficulties which are sometimes met with in our endeavours to determine the existence of pregnancy.

Menstrual Suppression.—In ordinary cases this is one of the earliest symptoms of pregnancy; but it may both co-exist with pregnancy, and occur from a variety of causes altogether unconnected with it. Thus cases are recorded in which conception took place previously to menstruation. In some women the return of the monthly periods is unusually prolonged; whilst at what is called the change of life the catamenia are often suppressed for two or three months together; and conception has been known to occur after menstruation had apparently ceased, as also in many persons who had not menstruated for many years previously. Menstrual suppression may, moreover, arise in married women from a variety of causes altogether independent of conception, such as different forms of disease, exposure to cold, hardship, and mental emotions, more particularly fear. Owing to constitutional peculiarity, menstruation becomes finally suppressed in some women at an unusually early age, and pregnancy may be erroneously supposed to be the cause. Conception may occur when the menses have been long suppressed in consequence of disease; or women may conceive while nursing, without any previous return of the catamenia. On the other hand, cases have been met with in which menstruation, or an appearance very similar, has appeared at one or more of the menstrual periods after conception has taken place; and in very rare cases, menstruation has either appeared for the first time after conception, or has continued to recur only during pregnancy. In a medico-legal point of view, it must also be remembered that menstruation may be simulated in pregnant females for the purposes of deception, whilst also the menses may be retained from an imperforate hymen, and accumulating within, and distending the vagina and uterus, may give rise to several of the sympathies of pregnancy. Such are some of the anomalies which occur in regard to menstruation as connected with the question of pregnancy.

Gastric Sympathies.—In general, when pregnancy has occurred, the stomach becomes irritable, and the woman is distressed with nausea and vomiting, especially in the early part of the day. Dr. Montgomery has met with cases in which this commenced almost immediately after conception, but it most frequently occurs for the first time after the first period of menstrual suppression. It may not, however, occur until after quickening, and in some it does not happen at all. In certain cases it may be a mere consequence of uterine disorder or disease, such as suppressed menstruation; but it may be observed that whereas the vomiting of pregnancy is not usually accompanied by any other symptom of ill health, an opposite condition is met with when it occurs from uterine derangement; and, moreover, that the sympathetic vomiting of pregnancy is often attended with a disposition to salivation or diarrhœa.

Mammary Sympathies.—These more particularly consist in an enlargement and increased sensibility of the breasts, the development of the areola, and the secretion of milk. When conception has taken place, and the catamenia have been suppressed for one or two periods, the woman generally becomes sensible of an alteration in the state of the breasts, consisting of an uneasy sensation of throbbing, or of fulness in them, accompanied by soreness and tingling pains about their centre and in the nipple. They now grow sensibly larger and more firm; the circle around the nipple becomes altered in colour and structure, constituting the areola, and as gestation advances, milk is secreted. In some instances these changes may be recognised very soon after conception, but in others not until gestation is far advanced, or even drawing to a close; the ordinary period of their appearance being about two months after pregnancy has commenced. These changes, however, it must be remembered, may be altogether unconnected with conception. In many women, the breasts enlarge merely in consequence of marriage; in others, this may happen from the person becoming fat; it may be caused by accidental suppression of the menses, or their retention by an imperforate hymen, or other causes capable of distending the uterus. In some women of an irritable habit, swelling and pain of the breasts accompany each return of the catamenia, especially if they are the subjects of dysmenorrhœa; but here the tension and uneasiness subside on the appearance of the discharge, and in the instance of enlargement produced merely by fat, the greater firmness of the breast, the presence of developed veins on its surface, and its being more knotty and uneven when pressed by the hand, sufficiently distinguish the enlargement of pregnancy. Lastly, a preternatural fulness of the breasts may be said to be natural to some persons, or it may take place at a period of life when the catamenia become naturally suppressed; here, however, the appearance of the areola affords an important aid to diagnosis. This usually begins to be developed towards the end of the second month of pregnancy; at first, the colour is little deeper than a shade of rose, tinged occasionally with a yellowish or light-brownish hue; but during the progress of the next two or three months, the changes in the areola are generally perfected, and it then presents the following characters—a circle around the nipple, whose colour varies in intensity according to the darker or fairer complexion of the individual, and varying in diameter from about an inch to an inch and a half—the extent of diameter, as well as depth of colour, increasing as pregnancy advances. In the centre of the coloured circle, the nipple is observed partaking of the altered colour of the part, and appearing turgid and prominent, whilst the surface of the areola is studded over by glandular particles, varying in number from twelve to twenty, and projecting from the surface from the sixteenth to the eighth of an inch. Lastly, the integument covering the part appears a little raised, softer, more turgescent and moist, than that which surrounds it; whilst on both numerous spots of a whitish colour may be observed, as though the colour had been discharged by a shower of drops falling on the part. This last appearance Dr. Montgomery has not observed earlier than the fifth month, and he considers it a strikingly distinctive character of pregnancy. Such, then, are the ordinary changes observed in the areola; but it is to be remembered, that it may not be developed until a much later period than usual, or it may remain deficient in one of its most important characters—viz., its darkened colour, and this more especially in persons of fair complexion. In some, the mammary sympathies are almost entirely wanting, or at most, very feebly exerted; and should the fœtus be blighted, the characters of the areola will soon decline and fade away, the breasts becoming flaccid, and losing sensibility. Again, in the case of a woman recently delivered, the breasts may exhibit all the true characters of the areola, without, of course, her being pregnant; and in nurses the characters of the areola are sometimes kept up for a long time in a state of considerable perfection. The last effect of the mammary sympathies is the secretion of milk in the breasts, which, although popularly regarded as an infallible proof of pregnancy, is yet found to occur in many cases under circumstances totally independent of it; moreover, milk may be retained in the breasts for an unusual length of time after nursing; and morbid causes acting upon the uterus may excite sympathetic changes in the breasts, even to the secretion of milk. Hence, this symptom in the abstract cannot be infallibly relied upon, but occurring in connexion with other symptoms of pregnancy, is of great importance, especially when occurring in the case of a person who has not previously been pregnant. It is scarcely, however, an available guide in doubtful cases; for, in the first place, in many instances milk is not secreted until after delivery; and, secondly, when it is formed during pregnancy, it is not until a period has arrived which presents other modes of judging less liable to uncertainty.

Quickening and the Movements of the Child are first felt from the tenth to the twenty-fifth week, but most frequently between the end of the twelfth and sixteenth week after conception; or, adopting another mode of calculation, between the fourteenth and eighteenth week after the last menstruation. Under ordinary circumstances, the phenomenon of quickening is attended with an unusual degree of nervous agitation, which not unfrequently ends in faintness, or even complete syncope. It has been known to occur as early as the twelfth, eleventh, or even tenth week after conception; and, on the other hand, may not happen until the sixth or seventh month. In some cases, however, its postponement is only apparent, as in those instances in which conception has occurred after some months of menstrual suppression; and besides the occasional postponement of this occurrence, it may be totally absent during the whole period of gestation. With the view of feeling or exciting the motions of the child *in utero*, such a manual examination of the abdomen should be made as we are accustomed to make when examining for a tumour in that cavity. Sometimes the simple application of the hand over the front of the abdomen is sufficient for the purpose; but at other times, we shall succeed best by the sudden application of the hand, previously rendered cold by immersion in cold water. During the fourth or fifth month the sensation communicated to the mother or examiner amounts to little more than a slight pat or throb; sometimes to scarcely more than a flutter, and at an earlier period will be found to be proportionately more feeble. In the sixth and seventh month, however, the distinctness of these motions is greatly increased, and we now become conscious that the moving body has considerable bulk.

Enlargement of the Abdomen, and State of the Umbilicus.—About the end of the third month the abdomen usually begins to enlarge, and from this period it continues to increase gradually from month to month in the same proportion as the development of the uterus proceeds. Two circumstances may, however, affect or retard this enlargement—first, the death of the fœtus; and secondly, any diminution in the inflation of the intestines, consequent upon early pregnancy. By external manipulation and internal examination, this enlargement may generally be distinguished from that dependent upon morbid states of the abdomen; whilst its coexistence with a healthy state of the patient, and the coincident mammary changes, materially aid the diagnosis. The changes in the umbilicus consequent upon pregnancy usually commence in the fourth month, at which period it is found to be less hollow than before conception; in the fifth or sixth month it is nearly on a level with the surrounding integuments; in the sixth or seventh month it is completely so; and towards the close of gestation it projects in most persons above the surface. In addition to these changes, Dr. Montgomery directs attention to the occasional existence of an areola around it, and the presence of a coloured abdominal line, about a quarter of an inch in breadth, running from the pubes to the umbilicus, and not unfrequently thence to the ensiform cartilage.

Changes in the Uterus, &c.—During the first four months of pregnancy, the cervix is found to be fuller, softer, rounder, and more elastic than before conception. In the fifth month, it feels swelled out towards its upper end; its sides have begun to diverge from each other, and are becoming a part of the body of the uterus; the cylindrical part of the cervix feels somewhat diminished in length, and, owing to the increased softness and yielding condition of the uterine texture, the finger passes more readily into its canal. In the sixth month, these alterations are still more distinct, the vaginal portion of the cervix is decidedly more abbreviated, and this abbreviation or obliteration continues to be gradually affected from above downwards until the close of gestation, when the projecting cervix is no longer to be felt in the development of the uterus; the fundus is first enlarged, then the body, and lastly the neck. In the fourth month, the fundus may generally be felt above the anterior wall of the pelvis; in the fifth, half way between the symphysis pubis and the umbilicus; in the sixth, it rises as high as the umbilicus; in the seventh, it may be felt half way between the umbilicus and the sternum; by the end of the eighth month, it has risen as high as the ensiform cartilage; and in the ninth, it continues to enlarge, but the degree of its increase is not very observable by an increased elevation of its fundus, which, on the contrary, very generally falls lower towards the close of the month, so that for a week or two before labour, the woman will often appear and feel smaller than before.

Of the different Modes of Examination, and the Method of conducting them.—The sensible signs of pregnancy are determined by the exercise of the senses of touch, hearing, and sight. By the hand, we institute the external examination through the parietes of the abdomen, the internal examination by the vagina or rectum, and perform the manœuvre of ballottement or repercussion; by the ear, we recognise certain sounds resulting from the state of the gravid uterus, the action of the fœtal heart, and the sounds produced by the motions of the fœtal limbs; by the eye, we judge of the volume of the abdomen, of the state of the breasts, the character of the areola, and of certain marks in the abdominal integuments, to which may be added the dusky hue of the vagina, as proposed by Kluge and Jacquemin. In any form of examination we may adopt, the bladder should be empty, the patient in bed, on her back, and in a posture between sitting and lying. One or both hands should then be spread over the abdomen, while, at the same time, we engage the patient in conversation during our manipulation. Ballottement or repercussion is best performed by introducing one or two fingers into the vagina, and carrying them upwards until their points are applied to the anterior portion of the cervix, as high up as they can be conveniently made to reach. If, now, the uterus be impressed with a quick and slightly jerking movement, directed upwards and forwards, something will be felt to have bounded away from the fingers, upon which it will be felt to drop again with a gentle pat in the course of a few seconds. This test will be found to be most available in the course of the fifth and sixth months. The next source of evidence to be considered, is that derived from auscultation, which furnishes two sounds; the one derived from the placental circulation, the other from the pulsations of the fœtal heart. The former, commonly known as the placental or uterine souffle, possesses the two following distinguishing characters: first, it is a sound without impulse or pulsation; and, secondly, it is always synchronous with the mother's pulse, and varies with its frequency. This sound is rarely available as an evidence of pregnancy until the fourth month, and, according to Dr. Montgomery, is most frequently heard in the situation of the right Fallopian tube. In estimating its value as an evidence of pregnancy, it must be remembered, that it may be imitated in the case of certain abdominal tumours which may happen to compress the arterial trunks, and that it has been distinctly heard in the case of a large liver or ovary; whilst in many cases of pregnancy it cannot be detected. The pulsations of the fœtal heart are scarcely ever appreciable before the end of the fifth month of pregnancy, although reported to have been heard earlier. They vary in number from a hundred and twenty to a hundred and sixty, and are commonly audible only over a plane of three or four square inches, corresponding with the middle of a line drawn from the anterior superior spine of the ilium to the umbilicus. It is to be noted that in certain cases an unusual rapidity in the maternal circulation has simulated the fœtal pulsation; that in cases in which the child has died, they are of course not to be heard; and that in cases in which pregnancy is complicated with tumours, uterine, ovarian, or otherwise, they are apt not to be observed, or to be rendered altogether inappreciable. The last physical sign to be noticed as evidence of pregnancy, is the livid or dusky hue of the vagina, which, although not invariably present, is, nevertheless, a very frequent concomitant of such condition. According to Dr. Montgomery, it is first met with in the course of the third or fourth month, rarely earlier, and it does not occur continuously, but rather irregularly, or in patches; portions of the vaginal mucous membrane being tinged with this appearance, whilst others are simply congested. Although this sign is often valuable as an indication of pregnancy, it has yet two drawbacks; first, that it may be absent in any given case; and, secondly, that it may have disappeared from the death of the child, and the consequent cessation of the actions of pregnancy.

Such, then, in the abstract, are the chief signs and symptoms of pregnancy, so far as our present knowledge of the subject is available; and to such evidence must we appeal in our investigations of the matter, under difficult and embarrassing circumstances. We have omitted to mention certain sources of information referred to by Dr. Montgomery, such as the sounds produced in the funis, and known as the funic souffle, those produced by the movements of the child or its limbs, or the employment of the metroscope of M. Nauche; believing them to have little or no practical value. Nor have we adverted to the corroborative information to be derived from the examination of the blood, urine, or pulse, for the same reason. The characters of the urine in pregnancy might seem to have demanded a somewhat lengthened consideration; but the observations collated by Dr. Montgomery, and the corollary drawn by him from them, as well as from his own experience, would tend to show that although certain appearances connected with the formation of kyestein are sometimes present in the urine, yet that it is at best but of doubtful occurrence, and consequently a doubtful and uncertain indication of the existence of pregnancy.

Nor have we, in treating of the signs and symptoms of pregnancy, in any way exhausted the scope and character of the work before us. Besides giving the most ample information upon every point connected with this subject, several interesting papers are appended upon important questions connected with midwifery. In these the complications of pregnancy with various morbid and unusual conditions of the uterine organs and general system—the phenomena of simulated pregnancy—of imitative labour—the appearances connected with the formation of the corpora lutea—the period of human gestation, and the signs of delivery, are all treated of in the most complete and elaborate manner. Nor, lastly, must we omit mention of the interesting chapter which concludes the work, upon the spontaneous amputation of the fœtal limbs in utero, in regard to which the author's personal investigations stand so prominent. Reviewing the work as a whole, we regard it as one of the highest scientific and practical importance—as one which is rich in valuable facts—in which deductions are submitted with judgment and discrimination, and in which the labours of the writer are made eminently subservient to the advancement of truth, and consequently to the best interests of the medical art.

Review XI.

1. *Etudes Médicales et Statistiques sur les Principales Sources des Eaux Minérales de France, d'Angleterre, et d'Allemagne; avec des Tableaux Synoptiques et Comparatives d'Analyses Chimiques des Eaux, classées d'après les Analogies de leur Composition et de leurs Effets Thérapeutiques.* Par le Dr. J. Ch. Herpin (de Metz), Lauréat de l'Institut, de la Société Impériale d'Agriculture, de l'Académie de Lyons, Membre du Conseil de la Société d'Encouragement pour l'Industrie Nationale, &c.—*Paris*, 1856. pp. 374.

Medical and Statistical Studies on the principal Sources of Mineral Waters of France, England, and Germany; with Synoptical and Comparative Tables of their Composition and their Therapeutical Effects. By Dr. J. Ch. Herpin (of Metz), &c.

2. *Guide Pratique du Médecin et du Malade aux Eaux Principales de France, de Belgique, d'Allemagne, de Suisse, de Savoie, d'Italie, et aux Bains de Mer; suivi de Considérations générales sur le Traitement Hydrothérapeutique.* Par le Docteur CONSTANTIN JAMES, Ancien Collaborateur de Magendie, Chevalier de la Légion d'Honneur. Troisième Edition. Avec une Carte Itinéraire des Eaux et de nombreuses Vignettes gravées sur acier.—*Paris*, 1856. pp. 594.

Practical Guide for the Physician and the Patient to the Principal Mineral Waters of France, Belgium, Germany, Switzerland, Savoy, and Italy; followed by general Considerations on the Hydropathic Treatment. By Dr. CONSTANTINE JAMES, &c. With a Map and numerous Plates. Third Edition.

3. *Einleitung in die Mineralquellenlehre Ein Handbuch für Chemiker und Aerzte.* Von Dr. M. LERSCH, Arzte zu Aachen. Erster Band, *Die Grundzüge der Pegologie und Hydrotherapie.—Erlangen*, 1855. 8vo, pp. 950.

Introduction to the Knowledge of Mineral Springs; a Handbook for Chemists and Physicians. By Dr. B. M. LERSCH, Practitioner at Aix-la-Chapelle. First Vol., *The Fundamental Outlines of Pegology (the Science of Springs) and Hydrotherapeutics.*

THE subject of mineral waters is one of the *Elegantiæ Medicinæ.* If mineral waters be actually in themselves, physiologically and medicinally speaking, powerful remedies in the cure of disease, their beneficial effects are assisted in most cases by every adjuvant that can render the remedy one taken under favourable circumstances. In the use of these remedies the practitioner and the patient meet on common ground. Every man of sense and education can judge of the benefits of pure air, abstraction from business and care, to a certain extent of the amount of benefit derived from hot and cold baths; and the patient who frequents mineral waters is pretty sure to ask himself the question, how much of the benefit he may derive is to be ascribed to hygienic conditions, how much to the actual potency of the remedy—the mineral water? In this way, perhaps, if his mind be of an inquisitive turn, he may be led to the consideration of questions vital to medicine —viz., how far drugs are beneficial? how far they may be abused? Such systems as homœopathy and hydropathy are sure to pass in review before him; and thus the frequenter of mineral waters may form conclusions inimical or otherwise to the orthodox practice of medical men, according to the lights placed before him.

When we consider the vast number of conditions which, in the case of recoveries from the use of a mineral water, may concur in the production of the beneficial result, it is easy to see how arduous a task it must be to assign those conditions which contribute more than others to this result. The more important is it, then, that medical men should have, if possible, fixed principles to appeal to with regard to the use of these remedies; principles to which they may refer as guides amid so many conflicting statements and opinions.

In this article we intend, whilst referring to some of the more prominent matters in the works before us, above all to keep in view the discussion of those general principles with regard to the action of mineral waters still open to controversy and doubt; and this discussion involves primitive questions which one would think ought to have been long ago resolved.

In the first place, what is a mineral water?

The ready answer would be, one highly charged with mineral salts; or otherwise differing in minute qualities from ordinary spring water. We shall find, however, that opinions differ considerably on this point; and some waters are said to produce powerful effects, physiologically and therapeutically, without differing materially from ordinary water. For this reason, therefore, another element has been introduced into the definition by most writers—viz., the therapeutical one; that is to say, besides the properties of a water, as regards its chemical composition, temperature, &c., its effects in the treatment of disease, real or supposed, are made to confer of themselves the title of mineral water. One of the writers before us, M. Herpin, relates a conversation which he had with a Government Inspector of mineral waters at Bains in the Vosges:

> "After a conversation," says he, "in which I contested with M. the Inspector Bailly many of the properties which he attributes to these waters, which are very little charged with mineralizing principles, I quitted him, saying, 'So your waters are only clear water, and you only see through the prism of illusions.'* A lady was passing in the street. The Doctor said to me, pointing her out, 'See, nevertheless, a lady who has derived great relief from our waters.'"

This was a case of a lady who had been long a martyr to daily attacks of hysteria, accompanied by convulsions, and was cured by the use of the waters in fifteen days.

Among important waters, those of Pfeffers, in Switzerland; Baden-Baden, Gastein, Wildbad, in Germany; Plombières, Neris, Aix, in France; Buxton, and even Tunbridge, in England — for this celebrated spring contains only a very small quantity of iron—may be cited as instances of waters having the reputation at least of producing powerful effects, without, in some instances, even temperature to account for the result. Buxton, for instance, has a very slightly elevated temperature, and few constituents to account for the effects ascribed to it. But what if, according to this mode of defining mineral waters, we were compelled to include waters with which this country and Ireland formerly abounded, called "holy wells," where cures were believed to be performed, at least? In the definition of a mineral water, therefore, we should be very careful in admitting any waters to this title, unless by their chemical composition or thermal qualities they differ materially from ordinary water. The sea has been always considered the type of a mineral water. In the Channel, a kilogramme contains 35·256 grammes of solids, and has a medium temperature of 15·5 degrees Cent. The Mediterranean, again, contains 40·74 grammes

* They only contain 0·302 of a gramme in the kilogramme, but have a temp. of 50° Cent. Buxton has only 18·16 grains in a gallon, at a temp. of 77° Fahr. The kilogramme is 2·035 imperial pounds, the gramme 15·434 grs. troy.

of solids, and has a mean temperature of 21·6 degrees. Those who have witnessed the "ocean stream" of the Bosphorus perpetually flowing into the Mediterranean, have a beautiful fact before them; and the same repeated at Gibraltar, which the superior concentration of the Mediterranean, by the enormous evaporations from its surface in that climate, explains. The sea and the sand must represent the composition of the surface of the earth, and one be the complement of the other. Hence, the sea must contain everything soluble from the surface of the globe, and the sand the insoluble ingredients. In all probability, therefore, the number of constituents which the sea presents is limited only by the extent to which our analyses are pushed. Almost all the writers on mineral waters adopt a theory of their action which is, to say the least of it, mysterious. They ascribe peculiar properties to the thermal heat as distinguished from ordinary heat, and superior power of action to the ingredients of the mineral waters from their greater state of solution.

The general opinions on the subject are perhaps stated as well by Dr. Sutro as any writer. The principles, he says, of mineral waters are more easily dissolved and assimilated. The reason probably is,

> "The greater degree of solution and intimate union which they possess when flowing out of the soil, where they have been kept in this combination for years. They certainly enter the digestive organs as more congenial bodies; and their active principles so finely divided are no doubt fit to be immediately imbibed into the absorbent vessels, and to enter the circulating fluids without performing, as is the case with many artificially-prepared medicines, the tortuous route of digestion before their action is exhibited."*

This is the general theory of writers on mineral waters, including that very shrewd observer, the late Dr. James Johnson. Herpin goes so far as to state,

> "The more feeble a thermal water is, that is to say, the less it is charged with mineralizing principles, the greater is its dissolving action, and in consequence, the more is it apt to charge itself with the heterogeneous and morbific principles which it encounters in its passage through the organs. This is the reason which explains why the purest thermal waters, those which contain almost nothing of mineralizing principles, produce nevertheless the most surprising and unlooked-for cures." (pp. 175–6.)

This doctrine is carried very far by some writers, who seem to maintain that some waters are powerful in the *ratio* of their poverty in solid constituents; while at other times, with strange inconsistency, they puff a spring on account of its strength. Dr. Knox, in speaking of a spring, says,

> "To the objection that these ingredients exist in too small quantity to be efficacious, we may reply, that the effects on the sensations are obvious, and probably increased by the state of permanent and minute division in which they are applied."†

* Lectures on the German Mineral Waters, p. 7. London, 1852.

† Knox on Irish Watering Places, their Climate, Scenery, &c., p. 50. Dublin, 1845. At p. 317 he repeats a similar statement in speaking of Kilkee Spa.

This reminds one of the line in the play,

> "My wound is great because it is so small;"

and the reply of the Duke of Buckingham—

> "Then 'twould be greater were it none at all!"

But in point of fact, this theory about superior digestibility, assimilation, &c., is contrary to the notions we entertain of medicinal agents; for in what respects do these differ from food? In our opinion, at least mainly in these—whereas alimentary matters are subjected by the organism, and compelled to obey its orders; medicines force the organism to obey their laws, and are therefore of the nature of poisons.

With regard to the statement often advanced, that chemical analysis does not give the true composition of a mineral water, there is some thing in it, but not so much as is generally assumed. It is true that chemical analysis does not always give the composition of a mineral water; but that the ordinary rules for calculating the composition—viz., by giving, after the determination of the bases and acids, the strongest acid to the strongest base—are sufficiently correct in the main, may be proved—1stly. By the fact that the taste of several spas indicates a composition like that given them theoretically; and 2ndly. By the further fact that many of them can be reconstructed synthetically in accordance with this chemical constitution, as is done with such success by Dr. Struve at Brighton.

With regard to the notion of thermal heat having properties different from ordinary heat, that, too, may be more than doubted. Attempts have been made to prove that the specific heat of mineral waters is greater than that of ordinary waters; that is to say, at the same temperature they are supposed to contain unequal quantities of caloric. This is at least a plausible supposition, and one which one can understand as more reasonable than the supposed mysterious properties of thermal heat; but experiment has not confirmed that view. Nor do mineral waters take a longer time in cooling than water heated to the same temperature. It may well be, however, that many baths prepared by mixing hot and cold water, may not have the same equal temperature as that which is prepared in Nature's cauldron. We can also understand the luxury of being stretched upon the sand of the bath, as at Wildbad, where bathers feel the hot water boiling from the sand, while bubbles of gas escape along with it, and agreeably titillate the body.

As an instance of some of the fallacies on which the doctrine of a mysterious essentiality in thermal heat reposes, we quote the following comparison from Paracelsus, given approvingly by Dr. Sutro. Paracelsus, in speaking of Pfeffers, says:

> "Heat may have a various entity in itself—the heat of the sun is one, that of dung another. . . . . What great things may be performed by congenial heat is shown by the hens brooding their young with it."

But unfortunately for this comparison, we have poultry hatched artificially, as in Egypt; crocodiles' eggs hatched by the heat of the sun; and a recent traveller says, in describing Manilla:

"Immense quantities of poultry are here reared in a manner differing widely from that commonly adopted in Europe, having been assured that the art of hatching eggs is constantly practised, and with the greatest success, by the young people of both sexes, who, for the sake of gain, do not scruple to resort to this tedious and, to say the least of it, monotonous employment."*

Water itself, cold or hot, internally or externally, is a powerful agent, as all writers on mineral waters concur in remarking; and we do not require to go further, when we find waters do good, not accounted for by their chemical composition, except perhaps in *some rare instances*, than the plain physiological considerations arising from the nature of the remedy and the hygienic conditions in which the patient is placed; the removal from business and care; the purer air, the better diet, the air perhaps that of the mountains or of the sea-side. Very wonderful cures are effected by such means where no mineral water is pretended to be used.

When waters contain more than forty grains of solids in the gallon, they approach, in our opinion, the character of mineral waters; or if they contain a large quantity of gases, carbonic acid, or sulphuretted hydrogen especially; or acids, as the sulphuric and boracic. The number of constituents of mineral waters has been most materially added to of late. Bromine and iodine are probably present in greater or smaller quantity in all saline springs. We have ourselves detected them in a great many that we have examined, and it appears to us that a ludicrous amount of importance has been ascribed in some instances to their presence. Considering that the chlorides are analogous to the bromides and iodides in all their properties, and that their chemical relations are exactly imitated by their physiological properties and medicinal effects, it seems absurd to attribute so much to a minute proportion of a bromide or an iodide, in a water abounding in chlorides, perhaps powerful chlorides like those of calcium and barium, long recognised as powerful remedies, especially as tonics and deobstruents in strumous diseases.† Again, in some of the analyses of waters containing bromine and iodine, these substances are sometimes estimated as if they were free, which is quite out of the question.‡ Besides bromine and iodine, various vegetables, organic principles, as crenic acid, are found. Other principles of an organic nature—as glairine, the organic matter of the Kochbrunnen—are supposed to be in some cases of animal origin, from organic remains perhaps buried in the crust of the earth; while manganese, lithium, arsenic, &c., have been added to the list of known ingredients. Arsenic has been shown to exist in waters in Algeria (which was the first discovery of the kind) in 1839; in Spa water, Wiesbaden, Schwalbach, &c.; and in France in no less than twenty-four different sources. This is clearly a discovery of importance, and might even have a toxicological interest.

The first and most obvious division of mineral waters is into thermal and cold.

The origin of the former is probably from more than two or three

* Recollections of an Aide-de-Camp, by Capt. Cunningham, vol. ii. p. 179.

† On the Physiological and Medicinal Properties of Bromine and its Compounds, &c. By Dr. Glover. (Edinburgh Medical and Surgical Journal, July and October, 1842.)

‡ See Johnson On the Spas of England—On the Woodhill "or Iodine Spa!"

sources. All the water which descends in rain does not flow into the ocean, for on boring at great depths, even near the sea-shore, streams of running water are discovered. On the other hand, a filtration must doubtless be going on from the bed of the sea into the interior of the earth, where probably vast masses of minerals, uncombined as yet—as potassium, phosphorus, sulphur, iron, lead—are stored up, waiting for some disturbing cause, as it were, to call their chemical energies into action. Water will support the combustion of some substances even better than air; and it is probably in some such way that most volcanic eruptions and earthquakes take place. Most of the thermal springs are in volcanic regions.*

It is well known that the water of Artesian wells is hot. In the great Artesian well on the plain of Grenoble, carried to the depth of eighteen hundred feet, the temperature of the water was found to increase at the rate of 1·8 of Fahrenheit for every hundred and one English feet. Whatever be the cause of the increased temperature of some mineral springs, or our opinion as to the mode in which the caloric may be combined with the water, it is certain that in all ages and in all countries invalids have preferred the natural hot baths to artificial ones. Witness the ancient reputation of Bath among the Romans, and at present of Broussa among the Turks.

There is yet another view with regard to the action of mineral waters as baths—viz., that electricity may have to do with their greater power, real or supposed. We take the following from the work of Lersch, especially as the passages quoted will give an idea of the very diffusive, but no doubt very scientific, style in which the subject is treated throughout the work; the author, in fact, is not contented to pick up every stone which lies in his way, but he must go out of his way to take up every one he can see. He says, quoting from Heidmann:

"Conducting and non-conducting of electricity in bathing (p. 510). . . . . Flowing water is a better conductor of electricity than air. The conducting power of water is lessened when it is frozen; still ice and snow are reckoned among conductors. Humboldt, however, convinced himself of the non-conducting power of ice. The conducting power of water is much increased when at a boiling temperature, or when converted into steam; so that if the electric fluid be passed from one pole to another through boiling water, no spark will appear."†

Humboldt says, on the contrary:

"Unconfined steam at the pressure of the atmosphere seems without conducting power. . . . . The conducting power of water is somewhat increased when it contains salt. Repeated experiments have shown that it is water which makes living bodies tolerable conductors. Dried blood is not a conductor. . . . . The living skin is a good conductor, except that of persons suffering under rheumatism, which isolates them; and an electric current even can be interrupted by such a person if his hand be wetted." (Humboldt.)

* The connexion of the sources of mineral waters with many of the phenomena of earthquakes has been shown in the various changes undergone by mineral waters at the period of earthquakes—as, for instance, at the period of the earthquake of Lisbon, a great many mineral thermal waters, especially in France and Germany, underwent notable changes; some disappeared for a time; at others new sources were formed; others underwent changes in temperature and in physical qualities.

† Heidmann's Theory of Electricity, vol. i. p. 204. 1799.

As water is a good conductor, springs which have their sources deep in the earth must become charged with some of the electricity which existed there, and must in their turn impart some portion of the electricity to bodies which are dipped in them, if the waters have not already imparted it to the conductors which have surrounded their course. In water itself, the processes of flowing, and various frictional movements which Lersch describes, must generate electricity. He then proceeds at great length, after showing the numerous processes in connexion with bathing to be capable of generating electricity, to argue that this principle may play a great part in the action of baths. We all know how electricity is generated by the friction of steam in the hydro-electric machine. Elevation of the bathing place may produce an important electrical effect; many other changes may do so. In a word, it amounts to this, that the solution of the superior effect of thermal springs over baths or waters of the same composition, as far as can chemically be determined, may depend on electrical causes. Thermal waters may be more highly charged with electricity. As a proof of the part which electricity may play in such matters, he gives the following:

"A person affected with rheumatism and difficulty of breathing, forty years of age, used the douche rain-bath. One evening during the fourth douche, she saw, at the wetted part of her body, particularly the lower limbs, a number of electric sparks, and the darker the room, the more perceptible were the sparks; and they were perceptible either near or at a distance from the light. After this patient had taken a few douche baths, the symptoms of the illness decreased, and the phenomena disappeared." (Heinrich, Baden, 1846.)*

Waters in Europe have not in general the high temperature of some in America and Asia, some of which issue from the ground at the boiling point—the waters of Chaudes Aigues, in France, having the highest temperature in Europe. These waters have a temperature of 81° of the Centigrade. The highest temperature of waters known to be used for thermal purposes is that of Malha, in Kamtschatka—viz., 212° But these temperatures are of course useless in practice, inasmuch as water, according to experiments that we have performed, begins to scald as it approaches 120° of Fahr. For practical purposes as a thermal spring, our own Bath is as efficacious as any water can be. Thermal waters may act in two ways—by the temperature (whatever theory be adopted on that point) or by the electricity, as Lersch has it, or by the absorption of the contents either through the skin or stomach. On this point, M. Herpin remarks:

"MM. A. Chevallier and Ch. Petit have proved, from direct experiments, that one half hour of immersion in water of Vichy is sufficient to modify the quality of the liquids of the economy, and to cause them to pass from the neutral or acid state to the alkaline. The urine becomes promptly alkaline. . . . . Some glasses of Vichy drank fasting often alone produce this effect." (pp. 185–6.)†

When we add the presence of important principles, as sulphuretted hydrogen and carbonic acid, salts, and iron, there is no mystery about

* Lersch, op. cit., p. 512.

† 115° is the generally received temperature. The Geysers are said to have a temperature above 212°, as if the water had come from a Papin's digester.

the effects of most mineral thermal springs; but in regard to such waters as Pfeffers and Wildbad, unquestionably there is a difficulty. It is stated in regard to the former, that there is a marked difference, in the opinion of the bathers and drinkers at least, between the effects of the waters at Pfeffers and at Ragacz; the latter supplied with the same water, at an elevation less, however, by 600 feet, and with the water at a temperature lower by one degree of Reaumur. Both Wildbad and Pfeffers are nearly pure waters. Special indications, too, are believed in for the use of these waters. The former is more indicated in nodous gout; Pfeffers more in atonic metastatic gout; while Gastein, another thermal water nearly chemically pure, is especially celebrated in irregular and depraved nervous action based on atony. We can understand why a strong mineral water like Ischl should act by its saline principles as a deobstruent in scrofulous affections. In the waters even less known in this country, as Shap or Shotley, it is easily possible for a patient to take powerful doses of chlorides like those of sodium, calcium, barium, &c.; but how the effects, and especially the specific effects, of the waters mentioned can be explained, independent of their heat, what are the general hygienic conditions in which they are used, and their chemical composition, are problems which we have started, but do not attempt to solve; only we strongly incline in these matters to be very sceptical. Meanwhile, we shall quote what is said of Wildbad by Dr. James:

"The temperature varies from 30° to 37° Cent., or 86° Fahr., to about blood-heat, so that it neither requires heating nor cooling. The water is remarkable by its perfect limpidity; it has neither smell nor taste. It contains in a litre,* 0·46 of a gramme of fixed principles" [a little carbonate of lime and soda, and chloride of sodium]. "These are waters quite insignificant, chemically speaking, nevertheless their action is very real, and it is translated by a series of phenomena, whose gradations I have experienced on myself. So, at the first impression of the bath, which we have said to be delicious, succeed more free, clear, vivid sensations; one finds oneself strongly excited; luminous sparks flash before the eyes; the temples throb; it seems as if a more subtle blood flew towards the brain. One would wish to stay in the bath, and always something unusual and strange advertises one to go out."

Dr. James goes on to ask what can be the cause of these phenomena; it cannot be the temperature which makes these baths differ from other baths; it cannot be the mineralizing principles. He says it must be a very subtle principle.† It cannot be the gases, for these are chiefly atmospheric air; and that if the water is passed through pipes, if only at a short distance, its action vanishes, and it becomes ordinary water. Thus, according to Dr. Kerner, this water makes the old young, and restores to persons exhausted by work and fatigues, new forces and a new youth.

* The litre is 1.7608 of an imperial pint.

† "The origin of this principle (barégine-zoogine glairine) is very obscure. It exists in a great many mineral waters; it is oily, grey, amorphous, translucent, burns on coals with an odour like that of burnt horn, disengages ammonia, and is animalized and azotised. Examined by the microscope, it shows a great many infusoria. The origin of the broth-like smell of many waters is attributed to this substance. According to some, it proceeds from organic matters buried in the earth; according to others, from the decomposition of vegetables, animalcules, &c." (Herpin.) Since this was in the press, Scherer has stated that he has discovered butyric, propionic, acetic, and formic acids in the water of Brückenau. Ann. de Chemie et de Physique, Feb. 1857.

"What constitutes the speciality of these waters, and ranges them in a separate category, is the action—sometimes marvellous—which they exercise in affections of the spinal cord. Look at the patients who frequent them; they are almost all paraplegic. Interrogate them: the greater part are ameliorated or in way of cure."*

Independent of what may be called the *mysterious* theory of the action of mineral waters, there are one or two more theories in special cases. It has been asked, why can the spring of the Hôpital at Vichy be freely administered, while that of the Grande Grille must be administered with great caution, though both possess nearly the same chemical constituents, and belong to the same spa? "Because the vegeto-animal substance incapable of imitation, is more abundant in the former."† Such is the principle called glairine, the substance, apparently of animal origin, found in the Kochbrunnen at Wiesbaden, and other obscure principles. A catalytic force has been ascribed to mineral waters; and the decomposition of sulphates in the blood, yielding oxygen to the blood, and disengaging sulphuretted hydrogen, is another mode or action ascribed as possible in the case of sulphates, as well as their purgative action. We should be inclined to believe that the action of almost all chemical remedies is partly vital and partly chemical, and that it consists, in part, in a series of molecular changes which depend on decomposition and recomposition, beginning in the stomach and ending in the urine. For instance, if, as Dr. Prout supposed, chloride of sodium be decomposed in digestion, and its elements separated, is it likely that the elements separated, in their nascent states, with their chemical energies increased, would remain dormant? Or if they form new compounds, will these compounds, in the immense number of changes going on in which chemical phenomena must be important—most important—features, will these compounds remain stable? No: a series of molecular changes will take place, leading to circumstances favourable to the reconstruction of the machine, especially if the principle of life, the *archaeus* be sufficiently powerful, the *vis medicatrix* still unbroken, to direct these changes in a manner favourable; but when the constituents of mineral waters unquestionably adequate to produce the medicinal effects ascribed to them in many instances, are assisted by bathing, by all the favourable hygienic agents brought to bear, then indeed we have little mystery left, in many cases at least.

Of the many classifications of mineral waters, we prefer the chemical; and that of M. James seems exceedingly satisfactory, although there are cases in which it is insufficient. Thus the presence of arsenic, or the animal matter of the Kochbrunnen, would almost constitute a special reason for isolating a spa; and thus the same water may be ranged under several heads. However, in looking over the various classifications, we find none more to our mind. Of course there is the division into thermal and cold. But before giving the classification of M. James, it will be as well to quote the geological classification of M. Brongniart. A physiological classification according to the therapeutic effects, would be even more difficult than in the case of ordinary remedies, on account

* James, pp. 344–5. † Sutro.

of the more complicated composition of the waters, and the number of diseases in which they are used.

Although the mineral waters of a geographical region resemble each other in general, yet there are numerous exceptions. Thus the mineral waters of the Pyrenees are generally sulphurous; those of the Rhine or of Auvergne, from volcanic regions, strongly charged with carbonic acid; those of low regions, generally saline. Many sources together are generally of the same composition, as we may see at our own Cheltenham, Leamington, and Harrowgate, where, however, waters of different composition do exist. The following is the classification of M. Brongniart, abbreviated:

1. Mineral waters of the primitive rocks, very often thermal, containing carbonic and hydrosulphuric acids, silex, alkaline sulphurets, salts of soda, and especially the carbonate, few salts of lime or iron. Such are, for the most part, the waters of the Pyrenees.

2. Mineral waters of rocks of lower deposit. These are somewhat like the former, but of a less elevated temperature. They contain much of the salts of soda, and almost always sulphate of lime. Such as Plombières.

3. Mineral waters of the upper deposits. These are cold; contain little carbonic acid; the chief salts are the sulphates of lime and magnesia, with carbonates of lime and iron. Enghien, Epsom, &c.

4. Mineral waters of transition rocks. These contain all united in the preceding. Spa, Vichy, &c.

5. Mineral waters of old trap or modern volcanic rocks. The composition of these is much like those of the primitive rocks. The Geysers are thermal waters of this class, containing carbonic and hydrosulphuric acids, yet with an alkaline reaction, and containing also a large quantity of silica.

> "Beyond the Rhine, in the Duchy of Nassau, in Bohemia, in countries which have been torn up by volcanic eruptions, we find in the mineral waters the same qualities which distinguish those of Auvergne. Thus the presence of carbonate of soda, joined to a quantity, more or less considerable, of chloride of sodium, characterizes these waters. Such are the sources of Ems, of Bilin, of Töplitz, Selters, Tönnistein, Schwalbach, &c." (Herpin.)

But it is clear that a source may proceed from more than one kind of formation. When a spring proceeds from a primitive rock, there may be some certainty with regard to its origin; but when it proceeds from a more modern formation, the more chance is there that it may receive impregnations from older formations. Sulphuretted hydrogen in some instances, as at Harrowgate, proceeds from the decomposition of salts and organic matters.

To return to the classification of M. James. He divides mineral waters into six great classes—the *sulphurous*, the *chalybeate*, the *alkaline*, the *gaseous*, the *saline*, and the *bromo-ioduretted*.

1. Sulphurous waters. He has three divisions, which we consider important: those sulphuretted waters which contain sulphuret of sodium, coming from granitic and schistous rocks, mostly thermal and alkaline; the waters which contain sulphuret of lime, which spring in secondary or tertiary deposits, generally near deposits of gypsum, of

feeble alkalinity, disengaging carbonic acid, mixed with hydrosulphuric acid; the greater number cold. Both these generally contain baragine; the former most. These distinctions are very important, because the sulphurets of these alkaline and earthy bases are very active salts, and may materially increase the power of sulphuretted waters. For instance, in the analysis of Gilsland water by Dr. Clanny, sulphuret of sodium does not appear. Nevertheless, no amount of boiling will expel the whole sulphuretted hydrogen, or prevent a black precipitate being obtained with nitrate of silver or acetate of lead. Then he has a class of degenerated sulphuretted waters, as he terms them—waters slightly impregnated with sulphur, which have lost it by contact with air, or where it has assumed the form of a hyposulphate.

2. Chalybeate waters, divided into carbonated, crenated, and sulphated. Perhaps a fourth class may exist,—the muriated. There exists a water at Lorton, near Keswick, which appears to contain the muriate of iron. We have no want of good chalybeates in England—indeed, some of them are far too strong for use, as that at Sandrock, in the Isle of Wight, Dorton, in Oxfordshire. Unfortunately, our chalybeates are not impregnated with carbonic acid, like the Brodelbrunn at Pyrmont, or the waters of Schwalbach and Spa.

3. Alkaline waters. A very important class, on account of their alterative and solvent action, and their utility in gouty and rheumatic and calculous diseases. They generally contain carbonic acid. This is a class of waters in which we are deficient in England. The waters of Carlsbad stand at the head of this list. In a litre they contain 5·45927 grammes of solids, according to the analysis of Berzelius—a model for the analysis of a water—of which 1·26237 are carbonate of soda; then, the principal ingredient in quantity is sulphate of soda 2·53743, carbonates of lime, magnesia, strontian, iron, manganese, fluates, and phosphate of lime and silica; and since have been discovered iodine, bromine, arsenic, and recently, boracic acid, while the sediment of the sources appears to contain copper, lead, tin, and antimony! Besides this, it contains no less than 0·40 of carbonic acid gas in the litre. The sources at Carlsbad are nearly identical in composition, and the Sprudel has a temperature of 75 C. or 147° Fahr. Such a composition and such a temperature leave no doubt about the power of the agent. It is not here or at Harrowgate (which Dr. Johnson denominates the Yorkshire Stingo) that we find the least recourse to mysterious theories; in all such cases, the real chemical composition of the spa is at once appealed to. Amongst other properties, the waters of Carlsbad are celebrated for their power of healing fractures; and this may follow from their composition. It is worthy of note that, according to M. Hochberger, a physician at Carlsbad, during the last days of the cure, a peculiar electric state often develops itself, as in the cat. "Thus, in passing the hand over the scalp, one draws sparks; if this is done in the dark, the sparks appear luminous." We can well believe in the authenticity of the cures effected at Carlsbad,—the affections of the liver, gout, &c. As one of our authors affirms, "*Il n'y a pas à Carlsbad de sources faibles;*" the reputation of no mineral water is more solidly established. The same may be said of the

waters of Vichy,—pre-eminently alkaline waters, containing from four to five grammes of bicarbonate of soda to the litre. Here, again, all the effects ascribed answer to what we would expect from the remedy. There is nothing mysterious in the matter.

4. The next class are the gaseous waters—i.e., those which contain carbonic acid or sulphuretted hydrogen out of proportion to the salts, or especially the former, with azote and carburetted hydrogen occasionally. Of the properties of the two latter we know little or nothing.

5. Saline Waters. An immense class, whose bases are generally sulphates of soda and magnesia, or muriates of the same, and with some analogy to sea-water. These are common in England, and some of them little enough known. Those which contain considerable quantities of chlorides of sodium, calcium, barium, and magnesium, like some of those already referred to, must be much more powerful in scrofulous affections, than the trifling quantity of bromides and iodides associated with them in Mr. James's sixth class—the bromo-ioduretted waters, can render them.*

Several new modes of applying mineral waters have of late been brought into operation; in sulphuretted waters, a certain inhalation of that powerful gas, sulphuretted hydrogen, must take place, also in the case of carbonic acid. Contrivances have been invented for exposing the surface of the body to these gases. In the case of carbonic acid, the greatest application that has been made of the gas is probably at Nauheim and Bocklet, where, while the water passes away, a tube conveys the gas into a closed box, where the patient is seated, with his face exposed, so that he cannot respire the gas. The effect is a great heat of skin with itching, and after a time abundant perspiration. This treatment, it is said, has been found very successful in commencing muscular debility, and in the commencement of paralysis; and douches of the gas are directed in a similar manner on various parts of the body, the eyes, and the ears, in cases of weakness of those organs ascribed to the deficiency in nervous power, caoutchouc tubes being employed. The effects of sulphuretted hydrogen employed in the same way hardly require to be dwelt on; but whatever effects may be thus produced cannot but be capable of imitation, or of being surpassed by artificial gases. This is a branch of therapeutics which has been much neglected, as those who have read the admirable monograph

* This is a subject on which there is great exaggeration, as has been already hinted. The physiological and medicinal properties of bodies are in strict accordance with their chemical relations—a great law in therapeutics; and in no group of bodies are these relations more exact than in the group of chlorine, bromine, and iodine, and their compounds. Moreover, if we traverse the compounds, from the electro-positive compounds, as those of potassium, to the electro-negative compounds, as of mercury, we shall find the activity to bear a strong relation to the solubility of the salts, and their facility of decomposition. After a certain point, in passing from the electro-positive to the electro-negative metals, the affinities become inverted, as chlorine and iodine begin to have stronger affinities than oxygen. Iodine will expel chlorine from mercury, but not from potassium. The chloride of potassium is the least soluble, the most stable, and the least active of the three compounds of these elements with potassium, and these properties are all inverted to a great extent in the compounds of these elements with the electro-negative metals. This seems to justify the idea we have put forth, of the activity of salts depending upon the decompositions which take place in the system, the presentation of their elements in a nascent form, and the molecular changes thence ensuing.

of Mr. Wallis, of Dublin, on the employment of chlorine in this mode in diseases of the liver, may recollect. He found an erythematous eruption of the skin to follow the application, similar probably to that produced by the external use of iodide of potassium, and this was followed by a profuse perspiration.* Finally, douches of mineral waters, descending and ascending; the latter, especially in female cases, have been used. The mud baths are also a comparatively new feature in the use of mineral substances. Their use is especially practised at Franzensbad, where a kind of ferruginous mud is mixed with the mineral water, and heated with steam to a temperature sometimes of 112°. The effect is to produce, first, excessive astringency, and afterwards the reaction so often experienced in other kinds of bathing; and there is reason to believe that these baths are powerful agents in cases of great weakness, and also in too great excitement of the cutaneous organ. There are few things new under the sun. This is a revival, under another form, of the celebrated earth baths of Dr. Graham, who used to exhibit himself towards the close of last century buried in earth, with only his head, duly powdered, and pigtail, above the ground; and beside him, also buried, his goddess of health—afterwards Lady Hamilton and the friend of Nelson.

We shall now consider the practical deductions with regard to the use of mineral waters to be drawn from the works before us. Those in the work of Dr. James are far the most clear, practical, and intelligible that have yet appeared. The following, then, may be considered as an abstract of his views on this important point.

Here, *in limine*, we may remark, that patients are often sent by their medical advisers, or directed by their friends, almost indiscriminately to spas, and very often cases are sent which are beyond recovery. We remember to have seen at a northern spa the arrival of a wretched invalid, nearly eighty years of age, afflicted with asthma, and all the symptoms of the last stage of heart disease, dropsical too, who had been sent to drink the sulphur water, which he actually took to his dinner! It can hardly be expected that a medical man is all at once to prefer spas at a distance to the resources in his own hands on the spot; but if a more rational use were made of mineral waters, doubtless the number of cures would be much increased. On the other hand, the physicians at spas are too apt naturally to exalt their favourite spas, and the extravagant laudation of some is quite ridiculous. The following is an abstract of this part of the work (pp. 43–55) of Mr. James.

1. *Maladies of the Nervous System.*—In hemiplegia and paraplegia, where there is an absence of excitement of the nervous system: the thermal waters; but they ought to be very carefully used, especially in hemiplegia. In neuralgic affections: thermal springs, used generally not at the highest temperature. In nervous deafness: local douches of carbonic acid gas or sulphuretted hydrogen. In amaurosis: the same treatment.

* It may not be generally known that near the Grotto del Cano a grotto has been discovered which exhales ammonia, and which kills animals more rapidly even than its neighbour.

2. *Affections of the Chest.*—Pulmonary tubercle, and phthisis laryngea, catarrh of the bronchi. We pass over this division, having little faith in the use of the mineral waters indicated by the author in these affections. The most likely waters to do good in these cases are the chalybeate class, as in incipient cases of phthisis, especially in young females where the proper secretions are deficient; in some cases of what is called dyspeptic phthisis, also, thermal and alkaline baths and waters are beneficial, and in many cases of chronic bronchitis. Of course the hygienic means which a residence at many spas implies, are another element in the question. Asthma and emphysema: the thermal waters of Cauterets (Pyrenees) and Mont Dore (Puy de Dome) enjoy in France a special reputation in these diseases. In diseases of the heart and great vessels, mineral waters are generally contra-indicated.

3. *Diseases of the Abdomen.*—Here there is an extensive field for the beneficial use of mineral waters. In gastralgia, anorexia, and flatulence: the gaseous, alkaline, and thermal waters. In diarrhœa from atony: the astringent chalybeate waters, or chalybeate thermal waters. In constipation and hypochondriasis, engorgement of the liver, or in biliary calculi or enlargement of the spleen, the saline waters are recommended; and very often those which are thermal or sulphuretted. In this class of diseases more especially, most of our English salines are found beneficial. In catarrh of the bladder: especially alkaline waters, and above all those which are thermal, especially Vichy, Carlsbad, &c. The same in gravel. In urinary calculi: the same class. In amenorrhœa and dysmenorrhœa: tonic waters, cold bathing, ascending douche, &c. In sterility and other affections of the sexual organs, male or female, no general rule can be laid down.

4. *General Diseases.*—The principal diseases in which benefit is likely to be derived from mineral waters in this class are—1st. The whole class, almost without exception, of chronic skin diseases, where the sulphuretted and saline waters act both as alteratives internally, and locally as stimulants. Where the disease is more acute, the more calmative and milder saline springs. But it is almost always for chronic skin diseases that patients have recourse to mineral waters; and the sulphuretted saline springs, and especially those which are thermal, are unquestionably most potent remedies, more particularly in the case of syphilitic affections, where mercury remains in the system, and they may be supposed to act as antidotes.

Next to syphilitic diseases of old standing, come the whole class of rheumatic and gouty affections, having regard of course to those symptoms of acute disease which are a general contra-indication to the use of mineral waters. The waters used in different forms of gout and chronic rheumatism are so well known that any minute reference to them here would be misplaced. In no disease or class of diseases have the effects of mineral waters been more appreciated, especially those of the thermal waters.* In diabetes, a great indication is to restore

* Perhaps as striking a proof of the great efficacy of mineral waters in popular opinion, is to be found in the great request in which natural thermal waters are held in the East, where ordinary hot baths are used to such an extent. Thus, Sultan Suliman the Great was cured of the gout, after all other means had failed, by the waters of Broussa. The

the functions of the skin; the waters of Vichy and ferruginous waters have been recommended. In albuminuria, the use of mineral waters has been little practised; but as in this affection the restoration of the functions of the skin and deviation from the kidneys should be considered a great object, the use of some of the thermal waters would seem to be indicated. In chlorosis and anæmia, tonic waters generally; but as these have a tendency often to constipate the bowels, their use might be alternated with salines, or saline and ferruginous waters selected. In scrofulous affections, all the saline springs containing muriates, and especially saline springs of this kind with iron. As to the excessive importance ascribed to minute quantities of bromine and iodine, bromides and iodides—that is, either in mineral springs or cod-liver oil, we regard it as an illusion.

5. *Surgical Diseases.*—The use of mineral waters in these diseases has been lately brought under the notice of the military authorities in this country in a pamphlet, noticed in this Journal, by Dr. Pincoffs, attached lately to the military hospitals in the East, especially in reference to surgical diseases resulting from wounds. This pamphlet contains very sound arguments in favour of the establishment of military hospitals in connexion with such waters as those of Bath. It may be mentioned, that in the East this point was brought under the notice of the military authorities superintending the hospitals, especially in reference to the waters of Broussa and Mitylene, both of easy access from Scutari. In France, as Dr. Pincoffs shows, such establishments are found exceedingly favourable in the cure of old wounds.

A great advance has been made in an important branch of therapeutics by the distinction of diuretics into two classes—according as they tend almost solely to increase the quantity of water in the urine, or as they increase the quantity of solids. These latter are the true diuretics, inasmuch as they facilitate the metamorphosis of the tissues by removing the effete portions. The first distinct enunciation of this fact is due to Dr. Lewins, of Leith, who showed the effect of colchicum in causing a great secretion of urea and uric acid. To this class belong such remedies as iodine and mercury. Water itself is the purest type of the other class; but in all probability water itself, both as a diuretic and diaphoretic, taken in large quantities into the system, assists in promoting metamorphosis by facilitating the carrying off effete principles, by the increased action of the excreting organs, and the humidification of the ultimate cells—much more so when it is charged with salts. Dr. Heinrich, in explaining the effects of Weilbach, first shows that where diarrhœa takes place, no good is done; that the increase of urine is in the ratio of the quantity of water taken, and the absence of diarrhœa and flatulent eructations, and that the presence of sul-

principal bath which he used, the Kurkoutlus, is too hot for use except diluted, having a temperature of from 63° to 65° of Reaumur, about 178° Fahr. The Greeks go in pilgrimage to this source of Kurttus, because it is said that a Roman proconsul threw the martyr Patricius into this source, where he was scalded to death, for refusing to sacrifice to the gods. The waters are powerfully saline, and contain carbonic acid, with an unascertained quantity of sulphuretted hydrogen.—Les Bains de Brousse en Bithynie, Turquie d'Asie, &c. Par T. A. Bernard, D.M. et C., Directeur et Professeur de Pathologie Interne et des Cliniques Médicale et Chirurgicale à l'Ecole Impériale de Médecine de Galata Serai. Constantinople. 1842.

phuret of iron in the stools is proved by their green colour; and the evolution of hydrosulphuric acid when muriatic acid is applied to them. This he supposes to be due to the sulphuretted hydrogen combining with the iron of the blood and disintegrating the red corpuscles; and he assumes that those corpuscles which are most effete will first fall a prey (so to speak) to the chemical powers of the agent, and thus the blood be depurated. Those sulphuretted waters which by means of their saline contents act powerfully on the bowels, are therefore not to be used when it is desired to act on the skin or kidneys. We have in the north of England, Gilsland, and in the south of Scotland, Moffat, which are pure sulphuretted waters, powerfully charged with this gas, and containing little else, and therefore admirably adapted for obtaining the pure diaphoretic and diuretic effects of sulphuretted hydrogen.*

From the various records which he has been able to collect in France and abroad, M. Herpin has formed several tables of the proportion of cures, ameliorations, &c., both in cases of disease generally, and in particular classes of disease. As very little reliance can be placed upon the reports of many of our hospitals and dispensaries, where it is too often the practice to put down the patients who do not return as cured, in order to present a flourishing report, it cannot be hoped that the records of spas should be more conscientiously kept.

In some of the statistics, the results, however, agree very well; thus, at Bath, Balaruc, Wiesbaden, Bourbonne les Bains, the number of cures of severe paralysis is in the proportion of seven per cent. Unfortunately it is the same in several other waters of far inferior potency. The cases in which the most favourable results have been obtained are wounds, scrofulous affections and tumours, diseases of the mucous membranes, and uterine affections; the last probably including a good many cases of hysteria.

Of all countries in Europe, France probably contains the greatest number of mineral waters, and Germany the most renowned. Spain is not deficient; Italy has the celebrated thermal-saline waters of Lucca.† Savoy possesses the thermal-sulphurous waters of Aix, a delightful place of resort. Belgium the chalybeate of Spa, which has given a name to medicinal mineral waters. In short, no country in Europe is wanting in some of these beneficent resources. England is not deficient in thermal, saline, sulphuretted, or chalybeate waters. Perhaps few countries are so little gifted in this respect as Ireland.

* The former, Gilsland, is most unduly depreciated by Dr. Johnson, in his Spas of England. It may be well therefore to take this opportunity to state, that for pure air and water, beautiful and romantic scenery, opportunities for pursuing angling, associations connected with ruined border castles, the Roman wall and camps, Gilsland has no superior. The neighbouring station of Amboglanna (Ambo glannæ, doubtless, from the British glen) has been recently excavated by a member of our profession, a distinguished archæologist. There is also a strong chalybeate.

† A few miles from Civita Vecchia, in Italy, there is a remarkable and powerful thermal sulphurous water, little known except to the people of the neighbourhood. The water issues from the ground hot enough to boil an egg, and flows some distance through the ruins of Trajan's villa. It passes through an ancient bath, still perfect enough to be in use. The water is there endurable. The writer bathed there many years ago. Pliny describes the place in his Letters, while on a visit to the Emperor. The water has a great repute in the vicinity in rheumatic and gouty affections.

It possesses numerous springs endowed with some feeble properties; but none, as far as can be gathered, entitled to the dignity of a spa. The spring of Mallow has a temperature of 70 degrees, and 10,000 grains contain 2·01 grammes of solid contents.

In appreciating the three works at the head of this article, it may be stated generally, that they are probably the three best works yet published on the subject. That of Dr. James, besides containing a good scientific account of the waters, is well calculated to act as a guide-book for practitioners and patients, from the description it gives of the different localities. The work of Dr. Herpin, within a limited space, presents a most excellent, clear, and comprehensive view of the subject; and besides, has appended very valuable and laboriously compiled tables of the composition of the waters. The work of Dr. Lersch is of a totally different character. The first volume shows what the work will be—viz., a mine of research; in fact, the author proposes to exhaust the subject. The commencement of the list of contents and the extracts already given will show what the work is. "Contents of the first volume.—Abbreviations and Chemical Signs —Measures and Weights—Composition of the Springs generally," &c. &c. The author then goes into the whole subject of the natural history of water with German minuteness. To give an instance: at p. 459, under the head of "Influence of Hot-water Baths upon the different Factors of the Imperceptible Perspiration"—in speaking of the normal constant loss of the weight of the body, he observes:

"The number and amount of the substances which a man daily or hourly loses from his body varies greatly, so that no general accuracy concerning it can be reckoned on. They change according to the humidity and temperature of the air; for a warm, or at least a dry, atmosphere must promote the perspiration; it is so much more abundant, the more copious the atmospheric currents; it appears to be greater when the body is full of nutriment. All researches serve to show that the greater part of this loss is through the skin and lungs."

He then goes on to give the researches of Dalton and others on the amount of loss through the skin and lungs—in fact, we receive a summary of the physiology of the subject. To analyse such a work at length within the limits of this article is impossible. It will probably prove a mine of wealth to future writers.

REVIEW XII.

On some Points in the Anatomy of the Liver of Man and Vertebrate Animals, with Directions for Injecting the Hepatic Ducts, and making Preparations. By LIONEL S. BEALE, M.B. Lond., Physician to King's College Hospital, and Professor of Physiology and General and Morbid Anatomy in King's College, London. Illustrated with upwards of Sixty Photographs of the Author's Drawings.—*London,* 1856. 8vo, pp. 80.

EVERY tyro in Physiological Anatomy must be aware that as to one of the most important features in the ultimate structure of the Liver, —namely, the relation of the gland-ducts to the secreting cells,—there is at present great difference of opinion amongst those who have investigated the problem; the facts hitherto obtained by the most careful scrutiny, not having been of such a kind as to justify any very positive interpretation. On *à priori* considerations, we should at first be naturally led to expect that the hepatic cells, though apparently forming a solid parenchyma traversed by bloodvessels, are really contained within some kind of tubular or follicular membrane, continuous with that which lines the hepatic ducts; since if it be not so, the liver of Vertebrate animals must be constructed on a plan so different from that of Insects, Crustacea, and Mollusca, that its homology with theirs might be seriously questioned. But to this it may be replied that the Liver of Vertebrate animals *is* a very different organ from that of any Invertebrata; since it is not merely a *secreting*, but also (and this perhaps is its principal function) an *assimilating* gland; and that we might therefore expect to find it constructed, in part at least, upon the plan of the Peyerian bodies, the Malpighian corpuscles of the Spleen, and other similar aggregations of assimilating cells, which cluster around capillary plexuses, without the intervention (so far as can be discerned) of any other membrane than that which belongs to the bloodvessels.

It is well known that Mr. Kiernan, whose admirable researches laid the foundation of all subsequent investigations into the structure of the Liver, advanced the doctrine that the hepatic ducts form a plexus in the substance of each lobule. He did not profess, however, to prove the existence of any such plexus; but he rested his belief upon various arguments, which had, perhaps, more weight at that period than could now be conceded to them,—one of the most cogent being the existence of such a plexiform arrangement of hepatic ducts (known under the name of *vasa aberrantia*) in the left lateral ligament of the liver, where they are accompanied by branches of the vena portæ, hepatic artery, and hepatic vein, so as to present all the elements of a lobule, save the secreting cells. Professor Müller, as might be expected, concurred with Mr. Kiernan in the belief that the secreting structure was essentially tubular or follicular; but he doubted the anastomosis of the ducts, thinking it more probable that they terminate in tufts of tubes, having free and blind extremities, as in the lower animals. Neither of these doctrines has found much favour since the discovery of the hepatic cells; and the prevalent opinion of late years has certainly been, that the hepatic ducts do not penetrate into the substance of the lobules, the secreting cells being distributed in such a manner as to fill up the spaces between the capillary network, and not being enclosed within any limitary membrane. Thus Henle conceived that the commencing ducts are mere interstitial channels in a mass of cells filled with bile, and that these nascent and imperfect ducts pass into others which have a distinct bounding membrane and lining epithelium. By Theile and Kölliker, moreover, the existence even of these intercellular passages is denied, the smallest ducts surrounding the lobule being supposed to impinge upon columns of hepatic cells which converge towards the centre of the lobule, the bile secreted by the interior of which is conveyed along these radiating columns, from cell to cell, until it finds its way into the ducts. Dr. Handfield Jones, who has given great attention to the comparative anatomy of the liver, maintains that the hepatic ducts terminate altogether on the outside of the cellular parenchyma of the lobule; and in this he is supported by Mr. Huxley, who first brought the analogy of the assimilating or "ductless" glands in support of this view. On the other hand, Prof. Retzius has affirmed the existence of some such cell-containing plexus as Kiernan supposed to pass through the substance of the lobules; but the results of his method of demonstration are far from satisfactory. Dr. Leidy, of Philadelphia, has supported the same view; but we are not aware that any one has verified his statements. For ourselves, we must confess that although we at one time adopted them on the strength of Dr. Leidy's reputation for care and accuracy, we have never been able to see anything like the biliary plexuses which he has described and figured.

Such being the state of uncertainty in which the question has lain for some time, it is obvious that any new data that seem likely to help in its determination, ought to be carefully considered by all who are interested in the subject; and such data have been recently obtained by Dr. Lionel Beale, of whose researches we shall now give a brief account.

Every Anatomist who had previously attempted to investigate the distribution of the Hepatic Ducts by means of injections, has been disappointed at the very imperfect results which he obtained; the injections seldom filling more than the ramifications of the ducts on the exteriors of the lobules, and only now and then entering a twig which passed a short distance into their substance. It occurred to Dr. Beale that this failure might be due to the circumstance, that the bile remaining in the ducts would be pushed back by the injection into the lobules, and that, having no means of escape from the terminal portions of the ducts, it would prevent the entrance of the injection into them. His first care, therefore, is to empty the bile-ducts as completely as possible, by distending the sanguiferous system with water, so as to make pressure on the lobular plexuses of ducts (supposing such to exist), and thus to force their contents into the larger ducts; and after this has been thoroughly accomplished, he finds that injection carefully thrown into the bile-ducts, will penetrate much further into the lobules than it had been previously seen to do. For the details of his method we must refer to his treatise, as we are only concerned with its physiological results. In order to harden the substance of the liver, so as to allow sections to be advantageously made of it, he treats it for some time with dilute alcohol, to which a few drops of solution of soda have been added. Of the value of this combination, in regard not only to this, but to other tissues to be prepared for microscopic examination, Dr. Beale speaks very highly; the soda rendering those substances transparent which the alcohol has hardened. For preserving these and other preparations illustrative of the structure of the liver, he mounts his sections sometimes in syrup, sometimes in glycerine, either of which fluids has a conservative effect upon this tissue, without diminishing its transparency.

The following are the general results of the inquiries prosecuted by

Dr. Beale in these modes. He considers that he has demonstrated the existence of a cell-containing network of tubes formed of basement-membrane, which interlaces with the sanguiferous network; the former, generally speaking, occupying the entire spaces left between the meshes of the latter, so that its outer surface comes into contact with the surface of the sanguiferous network, and the two membranes become incorporated; but in certain cases, especially in the fœtus, the distinctness of these membranes may be unmistakeably recognised. The hepatic cells are not attached (like epithelium-cells) to the walls of the tubuli, but lie in their cavity, free oil-globules and granular matters being commonly found amongst them; usually there is only room for one row of cells, but sometimes two or more lie across the tube. The diameter of the tubes of the cell-containing network is commonly about $\frac{1}{1000}$th of an inch. At the centre of the lobules, the cell-containing network terminates in loops, which lie close to the intralobular vein; and near the margin of the lobules it becomes continuous with the ultimate ramifications of the ducts, which are lined with a flattened epithelium, and are commonly not more than $\frac{1}{3000}$th of an inch in diameter, being often seen less. The following parallel is drawn by Dr. Beale, between the structure of the secreting apparatus of the liver, according to his view of it, and that of other glands:

"According to the observations just described, the cells of the liver correspond in all essential characters to the secreting cells of other glandular organs. They lie within a cavity of basement-membrane, which is here arranged so as to form a network, the tubes of which are directly continuous at various points with very narrow efferent ducts. Now this narrowing of the duct before it becomes continuous with the *secreting* portion of the organ, is seen in other glands. In the kidney, the total diameter of the *straight and ductal* portion of the renal tube is considerably less than that of the *convoluted and glandular part*, although the central cavity is wider, which allows of a very rapid removal of the secreted products found in the convoluted portion of the tube. The cavity of the very narrow ducts of the liver, although so small, would doubtless admit the passage of a larger quantity of fluid, within a certain time, than the variable and irregular interstices existing between the cells and the basement-membrane in the secreting portions of the network. A somewhat similar arrangement occurs in many other glands. In the liver, where the secretion is highly elaborated, and slowly removed from the secreting structure in a concentrated form, we should naturally expect to find the contrast between these two different portions of the gland even more remarkable than in the examples referred to. This is really the case." (p. 61.)

Our readers will naturally expect some opinion from us, with regard to the degree in which Dr. Beale has substantiated the somewhat positive statements contained in the above quotation and in the descriptions we have abridged in the paragraph which precedes it. Having ourselves had the opportunity of examining many of his preparations, we do not hesitate to say that they afford strong evidence of his doctrine; but this evidence is for the most part rather *inferential* than *direct;* and even if his facts were beyond dispute, the correctness of his deductions from them is not incapable of being called in question. The features which most strongly impressed us as being clearly exhibited by his injected preparations, are the penetration of fine ramifications of the bile-ducts into the margin of the lobules; their apparently abrupt termination after passing a short distance inwards; the diffusion of the injection beyond those terminations, not vaguely through the cellular parenchyma of the liver, but within definite boundaries imposed (as it appears) by a limitary membrane; and the apparent continuity of the flattened epithelium which lines the narrow duct, with the glandular cells which occupy its dilatation.

Altogether, we may unhesitatingly affirm, that Dr. Beale has added much to the scanty evidence which previously existed in favour of Mr. Kiernan's view; and that certain of his facts are not to be easily explained away by those who advocate the doctrine that the cellular parenchyma of the liver is *outside*, instead of *inside*, the terminal ramifications of the glandular tubuli. But knowing as we do, that so excellent and practised an observer as Prof. Kölliker, after a careful examination of the preparations selected by Dr. Beale as affording his most cogent evidence, remained unsatisfied by them, we hesitate in asserting that he has established the existence of the cell-containing network as an unquestionable fact.

The merit of Dr. Beale's essay, however, does not solely consist in its contributions towards the solution of this *quæstio vexata;* for he has added not a little to our knowledge on various other points which are by no means deficient in interest. Thus, having carefully investigated the degree of lobular division which exists in the livers of different mammalia, he has shown that this reaches its maximum in the pig, that it is less distinct in the horse, rabbit, and rodents in general, and least observable in the ox, sheep, and human subject; this difference, as he justly observes, corresponding with that which we meet with between the much-divided kidney of the porpoise, the lobulated but more compact kidney of the ox, seal, and human fœtus, and the solid kidney of the human adult and of mammalia generally. Dr. Beale has also given a much fuller account than we heretofore possessed of the curious sacculi which are found in the coats of the bile-ducts, and which have been commonly regarded as mere mucous follicles. These sacculi, in the pig and most mammals, are arranged entirely around the tube into which they open; but in the human subject they form two parallel rows, one on either side of the duct; and they frequently communicate with each other in the coats of the duct. Dr. Beale advances reasons which render it improbable that they are mucous glands; but he does not seem to us to produce any good ground for his hypothesis that they are *diverticula*, analogous to little gall-bladders, in which the bile may be temporarily retained, while it becomes inspissated, and perhaps undergoes other changes. He has made it probable, however, that they belong to the same category with the *vasa aberrantia*, which he has found in many other situations than those in which they had been previously recognised. Thus, he says—

"In the very thin edge of a horse's liver, which was composed principally of fibrous tissue, I have been enabled to trace the gradual alteration of the ducts through many intermediate stages, to the ultimate complete disappearance of the secreting cells, until at length nothing remains but a branched tube without any cells in the interior." (p. 33.)

It will be obvious, from this account of Dr. Beale's labours, that we consider them to reflect great credit upon his industry, sagacity, and manipulative skill; and we hope that he will not satisfy himself with what he has already done, but will follow up his investigations in a still greater variety of methods, until he shall have completely unravelled the mystery which he has set himself to disentangle. We must not forget to say a word or two respecting the illustrations of his treatise, which are in a style altogether new—namely, photographic reproductions of his original drawings. We cannot say that they impress the eye very favourably or pleasantly as works of art; but they answer the author's purpose in reproducing his delineations at a very small expense of money, although the multiplication of them must have involved a considerable amount of time. His reason for adopting this method in the present instance, was the costliness of engravings or lithographs, joined to the probability of a limited demand for the work.

Review XIII.

1. *Syphilizationen.* Studeret ved Sygesengen. Af Wilhelm Boeck, &c. 1854.

Clinical Observations on Syphilization. By William Boeck, Professor of Medicine in the University of Norway.—*Christiania,* 1854. 8vo, pp. 211.

2. *Die Syphilization bei Kindern.* Von W. Boeck, &c. 1856.

Syphilization of Children. By William Boeck, Professor of Medicine in the University of Norway at Christiania.—*Christiania,* 1856. pp. 54.

Two or three years ago, a bold young French physician startled the grave deliberations of the *Patres Conscripti* in the French Academy of Medicine, by the announcement of his having discovered a new method of treatment of syphilis, with which he proposed to extirpate that wide-spread malady from our nosology. Not only did Auzias Turenne aim at the cure of syphilis in persons already affected with the disease, but he shocked morality by the proposal to render individuals hitherto untainted with syphilis totally unsusceptible of the venereal virus. The French Academy of Medicine met, and an acrimonious discussion ensued. The moral and hygienic objections seem to have been those which were descanted upon; the facts do not seem to have been very carefully inquired into; no experiments were made to test the truth or falsehood of the new mode of treatment, and under the powerful influence of Ricord it was rejected by the Academy, in spite of the protest of Malgaigne and others against this summary decision. In this country, the subject seems to have excited very little interest. One or two journals briefly alluded to it in terms of unqualified condemnation, and the only notice of the controversy from an impartial point of view is given in 'Ranking's Abstract of the Medical Sciences,' p. 333, vol. xvi., by Dr. Radcliffe. Since then, with the exception of two papers by Victor de Méric, in the 'Lancet' for 1853, no notice has been taken of the subject, and the medical public in this country seem to regard the question as finally settled by the fiat of the French Academy. Not so, however, our brethren on the Continent. In Norway, in Sweden, in Turin, and elsewhere, the bold empiricism of Auzias Turenne has been carefully put to the only test capable of deciding the question at issue—viz., that of experiment. Not content with merely declaiming against syphilization as unheard of and unjustifiable, Professor Boeck in Christiania, Danielsen in Bergen, Carlsson in Stockholm, and Sperino in Turin, have for some years past been engaged in a series of careful experiments and observations to determine the truth or the fallacy of Turenne's practice. It is plain that experiment alone can decide the question; theory here is of but little avail, and would be of no more use in disproving stubborn facts—if such there really be—than if it were directed against the efficacy of mercury in primary syphilis, or of quinine as an antidote to ague. The French Academy seems to have rejected the practice of Turenne without putting it to the proof; indeed, as we observed before, the moral question alone was tried, and found wanting, while the actual facts seem hardly to have been discussed at all.

The two letters on this subject, by Victor de Méric, inserted in the 'Lancet' (vol. x., July, 1853, p. 203), are written with a strong bias in favour of the decision of the French Academy, and of the views of Mons. Ricord, with which the experiments of Turenne are so directly at variance. Dr. de Méric at that time was, however, only acquainted with the incomplete researches of Turenne himself, and had not seen the large work of Sperino of Turin, on the same subject. We must plead guilty ourselves to the same omission, for we have not been able as yet to obtain the work in question, and we are only acquainted with it through the notices of it interspersed in Professor Boeck's work, and the brief review given in Ranking's Abstract above referred to.

As the subject of syphilization has as yet been almost entirely unnoticed in this Review, it may be interesting to our readers to give a brief history of the rise and progress of this singular method of treatment up to the present time.

Auzias Turenne, a young French physician, commenced, about the year 1844, a series of experiments, with the view of testing the validity of John Hunter's doctrines of the non-communicability of syphilis to the lower animals. After many experiments and several failures, he succeeded in producing in monkeys inoculated with chancre matter a disease which had all the characteristics of true chancre. This was at first admitted in the French Academy, but at a later period was denied. However this may be, it is quite certain that a contagious disease was communicated to the poor animals, and that from these it was transferred to rabbits, cats, and horses. The malady was again from these returned by inoculation to the human species, the first trials in this regard having been made by Dr. Robert Weltz, of Würzburg, on his own person. On four separate occasions, Dr. Weltz succeeded in producing an unmistakeable chancre on his own person, by inoculation from animals, and this was acknowledged even by Ricord.

While Auzias Turenne was thus engaged in researches on the transmission of syphilis to animals, he became aware of the curious fact, that each succeeding chancre produced by inoculation became less and less in each animal, until at length a period arrived when inoculation apparently lost all its power, and no chancres or sores of any kind followed the application of the venereal virus. From these facts he drew the inference, that by prolonged inoculation with the syphilitic poison, a constitutional state or diathesis was at length produced in which the system was no longer capable of being affected by syphilis. This condition he terms "syphilization," and upon this asserted discovery all the subsequent experiments and peculiar mode of treatment are based. Auzias Turenne and his followers contend that by such a process of prolonged inoculation the system becomes protected for the future against the venereal poison, just as an individual who has had small-pox cannot take the disease a second time. To obtain perfect syphilization or immunity, the individual must undergo constitutional syphilis; but he must be forced rapidly through this disease by repeated inoculations, in order that it may not injure the constitution.

The abortive experiments of Diday in 1849 require but little notice. He proposed to inoculate with blood drawn from a person labouring under tertiary syphilitic symptoms, so as to prevent, as he imagined, the poison from entering into the constitution at all. Although this proposal was apparently based on one of Ricord's supposed "laws"—viz., that constitutional syphilis never affects an individual but once in his lifetime, it was also in direct contradiction with Ricord's positive opinion, "that tertiary syphilis could not be communicated by the parent to the child."

After a series of experiments, Auzias Turenne's doctrines were laid before the French Academy of Medicine (November 18th) in 1850; and, as might be expected, opinions so novel and so startling met with the most vehement opposition. Turenne had, it seems, only recently commenced at that time his experiments on syphilization in the human subject; he had, therefore, few or no data for the support of his opinions, and he not only proposed to employ syphilization for the primary and secondary forms of venereal disease, but suggested the use of this treatment as a prophylactic against the contagion of syphilis in persons as yet untainted with that malady. It was upon this latter point that the discussion mainly turned, and here the indignation of his opponents was unbounded at the audacity and immorality of such a proposal. We cannot deny that they had right on their side; the proposal was not only immoral, for the disease is one to which an individual voluntarily subjects himself by a lapse from the rules of morality, but it was also most injudicious to subject a perfectly healthy person to the danger of incurring a malady from which he might never again be able to free himself. The true mode of determining the question—that of experiment, carefully conducted and often repeated—was not adopted, and an application by Turenne for leave to prosecute his researches in the Hôpital St. Lazare was negatived by the Commission. Hitherto, not being permitted to pursue his investigations in a hospital, he had only experimented on a few cases in private practice, and these were necessarily too few and too scanty in the details to be implicitly relied upon. The real question at issue, that of the reality or non-reality of syphilization, was left untouched. Malgaigne, Depaul, and others, in vain protested against the sweeping condemnation of these proposals before the truth or falsehood of the doctrine had been determined by experiment; the great influence of Ricord and his partisans prevailed, and the proposals by Auzias Turenne were unequivocally condemned. Shortly after, a strong case appeared in favour of the opponents of syphilization, in the person of a Dr. L——, who had allowed himself to be inoculated to produce syphilization, and was now covered with venereal sores. While matters thus proceeded in Paris most unfavourably for the advocates of syphilization, the question was being investigated on a large scale, and in a more complete manner, by Sperino of Turin. This physician had great advantages for the prosecution of his researches, as he was attached to the Syphilicoma, or Venereal Hospital, of the city of Turin. He had long remarked that large suppurating buboes healed more rapidly when their syphilitic character was tested according to Ricord's plan, by inoculation of the surrounding parts; and, moreover, that when the primary chancres were large and obstinate, the inguinal buboes were smaller and less freely developed. The longer the local disease lasted, the less chance there seemed to be of constitutional syphilis. Sperino made his first report on the subject to the Medico-Chirurgical Academy of Turin on the 23rd of May, 1851. In this report he gives the full details of fifty-two cases treated by him in the Syphilicoma of that city. If Sperino was not the first to employ syphilization for the cure of venereal disease in the human subject, he at all events first performed a regular series of experiments and observations to test the truth or fallacy of Turenne's doctrines.

"The subjects of M. Sperino's experiments were fifty-two hospital patients, all prostitutes, and all suffering from aggravated forms of primary or secondary syphilis. The virus was taken from the person syphilized, or from a comrade —from the first, if possible. The inoculations were repeated once or twice a week in three or four distinct places, usually in the abdomen. The time required for the establishment of the artificial chancres was from two to three days. The effects of the second inoculations were less serious than the first, the third than the second, the fourth than the third, and so on, until the virus ceased to produce any effect whatsoever; contemporaneously with which epoch all former ulcers had healed, and buboes, recent nodular enlargement of bones, and cutaneous stains or blotches, had either disappeared altogether, or were rapidly going away."

The virus also which made no impression at that time was found to retain all its virulence when tried on an unprotected person.*

Sperino's observations were confirmed by similar results obtained by Dr. Gamberini at Bologna, and by Gulligo at Florence. The report of the Commission appointed in this case, as at Paris, was unfavourable, but it did not extend to the prohibition of further experiments, and Sperino has ever since followed up this treatment in the hospital under his charge. In 1853 he published a detailed account

* See Dr. Radcliffe's Report on Surgery: Ranking's Abstract, p. 334, vol. xvi.

of 96 cases of syphilization in a bulky volume of 903 pages. We presume that the 50 cases given in his former report of 1851 are included in the 96 here detailed; but as we have not seen Sperino's original work, we can only quote from Dr. Boeck's extracts. Of these 96 cases, 53 were of primary syphilis, and 43 of the constitutional disease. Fifty of the cases of primary syphilis were cured, 2 failed, and 1 was not treated by syphilization alone. Of the 43 cases of constitutional affection, 26 were treated by syphilization alone, and 17 by this method in conjunction with mercury or iodine. Twenty-five of the 26 in the first category are said to have been cured. In only 2 cases of the primary disease did any constitutional symptoms appear, and these symptoms rapidly yielded under a continuance of the syphilization. No relapse has yet taken place in any case. Many of these cases were of very severe character, and were such as were not likely to have healed spontaneously; while the numerous inoculations that were required produced no serious effects, except in one or two instances a slight tendency to form phagedænic sores.

We have placed only two of Dr. Boeck's publications at the head of this notice, and this because these two works contain the whole result of his experiences in syphilization up to the summer of 1856. Already in 1853, he published, in the seventh volume of the Norwegian Medical Journal, a brief notice of the results of a few experiments he had then made. Since that time he has closely investigated the subject in all its bearings, and has, as we see, even extended the practice to infants at the breast. The same plan of treatment is now pursued by Dr. Danielsen in the hospital at Bergen, though we believe that as yet he has published no report of his researches. We can, however, state that, on visiting this hospital in July, 1856, he assured us that he fully coincided with the views of Dr. Boeck, and that the results obtained in Bergen by syphilization were as successful as those recorded by the latter at Christiania. Dr. Boeck's position as a practitioner of eminence in that capital, and as Professor of Medicine in the Christiania University, entitle him to due consideration. His experiments, however, have been carried on in a large hospital, in the presence of intelligent colleagues, and of a large body of students; and if the results had been otherwise than as stated by Dr. Boeck, the truth of his reports would unquestionably have been challenged. As the author, in conjunction with Dr. Danielsen of Bergen, of the great Norwegian work on the 'Spedalskhed, or Norwegian Leprosy,' he has already made for himself a scientific reputation; and from his earnestness and love of truth, we feel assured that, should he discover any error in the conclusions he has drawn from his researches on syphilization, he would be the first to proclaim this to the world.

In reviewing the two works before us, we shall study brevity and concentration as much as possible, but it would be unfair to pass over anything that materially elucidates the question at issue.

That mercury has the power of driving away, or at least of allaying, venereal symptoms, both primary and secondary, few at the present day will deny; but no one who has practised much among syphilitic patients can have failed to observe how uncertain cures by this mode of treatment frequently are. How often do we not see a venereal eruption healed to all appearance by a judicious employment of mercurials, and yet, after a short lapse of time, that the patient returns in a worse condition than heretofore. Moreover, it is a well known fact that, even after the constitutional disorder is apparently healed, the children of such parents frequently exhibit unmistakeable signs of syphilis, though the parents may have deemed themselves completely freed from the malady by repeated mercurial courses. Mercury has now for centuries held its place as an agent in the cure of syphilis, and though from time to time likely to fall into disrepute, it has always been reinstated in favour in the absence of any better means of cure. It is in England especially that it is most employed for this purpose; while on the Continent, and especially in the north of Europe, it is now less resorted to even in primary syphilis than at any other period of its career. More than 80,000 cases have now been treated on the non-mercurial plan; and the result of these observations has been to establish that syphilis is cured in a shorter time, and with less probability of inducing secondary symptoms, by the simple treatment alone. That syphilis is of an infinitely milder character in foreign countries than in England, is perhaps owing in a great measure to our prejudices regarding the regular medical and police supervision of prostitutes; but it is possible, too, that the severe cases of syphilitic caries of the bones, and the other terrible sequelæ of the disease, may be due in part to our almost universal employment of mercury in its cure.

Although mercury is perhaps our most powerful agent in combating syphilis, it is yet confessedly not to be regarded as an absolute specific; and again, it is maintained by many, and we think with great show of reason, that many of the severest forms of secondary and tertiary venereal disease are produced as much by the mercury taken for their cure as by the original malady:

"Such being the case," observes Dr. Boeck, "and our present remedies for the cure of syphilis being either insufficient or exercising an unfavourable effect upon the constitution, it appears to me that we are justified in trying a new method of treatment, which has had reported success, even though its operation may be inexplicable, and its adoption seem in contradiction to common sense. The great question, in our opinion, is, in what cases should syphilization be employed? As a prophylactic its adoption is unjustifiable, and even its discoverer now holds this opinion. Syphilization can therefore only be adopted where venereal disease already exists; but here, in my opinion, it is far from being applicable to every case. Hitherto it has been used both in primary and in secondary syphilis, but with this practice I cannot agree—I believe it to be contra-indicated in the primary forms. In ordinary non-indurated chancres, I would not practise syphilization, for there is a strong probability that, in such cases, the patient will escape the constitutional affection altogether. I do not, however, subscribe implicitly to the dogma of Ricord, that a non-indurated chancre can never give rise to constitutional symptoms, for I am quite of a contrary opinion. Among many other examples, I may refer to the case of a prostitute, Karin Ellingsdatter, who was under treatment in my division of the hospital for simple chancre, without a trace of induration. She was watched most carefully during the weeks after her admission, and constitutional symptoms of an unusually severe character showed themselves at the usual time.

I do not consider myself justified in subjecting patients to a treatment necessarily long and painful, in order to avert evil consequences which possibly never would ensue. Ricord maintains that an indurated chancre is the first symptom of constitutional syphilis. On this point, too, I cannot agree with him, and I should not deem myself justified in employing syphilization until constitutional symptoms have really shown themselves. It is no easy matter to decide upon the degree of induration which constitutes an indurated chancre. The case is widely different when constitutional symptoms have already declared themselves. Here there is no danger of introducing into the system by inoculation, a malady which before had no existence there. Whether in every case of constitutional syphilis this peculiar mode of treatment should be employed, is a question to which I shall afterwards return. I may merely observe here, that the syphilitic dyscrasis seems, by the lapse of time, to become occasionally so modified, that primary matter ceases to exercise any influence over it.

"All the cases that I have treated by syphilization have laboured under constitutional syphilis in its most varied stages and forms. Some of these cases had previously undergone every mode of treatment that science could devise, while others had had no previous treatment at all. I thought it of great importance to collect observations from both classes of cases. If syphilization is not had recourse to till all other remedies have been tried, it is difficult to form a correct estimate of its powers—for under such circumstances we hardly know what symptoms belong to syphilis, and what are to be ascribed to the medicines administered, and particularly to mercury." (p. 15.)

Not only are certain cases ill fitted for syphilization from previous mercurial treatment, but the state of health of the patient must be taken into consideration before submitting him to this prolonged and painful treatment. Dr. Boeck advises that we should not syphilize when any inflammatory diathesis exists in the system, as in such cases the artificial chancres may take on a malignant action. Habitual spirit-drinkers, and persons of very weakly constitution, should not be subjected to this treatment. The bowels should be regulated, and the digestive organs should be brought into good order; but it is not necessary to enforce any strict rule of diet. In the hospitals of Bergen and Christiania, the ordinary full diet of the hospital was always allowed. With regard to obtaining the patient's consent to the treatment, no difficulty seems to be found either in the Scandinavian or the Italian hospitals. Both Sperino and Dr. Boeck mention the readiness with which patients submitted to, and even sought for, the mode of cure which they had seen to be so successful with their fellow sufferers.

Various methods of inoculating the venereal virus have been adopted by the advocates of this system. Auzias Turenne at first kept up a succession of single chancres; while Sperino made three or four separate inoculations at once, and repeated these two or three times in the week. After having in this way reached the number of twenty-four or thirty inoculations in all, he found that the chancres last produced were exceedingly small, and he then diminished the intervals, and made more inoculations at each sitting. He found that the first chancres were deeper, larger, and more inflamed than those which succeeded them; and that by diminishing the intervals and increasing the number of inoculations, the earliest chancres visibly diminished, and were less painful and inflamed. To test this still further, Sperino ventured upon as many as sixty inoculations at once upon the same individual; but the result obtained was, that *immunity* to further inoculation set in before the syphilitic symptoms were cured, and relapses of the disease frequently ensued. He therefore returned to his former plan, and now inoculates for six to ten chancres at each sitting. While these chancres are progressing, it is neither necessary nor advisable to inoculate afresh, nor should this be done until the former chancres are developed. Should the chancres be developed too freely, and threaten to produce active inflammation, or to extend as phagedænic sores, he checks their progress by inoculating afresh at shorter intervals.

The practice of Dr. Boeck differs very little from that of Sperino. At first, afraid of producing too serious an impression on the system, Dr. Boeck inoculated for two chancres only every six days, selecting that period of time because he found from experience that it required about five days to produce induration in a chancre; although he does not, as we have already seen, consider this latter circumstance absolutely essential. Subsequently he has shortened his intervals to three days, and increased the number of inoculations to eight or ten. Less time is thus required to produce immunity, but Dr. Boeck has a wholesome distrust of those cases which are pushed too rapidly through their course of syphilization.

With regard to the most favourable points in the body for inoculation, Sperino placed his punctures on the lower part of the abdomen, while Dr. Boeck prefers inoculating on the arms and thighs. Accompanying each of his observations in the volume before us is a lithographed outline-plate of the human figure, with the points of inoculation, and the date of each; while lines drawn from the arms to the thighs, enable us to follow the transpositions of the virus from one chancre to another. By this simple figure it is easy to trace the progress of the treatment, to see the number of inoculations at each sitting, and the source from which they are derived.

After these preliminary observations, Dr. Boeck proceeds to detail at great length the particulars of twenty-one cases where syphilization was employed. The history of these cases occupies not less than 156 pages, and to each case is appended a page or two of observations on its peculiar features and on the relative effects of the treatment. We shall select two or three of these cases, abbreviating their details; and shall give a brief *résumé* of the others.

"CASE I. Admitted into the General Hospital, October 27th, 1852. A female, aged fifty-six, suffering from ecthyma syphilitica of the whole body, with syphilitic tubercules (tubercula mucosa) between the toes, upon the membrane covering the fauces, and at the angles of the mouth. She had been infected with syphilis by a soldier some ten or twelve years previously, but the eruption did not appear until the previous summer. Inoculation was commenced October 29th; the matter was taken from an indurated chancre on the glans penis of a sailor, who had been infected in England three weeks previously. For some time only two inoculations were made, with intervals of six days; subsequently, the inoculations were increased to six and more. The matter from the first sore was carried through a series of fourteen periods, when it no longer acted, and fresh matter was obtained from another source.

On the whole, 260 inoculations were made at 56 sittings, 38 of these had no result at all, and several more inoculations were more or less abortive. The only affection of the general health was an attack of gastric fever thirty-seven days after inoculation was commenced; it lasted for two days, and then the treatment was resumed.

"Nine days after syphilization had been first employed, she remarked that the pains in her legs were not so severe as when she entered the hospital. Thirteen days after the first inoculation, the mucous tubercles on the palate began to spread out and flatten, and two days after that they began to disappear on the back part of the palate. On the thirtieth day, fresh tubercles showed themselves on the nape of the neck, so that the former semicircle of tubercles there was converted into a perfect circle. At the same time that these tubercles appeared, the mucous tubercles of the palate began to diminish, and had entirely disappeared in forty-four days. In fifty-seven days the sores on the legs were entirely healed. About the seventy-sixth day she complained of pains in both clavicles, and in both humeri (dolores osteocopi) which did not cease until the one hundred and twenty-seventh day. On the one hundred and thirty-third day traces of psoriasis showed themselves in the palms of both hands; and these, the last symptoms that remained, did not entirely disappear till after the lapse of two months. The patient's general health improved remarkably during her residence in the hospital, and up to the present time (August, 1856) the disease has shown no disposition to return." (p. 15.)

"CASE II. Admitted 22nd Feb., 1853. This patient was a girl with large syphilitic sores on the left thigh. Her mother had been in the hospital for constitutional syphilis in a high degree, in the year 1844, at which time the present patient, then aged six years, was also under treatment with mercury. At that time she suffered from roseola syphilitica, and syphilitic ulcers in the throat. At the close of that year (1844), she was again admitted with a papular syphilitic eruption, and an affection of the throat. She was then treated with liquor bellostii and decoct. sarsæ (liquor hydrarg. nitrat.). Again, in 1845, she was a patient in the hospital, and then suffered from lichen syphilitica, with affection of the throat. She was treated at this time on Dzondi's plan, with mercurials. Twice since then the disease has reappeared, but has *gone away without any treatment.* The present attack has lasted for twelve months, and is described as a syphilitic tubercular affection, with lupus serpiginosus syphiliticus.

"The treatment by syphilization lasted for about five months, during which time 124 inoculations were practised in twenty-seven sittings. Of these 124 inoculations, 33 entirely failed, and many more were abortive. Thirty days after the first inoculation, erysipelas showed itself in the vicinity of one of the chancres, and was accompanied with sharp febrile symptoms, which lasted seven days. In two months her general appearance was greatly better than when she had entered the hospital, and the sores on the lower limbs were healing rapidly. In three months, immunity to any venereal virus that could be obtained showed itself, and the sores had entirely healed. Her general health has since continued excellent, and she has had no symptoms of a relapse. Dr. Boeck admits, that as the syphilitic tertiary symptoms had twice receded of their own accord, this case is open to objection; but one thing he observes is certain, that while the sores which had then lasted for a year, rapidly healed under syphilization, her general health was improved to a degree far beyond that which she had ever previously enjoyed." (p. 29.)

"CASE III. Admitted 21st Feb., 1853. This was a prostitute, aged twenty, suffering from roseola syphilitica of the face, breast, back, and thighs, with tubercula mucosa over the inner surface of the labia majora.

"The symptoms in this case receded slowly, fresh tubercles showed themselves on the labia majora on the fourteenth day, and by the twenty-first day they had also invaded a considerable portion of the mucous membrane of the mouth. These appearances did not recede till after the lapse of five weeks. and about that time a papular eruption showed itself on various parts of the body. While the tubercular symptoms were receding, fresh ulcerations appeared in the fauces and on the organs of generation. Two months after syphilization had been commenced, fresh mucous tubercles showed themselves around the anus, and lasted for several weeks. A month after immunity was obtained, she was attacked with iritis, which continued for several weeks."

Dr. Boeck believes that in this case the syphilitic virus that was employed was too weak, and no fresh venereal matter could at that time be procured.

In the first and third cases mercury had never been taken; in the second case it had been employed to a considerable extent.

The fourth case is of much interest, as it seems to exhibit the influence of mercury on the system, so as to render it less susceptible of the action of syphilization.

"Case IV. A girl, aged twenty-four, was admitted on the 10th May, 1852. She had large syphilitic tubercles on various parts of the body, ulcerated tubercles on the legs, and syphilitic affections of the bones. She was treated, first with iodide of potassium and preparations of iron, which improved her general health, but did not alter the syphilitic affection. Subsequently, inunction of mercurial ointment was carried on to complete salivation, and after that iodide of potass was given again. After five months' stay in the hospital, she left, apparently cured. A month after leaving, she returned, in nearly the same condition as at first; and after undergoing Dzondi's cure for two months, she again went out apparently well. In a very short time she came back in her present condition, and after iodine had been again tried in vain, recourse was had to syphilization. Inoculation was commenced on the 9th March, 1853, and was continued for two months, when the treatment was stopped for want of a supply of the venereal virus. Salivation by mercurial inunction was now again had recourse to, but was of no avail. Four months later, Dr. Boeck was able to obtain a fresh supply of virus, and the treatment by syphilization was resumed, and was persevered in for three months longer, when complete immunity showed itself; but still the patient was not cured, for tubercular excrescences and sores still continued to appear on various parts of the body. Iodide of potassium was now again had recourse to, and, to Dr. Boeck's surprise, its beneficial effects were rapid and complete, though it had twice been tried before in vain." (p. 40.)

It is a remarkable fact, but apparently established by several observations of Dr. Boeck, that iodine is frequently of no avail before syphilization; while after that treatment, as in the present case, it will produce most marked good effects.

"Case V. is one of psoriasis syphilitica over the whole body. The patient was for two months under treatment by mercurials after Dzondi's plan, but without the slightest amendment, and she was exceedingly weak and emaciated. Her course of treatment by syphilization lasted eight or nine months; she had not less than 683 inoculations, only 90 of which proved abortive. About a month after syphilization was commenced, the eruption diminished on the lower extremities and trunk of the body; and five months afterwards, it was confined to a small spot on the nates. This in time disappeared, the general health had greatly improved, and since then she has continued well."

It would occupy more space than can be conceded to us, were we to attempt the analysis of the other cases in this book; all of them have evidently been carefully observed, and the symptoms recorded day by day.

We prefer to direct the reader's attention to Dr. Boeck's general *résumé* of his observations at p. 177 of the first work on our list, and with the analysis of this we shall combine such of his still more recent experiences as are contained in the second work, which, having been published this year (1856), may be said to contain the latest researches. In these general observations, Dr. Boeck tells us that he desires as much as possible to confine himself to simple facts, and to exclude theory. The observations having been made in a large hospital, under the eyes of his professional brethren, and of numerous students, he only claims for them the merit of accuracy of detail, without insisting that he is perfectly correct in his interpretation of the phenomena recorded. The local symptoms produced by inoculation first claim our attention. Much difference has been observed in different individuals, and even in the same persons at different periods of the treatment, with regard to the space of time required for the maturation of the pustule. This in part, no doubt, depends upon the idiosyncracy or state of health of the patient, but Dr. Boeck thinks that it is still more influenced by previous mercurial treatment. If mercury has been given, the development of the pustule and chancre is almost always slow. In general, the pustule produced by inoculation of the venereal virus has a more or less intense areola, and a depression in the centre like that of small-pox, with a black central point. When the pustule bursts, a regular chancre is soon formed, often of considerable size and depth,—especially after the first inoculations. As syphilization advances, these chancres become progressively smaller and shallower, and still later the pustules become exceedingly small, and have little or no areola. Inoculation, Dr. Boeck thinks, should be *commenced* on the arms, as the chancres formed there rarely become so large or so deep as those upon the thighs. If, as has happened in three or four instances, febrile reaction shows itself during the treatment, the artificial chancres generally put on a more threatening aspect. (May not the phagedænic tendency of the sores be the cause, and not the consequence, of the fever?) The cicatrices left by the artificial sores are remarkably small, and after some time leave very slight traces of their existence.

Syphilitic matter sent by Retzius from Stockholm, and by Sperino from Turin, has seldom proved efficacious in the Christiania Hospital.

The conclusions drawn by Dr. Boeck from the eighty-four cases of syphilization which he has treated up to March, 1856, are as follows:—

I. That in all cases, without exception, immunity to the venereal virus is obtained sooner or later by inoculation of this poison.

II. That the symptoms of syphilis present at the commencement of syphilization disappear during the employment of this mode of treatment.

III. That the general health does not suffer in the least from syphilization—on the contrary, if the patient has been in weak health before inoculation, he most materially improves in strength and appearance during the process.

We return to the first of these propositions. It seems indeed a bold assertion to maintain that one of the most intense animal poisons can be annihilated, as it were, by the introduction of fresh poison into the system, until at length the venereal virus has no more effect on the patient than a drop of water. That such immunity really does take place, we must concede as an undoubted fact. The unanimous testimony of Boeck, of Danielsen, of Sperino, and of Auzias Turenne, of Carlsson, and of Stenberg in Stockholm, all concur on this point; nor could we anywhere obtain a denial of this fact, either from the patients or from Dr. Boeck's colleagues, when we visited Christiania this past summer.

The explanation of this fact, however, has not as yet been given, and Dr. Boeck wisely abstains from indulging in any crude theories on the subject. He does not think it possible that a saturation of the system with syphilis can here take place, as in that case the symptoms would surely get worse instead of better; and if relapses occurred, as detailed in cases 13, 16, and 18, the return of the disease would be more severe than the former constitutional affection. This was so far from being the case, that these symptoms of relapse were remarkable for their mild character, and indeed seemed to hold the same place towards constitutional syphilis as varioloid disease does to small-pox. In most of the cases, the artificial chancres became successively less and less, but there were several exceptions to this rule, and the question has been raised whether the chancre matter has always the same degree of virulence. Dr. Boeck is of opinion that it gradually becomes weakened, for he has often found that matter taken from a single primary source becomes gradually less and less effectual. Still there are numerous exceptions to this rule; and in some of the recorded cases we remark, that for several inoculation periods the chancres produced were very small, and then suddenly appeared to acquire a greater degree of activity, so that they became as large or larger than those usually produced by the first inoculations.

Dr. Boeck believes that syphilitic matter may vary in its strength according to the degree of development of the chancre from which it is taken. We must also in this regard take into consideration the idiosyncrasies of the individual, and the changes that may take place in his constitution during the course of the treatment. As a proof of the gradual weakening of the syphilitic virus, Dr. Boeck notices the fact, that when, after a long course of inoculation of matter, taken each time from the most recent pustules, he found the inoculations beginning to fail, he has repeatedly gone back to some of the older chancres, which were still secreting pus, and inoculated from these, and has found that the virus from this source produced complete chancres of a more active character.

Dr. Boeck is of opinion that the virulence of syphilis is rapidly diminishing in Norway. He has found the greatest difficulty in obtaining fresh chancre matter of sufficient power; indeed his inoculations have been mainly carried on by matter primarily taken from chancres incurred in England or in Germany. For the last six years he has been in the habit of inoculating from every sore of suspicious character that came under his observation in the hospital; but twice during that period was he unable to find, for more than twelve months, a single inoculable chancre of native origin. He suggests, therefore, that a syphilitic sore may possibly infect *in coïtu*, and yet that the virus from it may be of no effect when inoculated artificially. We confess that we have strong doubts of the correctness of this opinion, but further observation alone can decide the question. The best virus for inoculation he generally found was that where the chancres were accompanied with suppurating buboes. The cause of the decrease of virulence of syphilis in Norway is, in our author's opinion, chiefly to be ascribed to the careful and regular inspection of the prostitutes in the towns of Norway, so that all fresh cases of syphilis are immediately subjected to treatment. If this is really the case, there is no danger of syphilis becoming extinct in this favoured and free land of ours, where the interference of sanitary regulations with prostitution is scouted alike by saint and sinner.

But it seems that there are likewise circumstances existing in Norway which may prevent the much-to-be-desired extinction of syphilis in that country. What these may be we shall present to our readers in Dr. Boeck's own words, perceiving that he details the observed facts merely as hints and guides to further investigations:

"When, in the course of my earlier investigations, I could no longer get the virus to act on the system, and no fresh virus could be procured, it occurred to me that perhaps I might regenerate the poison by passing it through the system of another individual. Auzias Turenne's experiments had shown that the virus from aborted pustules, in those who were fully syphilized, was yet capable of producing characteristic chancres in persons who had not been subjected to that treatment; but it remained to be proved that the virus from these chancres was more powerful than that which produced them. To ascertain whether this was the case or not, I transferred virus from No. 1 and from No. 12 to a person labouring under constitutional syphilis, but who had not been previously inoculated. The characteristic pustules and chancres were here produced, and from thence I transferred the virus back to No. 1, when it likewise proved efficacious for several periods in succession. To continue the experiment, I passed the virus thus regenerated through two other individuals, and when returned to No. 1 it again acted as on the previous occasion." (p. 190.)

Dr. Boeck tried this experiment on several occasions in other cases, and with the like result; but in three instances it failed, and he thinks that the virus was too much weakened here before the transferring it to others was attempted.

The contagion or non-contagion of secondary syphilis has been the subject of keen controversy. Although not bearing directly on the question of syphilization, it may be interesting to our readers to know the opinions of Dr. Boeck on this question:

"No one denies," says he, "that a chancre existing after constitutional symptoms have developed themselves can produce inoculable virus. I believe, however, that when the virus has resided longer in the system, it undergoes such changes that inoculation from it no longer succeeds, and perhaps this will explain the difference of opinion regarding the contagion of secondary syphilis." (p. 193.)

Dr. Boeck has been led to believe that great variety exists in the strength of the venereal virus. Some virus from a chancre incurred in England seemed to be of great activity, producing large and deep sores, but its efficacy did not continue nearly so long as the virus from a chancre contracted in Hamburg, which, however, never exhibited the actively destructive properties of the former. It is possible that this may depend on the greater or less dilution of the poison with other fluids, or upon its being respectively of a more serous or a more purulent character. The observations of Mr. Henry Lee in the October (1856) number of this Journal, throw some light upon this question, and we have no doubt that they will be read with interest by Dr. Boeck and his colleagues.

With reference to the immunity finally obtained by inoculation, Dr. Boeck says:

"The time that is required to produce immunity depends not only on the variable strength of the virus—upon the rapidity or otherwise with which the inoculations succeed each other, and upon the number of the chancres—but also upon the idiosyncracy of the individual. I have already stated that immunity in one case was obtained after seventy-one chancres, but I have also shown that the virus employed on this occasion was remarkably weak. The attaining to immunity depends on the length of the intervals between each inoculation—the more frequent the inoculation the more rapidly does immunity ensue. If there were sufficient virus to be obtained, we might, if we chose, inoculate every day; but if, as is generally the rule, we keep to obtaining the virus from the most recent inoculation, we cannot easily do this. From my own experience, I would say that the matter contained in a pustule of only one day's growth is generally capable of being inoculated; but I have also seen that pustules of three days' growth produced no effect; while three days later, the matter taken from them was decidedly contagious." (p. 196.)

Granting, then, that immunity to the syphilitic virus is really produced, the question immediately suggests itself, how long does this immunity last? Will it be for life, as in the case of small-pox, or will it endure only for a short time? And again, are we to believe that it is only the old constitutional poison that is eradicated by the inoculations, so that when all is finished, and the constitutional symptoms have entirely gone, the patient is exactly in the condition of an individual who has never had syphilis? We should ourselves think this cannot be the case, or the later inoculations, undertaken when the constitutional disease is fairly subdued, ought to affect the skin as they would the skin of a perfectly healthy individual. Dr. Boeck is evidently of opinion that those persons who have once reached perfect immunity to inoculation, are probably, for all the rest of their lives, insured against contracting syphilis again; but he confesses that he has not experimented to determine this question:

"I am much inclined to believe," says he, "that this is really the case, but to prove it is not so easy, for according to my views, this question cannot be determined by artificial inoculation. I have not considered myself justified in putting those healed by syphilization to the proof of inoculation after some time had elapsed since the cure, as I thought it possible that such inoculation *might* produce constitutional symptoms, as I believe that syphilization *destroys* the syphilitic poison in the system. Ricord's dogma, that an individual can only once in his life be affected with constitutional syphilis, may perhaps be erroneous, and the patient might then be in the position of a previously healthy person newly inoculated with syphilis. I have therefore always abstained from testing by inoculation those who had not previously been treated with mercury. Such, however, has not been my practice where the patients had undergone a mercurial course or courses before they came under our care. In my former work" (the first on our list) "I had remarked that, where mercury had been given before syphilization, we are never certain that relapses may not occur, although these last are of small importance in comparison to the previous constitutional affection. When in such cases I employed syphilization, and obtained a positive result, I did not consider that this proved the immunity before obtained to be of short duration, for this immunity was not the consequence of the entire destruction of the syphilitic poison in the system—as this had been hindered, if I may so speak, by the combination of the syphilitic with the mercurial poison.

"It has, moreover, often occurred to me that, when I have syphilized persons who had previously gone through a course of mercury, and when the powers of the virus began to fail, I obtained large pustules and chancres after the administration of iodine." (Syphilization of Children, p. 11.)

We now come to consider the second of Dr. Boeck's propositions—viz.: "That the symptoms of syphilis present at the commencement of syphilization, disappear during the employment of this mode of treatment." We think this cannot be denied; it is a fact proved now by hundreds of observations made by men worthy of all credit. In constitutional syphilis, where no mercury has previously been given, the cases have regularly progressed towards a complete cure under syphilization. Of 42 cases of constitutional syphilis, where no mercury had previously been used, *not one* had exhibited any relapse to the commencement of 1856, and many of these had been for three years and more without requiring any treatment whatsoever. Of the 21 cases recorded in Dr. Boeck's first work, 6 had been treated without mercury, and in all of these syphilization dispersed the symptoms, which have never since returned. The average duration of the treatment in these 6 cases was six months and two days; the average number of chancres was three hundred and twenty-two.

A second class of cases out of the 21 recorded in Dr. Boeck's first work, were those in which the constitutional symptoms were principally confined to the skin and mucous membranes, but all had taken more or less mercury. The average duration of treatment in these cases was six months and twenty-four days; the average number of chancres was four hundred and thirty-two. It must not be supposed that all these were fully developed chancres; on the contrary, two-thirds of the number at least were very small and transient. Two of these cases, Nos. V. and VII., were remarkably susceptible of the venereal poison, and consequently a large number of chancres was rapidly formed; and it is remarkable that these were the only two out of the five who had *no* relapses.

In the third category, Dr. Boeck includes those individuals who laboured under very inveterate forms of constitutional syphilis. The 7 cases belonging to this class had all had mercury, and some had been repeatedly subjected to mercurial treatment. The average duration of treatment was seven months and twenty-four days; the average of chancres, five hundred and seventy.

The increase in both of the last categories of cases in the number of chancres, and in the length of time required to complete the cure, is ascribed by Dr. Boeck to the previous administration of mercury. It is, however, probable, as he observes, that another circumstance may have retarded the cure—viz., that the syphilitic virus may have undergone a material change during the many years it had been resident in the system. The more inveterate, and especially the tuberculo-serpiginous, forms, were found to be extremely rebellious to treatment, and some of them—as, for example, Nos. IV. and XII.—were not cured when immunity was reached. It was necessary then to have recourse to iodine, upon the exhibition of which all symptoms rapidly disappeared, though previous to syphilization both mercury and iodine had proved inefficacious.

In one case of inveterate pains in the bones, syphilization was tried in vain. The patient was five months and thirteen days under treatment, and had three hundred and forty-six chancres, without the slightest benefit. Upon this case Dr. Boeck remarks—

"This unfavourable result may depend upon two circumstances. The syphilitic virus may have become, in the lapse of time, so modified in the system, that it can no longer be regarded as the same poison which produced the primary and secondary symptoms. It was here more altered than in the cases belonging to the third category, for in these the change had not proceeded so far as to prevent them from being susceptible to a certain degree of the influence of inoculation. This idea does not appear to me so improbable, if we take into consideration the peculiar properties of this poison, and compare the possibility of inoculating it in the primary and secondary forms, with the difficulty of transplanting it in more advanced cases. To go still further, let us suppose that the immunity and the cures obtained by syphilization depend upon an isopathic operation of the syphilitic virus, so we may naturally expect that the further we recede from the first and earliest introduction of the poison into the system, the less powerfully will primary virus operate on the disease, and that at length the virus becomes so changed in character, that it is no longer capable of being acted upon by inoculation. We have seen that the most recent constitutional affections require the shortest time for their cure, and that no relapses have occurred where no previous treatment had been employed. Where mercury had been previously given, the cases were almost always more obstinate, and the tendency to relapse occasionally manifested itself. Still, among the inveterate cases, we met with one or two where no mercury had previously been given, and yet they proved to be extremely obstinate. I suspect, therefore, that here some other obstacle besides the previous use of mercury has existed, and I believe this to be the change that the virus undergoes by long residence in the constitution." (p. 201.)

Dr. Boeck suspects that in those very obstinate cases a union of the syphilitic with the mercurial poison has taken place. In most instances this union seems to be dissolved by syphilization, and then iodine, which had before been ineffectual against the united poisons, acts readily on the mercury, and eradicates it from the system. In No. VII., however, the skin disease healed under syphilization, but the pains in the bones continued unaltered till they were cured by iodine. That affections of the bones may, however, ensue from syphilis alone, is proved by No. I., where they rapidly yielded to syphilization, while in Nos. VII. and XI. iodine was required to complete the cure;

for in both these cases mercury had previously been given. Mercury alone, observes Dr. Boeck, will not produce the peculiar affections of the bones; the most profuse salivations in other diseases have not been followed by affections of this kind.

Dr. Boeck then proceeds to consider the indications for treatment by syphilization. During the two years that have elapsed since the publication of the work first on our list, he has had sixty-three individuals under his care for constitutional syphilis, and of these thirty-six had never taken mercury. Whenever mercury had been previously given, as in the remaining twenty-seven cases, he has invariably found the cure more difficult, and in some cases impossible without the aid of iodine, the action of which he believes to be essentially anti-mercurial.

The relative merits of the two methods of treatment—viz., of that by mercury, according to established rule; and secondly, by syphilization—are next discussed by our author. Syphilis has been treated by mercury for centuries, and immense experience of its action has been obtained; but this experience has likewise made us acquainted with its deficiencies. The tendency of the disease to relapse under this treatment is acknowledged by all. Some individuals, indeed, when treated for constitutional syphilis by mercury, have been to all appear ance cured, and have remained so for years; and then affections of the bones, swelling of the testicles, and serpiginous tubercles of the skin, have shown themselves. Others continue healthy during their lives; but their children, though born several years after the mercurial course, may exhibit unmistakeable signs of syphilis in its various tertiary forms. Where mercury has not been previously employed, Dr. Boeck is decidedly in favour of syphilization; for as far as can be ascertained from the 42 cases of this character that have been subjected to the treatment, no single relapse has occurred up to the present period. We confess that this is a strong argument in favour of syphilization, for it is precisely in the matter of the tendency to relapse that the inferiority of mercury is shown.

Another objection to syphilization is the length of time required for the treatment. In the Christiania Hospital, the average duration of the mercurial cure is three months and a half, while syphilization averages a full half year. If, however, the latter be not liable to relapse—at least in non-mercurialized cases—it is infinitely preferable, even if it required a much longer period. To try the effects of mercury first, and to have recourse to syphilization only when the former had failed, would plainly be erroneous practice, for the administration of mercury has been always found to retard the operation of treatment by inoculation.

Is it possible, then, to syphilize, to produce immunity, and to heal permanently, the constitutional disease, with matter taken from one source alone? Dr. Boeck doubts much if such be the case. He has met with instances where complete immunity existed to the matter taken from one source; but upon obtaining fresh virus from other individuals, the inoculation succeeded perfectly again. However, in some instances—as in Cases I., II., and III.—he has been able to effect a complete and permanent cure with matter from one source alone.

This leads us to another question—viz., whether it is necessary to obtain absolute immunity when the syphilitic phenomena have disappeared in the course of inoculation. In those cases where mercury has not been previously employed, he does not believe that absolute immunity is always requisite; but where mercury has formed the staple treatment beforehand, he would inoculate so long as any matter could be found to take. Dr. Boeck does not agree with Auzias Turenne as to the identity of syphilis and blennorrhagia or gonorrhœa. A discharge similar to that of gonorrhœa may, however, result from a chancre in the urethra; but even here, though syphilization would probably cure, it would not be admissible, for the affection would be only of primary character. Immunity to the venereal poison does not confer the slightest protection against gonorrhœa, for one of the patients (No. VII.) contracted a severe gonorrhœa immediately after leaving the hospital.

We have now arrived at Dr. Boeck's third and last proposition—viz., "That the general health does not suffer in the least during syphilization—on the contrary, the health improves remarkably in those instances where it had been impaired before commencing the treatment."

Singular as this may seem, it is most certainly true, as we have ascertained from personal observation recently in the Christiania and Bergen hospitals. We conversed with several of the patients, and questioned them upon this point, and all declared that their general health had greatly improved under the treatment. Full diet was allowed, and it may be suggested that this contributed much to the improvement observed, as it is perhaps of a more nourishing character than the ordinary diet of the Norwegian labourer. The sensations of weariness, the sleeplessness, and the pains resembling rheumatism, rapidly disappeared, and the aspect of many of the patients presented an appearance of health such as could not have been expected. Moreover, the patients, when cured, could at once return to their ordinary occupations—they could expose themselves to the vicissitudes of the climate, to wet and to cold, without the fear of evil consequences, such as might justly be apprehended in those who had undergone a mercurial course.

With regard to the danger of the chancres assuming a phagedænic character, Dr. Boeck has indeed occasionally observed such a tendency, but believes that it arose in a great measure from employing venereal matter of too active a character at the onset. But the appearance of phagedænic sores did not prevent him from continuing the treatment—on the contrary, he regarded such symptoms as an indication for persevering with inoculation; and the result justified this proceeding. Perhaps the case of Dr. L——, which in the French Academy of Medicine was so prominently brought forward to prove the dangers of syphilization, was one of this kind. Very active matter had been employed at first, and phagedænic chancres were produced, but at this point, unfortunately, inoculation was stopped, and the disease continued to spread. Dr. Boeck's experience tells him that the remedy is at hand in a bold perseverance in inoculation. Of late, he has had no phagedænic chancres, as he has learnt to avoid matter of too active a character in the first inoculations.

Dr. Boeck, in the second work on our list, gives three cases where he has successfully syphilized children under two years of age, labouring under tertiary symptoms.

In another large work (on diseases of the skin), of which the first number only has appeared, he has given, in concert with Dr. Danielsen of Bergen, a long account in French and in Danish, of his experiences in syphilization. We intend to notice this work on its completion; suffice it to say that the execution of the plates reflects great credit on the artist.

We have now endeavoured to lay before our readers as concisely as possible the contents of a work from the pen of a man of high standing in the scientific world. We are well aware that position alone does not secure us from error. Professors of universities have more than once accepted the fallacies of homœopathy, mesmerism, and other pseudo sciences. All that the advocates of syphilization demand is a fair trial of the system; and without the aid of experience we can hardly venture to pronounce against it. In this country, from the almost universal employment of mercury in venereal disease, it would be difficult to meet with an individual labouring under constitutional syphilis, who had not undergone at least one mercurial course. Mercury is now but little employed in Scandinavia in the treatment of primary syphilis, so that in those countries more favourable opportunities will occur. Is our obstinate adherence to mercury in the treatment of this disease perfectly justifiable? and may we not really have laid ourselves open to the severe strictures recently passed by Dr. Bennett of Edinburgh, on the passion for mercurial treatment that prevails on this side of the Tweed?

At all events, we think that the advocates of syphilization have established a claim on the profession to a fair trial of their system. It is evident that its employment is not fraught with danger, as is the case with so many remedies proposed from time to time; and the investigation of the subject seems to open up a new field for the further study of one of the most malignant and most lasting and destructive poisons that affect the human frame.

Review XIV.

Ocular Spectres and Structures, as Mutual Exponents. A Treatise. *Eustachian Tube—why Opened in Deglutition.* A Paper. By James Jago, A.B. Cantab., M.B. Oxon., Physician to the Royal Cornwall Infirmary, and the Physician to the Truro Dispensary.—*London*, 1856. pp. 110.

The volume before us bears evidence of being the production of an original thinker and conscientious worker, and as such has claims upon our consideration. The originality, we may remark, is not confined to the thought, but extends to the style, reminding us of that of the author of 'Sartor Resartus.' From the first page to the last, the volume is mainly composed of the details of careful experiments, repeated, we imagine, many times, and of the inferences drawn from those experiments, which inferences are not unfrequently at variance with those deduced by the highest authorities.

As may be gathered from the title, the special object that Dr. Jago has had in view is, a methodical elimination of what he terms *ocular spectres* from one another. Some might take exception to the term, but that is of little consequence. The ground he considers untrodden; for though isolated "spectres" have been traced to their sources, he affirms that no one has hitherto devised and practised any plans for exploring the visual organ, capable of leading to the detection of the respective causes of these optical illusions.

To this task the author has applied himself with zeal, and his labours have not been barren. He finds—

"That in the transparent ocular media, structural spectres are created, which have begotten erroneous opinions upon these media; and that certain anomalies incidental to the use of the organ of sight, which have been regarded as evincing a capricious conduct in the retina, optic nerve, or brain, are purely mechanical in their origin." (p. 2.)

The first chapter is introductory; the second fairly takes up the subject with "optical effects of eyelashes, eyelids, and conjunctival fluids," in which are pointed out a number of phenomena as they appear in divergent and convergent light. The third chapter treats of the optical structure of the iris and of the crystalline lens; and the following observations, deduced from personal experiment, possess interest:

"I fall upon many objects fixed between the iris and the vitreous humour. These objects must therefore be connected with the crystalline lens.

"In my left eye I could enumerate from thirty to twice as many (or three times, for aught I know, if the pupil were dilated to the utmost) small objects resident in this region; in my right eye they are not so numerous, though it contains the largest examples. They are all either exactly round, or slightly oval. In divergent light they may be seen to have each a white centre, of the brightness of the ground-light used—except a few, in which the central luminosity is brighter and proportionally larger—within a wide black ring (this ring in the greatest cases shows coloured indications, if not a sub-annular series), which is again within an alternation of fringes. In strong light I have counted in a large example full ten such alternations. In convergent light they *all* present a small black spot within two or three fringes of either kind; and when the point of decussation is passed through the space occupied by these bodies, the few excepted will be dilated and dissolved in light, and all the others in shade. Hence the few are transparent, and the many opaque.

"The opaque bodies, when tested as to depth in the eye, are found to be many of them shallower than others, through a difference fully equal to what I should imagine correspondent to the whole thickness of the lens. It is certain, therefore, that those bodies are scattered throughout the lens; and it is probable that examples are located on the surface of that substance. Whether they are earthy concretions, any foreign bodies the accident of years, or a normal deposit in the lens, I am unable to affirm. I am, however, pretty well satisfied, from my remembrance of the uneven cloudiness in a puncture, that there must have been opacities in my crystalline lenses when I first explored my eyes with divergent light, though I had then my attention absorbed with another part of the eye. The two adjoined transparent ones which I am going to speak of, attracted my particular notice by their peculiar aspect and situation in a puncture, at the very beginning of my attempts of this sort of ocular examination. The bodies now expatiated on do not damage usual vision in any appreciable manner, and I should presume that no eyes are without such. The opaque ones, in the average run, do not exceed in diameter the breadth of a filament of the vitreous humour, and the transparent ones do not surpass this standard in size. The extreme examples of the opaque kind may present shadows of three or four times the average measurement.

"The transparent couple just now singled out are as near the cornea as the nearest of the opaque ones. There is a group of four of like kind in my right eye, as remote from the cornea as the deepest opaque one." (p. 19.)

"But not only are the objects fixed by the crystalline lens visible, but the 'stelliform' structure of the lens itself. In either eye, in divergent light, I find nearly a dozen and a half, with about two-thirds of the number more strongly marked than the remainder, of strongly luminous, nearly white lines, issuing from a like spot in or near the centre of the pupil, and going towards its circumference in a slightly crooked or undulating manner; thus radiating, as it were, to a little beyond the margin of the pupil. The figures in my two eyes do not precisely resemble each other." (p. 20.)

The structure of the vitreous body has occupied the attention of many able microscopists. Hannover, Virchow, Bowman, and Kölliker, for instance, have investigated it carefully. Dr. Jago, however, takes exception to the conclusions at which they have arrived, saying, "It is, I think, impossible to doubt that the web described in this chapter clears up the slender results they have obtained." This is the description of the web in question:

"From innumerable points of the wall of the posterior chamber of the eye, as far as the hyaloid membrane extends—though in no instance from the remaining part formed by a portion of the capsule of the crystalline lens—there spring fine beaded threads or fibres, consisting of rows of transparent globular and equal (or nearly so) cells, of less specific gravity than the fluid which fills the chamber. These threads quickly unite in pairs (or occasionally otherwise); single (or a less number of) threads proceed from the knots thus made to join again other threads as before, from which knots again in diminished number of threads, the process is repeated, and so on. Thus, a lax, irregular, sometimes decussating network, is woven from the hyaloid membrane, beginning with very small meshes, and extending into the interior of the chamber; but not so far into the chamber as to occupy its middle portion. The network is completed anteriorly by being woven from the surrounding part of the hyaloid, and threads prolonged from more distant portions across the back of the crystalline lens, so as to float freely over its capsule." (p. 25.)

The observations upon muscæ volitantes, though very confident, will, we suspect, not satisfy all those who have investigated the subject: we fancy that Sir David Brewster will hold his own upon that question, and upon some others mooted in this book.

Dr. Jago is especially sceptical as to the possibility of muscæ volitantes becoming visible to another person by aid of the ophthalmoscope. He more than hints that some of the ophthalmoscopists who have racily described the marvellous "shreds and flakes" they have seen in patient's eyes, have unwittingly written down the wonders of their own visual organs. They have assigned to others motes which properly belonged to themselves.

"Filaments and their individual beads of the vitreous humour, small objects in, and even the stelliform figure of the crystalline lens of the examining eye, have all been imagined to be things in the examined eye." (p. 42.)

We will not dispute the *possibility* of such confusion of ownership, when inexperienced persons are using the ophthalmoscope, but with skilful observers we doubt the probability. We suspect that the ophthalmoscope has not become familiar to Dr. Jago. When it has (and so acute an observer is not likely to neglect it), we believe that he will admit abnormal conditions of the vitreous humour which at present he rejects, and will promote the degraded muscæ to the position assigned to them by observers not inferior in reputation to himself.

A considerable portion of the work is devoted to the consideration of the optical anatomy of the retina, many ingenious experiments being detailed, and very decided opinions being expressed adverse to Sir David Brewster, Purkinje, and others. Those gentlemen are, however, perfectly well able to support their own views, and we have no space to discuss controversial points which are at present open questions. We have no doubt that if Dr. Jago be right, and these other observers wrong, full justice will be done to this earnest and confident labourer in the field of physiological inquiry. From amongst the many subjects mooted, we may select some interesting observations upon the *images of objects which press upon the retina through the coats of the eye*—a class of illusions familiar to all. Concerning them, Dr. Jago says:

"We gather from the series of facts recorded that it is not, or at any rate scarcely at all, by immediate pressure upon the retina that the sensations above implied are created. It is where the portion of the globe which has been thus flattened or hollowed towards the orbit, passes into the remainder, that preserves the globular form—that is, where the retina is bent towards the vitreous humour at an angle, as if to make a fold or crease on that side, from which, of course, will radiate perpendicularly short folds. This fact would seem to indicate that it is only by crowding into closer space the elements of the internal (at all events of an inner) surface of the retina that sensations are begotten, for if crowding together those on the outer surface would produce this effect, we should have the brightness at the bottom of a hollow or depression." (p. 63.)

The second portion of this volume is devoted to a disquisition upon "The Eustachian Tube—why is it Opened in Deglutition?" On this question Dr. Jago breaks a lance with Mr. Toynbee, who, it is well known, advocated the views that the guttural orifice of the Eustachian tube is closed, except during the act of swallowing or of violent expiration, and that it is essential to a perfect organ of hearing that the drum be a shut chamber, as the existence of any outlet would allow of the escape of sonorous vibrations, instead of their being concentrated upon the membrana fenestræ rotundæ. He also showed that the sonorous vibrations pass from the membrana tympani to the labyrinth by means of the air in the tympanic cavity, and not through the chain of ossicles.

Dr. Jago, whilst agreeing with the first of these propositions, is opposed to the second. For the sake of clearness, we will briefly take each proposition seriatim. 1st. The closure of the Eustachian tube, except during the act of deglutition, can be proved by experiment.

To those accustomed to descend in the diving bell, it is well known that the unpleasant sensation in the ears, amounting sometimes to positive pain, is capable of instant removal by the act of swallowing, during which act, the condensed air being allowed to enter the tympanum and come in contact with the membrana tympani, the pressure on its outer surface is relieved by being counterbalanced. Again, if an attempt be made to swallow while the nostrils are closed by the finger and thumb, a sensation of fulness and pressure is experienced in the tympanic cavity, in consequence of air being forced during the act of deglutition through the open tube into the tympanum; and this sensation continues until, by another act of swallowing, the tube is reopened, and the confined air escapes into the fauces.

2nd. Dr. Jago is of opinion that the advantage derivable from the closed condition of the Eustachian tube consists in the exclusion from it, and consequently from the tympanic cavity, of the sound of the speaker's voice, and of the air in respiration, deglutition, &c. This, so far as we can learn, is the only novel fact that Dr. Jago has to offer on this subject; and it may doubtless be considered one of the reasons why the Eustachian tube is usually closed. Mr. Toynbee has shown, in a paper published in this Journal (Jan., 1853), that the main object of the chain of ossicles is, not to conduct the sonorous undulations to the labyrinth, but to act as the analogue of the iris in the eye. We believe that the sonorous undulations are conveyed to the fenestra rotunda by the air in the tympanic cavity, and he thus accounts for the continuance of the hearing power after the membrana tympani and the malleus have been destroyed.

Whilst expressing our approbation of the diligence and ingenuity displayed by Dr. Jago, we cannot consider the work as free from blemish. One especial fault is a want of clearness of style, so that it is sometimes exceedingly difficult to discover the conclusions to which the author wishes to lead the reader. Experiment after experiment is detailed with exceeding minuteness, but the point to be established is not easily ascertained.

Then, again, we would have felt greater confidence in the work had (the author must excuse our saying so) it been free from egotism; this may have arisen inadvertently, but the impression forces itself upon the reader's mind, that due weight is not attached to others, at least equals in skill and in experience to Dr. Jago.

Nevertheless the book is highly meritorious; and as we scanned the minute details of the trying experiments upon his own eyes performed by the author, the fate of Plateau presented itself to our mind. Like Dr. Jago, he devoted himself to investigations which formed the basis of his work, 'Sur quelques Propriétés des Impressions produites par la Lumière sur l'organe de la Vue;' but, in the words of Rodenbach, "Malheureusement! ses travaux persévérants sur la lumière lui on fait perdre l'organe de la vue." We trust that neither Dr. Jago nor any of his readers who may be tempted to repeat his experiments will be visited with so great a calamity. Let them beware, however, or they may, by over diligence, earn the sad distinction of a place in the roll of the martyrs of science.

PART SECOND.

Bibliographical Record.

ART. I.—*The Principles of Surgery.* By JAMES SYME, Professor ot Clinical Surgery in the University of Edinburgh. Fourth Edition. —*London*, 1856. pp. 486.

THE distinguished author of the work before us aims by it to elucidate the principles which guide his practice, and are taught in his lectures. "They cannot," he adds, "now lay any claim to novelty." He does not present it as a comprehensive system of surgery, which it is not, but rather as a terse expression of the leading principles which ought to direct the practice of the surgical art; together with as sparing an admission of details as was consistent with such a manner of treating the subject.

Accordingly, although not less than fourteen years have passed away since the last edition appeared, very slight alterations can be noted in the present volume. Modifications of the author's views can be occasionally traced, in the change of form which a sentence has undergone, or in the addition of three or four lines here, or a similar omission there. With these and a few other exceptions, the changes consist chiefly in the removal of all the engravings and woodcuts, so that the bulk of the volume is slightly reduced.

Some of those topics which, at the present moment, excite more than ordinary interest, have received additional notice in the volume, and such shall be briefly indicated here.

The subject of compression in popliteal aneurism is introduced; and a few lines are devoted to its consideration, of which the following is the summary:

"On the whole, it would seem that the ligature is the quickest, easiest, most certain, and least painful means of remedy, while compression affords a useful substitute when circumstances prevent the operation being performed with due attention to the circumstances requisite for its safety." (p. 99.)

The author's own method of amputating at the ankle-joint is described in detail at p. 146.

In the former editions, the flap amputation was recommended as best adapted in all cases for the thigh, without exception. In relation to this we now find:

"The unqualified preference for the flap method in amputating the thigh, which I formerly expressed, has been considerably modified through the experience of more extended observation. I still think that when the limb requires removal at the middle or any higher point of the thigh bone, the operation should be performed by the formation of two flaps, one being anterior, and the other posterior. But when circumstances permit amputation at the lower third of the limb, and especially if it be muscular, I am fully satisfied that great advantage results from operating by circular incision; or, in other words, by providing a covering of integument for the bone, instead of the muscular cushion, which in this situation is so apt to retract, and leave it exposed." (p. 149.)

We are compelled to express our surprise that Mr. Syme still views with disfavour the method of treating fractures by the starch bandage, of which he says:—"There seems great reason to question its superiority over that previously in use." (p. 156.) For several years past it has stood the test of experience in London, and is regarded by those who have tried it as a valuable adjunct to other means, although undoubtedly it does not possess any claim to supersede the numerous other appliances available in the treatment of fracture.

On the subject of excision of joints, the following passage from the last edition remains unaltered in the present:

"As to the joints which may be subjected to this operation, it is evident that the extent to which the acetabulum is almost always affected in the hip disease, forbids any attempt at excision. Though experience has not yet fully decided whether the limbs that might be preserved by cutting out the knee and ankle-joints would be preferable to the artificial substitutes which may be worn in their stead, it seems pretty well ascertained that they would not." (p. 228.)

English practice appears to be confirming the correctness of these remarks, as far as they relate to the hip-joint; but not so as regards the knee-joint, which is now excised with certainly successful results.

The result of recent experience has led to the following observation on caries of the shoulder-joint:

"It should be known that this joint is remarkably distinguished by its liability to suffer from disease *in one of the bones composing it, while the other remains free from the same derangement*; so that the head of the humerus may suffer from absorption and caries, while the glenoid cavity is no otherwise changed than by the removal of its cartilage." (p. 231.)

A suggestion for the treatment of obstinate ganglia extending from above the wrist into the palm, is new. The author had lost a patient from constitutional disturbance excited by long suppuration from such an one opened in the usual manner, and has since treated the affection with more success by the following means:

"The easiest mode of operation is to puncture the swelling a little above or below the annular ligament, then introduce a blunt-pointed curved bistoury under the arch, and, lastly, divide it, together with the superjacent integuments. Light and superficial dressings are sufficient in the first instance; and if the swelling does not speedily subside, one or two blisters may be applied." (p. 250.)

The author's perineal operation for stricture is fully described in pp. 338–40. A few additional remarks appear on the practice of lithotrity, on lithotomy, and on the administration of mercurials in syphilis, in that portion of the work which relates to the genito-urinary organs. Besides these, there are, we believe, no alterations or additions of any great importance in the edition which has just appeared.

ART. II.—*On Artificial Digestion as a Remedy in Dyspepsia, Apepsia, and their Results.* By EDWARD BALLARD, M.D., Licentiate of the Royal College of Physicians, and Fellow of the Royal Medical and Chirurgical Society of London, Lecturer on the Practice of Medicine at the School of Medicine adjoining St. George's Hospital.—*London*, 1857. pp. 46.

THE employment of pepsine prepared from the stomachs of the lower animals, and especially from the rennet-bags of ruminants, was introduced into therapeutics some years ago by Dr. Corvisart; and in 1854, this physician published the results of his experience on the subject. Dr. Ballard has been the first to adopt Dr. Corvisart's suggestions in this country; and in the little book before us communicates his own views, and the conclusions he has drawn from his own practice.

When we read of the extraordinary power possessed by pepsine in producing the solution of albuminous substances out of the body, as originally shown by Wasmann, we join in Dr. Ballard's expression of suprise that no attempts should hitherto have been made to isolate this principle for administration to persons whose stomachs are unable to perform their duty efficiently. The whole merit of the introduction of pepsine into our Materia Medica belongs to Corvisart; his own extensive experiments, physiological and pathological, and the numerous trials since made with the preparation by other Continental physicians of eminence, render it a duty of British physicians to arrive at a definite conclusion with regard to its value. Our own experiments have been as yet of too limited a character to justify our expressing a positive opinion, but the physiological evidence is so strongly in favour of the employment of the remedy, that the additional testimony of practical men as to its efficacy in disease should not fail to secure for it an extensive trial. For this purpose, however, it will be necessary that our own chemists should prepare it, and at as low a price as possible, since the cost of transmission added to the original cost of the preparation in Paris, is an impediment to its very general employment.*

Pepsine is indicated in cases of dyspepsia connected with a deficient secretion of gastric juice, and is calculated to allay the unlimited variety of symptoms that are attributable to that cause.

"It is especially where these disturbances succeed the use of animal food, that the employment of pepsine is chiefly indicated. It often enables a patient, who has not dared to attempt it, and could not do so without suffering, at once to eat it with impunity. Nor is this operation tardy. The first dose usually in such instances produces an effect; and after two or three more, no further discomfort is perceived. Even the severest cases of gastralgia after

* It is sold under the name of Poudre nutrimentive, and prepared by M. Boudault, a chemist, in Paris. The cost of a drachm, which is equivalent to four doses for an adult, is half-a-crown, in London. The bulk of the powder consists of starch.

food are almost as by a miracle relieved by its assistance. When it fails to give relief to painful digestion after three doses have been used, and still more so when future doses equally fail to assist digestion, it is probable either that the dyspepsia does not arise from a defect of the gastric secretion, or that some other condition predominates as its cause, such as hyperæsthesia of the stomach, or atony of its muscular parietes." (p. 15.)

In these cases respectively, the addition of minute doses of morphia or strychnia is found beneficially to counteract the secondary morbid element, and thus to enable the pepsine to effect its peculiar operations. Many diseased conditions, resulting from enfeebled digestive powers, are stated to be benefited by this remedy in the adult as well as in the child. In the latter we would especially recommend that it be extensively tried, since there are few chronic infantile complaints that may not be traced back to defective nutrition as their *fons et origo mali.*

With these few remarks we introduce Dr. Ballard's book to our readers. The cases which he gives are generally confirmatory of the previous observations made by our foreign *confrères,* and like the remainder of the book, deserve a careful perusal.

ART. III.—*Torquay in its Medical Aspect as a Resort for Pulmonary Invalids.* By C. RADCLYFFE HALL, M.D., Licentiate of the Royal College of Physicians, Physician to the Torquay Hospital for Consumption, formerly Physician to the Bristol General Hospital, &c.—*London,* 1857. pp. 165.

Madeira, its Climate and Scenery. A Hand-book for Invalid and other Visitors. By ROBERT WHITE. Second Edition. Edited, and in great part Re-written, with the addition of much New Matter, by JAMES YATE JOHNSON. With a Map of the Island. *Edinburgh,* 1857. pp. 338.

THE therapeutical influence of climate on pulmonary disease is so important and practical a subject, that we are glad to see any contributions to our knowledge like those above-mentioned. Without going so far as to assert that every practitioner ought to be acquainted with the chief features of all the places generally resorted to by pulmonary invalids, we may at least conclude that it is highly advisable the confidential medical attendant of the *poitrinaire* should have some voice in determining the place his patient selects; it is not very likely or desirable that he should possess any such influence, if completely ignorant of this kind of medical geography. There can be no doubt that the remedial effect of climate in many chronic pulmonary affections, is quite as distinct (to say the least of it) as that of the drugs held in most estimation in the treatment of these diseases. And the benefits which experience proves, meteorology goes far to explain. So that whatever the effects of repose, change of scene, amusement, or the other circumstances which attend travel in search of health, they cannot account for more than a very small fraction of the benefits obtained.

Dr. Hall's little book, dedicated to a lady, and written at her suggestion, seems to be addressed at least as much to the public as to the profession. Its subject, however, goes far to explain what the history of medical authorship shows is generally an equivocal method of writing. An invalid who is meditating a toilsome (if not hazardous) journey from a distant part of England, has a right to expect somewhat more exact and decisive information than the few and oracular words in which he must often be content to learn the diagnosis and treatment of an ordinary indisposition. Indeed, it is obviously of the highest importance that he should know enough of the geography of the place to select his residence aright; the more so, that few persons would think of calling in a physician expressly to choose the street or terrace most advisable for their particular case, or to render into suitable Latin the scarcely translateable *dictum*, "Let him or her live (if possible) in Little Arabella Crescent."

In short, we are of opinion that Dr. Hall has an unusually good excuse for addressing the public as well as the profession, on the medical aspect of Torquay; and believe that the little book he has produced will be useful to those for whom it is intended, and creditable to himself, as a sound practical physician, residing in the neighbourhood of which he treats.

It is evidently the work of an accomplished physician, who writes in an easy and not inelegant style, whose statements (apparently derived from careful observations) are clear and explicit, without being too minute, and who especially recommends himself by the candour with which he points out the bad (as well as good) effects of the climate and seasons in certain cases. The medical reader may perhaps regret that many of the details supplied are not more fully gone into. But he will find quite enough to give him a good insight into the points which it is chiefly important for him to know; and especially, a comprehensive view of the effects of Torquay on various classes of disease, and of its local modifications of climate. Indeed, by a judicious introduction of collateral matter, the author has contrived to make his work an interesting sketch of the effect of climate on tubercular disease generally.

The second of the above works is to some extent contrasted with the first, in the fact that its strictly medical contents are not only a smaller fraction of the whole, but occupy a more subordinate rank in their treatment. A chemist would perhaps be tempted to describe the two as representing a super- and a sub-salt; the physic being the acid, and the climate the base of the combination in both treatises.

The "Hand-book for Madeira" fairly deserves this title, and emulates the merits of that red-coated English army of similar books which, under the generalship of John Murray, yearly invades all the accessible parts of Europe. The fat old exile of Ghent somewhat profanely said of the birth of Wellington in the same month as Napoleon: "Providence owed us this counterpoise." And, similarly, we really think that in this age of vapid Tours and Travels, nothing but the counterpoise of Guide-books preserves critics from maniacal delirium. Let the reader who rises from the perusal of this excellent though brief description of what is perhaps to him a hitherto unknown island—prepared, as a practised traveller ought to be by its pages, to perambulate the whole island with no more provision than a pocket compass, a loaf, and the map at the end of the volume—let him only reflect that he might have been reading "Dottings down Dahomey," "Trottings through Thibet," "Antics around the Andes," or any other of the various tours of alliterative title published and to be published! Let him, we say again, think what he has escaped, as well as what he has gained, and be grateful for a work which not only must have added to his knowledge, but also subserved what moralists tell us is one chief object of amusement, as well as of industry—namely, kept him from doing much worse!

The peculiar claims of the climate of Madeira have been so long before the medical profession of this country, that it is scarcely surprising to find that even the copious details collected in such a volume as that before us add little of importance to what is generally known respecting it. The excellent treatise of Sir James Clark pointed out its chief features so conclusively (and as later researches show, so exactly) many years ago, that its even temperature (about $1\frac{1}{3}°$ Fahr. being the average monthly variation), its uniform moisture, its warmth in winter, and its coolness in summer, require no mention. We confess, however, to a little alteration and correction of some others of our previous notions respecting it. With its volcanic geology, and its chain of mountains rising to six thousand feet in height, it evidently includes regions accessible (and, indeed, habitable), where any constitution especially requiring it might secure a far more bracing climate than that of the sea-coast during much of the year. At any rate, the British invalid proceeding to Madeira need have little fear of discovering that he has landed on a remote island, of which the climate is as a whole unsuitable to him during any part of the year. There is the more reason to notice this fact, because Dr. Hall has the merit of specifically informing his readers, that the climate of Torquay has an injurious effect in some maladies, and at certain seasons of the year—an objection which, if applicable to Madeira, would obviously add to the responsibilities of both physician and patient in deciding upon a sojourn there.

Art. IV.—*Remarks on Vesico-Vaginal Fistula, with an Account of a New Mode of Suture, and Seven Successful Operations.* By N. Bozeman, M.D., of Montgomery, Ala. 1856. (From the 'Louisville Review' for May.)

This *brochure* is a valuable contribution to the therapeutics of a lesion long a surgical opprobrium, which has of late engaged the earnest attention of many ingenious practitioners. The author has a new method to extol, and it must be admitted that he adduces good theoretical and practical arguments in its favour. He first briefly describes the anatomy of the region and structures concerned—the situation and peculiarities of the lesion. He insists that it is common for two fissures to co-exist, and remarks that this form of injury has escaped the notice of authors. He briefly adverts to the various methods of treatment that have been successively practised, and the indifferent success that has attended them. He insists upon the objections to the quill-suture, and finally, for the purpose of comparison or contrast with his own method, fixes upon the modifications of the quill-suture and the clamp-suture of Dr. Marion Sims for more especial criticism. Dr. Bozeman says that he has several times found the clamp-suture of Dr. Sims ulcerate and cut its way out, and that his failures by this method led him to devise the one which forms the object of his memoir. Another objection is certainly well founded, and it applies to every form of suture with which we are acquainted, excepting the brad-suture of Mr. Brooke.

"The sutures must be introduced exactly alike; each wire must be entered on the same line, at a proper distance from the edge of the fistule, and brought out in a similar manner, so that when the shot are secured in their places, the same amount of traction, and in the same direction, shall be exerted on each suture. Unless these precautions be observed, the clamp will not lie easy, and it is liable to do injury."

The *button-suture* is the name that Dr. Bozeman applies to his own contrivance. It is, he says, a modification of the twisted, as the clamp is a modification of the quill-suture. The essential parts of the apparatus consist of wire for the sutures, a metallic button or plate, and perforated shot to retain the latter in place. The button may be of lead or silver. The former hammered out to the thickness of one-sixteenth of an inch answers tolerably well. The latter can be made still thinner, and does better. The object of the button is to cover the fistulous opening after the introduction of the sutures, and its size and shape will therefore vary somewhat, according to circumstances. It is a matter of great importance that the under surface should be slightly concave, and the edge turned up. Along the middle of the button are arranged perforations for the passage of the sutures, which should be sufficiently large to admit two thicknesses of the wire readily. The number of these openings will depend upon the number of the sutures, which are usually placed about three-sixteenths of an inch apart. The edges of the fistula having been pared, the wire sutures are to be lodged in their respective places by attaching them to the ends of silk ligatures previously carried by means of a needle through the septum. The space between the entrance of the needle and the edge of the fistula need rarely exceed half an inch. It is not necessary to be over-scrupulous in entering and bringing out the sutures upon an exact line with each other, for each one in its action is entirely independent of the others. Thirdly, instead of being obliged to place the sutures parallel with each other, you may, if the peculiar nature of the case indicate, insert them in any direction, and thus bring within the sphere of successful treatment a large class of cases, which, owing to the irregular shape of the fistula, and the scarcity of tissue not admitting of extensive paring, cannot be subjected to the clamp-suture.

The wire for each suture should be about eighteen inches long. When passed, they are drawn together by a *suture-adjuster*—an instrument which pinches the wires on either side of the fissure into approximation with its fellow. The button of suitable size and shape is now placed upon the wires, the concave surface corresponding to the fistula, and carried down gently against the surface of the ports. The shot are then passed down over the approximated ends of the sutures, and fixed against the button. The author insists that one of the marked peculiarities of the button-suture is the separate and independent action of each wire. The only precaution requisite is to have the shape of the button made to correspond to that of the fistula, and its perforations to that of the points of suture. Quietude and accuracy of approximation are secured. "But probably one of the most important advantages of the button-suture is the protection that it affords to the denuded edges of the fistule." The edges thus covered by a sort of shield are secured from irritation by discharges and chafing.

The cases related in which this ingenious proceeding was employed certainly bear evidence to its efficacy. But we think it right to observe that Dr. Bozeman has earned for himself an easy triumph by comparing his suture with that of Dr. Sims, instead of with that of Mr. Brookes. It so happens that the brad-suture of this latter surgeon fulfils exactly the indications which the author points out as being exclusively accomplished by his own. No form of suture—not even Dr. Bozeman's—admits of being more effectually adapted to varied circumstances, or possesses the important merit of allowing each point of suture to exert an independent action. It even appears to us to enjoy the advantage of diverting the dragging more completely from the edges of the fistule, and to be in consequence less liable to failure from the sutures ulcerating their way out. In fine, Dr. Bozeman ought to have contrasted his operation with Mr. Brookes's. By leaving this out of sight—which in all probability he has done simply from not comprehending its exact nature and mode of action—he has overlooked the fact that two of the essential conditions of success in the cure of this troublesome lesion—accurate adaptation and independent action of each point of suture—had been gained to surgery before his own induction. The peculiar and great merit of Dr. Bozeman's button or shield lies in the protection it affords to the line of fissure. This contrivance is new, and we anticipate that it will be found to be a valuable accession to our means of ensuring the success of the operation for the cure of vesico-vaginal fistula. In this belief we have given Dr. Bozeman's description in detail, and commend it to the consideration of those who are interested in plastic surgery.

ART. V.—*On certain Painful Muscular Affections simulating Inflammatory, Neuralgic, or Organic Disease.* By THOMAS INMAN, M.D. (Lond.), Lecturer on the Principles and Practice of Medicine at the Royal Infirmary Medical School, Physician to the Northern Hospital, &c., &c.—*Liverpool,* 1856. pp. 49.

DR. INMAN details several interesting cases in proof of the fact that pains frequently occur in various parts of the body which may be shown to result from over-fatigue of certain sets of muscles, but which, unless traced to their true cause, may be, and frequently are, regarded as the result of a deeper-seated and more serious disease, to the no less serious detriment of the patient. The author observes that the abdominal muscles are more "frequently the seat of pain than any others," and "that it is always (when muscular) referred to the costal origin of the external oblique." The signs by which the nature of these pains is to be detected, are stated thus:

"They are usually dull and aching in the morning, then more acute, and at night they are severe and burning; they are scarcely relieved by pressure, stretching to the opposite side gives temporary relief; a recumbent posture on the affected side almost always cures for a time. The pain is commonly absent in bed and shortly after rising, but goes on increasing in severity towards night. Friction has little influence over it; taking a deep inspiration commonly aggravates it, or appears to do so, by bringing on a 'stitch'—i.e., a cramp in some of the muscular fibres—and care must be taken lest this symptom should suggest pleurisy." (p. 25.)

Dr. Inman's paper is instructive, and contains numerous suggestive remarks, which render it valuable to the practitioner, to whose attention we specially commend it.

ART. VI.—*Practical Observations on the Use and Abuse of Tobacco.* By JOHN LIZARS, late Professor of Surgery to the Royal College of Surgeons, and lately Senior Operating Surgeon to the Royal Infirmary of Edinburgh. Sixth Edition.—*Edinburgh*, 1857. pp. 42.

ANY one who has experienced the beneficial effects of a cigar after a day of intense bodily fatigue, will feel that there is something wrong in the sweeping denunciation to which the employment of tobacco is at present exposed. We entirely agree with Mr. Lizars, Mr. Solly, and others, who disapprove of the habitual resort to so powerful a sedative as tobacco is, even in its mildest form; but we much fear that the extravagant manner in which the war is commenced will fail to produce the effects that all sanitary reformers would desire. Thus, the frequency of cancer of the tongue, of which Mr. Lizars gives us three very vivid representations, and which he attributes to excessive smoking, cannot be proved to bear any very alarming proportion to the number of persons who indulge in the Virginian weed. Nor, as has been pointed out by a contemporary, does insanity bear any ratio to the extent to which smoking prevails. In short, a much more careful collecting and sifting of evidence will be necessary to place upon a scientific basis the assertions of the *coûte qui coûte* enemies of tobacco. We hope to bring this very important question more fully before our readers; but while we have no hesitation in expressing ourselves generally in favour of a *razzia* against Regalias, Paesanos, Cubas, Cavendish, high-dried Welsh, Rappee, *et hoc genus omne,* we would beg the energetic opponents of the tobacco-nuisance to bear in mind that there is something to be said on both sides, and that they are likely to damage a good cause by the excess of zeal and vituperation which they are now indulging in.

ART. VII.—*Varicose Veins; their Nature, Consequences, and Treatment, palliative and curative.* By HENRY T. CHAPMAN, F.R.C.S., &c.—*London*, 1856. pp. 99.

THIS little volume, the author states in the preface, consists, with the exception of a short section on the palliative treatment of varix, of a reprint of several papers on the curative treatment of the above complaint, from the pages of the "Medical Times and Gazette." The author lays stress on the intimate connexion which exists between dilatation and inflammation of the venous walls, and contends that pathological writers have not pointed it out with such distinctness as to attract attention to it practically.

In the first part, the author describes the nature, causes, and consequences of varix; in the second, the treatment. Under the latter head he justly condemns the various surgical operations that have been heretofore resorted to for the cure of the malady, because—

"*First*, there is more or less danger of extensive phlebitis supervening upon the section or ligature of veins, in whatever manner it may be conducted. And, *secondly*, were no such consequence to be apprehended, every operation hitherto devised fails, in the majority of instances, in accomplishing the object for which it was performed." (p. 40.)

Under the head of curative treatment, the author describes *his* appliances of wet strapping and bandaging, with ingenious modes of effecting local compression. We believe indeed that, at the present time, all right-minded surgeons are agreed that the best mode of remedying this complaint is by the aid of circular pressure. "In the present state of information upon the subject," says Mr. Syme,* "*it* seems that the most judicious course in treating varix is to be satisfied with remedying its bad consequences, and using means for preventing their occurrence."

The author has appended notes of eight cases corroborative of the utility of the measures which he advocates. The work is clearly written, and reflects credit on the intelligence and industry of the author.

ART. VIII.—*The Liverpool Medico-Chirurgical Journal.* No. 1. January, 1857.

WHETHER or not the conductors of the 'Liverpool Medico-Chirurgical Journal' have met a want by the establishment of a half-yearly periodical, is a point upon which we offer no opinion. That a town containing hospital accommodation for 1400 patients presents a large field for the cultivation of medical science, there can be no doubt of; but it is an open question whether, in the commonwealth of science, every province or provincial town should be individually represented by its own journal, or whether their honour and dignity would not be sufficiently maintained, and the true interest of science advanced, by seeking as much as possible to combine and interchange their labours in periodicals representing a still larger sphere. Time will afford the

* Principles of Surgery, p. 122. 1856.

answer. But we certainly think that the new Journal justifies the feeling of respect, we would almost say of veneration, which the name of Liverpool excites when we examine the records of our profession, and call to mind what men like Park, Alanson, and Currie have achieved;—Park, who was the first to perform, and successfully, excision of the knee-joint; Alanson, the chief reformer of the old system of amputation, according to which, out of forty-six cases, nineteen died, and none recovered without more or less dangerous symptoms; Currie, whose name stands equally high as a physician, a literary man, and a politician.

The new Journal is very properly introduced by a sketch of the medical history of Liverpool, by Mr. Fletcher; and then follow eleven papers on medical, surgical, and obstetric subjects, of a practical character, and of more or less interest. We would particularly direct attention to Mr. Higginson's cases of transfusion, and to Dr. Sinclair's case of idiopathic tetanus; to which we may have an opportunity of adverting more fully in our quarterly reports.

ART. IX.—*On Rheumatism, Rheumatic Gout, and Sciatica: their Pathology, Symptoms, and Treatment.* By HENRY WILLIAM FULLER, M.D. Cantab., Fellow of the Royal College of Physicians, London, Assistant Physician to St. George's Hospital, &c. &c. Second Edition.—*London*, 1856. pp. 464.

WE are not surprised to find that a second edition of Dr. Fuller's work on rheumatism is already called for. The book recommends itself so strongly to the professional reader, both on account of the scientific treatment of the subject and for the clearness and amenity of its style, that we have no doubt of its being very extensively referred to. Dr. Fuller has enriched the present edition by considerable additions, bearing chiefly on the therapeutical appliances at our command in rheumatic gout, in chronic rheumatism, and in sciatica.

ART. X.—*Defects of Sight; their Nature, Causes, Prevention, and General Management.* By T. WHARTON JONES, F.R.S., F.R.C.S., Professor of Ophthalmic Medicine and Surgery in University College, London; Ophthalmic Surgeon to the Hospital; late Fullerian Professor of Physiology in the Royal Institution of Great Britain; Fellow of the Royal Medical and Chirurgical Society of Copenhagen, Corresponding Member of the Imperial Medical Society of Vienna, Member of the Society of Biology of Paris, &c.—*London*, 1856. pp. 149.

AFTER an introductory chapter, in which the structure and functions of the individual parts of the eye are succinctly explained, Mr. Wharton Jones discusses the choice of light for working by, and the general precautions to be observed in the employment of sight. He then, in the third chapter, proceeds to place before the reader the special dangers which beset the organ. The succeeding three chapters are devoted to the defects of sight, cataract, mydriasis, myopy, presbyopy, amaurotic affections, and defects depending on loss of correspondence of the sensations and movements of the two eyes.

The third part, which comprises two chapters, is devoted to the prevention and management of morbid states of the eyes affecting different periods of life, and to the preservation of the eyes in certain general diseases. We have perused the work with interest, and have no doubt, from its practical utility, that it will find a good reception with the public. At the same time, we would suggest that the remarks on the methods of operating in cataract are not adapted to the general objects of the book; while other parts, especially the observations on colour-blindness, might very suitably receive further extension.

ART. XI.—*Die Cholera in der Schweitz.* Von HERMANN LEBERT, Professor d. Med. Klinik in Zurich. —*Frankfurt-am-Main,* 1856. pp. 93.

The Cholera in Switzerland. By HERMANN LEBERT. 1856.

LEBERT, the well-known micrologist, here presents himself to us in a field of research very different from that on which we are accustomed to meet him. He gives us the results of his observations in the epidemic of cholera in Zurich in the autumns of 1854 and 1855. In the first chapter we find a short account of the former epidemics of cholera in Switzerland; he afterwards describes the manner in which the disease manifested itself in the town of Zurich, and then passes on to the principal part of the memoir, his own observations in the hospital of the canton. Here we meet first with his notes on the diarrhœa accompanying the epidemics of cholera, and particularly on that form which usually precedes the real attack; and has, by many pathologists, received the designation "premonitory." We are struck with the large proportion of cases in which it had been altogether absent. As this is a *quæstio vexata,* we will introduce Lebert's own account:

"Among our 96 cases, it had been doubtless present in 45 cases—i. e., in 47 per cent. . . . . . 18 cases were doubtful. In some of them the prodromic diarrhœa had probably existed, but could not be proved with certainty; if we add, however, even these to the ascertained cases, we yet obtain scarcely two-thirds of the total number. The complete absence of the prodromic diarrhœa was demonstrated in 33 cases—i. e., in more than one-third of the whole.

"Concerning the relation of the diarrhœa to the fatal cases, and to those ending in convalescence, the following table shows:

	Among the 45 fatal cases,		Among the 51 terminating in convalescence,
It was ascertained in	20, or 44 per cent.	...	in 25, or 49 per cent.
It was doubtful in	13, „ 29 „	...	in 5, „ 10 „
It had been absent in	12, „ 27 „	...	in 21, „ 41 „

(p. 25.)

The duration of the prodromic diarrhœa varied from one day to three weeks; it was not found shorter in the fatal cases than in those who recovered.

The *cholera nostras*—or, as we should term it, sporadic or English cholera (cholerine)—is considered as essentially identical with Asiatic cholera, but only as a milder form of it. It was still more rarely preceded by diarrhœa than the severer form.

We forbear entering into the description of the various symptoms; we can, however, not omit mentioning Lebert's notes on the secretion of urine. The continuance of the secretion during the whole attack has been observed six times among the ninety-six cases; but also in these the quantity was considerably diminished, and albumen was always admixed. In some of the remaining cases the suppression of the urine ceased after the lapse of forty-eight hours, in others only after four or six days; on the average, however, urine was passed for the first time in the course of the third or in the beginning of the fourth day. Not more than once the urine first discharged was found free from albumen. The further examination of the urine by Messrs. Lehmann and Volk, of Zurich, showed the chloride of sodium and the urea to be considerably diminished during the first days after the suppression of the urine; that their amount afterwards becomes increased beyond the average, as is the case with the total quantity of the urine before it returns to the normal medium. Thus we find, in one of the cases, that the *quantity of urine* (twenty-four hours) fluctuated from the third to the sixth day between 405 and 470 cubic centimètres; that it rose on the seventh to 2879 cubic centimètres, and averaged on the following days nearly 1500 cubic centimètres; *specific gravity*, between 1006 and 1014: *chloride of sodium*, from the third to the sixth day, between 0·3272 and 0·9494 gramme; on the seventh, 7·7215 grammes; on the eighth, 5·5398 grammes: *urea*, from the third to sixth day, 6·266 to 7·094 grammes; on the seventh, 60·594 grammes; on the eighth, 40 grammes. These figures are the more valuable as they agree with those obtained by Professor Buhl of Munich.

The *cholera rash* was observed only three times, and in each of these cases towards the end of the first week; two of them terminated in convalescence, one fatally.

Regarding the age of the patients, the largest number were between thirty-six and forty-five years old. The proportion of fatal cases was largest after the sixtieth year.

For the author's remarks regarding the pathological anatomy of the disease, and the treatment adopted, we refer to the essay itself, which may be perused with pleasure and advantage. The various chapters are elucidated by a series of cases. The whole is written in an easy style, containing scarcely any references to other authors, which may be partly attributed to the circumstance that Lebert wrote it away from his usual haunts, in the delightful little town *Bex*, in Switzerland.

ART. XII.—*The Change of Life in Health and Disease.* By EDWARD JOHN TILT, M.D. Second Edition.—*London*, 1857. pp. 307.

WE have much pleasure in introducing to our readers a second edition of Dr. Tilt's work, because we believe that much ignorance on the part of the public, and no little negligence on that of our profession, have contributed to the prevalence of erroneous views of the subject of which he treats. The book embraces the physiology of the change of life, the general principles of pathology at that period, the general principles of therapeutics and hygiene, and the consideration of the diseases of the reproductive and other organs occurring at the climacteric. It also contains a number of tables of more or less interest, to which we must refer the reader.

The fact of protracted menstruation and fecundity is shown in a tabulated statement, that of 10,000 pregnant women, 436 were upwards of forty years of age; and of these, 51 exceeded the age of forty-five, and 3 the age of fifty—one being fifty-two, another fifty-three, and the third, fifty-four: on which our author remarks:

"I have insisted on the frequency of pregnancy late in life, because grievous mistakes have often followed the practitioner's persuasion of its impossibility. To my knowledge, pregnancy late in life has been mistaken in three cases for an ovarian tumour, and was treated by iodine, mercurials, and tight bandaging, which caused the death of the child, and greatly compromised the mother's health."

The following observation will interest those who may be called upon to advise in such matters:

"If I were consulted respecting the chance of issue from a lady aged forty-five, I should ask when menstruation first appeared; and if it were eighteen or nineteen, I should infer the prolongation of the menstrual flow beyond the average time, which would of course increase the chance of loss to be sustained by an insurance office."

In a brief notice like the present, we can only point out in general terms the views and intentions of the author, leaving the more intimate examination of the work to those who desire to investigate and study the subjects it contains. The particulars chiefly sought to be established may be thus summed up: the period of life comprised between the fortieth and fiftieth years, commonly called the change of life, is eminently critical; while in most women the critical phenomena with which this epoch abounds are instrumental in removing previous complaints and strengthening the constitution, there are a certain number of women in whom these critical phenomena give rise to numerous, and sometimes fatal, diseases. The natural history of the change of life can alone indicate the best modes of treatment for the complaints incidental to this period. For the preservation of the health of women at the change of life, it is necessary that they should adhere to a judiciously laid down code of hygiene. Many forms of nervous disorder, affections of the digestive organs, especially of the biliary apparatus, and of the skin (these last being rather tedious than severe), frequently occur at this period. There are ganglionic nervous affections, which should be carefully distinguished from the cerebral and the spinal nervous affections, with which they are now confounded, because they often coincide and alternate with them. There are several well-determined modes of cerebro-spinal disturbance to which the term hysteria is indiscriminately applied, and unless clearly defined, that term is a bar to the progress of mental pathology, by lending to ignorance a scientific cloak. Cerebral affections are so common at the change of life, that few women, if any, escape the milder forms of cerebro-spinal disturbance; and these, if neglected, sometimes merge into the many varieties of insanity, the worst cases being, however, peculiarly amenable to treatment, of which the local application of sedatives is an essential part. And lastly, women at the change of life are frequently afflicted with cancer, gout, and rheumatism.

The chapter (fourth) on the "General Principles of Treatment at the Change of Life," contains some very judicious remarks on the employment of remedies, calculated to be useful to the practitioner; and we quote the following observations in order to show the mode of reasoning adopted by the author:

"If nature bled, in different ways, 208 women out of 500, it evidently shows that this spontaneous effort of a hidden force to relieve the system often deserves imitation. If 326 out of 500 suffered from sinking at the pit of the stomach, from fainting, debility, and chlorosis, it shows that stimulants and strengthening treatment may be as indispensable at the change of life as at puberty. If 75 out of 500 had frequent diarrhœa or constipation at this period, it shows that purgatives may often be safely exhibited. If 285 out of 500 had unusual perspirations or sweating, it is a positive proof of the utility of sudorifics at this epoch. If 134 out of 500 suffered continually from biliousness, jaundice, water brash, vomiting, and dyspepsia, it is clear that alkalies will be often useful. If in 277 out of 500 the nervous system was actually steeped in a more or less intense state of stupor, it is an indication of the great utility of sedatives. If, in many, the organ most prone to disease suffered most at this period, does it not show the necessity of discovering this weak organ from the patient's previous history, so as to give it protection?

"In exhibiting remedies at this period, it is necessary to guard against a prejudice firmly rooted in the minds of many, that the change of life is synonymous with old age, for the principles of treatment applicable to diseases of old age will not suit those of the change of life. Then, as at puberty, there may be vital energy, but latent and oppressed, so that bleeding and lowering measures sometimes develop an unexpected amount of strength. I thoroughly believe in the efficacy of the modes of treatment suggested by the study of natural phenomena, for several cases recorded in this work will show how, by following the suggestions of nature, I have been able, in a few days, to relieve patients who had been suffering for years. With the exception of those afflicted with cancer or structural affections, the number unaffected by treatment is extremely small, though many, satisfied with a first instalment of recovered health, will not allow a perfect recovery to be made. They would rather bear their accustomed evils than submit to the tedium of following out a systematic plan; they oppose the stubbornness of prejudice to advice founded on fully-proved facts, and then impertinently talk of the 'deplorable inefficacy of medicine,' when, in fact, *they will not be cured*."

We heartily concur in the hope, earnestly expressed by the author, that an accurate study of this important period of life may diffuse a better appreciation of its beneficial influence, as well as lead to the prevention of the sufferings often attending it, and to more rational and systematic modes of treatment.

ART. XIII.—*The Mechanism of the Gubernaculum Testis, with an Introductory Sketch of the Development of the Testes, and an Appendix on the purpose of their Descent from the Abdomen.* By JOHN CLELAND, M.D.—*Edinburgh*, 1856. pp. 40.

THIS is an inaugural prize thesis, the nature of which is sufficiently displayed by the title. After a brief sketch of the development of the testes, Dr. Cleland enters upon the special subject of his work by quoting the observations of original inquirers from Hunter downwards, and then records the conclusions which he himself arrived at after the dissection of five subjects at about the middle period of fœtal life. These are summed up as follows:

"First, they show that there is no simple ligament running directly from the testicle to the scrotum, but that the fibrous tissue of the gubernaculum is composed of a superficial fibrous layer of peritoneum, and of the fibro-cellular tissue within the same, which occupies the plica gubernatrix; of ascending and descending fibres from the aponeurosis of the external oblique muscle; and of ascending and descending fibres from the fascia of the groin.

"Secondly, they show that there is no permanent definite sac, such as is described by Weber, but there is a sac-like space left, in the first instance between the serous and fibrous layers of the peritoneum, afterwards between the different fibrous layers, and lastly between the fascia, on the one hand, and the gubernaculum as made up of all the peritoneal structures, on the other.

"Thirdly, they confirm the existence of cremasteric fibres, arched downwards upon the gubernaculum, and also of an ascending set of muscular fibres; but these latter do not occupy the position which authors have assigned to them—namely, within the plica gubernatrix." (p. 22.)

The author dissents from the view which assigns to the muscular fibres of the gubernaculum (believed afterwards to become cremasteric) any agency in effecting the descent of the testicle from the abdomen to the scrotum, and combats the arguments adduced from certain facts supplied by comparative anatomical researches, believing that it is a vital and not a mechanical process.

An appendix, on the object of the descent of the testes in man, and three illustrative lithographed plates, complete the work.

Before closing our remarks, we are bound, in justice to the well-known labours of Mr. Curling, to claim for that gentleman, in this place, a measure of justice which he has not met in Dr. Cleland's hands. That this error has been inadvertent we doubt not, but it is not the less necessary on that account to point it out. In collating the observations of others, Dr. Cleland quotes M. Robin, and, in the succeeding paragraph, Mr. Curling; the former from the 'Gazette Médicale de Paris,' 1849; the latter from the 'Cyclopædia of Anatomy and Physiology,' article Testicle, 1850; stating that "the description of Mr. Curling is nearly identical with M. Robin's." (p. 15.) The inference from this passage that M. Robin was the prior observer, is natural and unavoidable. The fact, however, is, that Mr. Curling's observations were made and published many years before those of M. Robin, having first appeared in the 'Lancet,' April 10th, 1841, p. 72, as well as in the 'Medical Gazette' of the same period; a circumstance which is moreover pointed out, we observe, in the article referred to in the 'Cyclopædia,' in which place the author's original remarks in relation to this subject are quoted verbatim from the sources named.

ART. XIV.—*On Diseases of the Skin.* By ERASMUS WILSON, F.R.S. Fourth Edition.—*London*, 1857. pp. 747.

MR. WILSON'S work, 'On Diseases of the Skin,' has now been sufficiently long before the public to render any detailed notice of it on our part superfluous. In the present issue several additions have been made, which enhance the value of the book, and render it eminently a book of reference on this specialty. While we do not in any way underrate the importance of careful study of the local phenomena of skin diseases, we should be glad to see the questions regarding their relation to the viscera, to the organs of sanguification, and to the nervous system, answered by something more definite than a passing allusion to disordered assimilation and innervation. Whether we believe in the *diathèse herpétique* of French writers, and the *zurückgetretene Krätze* of some German physicians, or not, we cannot in daily practice fail to see the constant relation that exists between cutaneous affections and derangements of the internal organs. To place these vague notions on a firm basis of physiological pathology, is reserved for future dermatologists; in the meantime, we take pleasure in referring to Mr. Wilson's useful volume for the information now attainable, the more so as by the anatomical classification which he adopts, he makes a decided step in advance, and facilitates the acquisition of dermatology by removing much of the mystery that former writers had thrown around it.

ART. XV.—1. *Plates of the Brain, in Explanation of the Physical Faculties of the Nervous System.* By JOSEPH SWAN.—*London*, 1853. 4to. pp. 67. Twenty-two Plates.

2. *On the Origins of the Visual Powers of the Optic Nerve.* By JOSEPH SWAN.—*London*, 1856. 4to. pp. 45. Nine Plates.

THERE is confessedly no department of Anatomical inquiry more difficult, or less satisfactory in its results, than that which relates to the structure of the Cerebral Hemispheres. Beyond the general fact that the fibrous portion, or white substance, of these ganglionic masses connects the peripheral grey substance forming their convolutions with various masses of central grey substance—especially those forming the thalami optici, corpora striata, and corpora quadrigemina—and that it also forms commissures, uniting the two hemispheres with each other transversely, and the different parts of the hemispheres with each other longitudinally, nothing can be said to have been positively ascertained; and the statements of Foville and others, who have asserted the existence of special tracts of fibrous structure, taking various definite directions, have been received by all experienced anatomists with justifiable hesitation. No one who has never prosecuted this inquiry for himself, can have any idea of the sources of fallacy which arise out of the variety of directions in which the fibres of the white substance are laid down, and from the mutual interpenetration of their different fasciculi; and it has always appeared to us extremely easy for an expert dissector to demonstrate in a dead brain almost any set of fibres which the ideational action of his own living cerebrum might lead him to believe in.

We have every respect for Mr. Swan's labours in Neurology, and believe that there are few persons more competent than himself to trace the distribution of the peripheral termination of a nerve-trunk, or to make beautiful preparations of its minute ramifications. But we are satisfied that he mistakes his vocation, when he applies himself to the unravelling of the perplexed problem which the structure of the Cerebrum affords; still more, when he adventures into the domain of psychical inquiry. We appeal to any one who has ever tried to anatomize a brain, whether Mr. Swan's plates represent anything that *can* exist in Nature,—whether, in fact, they are not rather pictures of *carvings* executed out of the brain-substance according to certain preconceived ideas in his own mind, than portraitures of *dissections* in which the true course of the cerebral fibres is made evident by the simple clearing away of what previously obscured it.

There is another circumstance that indisposes us to attach much importance to these products of Mr. Swan's skill and perseverance. He has essayed to grapple at once with the most complicated form of the problem, that, namely, which is presented by the Human brain, instead of commencing with the simplest, which is exhibited in the brains of Fishes, and gradually ascending through the Vertebrate series, until he arrives at Man. We have long been convinced that it is only by such an extended comparative investigation, and by combining therewith the careful study of development, that any light will be thrown upon this difficult problem. We are sure that much *can* be done by any one who has the time, patience, skill, and opportunity for such a work; and that a rich harvest of fame would be the sure reward. Mr. Solly laid a good foundation many years since; but no one has yet come forward to build upon it.

After the general opinion we have expressed, our readers will scarcely expect from us any detailed account of Mr. Swan's so-called discoveries. In fact, we should find it difficult to give such an account, so obscure is the author's language, so confused appear to be his ideas. In the first of these two memoirs, he seems to start with the notion that there must be a distinct cerebral source for what he terms the "physical" or involuntary powers of the nervous system (apparently forgetting or not knowing that these powers are manifested without any cerebrum at all); and this source he tries to discover by dissection. According to him, the motor tract of the crura cerebri (termed by him the voluntary tract) passes on through the corpora striata to the anterior lobes of the brain. The sensitive tract, on the other hand, passes up through the thalami optici to the convolutions chiefly forming the posterior lobes of the brain. Between these two, he considers that an involuntary tract is interposed, which passes up through the anterior part of the thalami optici, and the narrow posterior part of the corpora striata, to terminate in convolutions placed at the outer sides of the summit of the brain towards the posterior part of the middle lobe. This tract he states to be continued downwards throughout the spinal cord, at the bottom of the deep anterior fissure, becoming combined with both the anterior and posterior columns of the cord.

In his second Memoir, Mr. Swan describes a special visual tract, arising from the part of the thalamus immediately adjacent to the soft commissure, and passing upwards to a special convolution, between the voluntary and involuntary tracts. We presume that Mr. Swan will ultimately discover a special tract for each of the other senses.

It gives us much pain to feel ourselves obliged to speak in these depreciating terms of labours which have been attended with so much expense of time, skill, and money, as those of Mr. Swan have evidently cost; but the interests of truth are paramount with us, and leave us no choice but to express our honest convictions.

ART. XVI.—*On the Drainage and Sewage of London, and of Large Towns.* By JAMES COPLAND, M.D., F.R.S., &c. &c.—*London,* 1857. pp. 30.

NOBODY can have followed the lucubrations of the Board of Works with reference to the proposed intercepting sewers, which are to carry off the whole of the London sewage to some unknown destination, without feeling that the Board are acting under compulsion rather than upon any fixed principle. We would advise them to peruse the pamphlet by Dr. Copland, in which they may find clearly stated the great sanitary objects that a proper system of sewering must achieve, forcible and well-grounded objections to the plans that have found favour with the Board, and proposals which, in a sanitary, agricultural, and economical point of view, merit serious consideration.

Dr. Copland very justly dwells upon the importance of utilizing the sewage, while he points out the numerous sources of danger likely to accrue from an accumulation into one focus of an immense amount of sewage. Hence he advises that numerous reservoirs should be formed, into which the drains of limited areas are to discharge their contents, and that deodorizing and disinfecting agents be there employed, to enable the sewage to be removed in the solid form for agricultural and horticultural purposes. It is a fair subject for discussion, whether the process of deodorizing and solidifying the manure be the most advantageous, or whether it might not be better, by the aid of steam-power and well-adjusted tubing, to convey the manure in a liquid form directly into the country; but of one thing we are certain, that any system of sewage which fails to make the excrementitious matter available for the farmer is unworthy of the support of the public, because thus only can we meet the sanitary requirements of the case, and shall at the same time secure a large additional return for the outlay, in the shape of an enormously increased fertility of our fields and our gardens.

Art. XVII.—*An Introduction to Practical Pharmacy, designed as a Text-book for the Student and as a Guide to the Physician and Pharmaceutist, with many Formulas and Prescriptions.* By Edward Parrish, Graduate in Pharmacy, Member of the Philadelphia College of Pharmacy, and of the American Pharmaceutical Association, and Principal of the School of Practical Pharmacy, Philadelphia. With two hundred and forty-three Illustrations.—*Philadelphia.* pp. 544.

While on this side of the Atlantic we are gradually seeking to separate the duties of preparing and dispensing medicines from the duties of the medical man, because the present state of science renders it impossible for one man to compass both, our American friends are doing their best to perpetuate this heterogeneous compound. In the comparatively thinly-populated districts of the United States, it can as yet scarcely be otherwise, and we have no doubt that a book like the present may there prove of great use to the physician. It is what it professes to be—an introduction to practical pharmacy; and speaking of it in reference to our own country, we should say that its elementary character, and the profuse illustrations which it contains of the various apparatus required in the preparation of chemicals, in compounding medicaments, and in fitting up a shop, would render it very useful to an aspiring druggist.

Art. XVIII.—1. *A Practical Treatise on Stammering: its Pathology, Predisposing, Exciting, and Proximate Causes, and its most successful Mode of Cure, Scientifically Explained. With Remarks on the Principles which should Guide the Practitioner in the Treatment of all purely Nervous Diseases.* By J. H. Ayres Poett, M.D., Member of the Royal College of Surgeons, L.A.C., &c.—*London,* 1856. pp. 50.

2. *A Treatise on the Cure of Stammering; with a Notice of the Life of the late Thomas Hunt, and a General Account of the various Systems for the Cure of Impediments in Speech.* By James Hunt, M.R.S.L., &c. Second Edition, considerably enlarged.—*London,* 1856. pp. 104.

The first of these two treatises contains a rational exposition of the nature and causes of stammering, a defect which but too often results from a careless education in early life, and is the source of much anxiety and trouble to the individual afterwards. Dr. Poett points out that there are two points to be considered in reference to the removal of this fault. We should first attend to the health of the patient; an excitable state of the nervous and muscular systems, analogous to that prevailing in chorea, being the predisposing cause, which is amenable to therapeutic and dietetic agents. When we have removed any morbid disturbance of this kind, it becomes necessary to subject the individual to a systematic discipline, by which the bad habit may be overcome. This habit may be easily checked by a careful parent or master in its incipient stage; but when once established, the stammerer will require to undergo a special training, demanding separation from his ordinary companions, and constant supervision. The sixth chapter contains remarks on nervous diseases generally, that are not at all to the point, and might in another edition be suitably omitted. With this exception, we can recommend Dr. Poett's pamphlet.

Of Mr. Hunt's Treatise on Stammering, we are constrained to speak in less favourable terms. It contains some sensible remarks on the causation of the defect, but we are not informed of the method pursued by the author for its removal; while he indulges in polemical and self-laudatory remarks, and favours us with personalities and testimonials, which offer too great a savour of egotism to be agreeable or profitable to our readers.

Art. XIX.—*Summary of New Publications.*

A considerable number of works, of more or less interest, are before us, to which at present we are able only to make a passing allusion. Several of them will receive a more detailed notice in future numbers; but we must now content ourselves with directing the attention of our readers to their titles.

The completion of Todd and Bowman's 'Physiological Anatomy' will enable us to go fully into the merits of this important work in our next number, when we hope to couple with it the large volume on Human Physiology published by Dr. Draper, of New York. In Physiology we have also to mention the translation by Mr. Dallas of a work by Professor Siebold, 'On True Parthenogenesis in Moths and Bees,' in which the hitherto-disputed question of the existence of a *Lucina sine concubitu* is set at rest in the affirmative. With this class of books we must also range Dr. Holland's 'Constitution of the Animal Creation, as expressed in Structural Appendages,' under which quaint title the author enters into a minute disquisition on the hair, nails, and other parts of the animal frame, which may be regarded as mere appendages to the body.

In Medicine we have first to mention Sir John Forbes' 'Inquiry into the relative Power of Nature and Art in Curing Disease,' which, we doubt not, will command an extensive circle of readers, and to which we shall pay especial attention in our next. Dr. Todd's Clinical Lectures 'On Diseases of the Urinary Organs' are similar in character to his recent lectures 'On the Diseases of the Nervous System.' The pathology and treatment of consumption has found an able exponent in Dr. Hall, of Sheffield; while the employment of compressed air in phthisis, first suggested by Messrs. Pravaz and Bertin, and scarcely as yet adverted to in this country, has been introduced into English therapeutics by Dr. Simpson, of Ben Rhydding. The subject is one that merits further investigation, but we cannot admit that the cases brought forward by Dr. Simpson in any way prove his statements that the compressed air-bath effects the entire removal of tubercle, and thus a complete cure of phthisis, even after the formation of a cavity.

Numerous Surgical works of importance deserve notice.

Dr. Mackenzie has published brief 'Outlines of Ophthalmology,' which cannot fail to be of use to the practitioner and the student. Dr. Hodgson's prize essay 'On the Prostate Gland' we shall have occasion to speak of more fully; the same applies to Mr. Holmes Coote's 'Practical Observations on Syphilis,' and the fifth edition of Mr. Coulson's work 'On Diseases of the Bladder and Prostate.' Mr. Hare's 'Cases and Observations illustrative of the Recumbent Treatment of Spinal Disease' merit the careful perusal of all interested in this distressing form of disease. We hope to find an opportunity of again devoting special attention to the subject of orthopædics, when we shall also review Dr. Wildberger's work 'On the Treatment of Obsolete Luxations of the Hips.' The bulky ninth volume of the 'Transactions of the American Medical Society' offers but little that is likely to interest our readers. We receive with much satisfaction the Report of the Statistical Society of London, 'On the Medical Charities of the Metropolis,' the labours bestowed upon which will, we hope, ultimately be productive of a greater concentration of the extensive powers now bestowed upon our various charitable institutions. The Report of the Imperial Society of Medicine at Constantinople 'On Typhus' will be noticed in our next, when we hope also to enter fully into the important subject of adulterations, *apropos* of Dr. Hassall's latest work on their detection. Mr. Toynbee's 'Descriptive Catalogue of his own Museum of Preparations of the Ear,' will not fail to impress the reader with the conviction of that gentleman's untiring industry. To the young student of the microscopy and chemistry of the urine, Dr. Beale's Table will be useful.

A new Medical Directory has appeared, under the title of 'The Medical List,' which offers one useful feature not contained in its rival—the names of the medical officers of both services; but we cannot approve of the caprice with which the appointments and writings of medical men are given or omitted. In this respect the 'Medical Directory' is more uniform, and much more satisfactory.

Dr. Speer's translation of Messrs. Becquerel and Rodier's work 'On Pathological Chemistry' we shall take another occasion to treat more in detail. And we regret that it is out of our sphere to examine closely the very interesting Edinburgh Essays published by Members of the Edinburgh University. The only article of decidedly professional interest contained therein is a careful and temperate essay On Homœopathy, by Dr. Gairdner, to which we accord our full approbation, both in regard to style and to matter.

Among minor works and reprints, we would especially advert to Dr. Brinton's volume 'On the Pathology and Treatment of Ulcer of the Stomach,' consisting mainly of the papers which have appeared by that author in our Review; Mr. Field's prize essay 'On the Therapeutical Effect of Purgatives in the Horse;' Dr. Williams's paper 'On the Treatment of Iritis without Mercury;' and Dr. West's 'On Cranial Presentations.' Nor should we leave unnoticed the fifth edition which has just been issued of that very useful compilation, 'The Anatomical Remembrancer, or Complete Pocket Anatomist.'

PART THIRD.

Original Communications.

Art. I.

On the Relative Mortality of Males and Females under Five Years of Age. By John William Tripe, M.D., Medical Officer of Health for Hackney District.

The enormous disproportion in the deaths of males under five years of age, as compared with females, has long been known, although, so far as we are aware, without receiving any extended statistical inquiry as to its causes. As a step towards working out this subject, we pointed out, in 1849, the great excess of deaths from scarlatina during the third year of life, and especially the preponderance of male deaths from this disease during the first decennial period of life. Some years afterwards, in an article published in this Review,* we dwelt very much on the vast difference in the rate of death of the two sexes from scarlatinal dropsy, where it was shown "that males suffer from scarlatinal dropsy in the proportion of 60·3 per cent., and females of only 39·7 per cent.; and that the per-centages vary between 61·5 and 57·5 per cent. for males, and 42·5 and 38·3 per cent. for females."

Before commencing the proper subject of our paper, we may state, that in the years 1845–53, which are those to which our inquiry is chiefly limited for the metropolis, the aggregate number of male births was 338,901, and of female 325,756, being at the rate of 1000 males to 961 females; and that the annual rate varied between 950 and 978 females to 1000 males. Had the rate of death been uniform in the two sexes, it is quite evident that there would have been a great excess of males over females under five years of age at the date of the Census in 1851. But this was not the case, for to each 1000 males under one year of age there were as many as 993 females of the same age; and the numbers at the ages of one to two years, two to three years, three to four years, and four to five years, were as follows: 995, 996, 996, and 995 females to 1000 males respectively. The statistics of the Registrar-General show, that in this country the rate of male births to those of females undergoes considerable variations, and that "the fluctuation in the proportions is greatest where the births are fewest in number."† We may also state that Mr. Sadler also long since observed the smaller proportionate number of male

* Vol. xiii. p. 236 et seq. † Annual Report for 1850.

births which obtains in towns as compared with the country; and he explains it by the fact that inhabitants of towns marry proportionably younger women, and at a later period of life;* conditions which produce this result.

We have just pointed out that a comparison of the number of births of males and females with the census returns proves an excess of male deaths to have taken place in the metropolis during the years 1845–51, and we now propose ascertaining the rate of this excessive mortality. For this purpose we have tabulated the deaths registered in the Annual Reports of the Registrar-General, which show that during the years 1838–53, no less than 178,556 males under five years of age died in the metropolis to 159,278 females of the same age, or at the rate of only 892 females to each 1000 males; and that in the whole of England, 2,375,170 deaths of children under five years of age were registered during the same period, of which 1,275,095 were males, and only 1,100,075 females, or 1000 males to 863 females. On calculating the number of deaths of females to males for each year during this period, we have ascertained that, on an average at all ages, 103 males died to 100 females; whilst of children under five years of age, no less than 116 males died to 100 females. Also, that the smallest proportionate mortality of males at all ages was 101, and the largest 105; the average, 103, having occurred in ten out of the sixteen years examined. As regards the mortality under five years, the rate of variation was about the same, the smallest male mortuary rate having been 112, and the highest 118, to each 100 female deaths; the average of 116 having happened in eight, or one-half only, of the sixteen years.

The limited space at my disposal prevents me from discussing so fully as could be wished the following table (p. 457), which indicates the mortality under five years of age in several European nations, and in the cities of London and Paris.

The table shows, that of 1,812,467 deaths under five years of age registered in England, France, Belgium, London, and Paris, 967,398 were of males, and 845,069 of females, or 8735 females to each 10,000 males. Also, that of 2,822,129 deaths under five years in the above countries and cities, and also in Prussia and Sweden, 1,508,017 were of males, and 1,314,112 of females; of 231,937 deaths under six years of age in Saxony, 125,771 were of males, and 106,166 of females; and of 275,346 deaths under ten years of age in Norway, 147,285 were of males, and 128,061 of females. The aggregate of these deaths, which may for practical purposes be all grouped as under five years of age (the per-centage of those in Norway being the same as of the whole), amounts to 3,329,412, of which 1,781,073 were of males, and 1,548,339 of females, or only 8693 female to each 10,000 male deaths.

The column of per-centages of deaths under five years of age, shows that to every 1000 male, the following number of female deaths occurred:—863 in England,† 862 in France, 871 in Belgium, 876 in

* See Quetelet's Treatise on Man, chap. 2.

† The rate was precisely the same in England for the years 1838–44, and 1848–53.

TABLE I.—*Deaths under Five Years of Age in England, France, Prussia, Sweden, Saxony, Belgium, London, and Paris; Norway, under Ten.*

—	Under One Year.		Between One and Two Years.		Between Two and Three Years.		Between Three and Four Years.		Between Four and Five Years.		Total under Five Years.		Per-centages.	
	Males.	Females.	Males.	Females.	Males.	Females.	Males.	Females.	Males.	Females.	Males.	Females.	Males.	Females.
England, years 1838–44	301,463	236,338	100,902	95,826	53,833	53,483	35,836	35,814	26,054	25,650	518,088	447,111	100·0	86·3
France (mean of deaths in each year), 1817–31	97,552	79,156	25,170	24,198	14,108	13,643	9,272	9,026	6,630	6,562	152,732	132,585	100·0	86·2
Belgium, 1848–50	34,444	27,009	9,965	9,744	5,406	5,499	3,712	3,862	2,793	2,976	56,320	49,090	100·0	87·1
London, 1838–44	37,310	30,431	16,771	15,953	9,250	9,043	5,936	6,013	3,963	3,949	73,230	65,389	100·0	89·3
„ 1847–53	45,797	36,905	18,944	17,836	10,052	10,000	6,279	6,257	4,268	4,031	85,340	75,029	100·0	87·9
Paris, 1836–53	45,370	38,891	16,148	16,135	9,324	9,578	6,418	6,635	4,428	4,626	81,688	75,865	100·0	90·6
Totals	561,936	448,730	187,900	179,692	101,973	101,246	67,453	67,607	48,136	47,794	967,398	845,069		
Per-cents. of deaths under 5 years at each annual period	31·00	24·76	10·37	9·91	5·63	5·59	3·72	3·73	2·66	2·63	100·0	87·35		
Totals brought down	561,936	448,730	Between One and Three Years.				Between Three and Five Years.				967,398	845,069	100·0	87·35
			Males.			Females.	Males.		Females.					
Prussia, 1839–40–41	171,093	139,424	Prussia		82,930	79,426	31,427		31,308	......	285,450	250,148	100·0	87·6
Sweden, 1821–40	173,895	143,776	Sweden		58,727	53,750	22,547		21,369	......	255,169	218,895	100·0	85·8
Saxony* (10 years)	92,967	74,498	......			......	......		......	Under Six Years.	125,771	106,166	100·0	82·0
Norway,† 1801–35	......	......	......			......	......		......	Under Ten Years.	147,285	128,061	100·0	86·9
Totals	999,891	806,428	......		141,657	133,176	53,974		52,677	......	1,781,073	1,548,339		
Per-centages	100·0	80·65	......			......	......		......	......	100·0	86·93		

* The Saxon returns do not show any ages of early childhood except under One and from One to Six years of age.
† The Norwegian returns of deaths are in decennial periods.

Prussia, 820 in Saxony, 858 in Sweden, 869 in Norway, 893 in London for the years 1838–44, and 879 for the years 1847–53, and lastly, 906 in Paris. These numbers differ far less than might have been expected, and show that the rate of male deaths at an early age is greatly in excess of that of females in most European countries; and also that the disproportion in the mortality is smallest in cities, especially in Paris, whilst it is largest in Saxony. The per-centages at the bottom of the table show that to every 10,000 deaths of males under one year of age in England, France, Belgium, Prussia, Sweden, and Saxony, only 8065 deaths of females happened; a most enormous disproportion.

The per-centage of deaths under five years during each annual period shows that no less than 55·76 of every 100 deaths under five years of age took place during the first year of life; or, by avoiding decimals, that of every 10,000 deaths of children under five years of age, 5576 occurred in the first, 2028 in the second, 1122 in the third, 745 in the fourth, and only 529 in the fifth year of life. That of these 10,000 deaths, 3100 were males and 2476 females in their first year; 1037 males and 991 females in their second; 563 males and 559 females in their third; 372 males and 373 females in their fourth; and 266 males to 263 females in their fifth year of life. This table therefore proves that by far the greatest excess of male deaths occurs in the first year; that the rate of male deaths approximates to that of females as age advances, and that it becomes nearly alike after the third year of life.

As regards the mortuary rate of males to females in town as compared with rural populations, but little definite information is obtainable; but from calculations which we have made, the following have been deduced. The rate of females to 100 males in London is 89·2, and in Paris 90·6, against 86·6 for England and 86·2 for France. In Belgium and Sweden, on the other hand, it was 87·4 and 84·9 in the capitals, against 87·8 and 85·8 in the country. Although therefore no definite conclusions can be drawn from this statement, still the balance of evidence (the rate of mortality in England and France, in London and Paris, being respectively very nearly alike) is in favour of a larger proportion of female deaths occurring in towns than in the country. Had space allowed, this would have been proved to be pretty constant in London and Paris, as compared with all England and France.

We now propose showing that the relative proportion of male still-born children is greater than that of male deaths during the first year of life, and is so constant and large that it cannot depend merely on the dangers of childbirth.

TABLE II.—*Still-born Male and Female Children.*

	Males.	Females.	Per-centages. Males.	Per-centages. Females.
France (three years)	67·356	46·637	100·0	69·2
Austria (four years)	25·288	17·351	100·0	68·6
Belgium	38·312	28·359	100·0	74·0
Saxony (ten years)	17·618	12·839	100·0	72·9
Prussia (three years)	24·838	19·036	100·0	76·6

Our own experience is also in favour of the opinion that, in a large proportion of miscarriages, the fœtuses are of the male sex. It is much to be regretted that for this kingdom we have no statistics of still-born children, so that no comparison can be made in this respect between this and other countries.

The results of Table II. are very striking, for we see that to each 1000 males who are still-born, there are in France only 692, in Austria 686, in Prussia 766, in Belgium 740, and in Saxony 729, still-born females. The variations in the ratios are by no means great, and they are yet smaller in each country during a period of years than those shown in the above table for different countries. This cannot be proved here, for want of space. It will be noticed that the variation does not amount to five and a half per cent., although the statistics are collected from such different nations and races; showing that the law is general, and that the cause of the excess of male deaths over those of females commences at the earliest period of life, and diminishes, as we have already shown, as age advances, even from the first month, and most probably week, of extra-uterine life.

This opinion receives very strong confirmation by a comparison of the ratios of still-born male and female children with those of children who die during the first month. We find in Belgium that the proportion of still-born female children to that of males is 740 to 1000; whilst that of deaths under one month old is 749 to 1000; and in England (years 1839–44), 765 to 1000. I will put these in form:

TABLE III.

	Males.		Females.
Ratio of male and female still-born children in France, Austria, Prussia, Belgium, and Saxony	1000	...	723
Ditto in Belgium	1000	...	740
Deaths under one month in Belgium	1000	...	749
Deaths under one month in England	1000	...	765
Deaths under one year in Europe	1000	...	806

We must now pass on to a comparison of the mortuary rates in the two sexes from different diseases, and propose discussing them first under eight groups, which have been selected from the Returns of the Registrar-General.

Class 1. Deaths from zymotic diseases.
" 2. " tubercular diseases.
" 3. " diseases of the nervous system.
" 4. " " respiratory organs.
" 5. " " digestive organs.
" 6. " atrophy.
" 7. " premature birth.
" 8. " all other specified causes.

CLASS I. *Zymotic Diseases.*—From Table IV. we learn that out of 72,652 deaths under five years of age from zymotic diseases, 36,857 were of males and 35,795 of females, or 1000 males to 971 females. We also perceive that the proportion of the one to the other in the different years included in Table IV. varied between 92 and 104 females to 100 males respectively; and that, with one exception, the gross number of male deaths was in excess of those of females in every year.

TABLE IV.—*Metropolis* 1845-53.—*Deaths from Zymotic Diseases.*

Ages.	Under 1.	1—2.	2—3.	3—4.	4—5.	Total under 5.
Males ...	14,053 ...	9349 ...	6061 ...	4375 ...	3019 ...	36,857=100
Females...	12,377 ...	9442 ...	6468 ...	4572 ...	2996 ...	35,795=97·1
Males ...	19·3 ...	12·9 ...	8·4 ...	6·0 ...	4·2	100
Females...	17·0 ...	13·0 ...	8·9 ...	6·2 ...	4·1	

To 100 deaths of males in each of the different years under consideration, the following number of female deaths occurred:—104, 99, 97, 95, 92, 99, 97, 94, and 97.

It will also be seen that out of each 1000 deaths, 193 were of males, and 170 of females, under one year; 129 of males, and 130 of females, between one and two; 84 of males to 89 of females between two and three; 60 of males to 62 of females between three and four; and 42 of males to 41 of females between four and five years of age.

CLASS II. *Tubercular Diseases.*—Under this head are included scrofula, tabes mesenterica, phthisis or consumption, and hydrocephalus.

TABLE V.—*Metropolis,* 1845-53.—*Deaths from Tubercular Diseases.*

Ages.	Under 1.	1—2.	2—3.	3—4.	4—5.	Total under 5.
Males ...	5433 ...	4297 ...	2027 ...	1114 ...	772 ...	13,593=100
Females...	4187 ...	3541 ...	1802 ...	994 ...	660 ...	11,184=82·3
Males ...	21·9 ...	17·3 ...	8·2 ...	4·5 ...	2·9	100
Females...	16·9 ...	14·3 ...	7·3 ...	4·0 ...	2·7	

The rate of female deaths in each year to 100 males was 89, 83, 87, 80, 84, 81, 80, and 77.

We perceive, in casting our eyes over the per-centages for each year, that in one year only 823 female children died to each 1000 males; that the nearest approach to an even number was 887 female to 1000 male deaths; and also that 13,593 males died to 11,184 females; or in the proportion of 1000 males to 800 females. On examining the per-centages in the different years, we perceive that in no instance did the mortality of females equal that of males. It also informs us that out of each 1000 deaths from this group of diseases, 219 were of males, and 169 of females, under one year; 173 of males, and 143 females, between one and two years; 82 of males to 73 females between two and three; 45 males to 40 females between three and four; and 29 males to 27 females between four and five years of age.

CLASS III. *Diseases of the Nervous System.* — The next diseases which we have to consider are those of the nervous system—viz., cephalitis, apoplexy, paralysis, chorea, epilepsy, tetanus, convulsions, and deaths registered as from "diseases of the brain." The majority of deaths from these causes are attributed to "convulsions," a most unsatisfactory nomenclature.

TABLE VI.—*Metropolis,* 1843-53.—*Deaths from Diseases of the Nervous System.*

Ages.	Under 1.	1—2.	2—3.	3—4.	4—5.	Total under 5.
Males ...	9849 ...	1805 ...	876 ...	511 ...	359 ...	13,400=100
Females...	7480 ...	1637 ...	735 ...	473 ...	275 ...	10,656=79·5
Males ...	40·9 ...	7·9 ...	3·6 ...	2·1 ...	1·4	100
Females...	31·1 ...	6·9 ...	3·0 ...	1·9 ...	1·2	

The per-centages of female deaths in each year were 83, 82, 83, 81, 83, 77, 78, 74, and 75 to each 100 males.

We perceive from Table VI. that 13,400 deaths of males were registered in London from these diseases, against 10,656 of females; or in the ratio of 1000 males to 795 females—a singular and vast disproportion. An examination of the per-centages for each year shows the excess to be very uniform—viz., less than ten per cent; the greatest difference in the rate of death in the two sexes being 100 males to 74 females, and the smallest 100 males to each 83 females. The per-centages of all deaths indicate that, of every 1000 who died under five years of age from these diseases, no less than 720 expired during the first, and 148 during the second, year of life. Of these 720, as many as 409 were males, and only 311 females. These per-centages also prove that comparatively few children above two years of age die in London from nervous diseases.

CLASS IV. *Deaths from Diseases of the Respiratory Organs.*—This group consists of laryngitis, bronchitis, pleurisy, pneumonia, asthma, and deaths registered as from "diseases of the lungs."

We have here a different class of diseases to any as yet considered, and in which we might expect the rule hitherto obtained to be wanting; but it will be found that these diseases, even although inflammatory, are no exception to the rule.

TABLE VII.—*Metropolis*, 1845-53.—*Deaths from Diseases of the Respiratory Organs.*

Ages.	Under 1.	1 — 2.	2 — 3.	3 — 4.	4 — 5.	Total under 5.
Males ...	10,192 ...	5453 ...	2280 ...	1037 ...	515 ...	19,477=100
Females...	7,617 ...	3070 ...	1343 ...	1142 ...	547 ...	16,719= 85·5
Males ...	27·9 ...	15·1 ...	6·3 ...	2·9 ...	1·4	} 100
Females...	21·1 ...	14·1 ...	6·5 ...	3·2 ...	1·5	} 100

The rate of female deaths to 100 males in each of the different years under consideration, was 86, 85, 89, 87, 91, 84, 89, 82, and 83.

This table shows that of 36,196 deaths from these causes, 19,477 occurred in male children, and 16,719 in females; or in the ratio of 1000 of the former to 858 of the latter. From the annual per-centages we learn that the rate of excess varies less than ten per cent., the largest number of females being 91, and the smallest 82, to each 100 males respectively. This result is much opposed to the opinion ordinarily entertained, and should make us give a more guarded prognosis of inflammatory pulmonary disease when it attacks a male than a female. The per-centages at the different ages show that, out of every 1000, nearly one-half—viz., 490—happened during the first, 292 during the second, 128 in the third, 61 in the fourth, and 29 only in the fifth year; that the greatest excess of male deaths took place in the first year, when there were 279 of males to 211 of females; the next in the second year; and that between two and three, and three and four years, the number of females was in excess of those of males.

CLASS V. *Deaths from Diseases of the Digestive Organs.*—This class includes a large number of diseases, very many of which have but little influence on the rate of mortality. They are teething, quinsy, gastritis, enteritis, peritonitis, ascites, ulceration of intestines, hernia, ileus, intussusception, "disease of the stomach," "disease of pancreas," hepatitis, jaundice, "disease of liver," and disease of spleen. Of these, teething and enteritis are the two to which the greatest mortality is attributed, about one-half having been registered as due to the former alone.

TABLE VIII.—*Metropolis*, 1845-53.—*Deaths from Diseases of the Digestive Organs.*

Ages.	Under 1.	1—2.	2—3.	3—4.	4—5.	Total under 5.
Males ...	3321 ...	1568 ...	374 ...	158 ...	123 ...	5544=100
Females...	2402 ...	1459 ...	385 ...	136 ...	122 ...	4504= 81·2
Males ...	33·1 ...	15·6 ...	3·7 ...	1·6 ...	1·2	} 100
Females...	23·9 ...	14·5 ...	3·8 ...	1·4 ...	1·2	} 100

To every 100 males in each of these years, the following numbers of female deaths happened:—87, 85, 81, 84, 81, 84, 77, 73, and 82.

We ascertain from this table that of 10,048 deaths registered under this class, 5544 were of males, and 4504 of females, or 1000 deaths of the former to 812 of the latter; that the proportionate minus of female deaths varied between 868 and 726 to 1000 males. We also perceive that out of every 1000 deaths, 570 occurred during the first year of life, 301 during the second, 75 during the third, 30 during the fourth, and 24 during the fifth. Of the 570 during the first year, 331 were of males, and 239 of females; and of 301 during the second year, 156 were of males to 145 females. In the other years, the difference in the mortality of the two sexes was but slight.

So large a proportion of deaths having been registered from teething, they will be considered separately. It appears that of the above 10,048 deaths, 5086 were stated to have been caused by teething, and 4962 only from all the others. Of the 5086 from teething, 2714 were males and 2372 females, or 882 females to each 1000 males; and of the 4962 from the other diseases, 2830 were males and 2132 females, or 753 females to each 1000 males. We therefore perceive that the mortality from organic diseases of this class was greater in males than in females, not only absolutely, but even relatively, when compared with those from irritative diseases; a result far different from what we might have expected from the known peculiarities of the female sex.

CLASS VI. *Metropolis*, 1845-53.—*Deaths from Atrophy.*

Ages.	Under 1.	1—2.	2—3.	3—4.	4—5.	Total under 5.
Males ...	3856 ...	743 ...	285 ...	98 ...	68 ...	5050=100
Females...	3422 ...	730 ...	256 ...	124 ...	67 ...	4599= 90·1
Males ...	39·9 ...	7·7 ...	2·9 ...	1·0 ...	0·7	} 100
Females...	35·5 ...	7·6 ...	2·7 ...	1·3 ...	0·7	} 100

In the period embraced by the table, the number of females who died to 100 males in the different years were respectively 91, 104, 101, 81, 89, 81, 95, 82, and 96.

On examining the per-centages for each year, we perceive that the rate of death in the two sexes varies very considerably—so much so as to make me believe that deaths from many causes are included under this head. Thus we find the rate of death in females to vary between 81 and 104 to each 100 males. We also learn that of 9649 deaths registered as from this cause, 5050 were males and 4599 females, or in the ratio of 901 females to 1000 males. On inspecting the deaths at different ages, we find that by far the greatest proportion occurs during the first year—7278 of the 9649, or 75·4 per cent. of the whole, being in children under one year old.

CLASS VII. *Deaths resulting from Premature Birth.*—Of all the causes of death, there is perhaps no one in which we should, *à priori,* have expected a more variable rate of death in the two sexes than in the one we are now considering; yet, as we shall see on considering the table, there is no one in which the preponderance of male deaths is more uniformly marked.

TABLE X.—*Deaths from Premature Birth.—Metropolis,* 1845–53.

Years.	Males.	Females.	Per-centage. Males.	Per-centage. Females.
1845	448	381	100	85·0
1846	569	473	100	83·1
1847	633	514	100	81·2
1848	614	506	100	82·4
1849	671	561	100	83·6
1850	692	549	100	79·3
1851	840	630	100	75·0
1852	835	702	100	84·1
1853	840	635	100	75·6
Totals	6142	4951	100	80·6

We perceive on examining the column of per-centages, that the rate of deaths in females varied between 75 and 85 to each 100 males, that the average was 806 to each 1000 deaths of males during those years, the total number registered being 11,093, and of these 6142 were males and 4951 females. It is impossible to ascertain the proportionate number in England of the two sexes which are still-born, but the experience of most accoucheurs is, that the preponderance lies on the male side, and the foreign returns quoted in Table II. prove this indisputably.

CLASS VIII. *Deaths from all other Classified Diseases, not included in the previous Tables.*—There are some deaths, the causes of which are not specified; but all deaths from specified causes not included in the classes already considered, are here grouped together.

TABLE XI.—*Deaths from all Causes not included in the other Tables.—Metropolis,* 1845–53.

Years.	Males.	Females.	Per-centage. Males.	Per-centage. Females.
1845	553	392	100	70·9
1846	500	414	100	82·8
1847	583	5o0	100	85·7
1848	654	574	100	87·7
1849	647	599	100	92·5
1850	642	583	100	90·8
1851	743	642	100	86·4
1852	781	699	100	89·5
1853	748	684	100	91·4
Totals	5851	5087	100	83·3

As we see from the totals, the number is not very large, considering the great number of diseases of which this class consists—viz., diseases "of uncertain or variable seat," "of the urinary and generative organs," "organs of locomotion," "of the integumentary system," "malformation;" "sudden deaths, cause unknown;" from privation, want of breast-milk, neglect, cold, and deaths by violence.

Although this class consists of such heterogeneous diseases, yet the rate of death in females, as compared with that of males, is as uniform as most others; for with the exception of 1845, it oscillated only between 82·8 and 92·5 to each 100 males, or nearly ten per cent. The table also shows that of 10,938 deaths, 5851 were of males and 5087 of females, or in the proportion of 833 females to 1000 males.

We shall next pass in review the mortality of London in comparison with that of England and Wales, exclusive of the metropolis. In so doing, we shall not give the gross numbers in the tables, as they have in part been shown before, but the per-centages only, as they afford a sufficient and ready standard of comparison.

TABLE XII.

	England.		London.	
	Males.	Females.	Females.	Males.
Deaths from zymotic diseases	100	98·8	97·1	100
" tubercular diseases	100	84·9	82·3	100
" diseases of nervous system	100	76·2	79·5	100
" " respiratory organs	100	82·7	85·8	100
" " digestive organs	100	83·9	81·2	100
" atrophy	100	89·0	90·1	100
" premature birth	100	77·2	80·6	100
" all other diseases	100	83·3	86·9	100
Totals	800	676·0	683·5	800

From this table we learn that to 1000 deaths of male children, 971 females died in London, and 988 in England, from zymotic diseases; 823 in London, and 849 in England, from tubercular diseases; 795 in London, and only 762 in England, from nervous diseases, being the greatest disproportionate ratio in all the classes of disease. From diseases of the respiratory organs, 858 female children expired in London, and 827 in England, to 1000 males; and from the other classes of disease at the following rates:—Of the digestive organs, 812 in London, and 839 in England; of atrophy, 901 in London, and 890 in England; from premature birth, 806 in London, and 772 in England; and from all other diseases, 869 in London to 833 in England to 1000 males respectively. The most fatal class of maladies to male children is therefore those of the nervous system and from premature birth; and to female children, zymotic diseases.

We next purpose contrasting the rate of death in males and females separately, so as to ascertain the proportionate fatality of each group of diseases in either sex.

From Table XIII. we learn that of each 1000 deaths *of males* in England, 308 occurred from zymotic diseases, 99 from tubercular diseases, 190 from maladies of the nervous system, 139 from diseases of the respiratory organs, 58 of the digestive apparatus, 46 from atrophy, 111 from premature birth, and 49 from other diseases. *In London* the ratios varied from those in all England; the deaths from zymotic and tubercular diseases, also from affections of the respiratory organs, were comparatively in excess, being 348, 128, and 184 respectively; whilst *in England* those from nervous diseases and from premature birth were considerably in excess, being 190 and 111 in each 1000.

TABLE XIII.—*Per-centages of Deaths from each Cause, to Total Deaths in each Sex.*

Class		Males. London.	Males. England.	Females. London.	Females. England.
1.	Deaths from zymotic diseases	34·8	30·8	38·3	35·2
2.	" tubercular diseases	12·8	9·9	11·9	9·7
3.	" diseases of nervous system	12·7	19·0	11·5	16·8
4.	" " respiratory organs	18·4	13·9	17·9	13·3
5.	" " digestive organs	5·2	5·8	4·8	5·6
6.	" atrophy	4·8	4·6	4·9	4·7
7.	" premature birth	5·8	11·1	5·3	9·9
8.	" all other diseases	5·5	4·9	5·4	4·8
	Totals	100·0	100·0	100·0	100·0

Of each 1000 *female* deaths in England, 352 arose from zymotic diseases, 97 from tubercular affections, 168 from nervous diseases, 133 from disorders of the respiratory organs and 56 of the digestive apparatus, 47 from atrophy, 99 from premature birth, and 48 from all other diseases. In London, as compared with England, deaths from zymotic and tubercular diseases, and from disorders of the respiratory organs, were in excess, the same as in the male sex; showing, therefore, that there are certain definite causes in operation which affect both sexes alike, accordingly as they constitute part of a rural or town population.

The ratios in the other diseases are too small to require special mention.

Having discussed the rate of mortality in the two sexes under five years of age, from various groups of disease, we now propose considering the influence of sex on several individual diseases. I have chosen chiefly those of the zymotic class, as they consist of disorders each of which induces very different manifestions of its presence in the human fabric, has special periods in which it runs its course, and, as will be proved, induces death in the two sexes after a tolerably uniform rate. The other classes embrace diseases so similar in their progress, and so allied in their nature, as not to require separate consideration. For instance, what practical advantage would be derivable from considering the mortuary rate of the two sexes in bronchitis, pneumonia, or pleuritis separately, or of the various diseases of the brain, &c.? Does any one of these diseases usually produce death *per se*, and is the mode of registration such as to enable us to make this separation? To both of these queries we answer no, and therefore do not purpose making an individual analysis of either. The first disease we shall consider is small-pox, the characteristics of which, as well as of the other eruptive forms, are so marked as not to be mistaken.

TABLE XIV.—*Deaths from Small-pox.—Metropolis,* 1845–53.

Ages.	Under 1.	1—2.	2—3.	3—4.	4—5.	Total under 5.
Males ...	884 ...	548 ...	431 ...	351 ...	247 =	2461=100
Females...	827 ...	568 ...	420 ...	351 ...	228 =	2394= 93·2
Males ...	18·2 ...	11·3 ...	8·9 ...	7·2 ...	5·1	100
Females...	17·1 ...	11·7 ...	8·6 ...	7·2 ...	4·7	100

The rate of female deaths in different years to each 100 males was 90, 101, 106, 91, 94, 114, 106, 92, and 86.

The ratio of female deaths to those of males is seen to be very different to that from any other disease or group of diseases we have hitherto examined; for in four out of the nine years, the number of female deaths exceeded those of males by 1, 6, 6, and 14 per cent. respectively. It was formerly pointed out that the mortality from zymotic diseases as a class, occurred at a very different rate in female children to that from any other. The variation in the rate of death is also very large—nearly 30 per cent., instead of from 10 to 15 per cent. Thus, the per-centage shows that in one year there were 86 deaths only of females to 100 males, whereas in another there were 114 to 100 male deaths, the average for the whole number of years being 93 females to 100 males.

It will be seen on inspecting the per-centages of total deaths, that this disease is most fatal during the first year of life, both to males and females, and that the ratio gradually diminishes in each year as age advances, the greatest difference in the mortuary rate between the two sexes occurring in the first year of life, the variations in the other years being unimportant.

The total number of deaths registered in three years from this disease was 4855; and of these, 2461 were males and 2394 females, of which 35·3 per cent. occurred during the first year of life.

We shall now pass on to *Measles.* This disease proved far more fatal during these years (1845-53) than small-pox, having destroyed 10,024 children under five years of age. Of these, 5066 were males and 4958 females, or 970 females to each 1000 males.

TABLE XV.—*Metropolis,* 1845–53.—*Deaths from Measles.*

Ages.	Under 1.	1—2.	2—3.	3—4.	4—5.	Total under 5.
Males ...	931 ...	1945 ...	1167 ...	667 ...	356 =	5066=100
Females...	756 ...	1891 ...	1281 ...	660 ...	370 =	4958= 97·8
Males ...	9·3 ...	19·4 ...	11·6 ...	6·7 ...	3·5	100
Females...	7·5 ...	18·9 ...	12·8 ...	6·6 ...	3·7	100

To each 100 male deaths in these years, 105, 88, 96, 96, 96, 97, 104, 87, and 97 of females happened.

During the nine years embraced by this inquiry, we perceive that two present an excess of female deaths over those of males, the range in the mortuary rate being very large, varying from 87 females to 100 males in one year, and 105 females against 100 males in another; the mean rate being 978 females to 1000 males.

Measles presents another peculiarity as compared with small-pox—the second instead of the first year being that in which it is most fatal, next the third, then the first, then the fourth, and lastly, the fifth year of life. But it agrees in proving comparatively more fatal to males in the first year. In the third and fifth years there was a preponderance of female deaths, in the former as much as 1·2 per cent. of the total deaths.

Scarlatina.—This, which is one of the most fatal of all the zymotic class of diseases, produced a mortality of 11,562 children in these years.

TABLE XVI.—*Metropolis*, 1845-53.—*Deaths from Scarlatina.*

Ages.	Under 1.	1—2.	2—3.	3—4.	4—5.	Total under 5.
Males ...	641	... 1327	... 1571	... 1468	... 1089	... 6096=100
Females...	476	... 1145	... 1456	... 1367	... 1022	... 5466= 89·7
Males ...	5·5	... 11·5	... 13·6	... 12·7	... 9·4	100
Females...	4·1	... 9·9	... 12·6	... 11·8	... 8·9	

To each 100 male deaths the following number of females happened in these different years:—89, 87, 88, 90, 85, 94, 86, 87, and 100.

The table shows, that of the above number of deaths, 6096 were of males and 5466 of females, or 897 females to each 1000 males. The proportionate mortality in females varied between 86 and 100 against 100 males respectively, being a smaller range than of the other zymotic diseases just considered. It also differs from them in the age at which it proves most fatal, the third year being that in which the mortality both of males and females is greatest. The proportions at the different ages are as follows: 26·2 per cent. in the third year, 24·5 per cent. in the fourth, 21·4 per cent. in the second, 18·3 in the fifth, and only 9·6 per cent. in the first. The greatest difference in the mortality of the two sexes is to be found in the second year, and not in the first; the next largest in the first year, then in the third, after that in the fourth, and the smallest in the fifth.

The next disease to be considered is *Hooping Cough.*

TABLE XVII.—*Metropolis*, 1845-53.—*Deaths from Hooping Cough.*

Ages.	Under 1.	1—2.	2—3.	3—4.	4—5.	Total under 5.
Males ...	2708	... 2529	... 1226	... 687	... 343	... 7493=100
Females...	2826	... 3046	... 1724	... 945	... 466	... 9007=120·2
Males ...	16·4	... 15·4	... 7·4	... 4·2	... 2·1	100
Females...	17·1	... 18·5	... 10·5	... 5·7	... 2·7	

To 100 deaths of males in each of these years, the following numbers of females happened:—130, 125, 118, 118, 117, 110, 125, and 122.

A glance at the per-centages reveals a very different rate of female mortality, as it will be seen that in no one instance were the deaths of males greater than those of females; but, on the contrary, those of the latter sex were far the largest. This disease, during the years embraced by this inquiry, was more fatal to children under five years of age than any other—16,500 having died from it, of which no less than 9007 were females, and only 7493 males, or at the rate of 1202 females to each 100 males. The largest disproportion was 130 female to 100 male deaths; and the smallest, 110 to 100 males; the average being 1202 female to each 1000 male deaths. The disease also varies from every other, in the highest male and female mortalities having happened in different years of life; the highest rate of male deaths taking place in the first year, and that of females in the second. We also perceive that the two first years of life are those in which the disease is most fatal, 33·5 per cent. of all the deaths having supervened in the first year, and 33·9 in the second, against 17·9 in the third, 9·9 in the fourth, and 4·8 in the fifth.

TABLE XVIII.—*Metropolis, years* 1845-53.—*Deaths from Diarrhœa.*

Ages.	Under 1.	1 — 2.	2 — 3.	3 — 4.	4 — 5.	Total under 5.
Males ...	5719 ...	1448 ...	361 ...	120 ...	77 =	7725=100
Females...	4792 ...	1394 ...	317 ...	127 ...	53 =	6683= 86·5
Males ...	39·7 ...	10·0 ...	2·5 ...	0·9 ...	0·5	100
Females...	33·3 ...	9·6 ...	2·2 ...	0·9 ...	0·4	

To 100 male deaths in each of these years, there occurred the following number of females:—92, 90, 88, 89, 88, 84, 87, 83, and 81.

We perceive that of 14,408 deaths from this disease, 7725 were of males and 6683 of females, or 865 females to each 1000 males; and also, that in no one instance was the total mortality for any one year of females equal to that of males; but, on the contrary, that the proportions varied between 81 and 92 females to 100 males. The rate of death, both for males and females, is very much larger during the first year of life than in any other, as we see that of the 14,408 deaths, no less than 10,511, or 73·0 per cent., happened during that period, and 19·6 per cent. in the second year. The disease was not only most fatal to both sexes during the first year, but also in a far larger ratio to males than females, the male deaths having exceeded those of females by 6·4 per cent. of the whole number. The difference in the mortality of the two sexes was not great after the second year.

The last disease which we shall examine is *Convulsions*. By considering this separately, it must not be considered that we believe convulsions to be a disease *per se;* but so very large a number of deaths are referred to this cause, and so large and so uniform a disproportion exists in the mortality of the two sexes, that we did not feel justified in passing it by, more especially as it shows either that convulsive diseases are more lethal to males in infancy than to females, or else more frequently attack children of the female sex.

TABLE XIX.—*Metropolis, years* 1845-53.—*Deaths from Convulsions.*

Ages.	Under 1.	1 — 2.	2 — 3.	3 — 4.	4 — 5.	Total under 5.
Males ...	8559 ...	983 ...	403 ...	183 ...	97 =	10,225=100
Females...	6504 ...	940 ...	367 ...	186 ...	81 =	8,078= 79
Males ...	46·9 ...	5·4 ...	2·2 ...	1·0 ...	0·5	100
Females...	35·5 ...	5·1 ...	2·0 ...	1·0 ...	0·4	

To 100 male deaths in each of these years, 81, 84, 83, 79, 79, 77, 80, 73, and 73 female deaths happened.

The total number of deaths registered as having been caused by convulsions in the years 1845–53, is 18,303; and of these, 10,225 were of males, 8078 of females; or 790 females only to each 1000 males. On glancing over the per-centages, we perceive that the ratio of female deaths was far less than of males: the nearest proportions were 84 females to each 100 males, whilst the greatest variation was 73 females to 100 males. The number who died in the first year of life was much larger than at any other age, 8559 male and 6504 female deaths, or 46·9 per cent. of males to 35·5 per cent. of females, having been registered, making a total of 15,063 children, or 82·4 per cent. of the whole number. The difference in the relative mortality of the two sexes was but small at the other periods of life under consideration.

It will perhaps be useful to include the results of this examination of individual diseases in one table.

TABLE XX.—*Metropolis, years* 1845–53.

—	Number of deaths from each disease.	Ratio of deaths in males and females.		Age at which the greatest mortality occurs.		
		Males.	Females.	In both sexes.	Males.	Females.
Small-pox ...	4855	1000	932	1st year	1st year	1st year
Measles	10,024	1000	978	2nd year	2nd year	2nd year
Scarlatina......	11,562	1000	967	3rd year	3rd year	3rd year
Hooping-cough	16,500	1000	1202	2nd year	1st year	2nd year
Convulsions ...	18,303	1000	790	1st year	1st year	1st year
Diarrhœa	14,408	1000	865	1st year	1st year	1st year
Teething	5086	1000	882	1st year	1st year	1st year

The peculiarities may be briefly enumerated as follows:—Of all the zymotic diseases, hooping cough was the most fatal in the years 1845–53, *and was more fatal to female than to male children, being, therefore, an exception to the otherwise universal law, that during the first year of life, more males than females die from all the great groups of maladies with which the human race is afflicted.* The largest proportion of males died in their first year of age, and of females in their second; the disease being most fatal in both sexes conjointly in the second year. Small-pox is the least fatal of the exanthemata, and produces the greatest mortality in the first year, measles in the second, and scarlatina in the third; the order of fatality being that in which the diseases are enumerated. Convulsions, diarrhœa, and teething are all most fatal in the first year of life, the former (convulsions) producing the greatest excess of male deaths.

In conclusion, we would again express our belief that male infants are predisposed to disease in a far greater ratio than females, especially during intra-uterine life; that this predisposition, which in extra-uterine life is most marked during the first month, gradually diminishes after the child ceases to obtain its nutrition direct from its mother, and is almost removed shortly after the ordinary age of weaning—viz., at one year and a quarter. From these considerations we infer that the greater mortuary rate of males during the first years of life depends on some influence derived from one or the other, or both, of its parents, but most probably chiefly, if not entirely, from the mother. It certainly might arise from some unknown influence of the child's own nervous system; but this is scarcely probable, for as age advances, and the sexual characteristics become markedly developed, the disproportionate mortality of males ceases, and in the period between ten and fifteen years of age, gives place to an increased rate of death in females. The facts adduced in this paper may not warrant these conclusions; for it may only be a coincidence that the period of greatest male mortality corresponds with the periods of gestation and lactation. The greater longevity of females certainly points to another vital difference in the sexes; and it may perhaps be true that one cause (greater vitality of the female sex) induces the large differential mortality which we have shown to exist.

ART. II.

On the Pathology of Cancer of the Stomach. By WILLIAM BRINTON, M.D., Fellow of the Royal College of Physicians, Lecturer on Physiology in St. Thomas's Hospital, Physician to the Royal Free Hospital.

(*Continued from No. 37, p. 228.*)

IN alluding hereafter to the possible healing of cancers of the stomach, I may take the opportunity of doing more justice to what Rokitansky and Dittrich have advanced respecting it. As regards the simulation of an ordinary ulcer, the chief facts I have collected are one or two instances in which, concurrently with what has offered every appearance of a simple ulcer, with a but slightly thickened (or even healthy) margin of mucous membrane, there have been suspicious (if not absolutely cancerous) deposits in the liver and lungs, such as seemed to indicate secondary cancer. Unfortunately, I have had no opportunity of a minute examination of these ulcers and deposits, and am therefore obliged to leave the question in its present form;—a vague (though important) suspicion that may perhaps be useful in stimulating further inquiry.

As regards mere combinations of cancer and ulcer, little need be said. Since the gastric ulcer has no protective influence against cancer, we need scarcely wonder to find that its scars are often found in stomachs that have subsequently been attacked by the deadlier malady. The coincidence of the open ulcer with cancer is scarcely less frequent. But, as might be expected, in these cases also it is the cancer which is invariably added to the ulcer, and never *vice versâ*—the ulcer to the cancer. Indeed, the formation of a simple ulcer in some part of the stomach unaffected by the cancerous growth, is, so far as I am aware, a circumstance quite unknown in the history of gastric cancer. Nor is there a single authenticated instance, in my knowledge, in which an ulcerous stomach has been attacked with malignant disease in some other part of its cavity, without the ulcer itself becoming implicated to at least the extent of having its base or edges infiltrated with the new deposit. More frequently, indeed, it is these parts alone which become the site of the cancerous deposit. An ulcer, for example, which has lasted many years, suddenly ends in death;—the necropsy revealing considerable cancerous infiltration in the thickened margin of the ulcer, or in that chronic thickening of the walls of the stomach which often extends some distance beyond this margin; or a fungous mass of comparatively recent medullary cancer springing up from the centre of the excavation. In rare cases, the same marginal cancerous infiltration seems to have occurred after the original ulcer has perforated the stomach; and has thus affected the parietes of the chronic abscess to which the accident under favourable circumstances gives rise. In short, there appears to be scarcely any limit to the varieties of this kind which may occasionally occur. And, on the whole, the relative frequency of these combinations is quite sufficient to suggest that the presence of such an open ulcer does occasionally in some measure provoke the development of the cancerous cachexia; as well as (more frequently?) aid in determining the deposit of cancer in this particular organ.

It may be useful to compare with the gastric cancer, a rare disease which, though not known to be ever combined with it, occasionally simulates its appearances to at least a sufficient degree to be mistaken for it. And though such an error is practically of far less importance than that of confounding cancer and ulcer, still the pathological distinctness of cancer and "hypertrophy" seems to be just as complete, and the typical course of their symptoms just as diverse, as is the case with cancer and ulcer.

The chief characters which distinguish this lesion from scirrhus may be briefly enumerated as follows. In marked specimens, the change generally involves a considerable extent of the stomach (often the whole organ) in a moderate thickening which, while it allows the three coats to be still distinguished and separated from each other, frequently increases their bulk in a tolerably equal proportion. The uniform expanse of stomach involved in the change is yellow, tough, and elastic; instead of having the peculiar pearly-white and gristly appearance and section of cancer. The muscular tissue is almost always discernible—often even hypertrophied in the earlier stages of the disease. And not only does the perfectly homogeneous deposit offer none of those varieties which commonly mark the later progress of a cancerous deposit in the shape of colloid and medullary matter surrounding the original and central scirrhus, but its microscopic examination shows a complete absence of anything like the characteristic cells of cancer, or at most a few spindle-shaped cells of fibrous import scattered sparingly throughout a mass of laminated (but scarcely otherwise organized) exsudation. Of course, the absence of secondary cancer in other organs would aid the diagnosis of this benignant lesion, though its mere histology would generally be sufficiently conclusive. Ulceration, too, is less frequent and extensive than in cancer. Finally, the pathological contrast of the two diseases is rendered complete by cases which affirm that the lymph thus deposited by a kind of cirrhotic (?) inflammation around the vessels of the stomach, occasionally undergoes a development into cartilage—rarely even into bone.*

The presence of *secondary cancerous deposits* in other organs is a very frequent complication of cancer of the stomach. Out of 214 cases I have

* It would lead me too far from my present subject to pursue this topic further, or to analyse these cases. And without definitely urging the adoption of the term "cirrhosis," I will only add, that the above characters sufficiently show how little such a change is really akin to "hypertrophy."

collected, 96, equal to 45 per cent. (or nearly one half), exhibit this complication. To these we may probably add Dittrich's* cases as affording 160 cases with 82 of secondary deposit; as well as Lebert's, 32 in 57. In all, therefore, we obtain 437 instances of gastric cancer, with 210 of secondary cancer: a proportion equivalent to 48 per cent.

The comparative liability of the different varieties of cancer to be accompanied by secondary deposits, I deduce from 81 of the above 96 cases; in which 81, the primary cancer of the stomach is divided amongst the scirrhous, medullary, and colloid species, in the numbers of 51, 17, and 13 respectively. Comparing these numbers with the relative frequency of these three forms (as deduced in page 297), we obtain the liability of each to be associated with secondary cancer as the respective fractions $\frac{51}{130}$, $\frac{17}{32}$, $\frac{13}{17}$; or, ·39, ·53, and ·76. These numbers nearly correspond with the simpler figures of $\frac{3}{7}$, $\frac{4}{7}$, and $\frac{6}{7}$. In other words, nearly one-half the cases of gastric scirrhus are associated with a deposit of secondary cancer in some other organ of the body: and this proportion, which is increased by one-third in the case of medullary cancer, is doubled in that of colloid. Such facts well illustrate one at least of the causes of that greater or more direct fatality of the two latter forms which has long been ascribed to them.

Respecting the variety of cancer which forms the secondary deposit, I have no large and exact numerical data to offer. In a majority of instances, however, it is medullary. Sometimes we meet with secondary cancer of the liver or peritoneum, which affects the areolar arrangement of colloid: and a similar arrangement, which is sometimes met with in the glands of the belly, is evidently due to a differentiation akin to that of the original structures.

Among the organs which form the seats of these secondary deposits, the liver claims that precedence which might be expected. Out of 431 cases collected by me, 105 (or about 25 per cent., or $\frac{1}{4}$th) exhibited a cancerous deposit in the liver: a proportion at least twice as great as that of deposit in the lymphatic glands adjoining the stomach, and thrice as great as that of secondary pulmonary cancer.

In many of these cases, the presence of cancer in the liver coincided with its deposit in other organs. The only exact numbers I have been able to obtain in respect to this further complication, are derived from 47 instances of secondary hepatic cancer, occurring in 214 cases of cancer of the stomach. Out of these 47 instances, such a coincidence was present in 13: nine times in the glands or peritoneum adjacent to the organ, twice in the kidney, and once in each of the following organs—the ovary, pancreas, intestine, spleen, thoracic glands, and lung. The immediate proximity of the abdominal glands and peritoneum renders it scarcely important to lay any stress on their frequent share in the hepatic mischief. But of the remaining sites of deposit, it is interesting to notice that the lung is affected with only $\frac{1}{4}$th or $\frac{1}{5}$th of its average frequency; while the abdominal viscera are involved about $1\frac{1}{2}$ times as frequently as their similar average.† In other words, it seems as if the secondary

* Loc. cit. In his 160 cases, Dittrich mentions 43 as being combined with cancer of the liver, 22 of the diaphragm, 9 of the lungs, and 9 or 10 of the retro-peritoneal glands and other abdominal organs.

† Their averages are stated below.

deposit of cancer in the liver somewhat increases the risk of other abdominal viscera sharing in the deposit; while it much more decidedly diminishes the chance of any pulmonary ingrafting of the disease.

In the reports I have brought together, it is sometimes so difficult accurately to distinguish secondary deposits in the lymphatic glands that adjoin the stomach, from similar deposits in and between the layers of peritoneum that form the gastro-hepatic and greater omenta, that it has seemed best to group them together. Adopting this arrangement, and adding to them Lebert's 57 cases, we obtain 271 cases, in 69 of which some of these structures were thus involved. This number corresponds to a per-centage of about $25\frac{1}{2}$ per cent., or rather more than one-fourth.

The lungs seem to be affected about 1 in 12 times, or in $8\frac{1}{6}$th per cent. of the total number of cases of gastric cancer. This estimate is based on 35 and 431 such cases respectively.

But in adducing these numbers, it becomes imperative to consider some circumstances which materially detract from their value; the more so that even in doing so, they furnish no data for any specific correction of the deficiencies they indicate.

And firstly, these numbers omit all mention of a morbid change in the pulmonary tissue which, in the shape of pleurisy or pneumonia, or both these diseases in combination, occurs so frequently in connexion with cancer of the stomach, that it is impossible to doubt its general significance in relation to the primary disease. The lungs offer in some part of their mass—almost always in the lower lobes, more frequently (I think) in the left than the right organ—a kind of reddish-grey hepatization; which generally extends to the nearest pleural surface, and gives rise to more or less adhesion here. The adherent tissues are united by a small quantity of lymph, of very moderate tenacity, and pasty (rather than fibrinous) consistence. And the pleural cavity itself is often occupied by a variable amount of serum, much of which, however, is doubtless in some cases effused during the very act of dying. This pleuro-pneumonic complication seems to be no very unusual cause of death.

Another (and still more frequent) complication of gastric cancer, relates to the association of what is called "tubercle" with the primary disease —an association of such remarkable frequency, as to raise suspicions similar to those already mentioned in speaking of the pleuro-pneumonic complication.

It is the more important to institute some inquiry into the real foundation for such suspicions, inasmuch as they are intimately connected with the general pathology of cancer and tubercle themselves. And if such an inquiry serves only to point out any sources of error in the facts on which are built the existing doctrines relative to these two comprehensive forms of disease, it will probably be regarded as not altogether useless.

The incongruous ideas prevalent with respect to these two varieties of blood-krasis, may perhaps be well illustrated by the fact that, while Rokitansky has long ago laid down the proposition that they may almost be regarded as mutually exclusive of, or incompatible with, each other, so large a number of cancerous necropsies reveal tubercles in the lungs or other parts of the body, as almost to confirm, relatively to individuals, the propositions stated by Dr. Christison* relatively to families: namely—that "the malignant diseases belong to the scrofulous constitution;" and that "consumption in early life, and malignant disease at a later age, seem not infrequent in the same family." In short, it would seem that not only do the two krases attack similar constitutions and temperaments, but that in a vast number of instances they merge into each other in one and the same individual; and in a smaller but still not inconsiderable number, their respective products are present in such a quantity and state as conclusively to indicate a simultaneous—if not indeed a connected—activity of both these diseases.

Without, however, diverging from the subject of the present essay to criticise the view thus concisely put forward by Dr. Christison, I venture to believe that the more carefully the two diseases are analysed by the light derived from pathological anatomy, the more conclusively will Rokitansky's dictum be established. With much to suggest caution in the implicit reception of any wide theory of either krasis, the few conclusions respecting the mutual relation of cancer and tubercle to which I have been led by the study of cancer of the stomach, might almost be summed up in the very words of this eminent pathologist. At any rate, they are so perfectly compatible with his brief but masterly outlines of these diseases, that, while I feel it would be presumptuous in me definitely to link them with the authority of his name, I would suggest them for the consideration of those whose opportunities of pathological research may not have supplied them with all the requisite materials for a commentary on Rokitansky's text.

Firstly, as to the mere coincidence of the products of the two diseases in the same person. The comparative fatality of the two diseases, and the different epochs of life which they specially affect, are circumstances which, apart from all others, sufficiently explain why cancer follows tubercle, but tubercle does not follow cancer. And the frequency with which arrested or absolute deposits of tubercle are found scattered in sparing quantity throughout the lungs of persons dying of cancer, is a fact which in itself not only fails to establish any essential connexion between them, but does not even militate against Rokitansky's view. However different may be the numbers by which different observers represent the frequency of such deposits in necropsies of persons dead from all causes indifferently, there can be no doubt that their range (30 to 70 per cent.?) quite equals anything as yet established respecting the frequency of similar deposits in cancer. Hence all that we are really entitled to infer is, that the deposit of tubercle in the earlier epochs of life does not afford any valid protection against that of cancer at a subsequent period: a proposition which few would question, and to which Rokitansky nowhere either expresses or implies a denial. "But," it might be suggested, "granting that the tubercular krasis of early life does, in more or less frequent instances, diminish and disappear as age advances, can we claim to distinguish these instances with all that accuracy that would be necessary to justify us in accepting its exchange for a disease which is assumed to be incompatible with it? In other words, can we always predicate

* Suggestions to Medical Referees of Standard Life Assurance Company, pp. 11, 12. Edinburgh, 1852.

the exact age and condition of a deposit in respect of its progress or regress, much more the extinction of that krasis by which (in a rather indefinite and subjective way of speaking) the deposit is assumed to be produced?"

In attempting a reply to such a question, I shall not even allude to the various weighty arguments that might be adduced from the general pathology of the two diseases, but shall confine myself exclusively to the mere histology of their products, as illustrated by cancer of the stomach and its complications.

In the first place, I may briefly state that it is by no means impossible to confound some cases of secondary cancer of the lungs with ordinary tubercle in the earlier stages of its deposit, and at the commencement of its suppuration and softening. A large proportion of the secondary cancerous deposits that involve the lung, possess either a firm and cartilaginous texture akin to that of ordinary scirrhus, or a somewhat less dense (but still tolerably firm), white, solid appearance, that can scarcely be mistaken for either the miliary or caseous form of ordinary tubercle. But in less frequent cases, the cancerous deposit, even when equally discrete and scanty, undergoes a process of softening and suppuration which can sometimes be scarcely distinguished, in certain of the small spherical masses in which it is deposited, from the similar appearances presented by tubercle. Nor does even the microscope always afford a definite decision. As a rule, the medullary mass of which such secondary cancerous deposits are composed, may be easily shown to consist of scarcely anything but a vast number of cytoblasts, or minute cells, which occasionally distend the lobules of the lungs so as to afford a complete demonstration of their arrangement; while in tubercle we find, in addition to what are often characteristic differences in the cells themselves, a comparative preponderance of the unorganized or amorphous constituents, which is, on the whole, even more distinctive of the nature of the mass. But occasionally these grounds of distinction altogether fail us. In such instances the process of softening itself breaks down the structure of the cancerous deposit to a degree which sometimes leaves scarcely more of the malignant cell-growth that formerly distended the pulmonary lobules than might be readily mistaken for the epithelium so plentifully found in recent (even if caseous) tubercle. Indeed, the cancerous deposit of the lung sometimes so closely imitates the structure of the normal epithelium of the pulmonary lobules, that the individual cells, if seen apart from each other, would probably deceive even an experienced histologist. In short, I have no hesitation in stating that I have seen cases in which, for all practical purposes, it might fairly be said that the microscope, even while indicating strong suspicions of the truly cancerous nature of a deposit which at first sight closely resembled tubercle, could scarcely have decided the question but for the aid afforded it by the symptoms and history of the disease, and the appearance witnessed in other parts.*

* An instance which may well illustrate these considerations was contributed by the author to the seventh vol. of the Transactions of the Pathological Society. An old omental hernia had become the seat of a cancerous degeneration, of which the patient (a young woman) ultimately died, after symptoms of slight peritonitis and indistinct pulmonary mischief. The cancerous nature of the deposit in the lungs could scarcely be regarded as decided

It is impossible to doubt that not only were all these points thoroughly considered by Rokitansky before coming to the conclusion already referred to, but that that conclusion was especially intended to sum up and contain them. Indeed, he specifies† a variety of "croupy tubercle of the lung, which occurs during the inflammation and suppuration of cancer, as a result of the cancerous degeneration of the fibrin; and which is distinguished by its whitish colour, its soft glutinous consistence, and by its breaking up into a creamy whitish ichor." But, so far as I have seen, there is every reason to suppose that it is *not* as a croupy tubercle that this mass is originally deposited: that, on the contrary, it is neither more nor less than pulmonary deposit of secondary cancer, consisting originally of cell-growth. In favourable specimens, the various masses of this cell-growth may be seen offering all the various stages of this change in one and the same lung: the smaller nodules white, dense, or even semi-cartilaginous; the larger, it may be, softened in their centre, or even completely broken down, and already partially emptied through a neighbouring bronchus. And instances are not wanting which seem to indicate that—though it is easy to imagine that the free access of air to such pulmonary deposits constitutes a chief cause of the proneness to suppuration of this form of secondary cancer, as contrasted with other and similar deposits in less exposed organs—still the immediate and effective impulse to the process of softening is given by the perishing of the original lung-tissue, which is cut off from the sources of its nutrition by the adventitious mass of cells that surrounds and encloses it.

It is not for me to judge how much either of novelty or truth this modification of Rokitansky's views possesses. But I venture to believe that careful inquiry will in great measure confirm it. In any case, I think few unbiassed observers could study the pulmonary appearances of a series of cases of gastric cancers, without coming to the alternative—either that the two diseases really have that exclusive relation to each other which Rokitansky deduces, or—what is at least as startling a conclusion—that they have an intimate causal relation, such as has never hitherto been suspected. The frequency of their apparent coincidence demands an explanation of one kind or the other.

Want of space forbids me to pursue this digression any further; or to attempt (did any facts warrant me in doing so) to define the exact relation of this peculiar form of pulmonary cancer with the pleuro-pneumonia, or with the obsolete tubercle, before referred to. And in dismissing it, I need hardly say that the caution I have suggested with respect to the histological evidence sometimes furnished by the microscope in such cases, is not in any degree intended to depreciate the value of this indispensable means of research, but rather to guard against the errors into which a hasty and imperfect reasoning on that evidence, or an exclusive

by the mere microscopic examination. But on a comparison of this deposit with the primary disease and with some of the implicated lumbar glands, little doubt could be entertained. The severity of the general symptoms, which was quite disproportionate to the local mischief detected, had raised the same suspicion during life, and of course helped to confirm it after death. The only mass of pulmonary deposit which reached the size of a hazel-nut, had softened and suppurated in its centre; and would evidently have been emptied by expectoration, had the patient lived a little longer.

† Op. cit., vol. i. pp. 422, 424.

attention to it, would occasionally lead us. If ever the disease now regarded as a single one under the name of pulmonary tubercle, should be distinguished into several different maladies—a contingency which seems by no means improbable—it is difficult to avoid the suspicion that *clinical* research will be at least as instrumental in this result as any mere morbid anatomy.

The other organs which have been the seat of secondary deposit, scarcely deserve more than a mere enumeration. The intestine was thus affected in 7 out of 431 cases: twice in the small intestine, twice in the colon, and thrice in the rectum. The same number also affords six instances of deposit in the ovary; and three in the uterus, spleen, and pancreas. The kidney, the bladder, and the ribs, were affected in two instances. And finally, the following organs only appear to have been involved once in 431 cases: the vertebræ (fourth lumbar), the sternum, the humerus, the supra-renal capsule, the thoracic duct, the seminal vesicle, the diaphragm,* and the pericardium. Among rarer forms of secondary deposit, we may allude to the obstruction of the vena portæ by a soft cancerous mass; a condition present in three or four of the above cases. This generally coincides with the presence of a similar deposit in the liver, and, for obvious reasons, is almost always accompanied by much ascites.

The remaining pathological phenomena of gastric cancer may be next briefly adverted to, in the usual order of their occurrence.

The *obstruction* produced by a cancerous thickening of the gastric parietes, often gives rise to a variable degree of one or more of the following results:—hypertrophy of the muscular coat, dilatation of the cavity of the organ, or contraction.

The *hypertrophy* is a change which really deserves this title, inasmuch as it essentially consists of an increased growth of the muscular fibre-cells, and in favourable specimens can be seen to be entirely limited to such a change. The calibre of the stomach being constricted by the tumour, an increased effort is required for the propulsion of its contents; and this addition to its function necessitates (and indeed brings about) an unusual development of its structure. The thickened muscular coat retains, however, its normal texture; its areolar bundles certainly appear somewhat more distinct and larger than usual; but their size and strength, in proportion to the true contractile tissue, remain unchanged. The fibres themselves are perhaps rather redder and darker than normal; but even this alteration may fairly be referred in part to the enlargement which their bundles have undergone.

The above purer form of hypertrophy is precisely identical with the condition that may often be seen in the muscular fibre adjoining (and especially behind) the cicatrix of an ordinary gastric ulcer. Like the latter, too, it may extend for a variable distance through the organ. It is usually limited to the neighbourhood of the pyloric region, which the cancerous deposit especially affects.

Its deficiencies and complications may many of them be explained by the circumstances under which they occur. In the softer varieties of

* Apart from mere continuity of the adherent diseased mass, which occurs very frequently; (according to Dittrich, 22 times in 160 cases).

cancer, and in tumours of rapid growth, such hypertrophy is generally indistinct or even absent. In instances where the deposit is traceable by no very abrupt line of demarcation into the unaffected part of the stomach, it is often less marked; or is accompanied by such thickening of the areolar septa that divide the muscular bundles, as sometimes to render it very difficult for the naked eye to discern how much of the alteration evinced by these structures in bulk and colour, is due to mere hypertrophy, how much to their implication in the cancerous disease. On the whole, the most marked degree of this hypertrophy is scarcely ever found, save in conjunction with a hard deposit, of slow growth, and possessing a tolerably abrupt edge towards the thickened muscular coat.

Dilatation generally accompanies the above hypertrophy, and is rarely met with in its complete absence. Moderate degrees of either of these associated changes are of course difficult to verify. And this difficulty is increased by the circumstance (too often, perhaps, overlooked in post-mortem examinations) that these two appearances are mutually convertible. In other words, the mere slitting up of a moderately distended stomach will sometimes allow what appears to be marked dilatation, to merge into what is evidently just as marked hypertrophy, by a simply passive contraction of the muscular coat:—a fact which, however usefully it may illustrate some of the mechanical relations of these two states, ought to render us cautious not to exaggerate the real degree of either by calculating their total and joint amount as their sum instead of their difference.

It is probable that a very large proportion of cases of cancer of the stomach offer both these states in a moderate degree. But I think the proportion mentioned by Lebert (1 in 4) as evincing a "notable dilatation," must be in part explained by the variable estimates which anatomists adopt respecting the average size of the organ. At any rate I believe that such a proportion could only have been deduced from the cases I have collected by including many instances in which the stomach was scarcely at all larger than a full meal would have rendered it in perfect health. Of excessive dilatation beyond any such degree, the records of 214 cases afford me only 13 instances: a proportion equivalent to $6\frac{2}{3}$ per cent. In every one of them the pylorus was the seat of the tumour. And even with respect to these extreme instances of dilatation, I venture to think that, as a rule, it is very rare for cancer to bring about a degree of distension at all approaching to the maximum of this state, which is sometimes witnessed as the result of contraction of the cicatrix of a pyloric ulcer.

The *contraction* which is sometimes found in cancer of the stomach, is very rarely connected with any true hypertrophy of the muscular coat. It may be regarded as of two kinds: each depending on a different process; and each (we may add) finding its parallel in another disease of the stomach. In some cases it is the physical result of a specific pathological phenomenon: a slow shrinking or contraction of the scirrhous mass which occupies a large portion of the parietes of the stomach; constricting and diminishing its cavity in the same way as the contracting tissue of that chronic or cirrhotic inflammation which generally attacks the organ with even greater diffuseness. In other cases, in which the tumour occupies the cardiac orifice of the stomach, and is occasionally limited to it, the stomach contracts (just as it sometimes does when an ulcer encircles the same aperture) simply because the constant regurgitation which this occlusion produces, entirely prevents the cavity of the stomach from undergoing its normal distension by receiving any quantity of contents. Here much of the contraction is temporary, and may be readily removed by artificial dilatation of the stomach. In rare instances both these sources of contraction are combined.

As regards its frequency, extreme contraction is far less common than dilatation. The 214 cases mentioned, only include 3 instances of contraction; of which 2 seem chiefly referrible to the situation of the tumour at the cardiac aperture: and the remaining one appears to have been due to contraction of a scirrhous mass that engaged the greater part of the stomach.

The *ulceration* that generally engages the cancerous deposit has already been alluded to, both as regards the local changes by which it is introduced, and the variable admixture of suppuration and sloughing by which it is often accompanied. Its remaining peculiarities require little notice. That it is rarely or never arrested and repaired, the known features of cancer would be sufficient to inform us. That, as a rule, death intervenes before any very large extent of mucous membrane has been devastated by its extension, is equally explicable.

The *sequelæ* of cancerous ulceration evince a marked contast with those seen in the ulcer of the stomach. Out of 507 cases, there are 21 cases in which *perforation* had taken place, with its usual result of peritonitis, rapidly ending in death. In 4 of these 21 cases, however, the contents of the stomach were not effused into the general cavity of the belly, but into an intermediate cavity, corresponding to the sac of the omentum, and bounded by the adherent viscera that enclosed this sac. In 10 other cases the accident of perforation was shown to have been imminent by the necropsy; and had probably so far taken place, as to have allowed that leakage of the contents of the stomach to which the fatal suppurative peritonitis seemed due. As regards *fistulous communications*, the same number include one instance in which an abnormal opening of this kind led from the cancerous stomach to the anterior wall of the belly: one in which its cavity was thus thrown into communication with that of the jejunum; and no less than 11 in which the transverse colon was the seat of a similar aperture (twice by an intermediate cavity formed exclusively of cancerous deposit).

Each of these results is in striking contrast with its respective parallel in the ulcer of the stomach. In gastric cancer, the perforation would seem to be far less frequent, the per-centage being from 4 to 6 instead of 13 (or about 1 to 2 or 3). While conversely, the formation of a fistulous communication between the stomach and colon occurs far more frequently: how much more I should hardly like definitely to estimate, though I may conjecture that its proportion in the malignant disease is at least thrice (and probably six to ten times) as great as in the ulcer.

This converse disproportion is obviously in great part due to the peculiarities of the destructive process which occurs in the course of gastric cancer. Growth and decay, deposit and ulceration, are generally going on at one and the same time in different parts of the diseased mass. And thus, even at the very time that the sloughy or ulcerous surface by which the cancerous tumour abuts on the gastric cavity is hourly losing a certain proportion of its bulk, the opposite or peritoneal aspect of the tumour is rapidly throwing out a cell-growth that more or less replaces these ravages. Hence it is quite possible that, even after long ulceration, the thickness of cancerous deposit between the cavity of the stomach and that of the peritoneum may remain comparatively undiminished. No doubt this process may be regarded as to some extent paralleled by the deposit of lymph at the base and margin of a gastric ulcer; especially where (as is occasionally the case) the symptoms point to an uninterruptedly open state of the ulcer during a long period of time. But the analogy is a very remote one. For while it is chiefly the situation and amount of such lymph which determine the occurrence or non-occurrence of perforation in the gastric ulcer,—and such a quantity as is generally present in the ulcer of the posterior surface is (in the majority of cases) an efficient barrier to this incident during an almost indefinite period,—no quantity of cancerous deposit can have any such protective efficacy. The newly-interposed mass may indeed, for the time, intervene between the stomach and the abdominal cavity: but the protection temporarily afforded by its quantity is sure to be soon abolished by its quality. In other words, its cancerous nature shortly brings about an extension of the same softening or ulceration as that which already occupies the neighbouring mass, and the barrier gives way.

These circumstances are well illustrated by the fact that the situation of the cancerous mass exercises no influence on the accident of perforation at all comparable with that seen in ulcer of the stomach. Indeed, from the posterior and diaphragmatic aspect of the cancerous stomach being the earliest and most frequent seat of adhesion, it is precisely in this situation (the safest a gastric ulcer can occupy) that the cancerous perforation most frequently takes place.

In both cancer and ulcer we are bound to recollect that the occurrence of perforation, as a pathological event, is by no means synonymous with the characteristic and fatal group of symptoms we generally associate with this word. Just as in the latter disease the true perforation of the gastric coats is often accomplished months, or even years, before an extension or renewal of the ulcerative process penetrates the new tissue which has hitherto warded off the accident; so in the cancer, the portion of the stomach that corresponds to the diseased mass has often been destroyed, long before the destruction of the subsequent deposit brings about a communication between the gastric and abdominal cavities. But the degree in which any of the original structures of the part are left, it would often be impossible to define. The frequency of partial perforation—or rather of a leakage of the gastric contents through such a spongy mass—it is not easy to compare with that of the similar accident in ulcer. But its apparently greater frequency is readily explained by the above allusions. And lastly, in any strict comparison of the pathology of the two diseases with respect to this accident, the date and mode of death in cancer ought not to be overlooked. It is scarcely too much to conjecture that a much larger proportion of cancerous tumours would end by perforation, were it not that the collateral circumstances of the disease often destroy life before the local mischief has reached this stage of development.*

Any satisfactory hypothesis for the relative frequency of communication between the cancerous stomach and the colon, it is not easy to offer. The selection of this part is of course explained chiefly by its situation. But while the above characters of the process of cancerous deposit no doubt constitute the main cause of this particular variety of perforation, as well as of the accident in general, the equal frequency of this one variety with all the others put together, suggests some peculiarity, favouring either the deposit or the removal of cancerous substance; and connected with the colon, rather than with the various other structures that adjoin the stomach. Perhaps, however, the mere thinness of the intestinal coats, as contrasted with these structures, will account for their being more rapidly and frequently penetrated when adhesion has once taken place. At any rate we are scarcely at present entitled to assume any specific liability of the colon (in virtue of its structure or function) to an accident of which the more immediate conditions seem so evidently local.

The *hæmorrhage* which occurs in the course of gastric cancer affords in some of its varieties an equal contrast with that witnessed in gastric ulcer. As a rule, it only occurs after the access of ulceration; though prior to this event it may be produced by mere passive or active congestion,—a form of bleeding which, from obvious reasons, seems to be much more frequent in cancer than in ulcer. The exact frequency of moderate hæmorrhage can scarcely be estimated in either malady. But those larger bleedings which occur as a result of the lesion of a considerable artery, seem to be much rarer in cancer than in ulcer. Out of 374 cases, only 4 exhibit such a hæmorrhage: a proportion of barely more than 1 per cent., or one-fifth of that calculated for gastric ulcer. As might have been expected from the usual situation of the cancerous deposit, all of these appear to have been lesions of the superior pyloric artery (or *coronaria dextra ventriculi*).

As regards the obsolescence of the cancerous deposit in the stomach, as evidenced by the detection of cretaceous matter, the 214 cases I have collected only afford one instance of this kind,—a woman, aged thirty, affected with colloid cancer of the pylorus, the liver being also occupied by medullary deposit. A similar case described by Dittrich, suggests equal doubt how far this process really deserves the above name. While these cases may suffice to modify Rokitansky's opinion respecting the exclusively hard and fibrous character of the cancers amenable to this process, their rarity reduces them to a very exceptional (and practically unimportant ?) variety of the disease.

With just as cursory a notice I must dismiss the subject of the healing of cancer. In every instance which has hitherto come under my personal

* For example, if we may regard the establishment of an unnatural opening between the cancerous uterus and the bladder or rectum, as analogous to perforation of the peritoneal cavity by a cancerous stomach, it may be interesting to notice that there is good ground for estimating that the former accident is from two to four times as frequent as the latter, proportionally to the numbers of the two localizations of cancer.

notice, I have been able to assure myself that the cicatrices which suggested such an explanation were precisely similar to those of ordinary ulcers. Of course such a statement does not claim to invalidate the observation of others who have found scars covering a scanty scirrhous deposit. But is it not quite possible that some of these instances may have been deposits of scirrhus in the cicatrices of ordinary ulcers? Or, in the observations that assert such a healing process, have the fusiform cells and fibres of the dense fibrous tissue that forms such scars never been mistaken for those of scirrhus—from which it is hardly too much to say that they are sometimes scarcely distinguishable by the most sedulous examination? At any rate, are these cases authenticated (as they certainly ought to be in order to establish so striking a fact) by a careful comparison with the symptoms of the patient during life? Lastly, is it safe to accept statements which (like Lebert's) allude to the appearances in language so equivocal as the term "scars of cancerous ulcers" seems to be, when brought forward by a pathologist whose devoted industry in clinical research does not allow him to depict so common a disease as ulcer of the stomach from his own experience?*

The *ætiology* of cancer of the stomach has so little direct connexion with the symptoms which attend the malady during life, that one may take this opportunity of summing up those pathological details which, from their prominence and constancy, seem most to suggest a causative relation. The most obvious (and at any rate the most convenient) hypothesis of the disease we have been considering, would refer it to a causation which is probably itself the co-efficient of at least two elements—the disease, and its site; the cancer, and the stomach it invades.

In respect to the former of these two elements of causation, there do not seem to be any facts which entitle us to suppose that the disease (whether exclusively humoral or not) presents any specific modifications in the stomach. At any rate, the larger features of age, sex, &c., so far as our information extends, afford little countenance to such a supposition.

Assuming so much of the ordinary theories respecting the cancerous diathesis, as to infer that the cancerous deposit expresses and measures its intensity with tolerably equal accuracy in all the organs it generally affects, it is chiefly as to the selection of the stomach by this deposit, that we may examine into the facts brought together in the preceding pages.

We have successively seen, that the disease selects the stomach in a large proportion of cases; that in this organ itself, it further chooses out the cardiac and pyloric orifices, and especially the pyloric. Its situation, in the earliest stages in which we detect it, conclusively shows that it cannot be attributed to any mechanical or chemical effects of the ingesta; that it is not due to any change in the secretory apparatus of the stomach, or even to a lesion of any part of the mucous membrane, or of the tissues immediately subjacent. In short, that, for all practical purposes, we may sum up the histological site of the deposit as the loose sub-mucous areolar tissue, at some little distance from the active cell-

* Loc. cit., p. 526.

growth of the gastric surface; and generally, so much nearer (or more closely allied) to the similar connective tissue between the bundles of unstriped fibre, as to involve these long before reaching the mucous membrane.

The organ thus affected, is, with the exception of the rectum, the œsophagus, and the uterus, the thickest and strongest mass of unstriped muscle in the human body. Relatively to its function, indeed, it transcends the contractile structures of all three of these organs: because that function implies constant and protracted movement, instead of such an intermittent and brief contraction as that by which every one of them might readily be shown to impel their contents. How violent as well as protracted that movement is, may be easily conceived when we recollect that, for at least six of the twenty-four hours, the stomach is actively contracting upon (and propelling) its contents; by a movement which, during a great part of this period, almost obliterates the cavity of the pyloric half of the organ every two or three minutes.* Hence, quite apart from its mere bulk, we may fairly suppose that the muscular coat possesses an exalted nutrition—a rapidity of growth and decay—which proportionally exceeds that of either of the other masses of this tissue with which we have contrasted it.

Should future researches establish either the absolute commencement of the disease in the unstriped fibre-cells themselves, or (what seems more probable) an exactly analogous situation of its development in all four of these organs—stomach, uterus, œsophagus, and rectum—of course the above conjecture would acquire a somewhat firmer basis. In any case we have to recollect that the areolar tissue on and between a given mass of muscle, necessarily shares in (and often, from its very office, in a far higher degree) the mechanical displacement which that muscle executes: and therefore as necessarily becomes the seat of a nutrition exalted beyond that of the same tissue in less active parts.

It is therefore to the more energetic movement of the pyloric half of the stomach, and to the passive as well as active relations of the pyloric and cardiac valves at the extremities of the organ, that the frequent selection of these parts by the cancerous deposit may probably be directly or indirectly ascribed. Whether the peculiar structure of the organic muscle, as a cell-growth, invites the access of a disease the morphology of which is closely akin to its own, is a question which there are no means of deciding, and which little concerns those gastric peculiarities to which we are limiting our attention.

Such an hypothesis, however, as that we have advanced, ought never to be stated without an exposure of its chief deficiencies; a knowledge of which restrains any conjecture to the useful office of grouping facts, and at the same time prepares for its confirmation or rejection. While it is not impossible (for instance) that the conditionating cause (*causa causativa*) of the preference of this or that particular organ by cancerous disease, may be a different (or even compound) one in each, it is difficult to fit into such an hypothesis as the above the frequency of uterine and mammary cancer. In the former organ, we may perhaps indistinctly shadow

* Compare the author's Essay, "STOMACH," Cyclop. Anat.—Supplement, pp. 314, 315.

out something compatible in that remarkable activity of growth and decay which its muscular wall from time to time undergoes, connected as the disease is with a period of life when we might readily imagine a transference of nutritional activity from one part of the reproductive apparatus of the female to another. But in the latter, this vague formula of transferred activity is alone left us; and, if we except the doubtful influence of mechanical violence in producing cancer, it is difficult to trace any community of causation between the gastric and mammary lesion. The conjecture hazarded with respect to cancer of the stomach therefore remains but a doubtful one;—opposed though not absolutely contradicted, by some of the most valid evidence which can at present be adduced.

Art. III.

*The Relation of Cataract to Heart Disease.** By T. Furneaux Jordan, Demonstrator of Anatomy at Queen's College, Birmingham, and Medical Tutor to the same College.

The use of the stethoscope in modern medicine has revealed, in many diseases, abnormal thoracic conditions formerly unsuspected; which, in truth, constitute the gravest phase of the affections in which they occur. It is a matter of surprise that the stethoscope has not been more commonly used as a surgical instrument whereby might be deciphered the many anomalous phenomena whose appearance too often perplexes the surgical eye, and to whose treatment the surgical mind is so often inadequate.

Not unfrequently are the most brilliant operative proceedings attended with a signal and unexpected failure—a failure which might have been averted, and possibly life saved and credit preserved, by the prior application of an educated ear to the infra-clavicular or the præcordial region. An inflexible routine treatment in any disease is ever indicative of the empiric. From such degradation, medicine has to an eminent degree emancipated itself. The same engine of delivery is available to the surgeon. To the most thorough collation of general symptoms—say in a strumous joint-affection—let the surgeon add the auscultative condition of the thorax, then will his treatment be based on a philosophic induction from all the ascertainable data, and not on a merely arbitrary categorical distinction. The surgeon would not care, on the one hand, to amputate at the thigh in a rapidly-extending tubercular deposit in the lung; or to allow his patient, on the other hand, to die for the want of an amputation, because night-sweats, diarrhœa, hectic, emaciation, and debility led him to fancy a condition of the chest which the stethoscope only could tell him did not exist. Nay, in practice, finer distinctions than these must direct the conscientious surgical treatment. An actively-increasing deposit of tubercle in the lung, a softening of a deposit already occurred, or a limited inactive deposit with no tendency to degenerative changes, will each modify remedial action.

* In making the investigations embodied in the following article, every facility was kindly afforded me by Messrs. Cheshire, Solomon, and Townsend, the able surgeons of the Birmingham and Midland Eye Infirmary.

To turn from the surgical value of a humid râle to the surgical value of a *bruit de soufflet*, I know no better illustration of the latter than may be drawn from a class of cases not yet described in medical literature, by no means frequent, yet sufficiently so to lead both learned physicians and learned surgeons into occasional completely erroneous diagnoses. A man is brought into a hospital with a total inability of motion, an excruciating pain in the joints—aggravated by the slightest movement, a furred tongue, hot skin, extreme thirst, an accelerated pulse, nay, even a sour perspiration and a cardiac bruit; universally, the case is diagnosed as acute rheumatism; alkalies, colchicum, and opiates are given; the joints sweated; the diet lowered; and in a week we are called upon to open two or three large diffused abscesses from acute necrosis of the larger bones. Probably a closer examination would have told us that the bruit was aortic, and presumably of a functional or blood character; while, in acute rheumatism, the mitral valve rarely escapes some lesion—a point perhaps not sufficiently insisted upon by authors.

Another reason why it behoves the medical investigator to scrutinize carefully the condition of the thoracic organs, especially the heart, is, that recent research is gradually unfolding, to an unexpected extent, the principle that many diseases, more especially those of the inflammatory type, are merely partial expressions of some all-pervading constitutional state. On this ground it is common to call what seems an idiopathic inflammation of the pericardium or endocardium, rheumatic pericarditis or endocarditis, as the case may be. Other examples are found in the pseudo-inflammatory affections which occur in Bright's disease, fever, the exanthemata, &c. In these abnormal diathetic conditions, what more delicate index have we of the states of the fluids, or of the solids made out of the fluids, than the central engine of their propulsion?

That there should be an intimate connexion between cardiac and ophthalmic disease cannot, even *à priori*, be deemed improbable to any one prepared to admit the connexion between diseases of the heart and diseases of the brain—a connexion, the existence and importance of which are placed beyond doubt by the labours of Reid, Burrows, and Watson, whatever of contradiction may appear in their elaboration. Shall the central artery of the retina maintain its integrity amid the ravages of a disease which does not leave the divisions of the internal carotid itself competent to the performance of their duty? The purely mechanical protrusion of the eye attending a hypertrophic heart is a condition now commonly appreciated. But there are probably other and more delicate conditions of the visual organ, telling of cardiac states so palpably that they shall challenge the credence of the accomplished physician and the accomplished surgeon.

The foregoing remarks are intended to introduce the record of some cases illustrative of a hitherto unsuspected relation which exists between an important surgical affection of the eye—cataract—and disease of the heart; and this relation simply will occupy our attention at present. The operative department of cataract is extensive in remedial contrivance and rich in literature. But the causes, pathology, and morbid anatomy of cataract are yet wrapped in grave doubt.

The object of the following cases is to support the proposition—that non-traumatic cataract is frequently associated with, and in many instances may fairly be regarded as a result of, cardiac impairment. The nature, extent, and locality of such cardiac lesion will be more fully referred to after a statement of the cases which have led to its inference has been placed before the reader. The cases are not selected, but are all that came before me from one certain date to another. It is presumed that they furnish data for all the conclusions which it is the object of this paper to set forth. They are about twenty in number, and constitute but a third of the cases of cataract in which I have most carefully ascertained the thoracic conditions; and in no one of the whole number of cases could a perfectly healthy condition of the heart be confidently affirmed to exist. The cases given are at some length, in order that the conclusions drawn might receive confirmatory evidence from the general symptoms, and the general and clinical history of each individual case.

CASE I.—*Cataract—Mitral Regurgitation—History of Acute Rheumatism—Limited Tubercle.*—Emma S., aged forty-four. Married. Dark hair and irides. Slight venous hue of the lips. Has had five children, all of whom are living. Her father died apoplectic; the mother apparently from old age. She states herself to have had tolerably good health until her twentieth year, when an attack of acute rheumatism confined her to bed several months. From that time to the present has had eight or nine similar though less severe attacks. In one of them local treatment was directed to the heart. She has constantly been subject to pains in the left temple, and in the larger joints. About six years ago, the left eye began to grow dull from the formation of a cataract. Eight months ago, the right eye became affected in a similar manner. Has suffered from occasional palpitation of the heart and dyspnœa. The impulse of the heart is against the sixth rib. Præcordial dulness extends upwards to the upper border of the fourth rib, outwards to a vertical line half an inch external to the nipple, inwards to the centre of the sternum. A loud, blowing, slightly roughened systolic murmur is heard under the nipple, having its maximum intensity over the apex of the heart; while at the base of the same organ, and along the course of the aorta, it is inaudible. The pulse is small and irregular. The external jugular veins are more than usually distended. Thoracic expansion is slightly impaired under the right clavicle. Vocal fremitus is alike on both sides. Over the right infra-clavicular region there is a shade of comparative dulness, with, on applying the ear, increased vocal resonance, and one or two moist clicks at the end of the inspiratory act. Similar phenomena are audible in the supra-spinal fossa of the same side. Under the left clavicle, inspiration is rough and loud. Flesh has been gradually lost for two years, and for double that period slight cough has been present in the morning. Has never had hæmoptysis, diarrhœa, or night sweats. The appetite is good. The bowels are regular. The catamenial periods are prolonged. The urine natural.

CASE II. *Cataract—Aortic Obstruction—Fat, flabby, pale subject.*—Mary D., aged forty-six, a widow, flabby and pale, lips exsanguine. Her mother died dropsical at sixty-two. Does not remember the cause of her father's death. She married at twenty, and has had six children. Since her marriage she has suffered from debility, "bilious" attacks, and hæmorrhoids. Two years ago the left eye grew dim from the formation of cataract. In a few months subsequently the right eye followed a similar course. The patient's manner is singularly reserved. The special senses, with the exception alluded to, are unimpaired. The impulse of the heart is slightly increased, its site being behind the sixth rib. Cardiac dulness extends upwards to the lower border of the third rib, three inches beyond the nipple externally, to the centre of the sternum internally. A loud systolic bruit is heard at a point midway between the chondro-sternal articulations of the third and fourth ribs, being distinctly audible up the aorta, very much less so towards the apex. The pulse is slightly diminished in calibre. The respiratory phenomena are those of health. The catamenia ceased ten years ago; pain and flatus are complained of after food. The bowels are open. The urine natural.

CASE III. *Cataract—Mitral Regurgitation—History of Acute Rheumatism—Limited Tubercle.*—Samuel G., aged forty-five, a farm labourer, rather under ordinary stature, brown hair, grey irides. Father and mother living. He enjoyed good health until his twentieth year, when he was laid up sixteen weeks with acute rheumatism. Local treatment was directed to the præcordia. Two or three years subsequently he was said to have brain fever, immediately after which both eyes began to grow dim from the formation of cataract. For the last two years he has been subject to occasional pain in the infra-lateral region of the left side, accompanied with some dyspnœa and cough. The intelligence is average. With the exception of vision, the special senses are unimpaired. The heart's impulse is natural, and cardiac dulness is of normal extent. A well-marked systolic prolongation is heard below the nipple, which is inaudible up the aorta. The pulse is small and unequal. The chest is of normal conformation. Expansion is impaired under the left clavicle, where there is slight increase of vocal fremitus, and, on percussion, a slight degree of comparative dulness. In the same spot the inspiratory murmur is rough, and the expiratory is distinctly audible. Occasional friction-sound of the left infra-lateral region is heard. There is some perspiration at night, and flesh has been gradually lost during the last two years. The bowels are usually constipated, with occasional attacks of diarrhœa. The appetite is good, the urine natural.

CASE IV. *Cataract—Mitral Regurgitation—Fat, flabby subject—Inactive Tubercle.*—Elizabeth B., aged sixty-four, married, had one child. Her father died at eighty, apparently from old age. Her mother at sixty, from cancer of the breast. Has lost two sisters from phthisis. Has always enjoyed tolerably good health. The catamenia ceased in her fifty-second year. Six years ago the right eye began to grow dim, and very shortly afterwards the left also, from the development of cataract. The mammæ are so large that the precise limits of præcordial dulness are not ascertainable—it is certainly increased. A prolonged systole may be heard at the apex only. The chest is of average conformation. Thoracic expansion is not impaired on either side. Vocal fremitus is increased under the right clavicle, where percussion evolves an inconsiderable degree of comparative dulness. In the same spot the expiratory murmur is distinctly audible, as well as the inspiratory. The appetite is moderate; the bowels regular; the urine natural.

CASE V. *Cataract—Mitral Regurgitation—Rheumatism—Inactive Tubercle.*—Mary S., aged forty-six, appears fifty-six; married; emaciated. Her father died in his seventieth year from apoplexy. The mother died in middle age from cancer of the breast. When seventeen she suffered severely, and for a long period, from "green sickness." At twenty-three she married. Has had eight children. Five years ago, during her last pregnancy, she was seized with rheumatic fever. The child was born dead at the seventh month. Three years ago the right eye began to grow dim from the formation of cataract. Twelve months ago the left eye became affected in a similar manner. Intelligence and the special senses, except vision, are unimpaired. The impulse of the heart is slightly increased. Præcordial dulness is of normal extent. A loud systolic bruit is heard at the heart's apex, inaudible under the sternum. The pulse is small and irregular. No palpitation is complained of. There is an occasional sense of constriction across the lower part of the chest on a level with the ensiform cartilage. Thoracic expansion is impaired on both sides. Vocal fremitus is increased under the right clavicle, where there is a slight degree of comparative dulness. In the same region vocal resonance is augmented, and the inspiratory murmur is blowing and wavy. In the supra-spinal fossa of the same side no sound whatever can be heard. Under the left clavicle inspiration is slightly puerile. She never had hæmoptysis. There are no night sweats or cough. The bowels are alternately constipated and relaxed. The appetite is not good. The tongue is small and pale. The urine is natural; the catamenia regular.

CASE VI. *Cataract—Prolonged Systole at Apex—Fat, flabby subject.*—Elizabeth H., aged forty-nine, married; had six children and two miscarriages; is fat and flabby, but states herself not to be so stout as formerly. Her father died, seemingly, from old age; the mother died at forty-nine, probably from cancer. The patient has enjoyed only indifferent health. Seven years ago the left eye lost its visual power from cataractous opacity. The right eye soon followed a similar course. During the development of the cataract she was subject to frequent attacks of vertigo, dyspnœa, weight in the epigastrium, and sense of faintness. In these attacks she took stimulants with relief. Five years ago the catamenia ceased, after having always been scanty. Intelligence is weak, with great loquacity. From the fat and flabby condition of the mammæ, it is difficult to ascertain the precise limits of præcordial dulness; it is undoubtedly increased. A slight prolongation of the systole is heard under the nipple, but which is inaudible up the aorta. The sounds of the heart are feeble, and limited to the cardiac region. Thoracic expansion is slightly impaired on both sides. There is a slight shade of dulness under the right clavicle, where also vocal fremitus and bronchial voice are more clearly pronounced than on the other side. The inspiratory murmur also is feeble, while under the left clavicle it is obviously exaggerated. At present there is no cough; two months ago, however, she spat up a little blood. The appetite is good; the bowels regular; and the urine natural.

CASE VII. *Cataract—Mitral Regurgitation—Slight Aortic Incompetency—History of Acute Rheumatism.*—Elizabeth H., aged seventy; single; emaciated. Mother died at forty-two, dropsical; the father and two of her sisters also died dropsical. At ten she had "spotted fever;" and at thirty she had what was called "typhus" fever. At fifty she was laid up seventeen weeks with rheumatic fever. Since then she has suffered slightly from some dyspeptic symptoms—nausea and pyrosis. She never had palpitation, dyspnœa, or cough. Three years ago she spat up on one or two occasions a small quantity of blood. At thirty, the catamenia were absent for fourteen months; at forty-four they ceased. Four years ago the right eye grew opaque, as, twelve months subsequently, did the left. Præcordial dulness extends upwards to the upper border of the fourth rib, inwards to the centre of the sternum, outwards an inch beyond the nipple. A marked systolic prolongation is heard, having its maximum loudness two inches below and a little to the inner side of the nipple. It is not heard up the aorta, but in the latter region the second sound is dull and slightly prolonged. The heart intermits every fifth or sixth beat. The pulse is small, but at the same time rather jerking. The respiratory phenomena are normal. The appetite is good. The bowels are confined.

CASE VIII. *Cataract—Aortic Constriction and Patency—Hypertrophy of the Left Ventricle*—Elizabeth K., aged sixty-four, married. Had ten children. Slightly emaciated. Her father died at fifty, consumptive. The mother, dropsical, at seventy-two. States that she was married at twenty-one, and enjoyed tolerable health until the birth of her youngest child, twenty-one years ago, since which time she has been subject to attacks of vomiting, with great pain in the left scapular region, and beneath the left nipple. Eight years ago the left, and three years ago the right, eye gradually became opaque. A slight degree of *frémissement cataire* is appreciable over the præcordia; percussive dulness extends upwards to a level with the nipple. At the upper margin of the dulness the thrill is best marked. A prolonged systole is heard over the aortic valves, where the diastole also is muffled and slightly prolonged. The heart's action is irregular, and occasionally intermittent. Was formerly, she states, subject to vertigo has now occasional dyspnœa. The respiratory phenomena are normal. No cough, diarrhœa, or night sweats. The appetite is bad; the bowels are usually constipated. The catamenia ceased on the fiftieth year. The urine is natural.

CASE IX. *Cataract—Mitral and Aortic Regurgitation—Rheumatism.*—Richard R., aged seventy-five, farm-labourer; married; tall, fat, and flabby. His mother died in childbed; the father reached old age. Has lived regularly and enjoyed good health until twenty years ago, when he began to be troubled with the subacute form of rheumatism, the head, feet, hands, and fingers being affected. Nine years ago, it would appear he had œdematous extremities for seven months. It was about that time that cataract began to develop itself in the left, and two years subsequently in the right, eye. Formerly he was much subject to cough and palpitation of the heart. The impulse of the heart is not markedly increased; præcordial dulness reaches the upper margin of the fourth rib, and is increased laterally to a corresponding degree. Two inches below, and a little behind the nipple, a loud systolic bruit is heard, which masks both sounds of the heart. This bruit is inaudible up the aorta, but in the latter region a harsh diastolic one may be heard. The heart's action is unequal, irregular, and intermittent. The inequality of the pulse is exceedingly marked—two or three feeble beats being followed by a large one. The respiratory phenomena are not unhealthy. The appetite is moderate; the bowels regular; the urine clear on the application of heat and nitric acid. Never had nocturnal micturition.

CASE X. *Cataract—Mitral Regurgitation—Aortic Constriction—History of Acute Rheumatism.*—Charles B., aged sixty-one, a widower; a boat-loader; above the average height; rather stout; grey irides. Both parents are dead, but does not know from what cause. He enjoyed tolerably good health until twenty years of age, when he was laid up fifteen weeks with acute rheumatism. From that period to his fortieth year, he had occasional smart attacks of a rheumatic character; since the latter period they have given him no trouble, but he has had occasional attacks of dyspnœa and palpitation of the heart. Eight years ago, a cataract began to form in the left eye; and twelve months subsequently, the right eye became affected in a similar manner. Cardiac dulness reaches upwards and outwards to the nipple, and inwards to the centre of the sternum. A systolic prolongation is heard most loudly at the apex; less so up the aorta. The pulse is small, irregular, and intermittent. The respiratory phenomena are healthy, save that under the left clavicle the vesicular murmur is partially augmented. Has slight cough; never had hæmoptysis. No nocturnal perspirations. Has not lost flesh. The appetite is moderate; the bowels regular; the urine natural.

CASE XI. *Cataract—Mitral Constriction—Aortic Dilatation—Rheumatism.*—Samuel L., aged seventy, a forgeman, married. His father died "asthmatic" at sixty-seven; the mother in her seventieth year, from cancer. Is of average height, very fat, and flabby. Has always enjoyed good health, with the exception of rheumatic pains. In his fiftieth year, he was confined ten weeks with a severe sciatica. Two years ago the right eye began to grow dim, and soon afterwards the left, from the formation of cataract. Cardiac dulness extends upwards two inches above the nipple, outwards one inch beyond the same point, and inwards to the centre of the sternum. The heart-sounds seem reversed, the diastolic being the longer, especially at the apex; less vividly so up the aorta. The pulse is small, rather sharp, and occasionally intermittent. The respiratory, digestive, and urinary phenomena are normal.

CASE XII. *Cataract—Mitral Regurgitation—Rheumatism—Tubercle—Lead Poisoning.*—John M., aged forty-four, a japanner; married; dark hair and irides; slightly emaciated. Father living. The mother died in her sixty-fourth year, passing gall-stones. He states that his habits have been regular. In his twenty-first year he had an affection of the heart, accompanied with great pain. Local treatment was directed to the præcordia. Since that time he has been subject to occasional pains of the heart, attended with more or less palpitation and dyspnœa. Nine years ago the right eye grew dim from cataractous opacity. During the last twelve months the left eye has followed in a similar path. The cardiac region is distinctly prominent over the cartilages of the fourth, fifth, and sixth ribs. Cardiac dulness extends upwards and outwards to the nipple. Over the apex, which is two inches below, and a little external to, the nipple, a systolic bruit is audible, which cannot be heard up the aorta. Thoracic expansion is slightly impaired under the right clavicle. In the same spot, there is a shade of comparative dulness, with augmented vocal fremitus; while the stethoscope reveals in the same region increased vocal resonance, and a semi-tubular inspiratory murmur. The respiratory phenomena are normal, save some exaggeration under the left clavicle. There is occasional pain between the shoulders. Never had hæmoptysis; no nocturnal perspirations; no diarrhœa. Has lost flesh during the last six months. The appetite is moderate. There is slight blue line on the gums. Lead is used in his occupation. There is no colic. The bowels are not constipated.

CASE XIII. *Cataract—Mitral Regurgitation—History of Acute Rheumatism—Limited Tubercles.*—Henry F., aged sixty-nine, a widower; works in iron; fat and flabby. Does not remember his mother's death. His father died apparently from old age. Has lost three brothers and five sisters from chest affections. States that he had good health until seventeen years ago, when he had rheumatic fever; since then he has suffered from occasional attacks of dyspnœa and palpitation of the heart. Two years ago the left, and six months ago the right eye, began to grow dim from the formation of cataract. The heart's impulse is not markedly increased. Cardiac dulness extends one inch and a half above the nipple. A marked systolic bruit is heard over the apex, which is inaudible at the base of the heart. The pulse is very small, and occasionally intermittent. Thoracic expansion is diminished under the right clavicle, where vocal fremitus is increased, and where percussion gives a shade of comparative dulness. The stethoscope reveals in the same region increased vocal resonance, and a semi-tubular, occasionally wavy, respiratory murmur. The respiratory phenomena of the left lung are healthy; the appetite good; the bowels are occasionally relaxed; the tongue is pale; the urine natural.

CASE XIV. *Cataract—Aortic Constriction and Patency—Limited Tubercle—Rheumatism.*—Jonathan K., aged sixty; married; a farm-labourer; average height; stout; lips exsanguine. His father died in his sixtieth year, probably from psoas abscess; the mother died at seventy, apparently from old age. In his seventeenth year he had the ague. Fourteen months ago he had severe pain in the frontal region, apparently of a rheumatic character. Twelve months ago the right, and subsequently the left eye, became dim from the formation of cataract. Præcordial dulness reaches upwards and outwards to the nipple, and inwards to the centre of the sternum. The heart's impulse is slightly increased. Over the aortic valves, and up the aorta, a prolongation of both sounds is heard, which cannot be heard at the apex. Thoracic expansion is diminished under the right clavicle; in the same spot vocal fremitus is augmented, while percussion yields a comparative degree of dulness. The inspiratory murmur is exceedingly feeble, and expiration is attended with a short blowing sound. The respiratory phenomena are exaggerated on the left side. Never had hæmoptysis; no night sweats or diarrhœa. The appetite is good; the bowels regular; the urine natural.

CASE XV. *Cataract—Mitral Regurgitation—History of Heart Disease.*—George T., aged forty-four; married; a plasterer; very stout; arcus senilis present. The left eye was lost twenty years ago from injury. Both parents died from old age, the father being seventy-five, the mother eighty. Was very intemperate in his youth. In his twenty-fourth year was under medical care for some cardiac affection, for which the præcordia were leeched and blistered. Has constantly been subject to attacks of vertigo, tinnitus aurium, and dyspnœa; otherwise has had tolerable health. Has no cough. Never had hæmoptysis. Cardiac impulse is diffused. Dulness extends one inch and a half above the nipple, to the nipple externally, and to the middle of the sternum internally. At the apex, a prolongation of the systole is heard, which is inaudible up the aorta. The respiratory, digestive, and urinary phenomena are healthy.

CASE XVI. *Cataract—Mitral Regurgitation—Hypertrophy of Left Ventricle.*—Henry B., aged fourteen; a smith; tall and stout; dark hair and irides. Father and mother living. Has always had good health, save that eighteen months ago he had some affection of the left eye, which in a month lost all useful sight. Never had any injury to the eye. The cataract is of a bluish-white appearance. The iris acts well. The lower half of the sternum is much depressed, while the præcordia is unusually prominent over the cartilages of the fourth, fifth, and sixth ribs. The impulse of the heart is stronger than usual. At the apex of the heart there is a prolongation of the systole, not heard up the aorta. Cardiac dulness extends upwards and outwards to the nipple, and inwards to the centre of the sternum. The respiratory, digestive, and urinary phenomena present no marked deviation from health.

CASE XVII. *Cataract—Mitral Regurgitation.*—George S., aged nine; a pale, thin lad; light hair; blue irides. Four years ago, the lens of the left eye gradually became opaque, without any obvious cause. Cardiac dulness is slightly more extensive than usual. A roughened prolongation of the systole is audible over the apex of the heart, inaudible at the base. Never had rheumatism. Is said to have had several attacks of inflammation of the brain. The cerebro-spinal functions are unimpaired. The respiratory, digestive, and urinary phenomena are those of health.

CASE XVIII. *Cataract—Mitral Regurgitation—Hypertrophy of the Left Ventricle.*—John G., aged seventy, furnace-worker; married; neither stout nor emaciated. His father died at eighty-three, seemingly from old age; mother at sixty-three, from apoplexy. A sister also died from an apoplectic seizure. Three or four years ago, had a severe attack of rheumatism, of a doubtful acute character, which invalided him between two and three months. With the exception alluded to, his health has always been tolerably good. Two years ago, the left eye gradually lost the power of vision; as subsequently did the right, from the development of cataract. The intellect is not of average power. The cardiac impulse is slightly increased; its situation is two inches below, and a little internal to the nipple. Percussive dulness reaches upwards to a level with the nipple, and outwards to a line vertical from that point. The stethoscope reveals a distinct systolic bruit at the apex. Along the aorta, both sounds are natural. The pulse is small. The respiratory phenomena are healthy.

CASE XIX. *Cataract—Mitral Regurgitation—Aortic Constriction—Chest Injury.*—William A., aged seventy-one; a labourer; married. Father died at seventy-three, "worn out;" mother suddenly at sixty-five. In his twentieth year, a fall of rubbish so injured his chest, that his life was despaired of. Treatment was directed to the heart. The impulse of that organ is slightly more marked than natural. Cardiac dulness is of normal extent. A loud systolic bruit is heard at the apex; it is also heard, though less distinctly, at the base of the heart, and up the aorta. Three years ago, the left eye began to grow dim, from the formation of cataract. The right visual organ quickly followed in the same path. The respiratory phenomena are healthy. The appetite is good; the bowels are regular; the urine natural.

After a fair consideration of the above cases, there can be no impropriety in making the affirmation with which they were introduced—that heart disease is in numerous instances found in conjunction with non-traumatic cataract, and that consideration of the history of the cases where it is found warrants us to look at it in the light of a cause. It cannot be presumed that the heart disease is a product of the same cause which induced the cataract, because then some other and prior cause of both would need to be eliminated. Such cause it would be difficult to demonstrate. No cause of cataract is known, unless heart disease be admitted to act as such. That admission being made, the causes of heart disease are numerous and undoubted.

The questions which now most naturally arise are these:—What extent of heart disease shall favour the development of a cataractous opacity? Is there any particular lesion of the heart which, more than another, predisposes to the affection in question? The cardiac disease exists only in a slight degree—a degree, however, unmistakeably appreciable, whether we consider general symptoms or physical signs. The cardiac impairment is indeed so limited, that old age in cataractous patients is a familiar phenomenon to the ophthalmic surgeon. Nor does the true explanation of this circumstance rest on the inference that old age is itself a cause of cataract. A large number of cataractous patients are not old. In one-third of the above cases, the age is between forty and fifty, while two of the cases are under twenty years.

It being presumed that cataract is a gradual degenerative change in the crystalline lens from a partially impaired heart, it is natural to infer that the causes of so limited lesions would accumulate in old people—in other words, that younger people would be cut off by more extensive lesions, either of the heart or other organs.

Sudden death is not unknown in cataract—occasionally as the mortifying result of an operation; but so exceptional is it, that where so extensively a diseased heart is found as to render either death probable or life uncomfortable, cataract is one of the results least to be expected. Hence one common cause of cardiac mischief—Bright's disease—is unknown in cataractous cases, clearly because Bright's disease leads to other and graver results. Rheumatism, which may leave only slight impairment of the central organ of circulation, we have already seen to be a frequent incident in the history of cataractous cases—possibly in the same category with rheumatism, future research may include influenza, scarlatina, small-pox, and the various fevers.

In reply to the second question—"Whether any particular lesion of the heart more than another predisposes to the affection under consideration?" a negative would seem the more correct reply. Cataract obeys the general law which regulates for the most part all the secondary results of heart disease—namely, that the result is determined rather by the amount than by the precise locality or nature of the abnormal condition. Of course the infinite rarity of disease of the right side of the heart is understood. In the above cases, slight mitral regurgitation is the cardiac infirmity found in the greater number of cases. In some of the cases, the mitral and aortic orifices were both partially implicated; in one or two, the aortic only. In several of the cases, a fatty condition of the heart might be reasonably predicated. It will be seen that an extended præcordial dulness without a proportionate increase of the heart's impulse, was a not unfrequent phenomenon. Hereditary heart disease was found in more than half the cases where the hereditary tendency could be discovered. In case No. 19, there had been a chest injury, evidently implicating the heart.

It is an undoubted disadvantage that the foregoing conclusions have not, from entire absence of opportunity, received the confirmation of post-mortem dissection. But such additional proof, while desirable, is not absolutely essential. A morbid sound is assuredly an appreciable phenomenon, and cannot exist without a cause.

Let us turn now for a moment to the collateral evidence confirmatory of the inferences above drawn. It has already been stated that the results of considerable cardiac lesion are not present, and cannot be expected to be present, in cataractous cases. The less grave symptoms, however, are frequently obvious, as vertigo, tendency to faintness, dyspnœa, palpitation. Those, too, who have mixed much with cataractous patients, must have observed frequently the peculiarity of their mental states—states not rarely found associated with heart disease. Extreme loquacity on the one hand, and obstinate taciturnity on the other, are psychological indices by no means rare. Nor are these results mere accidental sequences of blindness—they are not found in the blindness occasioned by injuries.

Probably much light may yet be thrown on the pathology of cataract by future microscopic examination of the opaque lens. In one opportunity I have had of examining a non-traumatic cataractous lens, the microscope revealed fat globules in the nuclei of the delicate cells covering the surface of the crystalline lens, and here and there a few delicate plates of cholesterine might be detected. May not cataract be the result of a process identical with or analogous to that of fatty degeneration? That fatty degeneration of a portion of the lens may exist, is proved by the researches of Drs. von Ammon and Schön, as quoted by Dr. Mackenzie. The former found, in cases of arcus senilis, a fatty arcus on the corresponding margin of the lens. Dr. Schön has found both the lens and posterior capsule affected with fatty degeneration.

Authors, when speaking of the causes of cataract, have been universally cautious. One only that they have advanced needs any consideration, which is, the influence of occupation in those who are exposed to the glare and heat of furnaces. A sufficient refutation of this opinion is found in the statements of the most reputed authors themselves. Mr. Middlemore, whose extensive ophthalmic practice lies in Birmingham—the very hotbed of furnaces—says, speaking of such occupations, "They are much more likely to produce glaucoma or amaurosis, a varicose enlargement of the vessels of the eye generally, or some form of chronic inflammation of the deep-seated textures." Dr. Mackenzie, too, throws equal doubt on the same class of causes. If, indeed, cataract could be demonstrated to be more frequent in those whose occupations are in the vicinity of furnaces, would not the rational explanation of so increased frequency be, that the arduous occupation, the lifting heavy weights, and the extreme heat, would affect the circulation and its central organ, rather than the well-protected crystalline lens?

PART FOURTH.

Chronicle of Medical Science.

HALF-YEARLY REPORT ON FORENSIC MEDICINE, TOXICOLOGY, AND HYGIENE.

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I. Toxicology.

Poisoning by Stramonium.—At one time stramonium had a great reputation in medicine as an anodyne, and the fumes arising from it when burning are in country districts commonly inhaled by persons suffering from asthma. We have now to record a case in which the extreme effects of the stramonium seeds, when taken by the stomach, were exhibited. Dr. G. T. Elliot, physician to the Bellevue Hospital, and to the Nursery, New York, was called, on October 11th, 1856, to the Nursery, to see a robust little boy, between four and five years old, who, about an hour and a half before, had eaten some stramonium seeds, with the plant of which he had been playing. The matron had first observed the effects; she saw that his face was very flushed, and on noticing that there was no perspiration, she watched him, and soon found him staggering and behaving as though intoxicated. Just before the arrival of Dr. Elliot, he had thrown up some undigested food and about thirty seeds; his skin was very red, very hot, and moist; expression of countenance wild and staring; pupil nearly fully dilated, and utterly insensible to light; a lighted candle could be so held as almost to singe the lids, without inducing contraction of the iris, and without attracting the patient's notice; the child was so wild and restless as to be controlled with difficulty, and in raging delirium, biting with fury at those who restrained him. He was unable to stand, and yet plunged in a restless manner in whatsoever position he could be placed, all the movements being ill co-ordinated, and resembling those of chorea. The pulse was too rapid to count. The respiration greatly hurried, and at times gasping and choky. He was constantly talking, and yet was unable to articulate a syllable; while, from the expression of his face and movements, he seemed at times to be chasing or fleeing from imaginary objects. A teaspoonful of mustard was ordered, dissolved in water; it caused him to gasp in a somewhat alarming way, but was promptly ejected together with more seeds. In a short time a half teaspoonful of alum dissolved in warm water was administered, and after that had operated, warm water was freely given. He vomited three times, a few seeds appearing in the two first, and none in the third vomit. After an hour the patient could slightly articulate, and called the matron a "nasty thing" distinctly. He then slept two hours, with much jactitation and restlessness, and had one large movement of the bowels unconsciously; when he awoke his skin was cool, and the pulse 150; pupil the same; no apparent thirst; restlessness, intolerance of control, and chorea-like movements; an hour later had two more evacuations from the bowels, one quite copious, a few seeds in the second. Two hours later he had improved, pupil answered slightly to the light, and he was conscious of the proximity of the light; has had two more passages, with a few seeds; can articulate pretty distinctly; can stand, and has taken three steps; chorea-like movements continue, and patient, when not roused, lies in a state of "mild delirium;" sings, talks, and fancies there are dogs in the room, which he describes and attempts to chase, springing suddenly to his feet, and as suddenly toppling over; sometimes talks of events of the day as though they were now taking place. An hour later is better, bowels have acted twice, once freely, with passage of more seeds; is thirsty, movements co-ordinate, but continued restlessness. From this time, three A.M., the patient slept until five, with about the same amount of jactitation, and on awakening had a passage, containing about twenty seeds; at eight another, containing more seeds, and then a quiet sleep. Is now perfectly conscious, voice and articulation natural, face still markedly red, pupil still slightly dilated, pulse 120. The pupil did not resume its natural state until the 14th, but recovery was perfect.

Dr. Elliot thinks that poisoning from stramonium cannot but be more frequent than is generally supposed.—*New York Journal of Medicine*, Nov., 1856.

[This case is unique in forensic readings. The symptoms, markedly different from those of opium, indicate that the treatment in another case of a similar kind should be emetics and a brisk purge.]

Transformation of Amygdaline into Hydrocyanic Acid within the Body.—Professor Kölliker, and Dr. Müller of Würzburg, have arrived at the following results from a series of experiments:—1. Amygdaline and emulsine introduced separately into the circulatory system by different channels, form prussic acid in the blood. 2. When the quantity of these substances is sufficiently large, death soon occurs; it takes place more slowly with weaker doses. When amygdaline is first injected into the blood, and emulsine an hour afterwards, death speedily occurs. 3. When emulsine is first injected, and amygdaline forty-five minutes afterwards, death is retarded. Is the emulsine changed in the blood, or rapidly excreted? 4. Poisoning is not produced by injecting amygdaline into the blood, and emulsine into the alimentary canal. The emulsine, therefore, does not pass from the blood into the digestive canal, at least not without some change; on the other hand, it is not found in the intestines on post-mortem examination. 5. When emulsine is injected into the blood, and amygdaline into the intestines, poisoning occurs, though slowly. Death has been produced by introducing amygdaline into the digestive canal of rabbits, without any emulsine. The intestines of these animals contain a ferment, capable of converting amygdaline into prussic acid. 6. Amygdaline injected into the veins, or into the intestines, passes off in large quantities, sometimes rapidly by the urine; some experimenters, as Wöhler and Frerichs, have not found amygdaline with certainty in this excretion; others, as Ranke, suppose it to be converted into formic acid.—*Allgem. Medicin. Central. Zeitung*, 1856. p. 72.

Poisoning by Cyanide of Potassium.—At a meeting of the Boston Society for Medical Improvement, on September 22nd, 1856, Dr. C. E. Ware related the following case.

A woman, who at the time was under medical treatment, took by mistake a teaspoonful of a solution of cyanide of potassium, this quantity containing about seven grains of the salt. Immediately after taking it she complained of severe burning in the stomach, and a feeling as if the bowels were about to act. She went to the water-closet, and almost immediately began to sink. She was removed to bed, and very speedily became unconscious. It was impossible to introduce anything into the stomach. She died in less than an hour. There was *no* convulsion before death, but a sudden convulsive action of the body took place about ten minutes after the heart ceased to beat. There was no post-mortem. The colour of the body was so natural, even on the day following death, that Dr. Ware was sent for, the friends surmising that there might still be life.—*Boston Medical and Surgical Journal*, Dec. 11, 1856.

Poisoning by Cider containing Salts of Lead.—On February 2nd of the present year, a boy, named Vialet, was admitted into the Children's Hospital in Paris. The patient, who was of a strong constitution, had been ill during a fortnight, but had undergone no medical treatment. His symptoms commenced with loss of appetite, a disagreeable taste in the mouth, constipation, and violent pain in the epigastric region. For two days the pains had continued over the whole abdomen, and had become very intense. At the time of visit the patient was in a state of extreme agitation; he had not had an evacuation for a week, but the abdomen was much retracted. There was a yellowish coating at the roots of the teeth, and a slate-coloured border round several. There was complete loss of appetite. The patient had been unable to sleep for the previous two nights, on account of the pain. There was no vomiting, no pain in the joints, no paralysis. The symptoms of lead poisoning being apparent, and the occupation of the boy—a carpenter's apprentice—giving no clue to their origin, M. Bonfils, who reports the case, visited the boy's family, and learned that the father had a distinct attack of lead colic; and that the mother, who was six months advanced in pregnancy, was in the same state. She had the characteristic border on the gums, with constipation and colic, sufficiently intense to cause fear of abortion. Of five children, one was the patient at the hospital; the eldest daughter, aged fourteen and a half years, had had several attacks of colic and constipation. A boy, aged four years, was beginning to complain of the same symptoms. Two girls, aged two and nine years respectively, presented no symptoms. On inquiry, it was found that in making cider, of which all of the family who suffered drank (the two girls who escaped partook of water only), they had been accustomed to pass the fluid through a leaden filter. Salts of lead, probably acetates and malates, were formed and dissolved in the cider. On analysis, the presence of lead in the cider was distinctly shown. The proportion was calculated to be one part in four thousand. The urine of the boy in the hospital was found to contain lead. After treatment by castor-oil and sulphur baths, the patient recovered in a week.—*L'Union Médicale*, Feb. 17, 1857.

Poisoning by Wine of Colchicum.—Dr. Rennard, of New York, relates the following case:—Mary Mullan, an Irishwoman, aged fifty-six, came under his care at the Alms' House, on September 23rd, 1856, suffering from chronic rheumatism. An ounce of the wine of colchicum seeds was ordered, with directions to take twenty drops three times daily. Twenty-four hours afterwards, Dr. Rennard was called to see her, and found her in a state of extreme prostration, suffering from excessive nausea and vomiting, with slight purging, heat and burning in the fauces, inordinate thirst, cold, clammy skin, feeble pulse, violent supra-orbital pain, and distressing gastralgia, with a very anxious countenance. On inquiry, he found that she had taken an ounce of the wine of colchicum, in twelve hours, and shortly afterwards had been seized with violent and profuse vomiting, which had gone on increasing. A large sinapism was applied over the epigastrium, and subnitrate of bismuth was given with opium until the vomiting ceased; mucilaginous drinks were then ordered, and a cataplasm was applied over the region of the stomach.

On September the 25th, the patient had slept but little during the night; she had been vomiting at intervals, and seemed in a hopeless condition from gastritis. The bismuth with opium was repeated, and after many attempts, the vomiting was checked, and sleep was procured.

On September the 26th, the patient seemed much easier, but was still troubled with retchings and vomiting occasionally, as well as with constant thirst, slight cough, and pain in the stomach. The same treatment was continued; and on September 29th she was discharged well.—*American Journal of the Medical Sciences*, January, 1857.

[For the particulars of five other cases of poisoning by colchicum, see Forensic Report, British and Foreign Medico-Chirurgical Review, October, 1855, p. 504.]

Poisoning by Ammonia.—At the beginning of August, 1854, Dr. Pellérin was sent for at Fontainebleau, to see a young lady who had attempted suicide. On his arrival in twenty-five minutes, he found the patient, Madame H., suffering from an intolerable sense of suffocation, and very restless. She had come to pass some months with her husband in Fontainebleau. In the evening, after some trifling disagreement with her husband, she retired to her room with the intention of committing suicide; she soon returned, with her face pale, her eyes haggard, and her hair disordered. The patient by signs informed Dr. Pellérin that she had taken from her table a phial containing solution of ammonia, that she had poured upwards of ten drachms into a glass, and swallowed off the whole at a draught. As soon as she had done this, she threw the glass from her, and rushed into the adjoining room in a state of extreme anxiety. Dr. Pellérin found her supported with difficulty in the sitting position, having on her knees a basin containing a large quantity of stringy salivary fluid with a few streaks of blood. The face was pale, the eyes were haggard and injected, the lips presented much swelling, and also redness, which extended to the mouth and fauces. There was complete aphonia. There was pain in the pharynx and epigastrium. Pulse was slow, limbs cold. Some spoonfuls of vinegar were given, but were swallowed with difficulty, from the pain produced in the pharynx. The pain in the epigastrium was very severe, and was increased on pressure. Twenty-four leeches were applied to this region, followed by a poultice. The neck was rubbed with oil and opium, and was wrapped in a warm linseed-meal poultice. Dr. Pellérin gave a draught of cold milk which happened to be at hand, and which gave relief. The impediment of breathing indicated leeches, as Dr. Pellérin thought, but the patient objected because of the cicatrices. Sinapisms were applied to the insteps and calves, and emollient gargles were used with milk. The aphonia lasted three days. Deglutition was almost impossible; a large quantity of saliva with a sanguinolent pellicle was excreted; the epigastric pain continued. In a week, under the same treatment, Madame H. was convalescent.—*L'Union Médicale*, Feb. 19, 1857.

Poisonous effects of the Narcissus Poeticus—White Jonquil.—Mr. Mellett, of Henley-on-Thames, gives some cases of poisoning by the bulbs of the white jonquil amongst some pigs. The plants were not in leaf at the time the bulbs were eaten. The symptoms were, obstinate constipation of the bowels, associated with cerebral disturbance very analogous to apoplexy. In some of the animals which died, the vessels of the stomach were found much congested. By the free use of aperients, other of the animals recovered.—*Veterinarian*, Dec. 1856.

Antimony, Slow Poisoning by.—Dr. Nevins relates the history of several experiments on rabbits with tartar emetic. Ten were experimented on. The doses were half a grain, a grain, and two grains respectively; and the animals were killed from day to day by a blow on the head, in order to observe the morbid appearances produced day by day. Some, however, died in the end from the poison alone. They had food in abundance, and were kept in a large and airy hay-loft. The fatal quantity of the poison ranged from twelve to seventy-three grains, and the period requisite for causing death, from four to seventeen days. For the first few days no striking symptoms were evident; but after this, the animals lost spirit in a great degree, and gradually became emaciated. They continued to take food until almost the hour of death. In the earlier animals killed by the poison, there was an absence of diarrhœa, while this symptom was evident in all that lived beyond the tenth day. None of them vomited, for rabbits are incapable of this action. Cramps were not present in a single instance, but several of the animals died in violent convulsions, which lasted from a quarter of an hour before death. The mouth was severely ulcerated in several of them, "from the local action of the tartar emetic whilst being introduced into it." One of the rabbits proved to be with young, and continued to increase in size and weight for about a fortnight, after which all motion ceased in the belly. She lost weight and flesh, and died whilst giving birth to the sixth of a litter of seven dead, immature fœtuses. The pathological appearances were in many cases insufficient to account for death. In the latter cases they were generally strongly marked. Their general outline was as follows:—*Emaciation* sometimes extreme, so that not a trace of fat remained in the body. *Mouth* ulcerated. *Stomach* frequently inflamed in patches, but not throughout; sometimes but rarely ulcerated; always more than half full of food. *Pylorus* frequently so thickened and indurated as to resemble cartilage under the knife. *Small intestines* frequently inflamed in patches, but rarely ulcerated. Intestinal glands sometimes excessively enlarged, and deeply coloured by sulphuret of antimony. *Large intestines* frequently empty, but seldom presenting any diseased appearance. *Liver* generally congested in parts; occasionally inflamed, hard, or brittle. *Kidneys* generally more or less congested, but never alike in this respect; sometimes one only; and sometimes the upper portion of one and the under portion of the other. *Bladder* generally distended with urine, and more vascular than usual. *Brain* (seldom examined) healthy. *Lungs and trachea* frequently congested, sometimes highly inflamed; the two lungs seldom alike. *Heart* healthy; generally full of black coagulated blood. In several cases there was extensive *extravasation of blood* upon the surface of the lungs and of the liver and stomach, and beneath the mucous surface of the cæcum. *Distribution of the antimony in the tissues:* It was always present in the *liver* after five grains had been given; and it appeared in this organ before it was clearly proved elsewhere, except in the coats of the stomach, about which some doubt existed whether its presence there might not be owing simply to adhering antimony. It appeared next in the kidneys, and after the fifteenth day it was present in the bones. Its presence was also easily proved in the blood and lungs, in the urine and in the fæces, both hard and soft. In the *brain* it was never clearly present, and its evidence in the muscles was very slight. *Elimination from the system:* The antimony was constantly passing off by the urine and fæces, and it was discovered in both these excretions abundantly in some rabbits which had survived twenty-one days after the last dose had been given; and in the fæces it was slightly present thirty days after the last dose. It disappeared from the liver about the fifteenth day, and from the kidneys somewhat later; whilst it was found abundantly in the bones thirty-one days after the last dose. *Effects upon the fœtus in utero:* A whole litter, consisting of seven young ones, was killed by the poison previous to the death of the mother, and antimony was found in abundance in the placentas and livers examined separately; in the remaining contents of the abdomen examined in a mass; in the contents of the chest; and, lastly, in the remaining flesh and bones of several fœtuses, which were well washed after all the viscera had been removed. The copper-slip test was the one employed in the analyses.—*Pharmaceutical Journal,* Feb. 2, 1857.

[For comparison, refer to the experiments on antimonial poisoning recorded in our last Report; these, in many points, are identical in their results with those described by Dr. Nevins. The absence of antimony in the brain seems to be a general rule.]

Cases of Poisoning by Strychnia.—A case of accidental poisoning by strychnia, in which four grains were swallowed, and recovery took place, under chloroform treatment, is recorded in a West Canada newspaper, but has not as yet, we believe, been confirmed by medical testimony. The patient is said to have discovered his mistake as soon as he had swallowed the poison, and applied to a Dr. Swinburn, who at once gave him an emetic. Two large emetics failed to produce vomiting. Twenty minutes after the poison had been taken, tetanic symptoms showed themselves. Dr. Bly applied chloroform, which relieved the spasms in about three minutes, and stopped them completely in ten, when a third emetic was given; in about ten minutes vomiting was produced, and this effect was kept up by the free administration of warm water. The chloroform was constantly administered for seven hours, after which time the spasms ceased.—*Pharmaceutical Journal,* Jan. 1st, 1857.

Poisoning by Strychnia: Recovery.—The following is a properly authenticated case of recovery from strychnine. On the 17th of September, 1856, Dr. H. J. Givens, of La Grange, Kentucky, was called to see a young man on whom sentence had been passed for a misdemeanour. Being very sensitive, and feeling the disgrace, he determined on self-destruction, and took two ounces of tincture of opium. Dr. Givens found him labouring under much excitement, with a full, frequent pulse, and vomiting freely. No coma or lethargy supervened, and the vomiting being encouraged, he was soon left doing well. In about an hour afterwards, Dr. Givens was again called to him, and found the muscles of the neck, throat, chest, and arms in violent action; while the lower limbs remained in a passive, straight, and rather rigid condition. In reply to inquiries whether he had taken strychnia, he at first gave an evasive answer, saying that he wished to die; but, on being assured by Dr. Givens that he desired to mitigate the violence of the pain and spasm, as there was little or no prospect of arresting the disease, he confessed that on ascertaining that the laudanum had failed to produce effect, he had swallowed two large pills of strychnia, which he had preserved for the emergency. Two large draughts of tartaric acid were at once given, with the object of neutralizing the strychnia; this was followed by tablespoonfuls every half hour of camphor mixture, alternating with doses of ether and turpentine in sugar and water. Deglutition was very difficult, and the spasms were violent for five hours, and tetanic in character. In seven or eight hours the spasms entirely subsided, leaving the patient quite prostrated, with much distension and tenderness of the epigastrium, and stricture and soreness of the throat, and of the muscular system in general. Two ounces of castor-oil, with thirty drops of spirit of turpentine, were given, and operated well. The symptoms of gastro-enteritis gradually subsided; but the inflammation and abrasion of the membrane of the throat continued, and there was hæmoptysis for three or four days. In less than a week he was convalescent, having only a little soreness of the throat and of the general muscular system. From subsequent inquiry, it was found that there were not less than ten or twelve grains of the strychnia in the pills; but Dr. Givens thinks it probable that the greater part was ejected by vomiting, which occurred soon after the pills were swallowed.—*American Journal of the Medical Sciences,* January, 1857.

[The case is one of considerable interest, but many facts are wanting to render it complete. The vomited matters ought to have been examined for strychnia; and the urine passed by the patient should have been subjected to the same process. The treatment by tartaric acid, to neutralize the alkaloid, is surely doubtful as a general line of practice, if not dangerous. A question arises, whether the laudanum, taken in such large quantities just before, had not the effect of subduing the force of the spasmodic contractions. As chloroform freely given seems to arrest the strychnia convulsions, so opium, the action of which is virtually the same, may possibly in this case have had the same beneficial influence.]

Lard as an Antidote to Strychnia.—In our Report twelve months ago* we gave the views of Dr. W. N. Pindell, to the effect that lard acts as an antidote to strychnia. Dr. W. Hammond, of Fort Riley, Kansas, in a letter to the editor of the 'American Journal of the Medical Sciences,' thus disposes of Dr. Pindell's antidote:—"I must tell you, that I have tried Dr. Pindell's antidote to strychnia—lard. I gave two grains of the poison to one dog without the antidote, and two to another with the addition of a pint and a half of melted lard. The best of the joke is, that the *latter* died in four hours, and the *former*—miserable worthless cur, who doubtless was too mean to die—is still running about in the finest possible state of health. So much for lard. We are of the opinion here that strychnia is quite harmless unless lard is indulged in."—*American Journal of Medical Sciences*, January, 1857.

Strychnia: Tests: Symptoms.—Dr. Macadam has carried on a very laborious and useful inquiry on the subjects of strychnine, its detection, and effects. In testing for this poison, he deprecates the employment of hydrochloric acid in the first part of the process. He commends the following as eminently serviceable, and to be depended on when animal matter is being examined:—"The animal matter is chopped into minute fragments, and treated with a dilute solution of oxalic acid. After standing for twenty-four hours, during which time the mass is repeatedly agitated, the whole is filtered through muslin. The contents of the filter are well washed with water, and the washings added to the filtrate. The liquid so obtained is heated to ebullition, when albuminous matters separate, and whilst warm is filtered through paper. Animal charcoal is added to the filtrate, and after repeated agitation during twenty-four hours, the supernatant liquor is decanted off, and the charcoal received on a paper filter, where it is well washed with cold water. The charcoal, now retaining the strychnine, is allowed to dry spontaneously, thereafter placed in a flask, drenched with alcohol, and the whole kept for two hours at a temperature barely short of ebullition. The alcoholic extract is separated by filtration from the charcoal, and is evaporated down to dryness in a porcelain vessel, in the water bath. The residue so obtained will generally be found in a fit condition to be at once tested for strychnine; but should such not be the case, a few drops of oxalic acid solution are again to be added, and the process repeated from the action of charcoal downwards.

Tartaric acid gives results equally successful with oxalic, whilst acetic appears troublesome on the application of the colour test. Dr. Macadam has full faith in the colour tests, and finds in practice the sulphuric acid and bichromate of potash test to be the most delicate. He adds the following table of STRYCHNINE TESTS, which may be useful to our readers:—

A. *Potass.*—A white precipitate, insoluble in excess.
B. *Bicarbonate of Soda* (in acid solution).—No precipitate.
C. *Sulphocyanide of Potassium.*—A white precipitate.
D. *Perchloride of Mercury.*—A white precipitate.
E. *Perchloride of Gold.*—A lemon-yellow precipitate.
F. *Chlorine Water.*—A white precipitate, which dissolves in ammonia to a colourless liquid.
G. *Nitric Acid.*—(Cold) colourless solution.—(Heat) yellow solution.
H. *Sulphuric Acid* (*with trace of Nitric Acid*), and *Binoxide of Lead.*—A violet, changing to a yellow colour.
I. *Sulphuric Acid*, with *Binoxide of Lead.*—A violet, changing to a red colour.
J. *Sulphuric Acid*, and *Bichromate of Potash.*—A violet, changing to a red colour.

* See British and Foreign Medico-Chirurgical Review, p. 514. April, 1856.

Dr. Macadam sums up his remarks on strychnine poisoning as follows:—

1. That when administered to an animal, strychnine is absorbed and retained in its system.

2. That strychnine is not sensibly destroyed in the animal system during life, nor by the partial decomposition of the animal tissue consequent on death.

3. That minimum doses of strychnine may cause the animal to exhibit but partially, or not at all, the physiological effects; but such doses are the most favourable to the chemist; so that, as the physiological evidence decreases or sinks to a minimum, the chemical proof increases or rises to a maximum.

4. That tartar emetic, muriate of morphine, extract of hemlock, and coniine may retard or relieve the spasms, but they do not in the slightest degree hinder the chemical isolation and detection of strychnine.

5. That by proper treatment, strychnine can be separated from organized tissue and organic material in general, as easily as any other poison, arsenic not excepted: and much more easily than most other poisonous substances.

6. That, when isolated, strychnine can be distinguished by a special test, which is unerring and most delicate, and which will detect the merest trace.

7. That the decomposition or natural decay of the animal frame *may* cause the destruction of the strychnine, but in this, TIME will no more easily blot out all traces of strychnine, than it will obliterate the mark of the knife of the assassin.

In conclusion, the author suggests that our law authorities in future should never hand over to the chemist fractional pieces of a subject supposed to be poisoned, but should give over the entire body for chemical analysis.—*Pharmaceutical Journal*, August, 1856.

Urari and Strychnia.—Professor Albert Kölliker has sent to the Royal Society a statement of the results of some experiments which he has lately made on the actions of urari poison and strychnia. The urari is the poison from Guyana, called also curare and woorara. The conclusions run thus:—

1. The urari causes death very rapidly, when injected into the blood or inserted into a wound; when introduced by way of the mucous membrane of the intestinal canal, its effects are slow, and require a large dose for their production, especially in mammalia. When applied to the skin of frogs, it is altogether inoperative.

2. Frogs poisoned with very small doses of urari may gradually recover, even after it has produced complete paralysis of the nerves. Mammalia may also be restored, even after large doses, provided respiration is maintained artificially.

3. The urari, acting through the blood, destroys the excitability of the motor nerves. In frogs, under its operation the terminal branches of these nerves within the muscles lose their excitability in a few minutes, while their trunks become affected in an hour or two later. If, after the nervous extremities have become paralysed, the heart of the animal be excised, so as to prevent the nerves from receiving any further share of the poison, the nervous trunks may retain their excitability for three or four hours.

4. The brain is less affected by the urari than the nerves in the muscles; still, when by ligature of the two aortic arches in frogs, the poisoning is confined to the anterior half of the body, the voluntary movements of the limbs speedily cease, while automatic movements of doubtful nature, and probably proceeding from the medulla oblongata, may be still observed for half an hour or an hour after the poison has begun to operate.

5. The spinal cord is considerably less affected than the brain by this poison, and by local limitation of the poisoning (as in No. 4), it is found that the cord retains its reflex activity from half an hour to an hour and a half; and the excitability of its white substance, or its conducting power, from two to three hours after the poison has taken effect. It is worthy of remark that in

such cases the impaired reflex activity of the spinal cord may be revived by strychnia directly applied to it.

6. The sensory nerves, as shown also by locally limited poisoning, retain their functional activity as long, at any rate, as reflex actions can be excited; and when the depressed reflex activity has been revived by means of strychnia, these nerves are found not to have been in the slightest degree injured; so that it seems doubtful whether the urari in any way affects them.

7. The nerves of the involuntary muscles and of the glands are also paralysed by the action of urari; at least, I find this to be true in the following cases—viz.:

a. The pneumogastric, as regards its influence on the heart.
b. The sympathetic (its cervical portion), in its relation to the iris.
c. The nerves of the posterior lymph-hearts of the frog.
d. The nerves of the vessels in the web of the frog's foot.
e. The splanchnic nerves of the rabbit, as affecting the peristaltic motions.
f. The nerves governing the secretion of the sub-maxillary gland in dogs.

8. The voluntary muscles remain perfectly excitable, but show a greater tendency than usual to merely local contractions. In general, the cadaveric rigidity of these muscles appears to set in later than usual.

9. The plain or non-striated muscles also remain long irritable after poisoning by urari.

10. The heart in amphibia is little affected by urari. Its pulsation, as well as the circulation of the blood, goes on regularly for many hours after the poisoning is established. The only thing worthy of note is, that the beat of the heart appears to be somewhat quickened, probably from paralysis of the pneumogastric nerves. In frogs poisoned with urari, the heart, when cut in two, shows the usual phenomenon—namely, that the half which contains the ganglia continues to pulsate, while the other does not; from which it may be inferred, that these ganglia are not paralysed. As to the nerves in the substance of the heart, those, at least, which are derived from the pneumogastric are unquestionably paralysed (vide No. 7).

11. The blood of frogs poisoned by urari is fluid and dark, but coagulates when drawn from the vessels, and forms a weak clot which is but little reddened by exposure to air. Directly mixed with blood, urari does not prevent coagulation; but the blood in this case also remains dark, and scarcely reddens on exposure.

12. The blood of animals poisoned by urari has the same poisonous qualities as the substance itself, but not in a degree sufficient to produce the full effects of the poison. Urari, when directly mixed with blood, loses none of its efficacy.

13. Urari in concentrated solution, applied locally to nerves, extinguishes their excitability, but only after a considerable time, and it appears to act similarly on the nerves in the substance of the muscles. Dilute solutions have no injurious operation. Applied directly to the brain and spinal cord, urari is altogether harmless, provided its absorption be prevented.

14. When artificial respiration is kept up in quadrupeds poisoned with urari, I find that, as observed by Bernard, many of the secretions become increased—as the tears, saliva, urine, and mucus of the air passages; which effect appears to be owing to the paralysis of the vascular nerves, and consequent dilatation of the vessels caused by the poison.

15. In mammalia, urari causes death by paralysis of the respiratory nerves and suppression of the respiration, which brings on convulsions in these animals as a collateral effect. In frogs, the final extinction of the functions may also be partly ascribed to suppressed action of the lungs and defective oxidation of the blood, which at length renders the heart unfit to perform its office; but it must be observed, that in this case the cause of death is not so plain,

inasmuch as in these animals the functions are in a great degree independent of the pulmonary respiration.

II. *Strychnia.*—Some experiments with strychnia (the acetate) gave the following results:

1. Strychnia has not the least influence on the peripheral nerves through the blood, which is best shown by cutting the nerves before administering the poison.

2. Strychnia paralyses the motor nerves of the voluntary muscles by exciting them to too energetic action, a paralysis which may be compared to that caused by too powerful electric currents acting upon the nerves. In frogs, when the tetanic spasms are over, the nerves often show no trace of excitability; in mammalia, they generally retain it in a slight degree, but never show the same energy of action as when uninjured.

3. Strychnia does not affect the sensory nerves.

4. The heart is not affected by strychnia, not even during the tetanic spasms, with the exception only that its pulsations are a little slower during the tetanic state. On the contrary, the lymph-hearts of frogs contract as soon as the tetanus begins, and remain in this state as long as the spasms last.

5. The tetanic fits can be brought on in two ways: first, through the sensory nerves, which, by irritating the grey substance of the spinal cord, produce the tetanic contractions as reflex movements; and secondly, through the brain, which is not affected at all by strychnia, and preserves its powers of volition and sensation. Accordingly, animals poisoned with strychnia try to move in the ordinary way, but every attempt brings on a tetanic fit, so that it is plain that the spinal cord may also be excited by the brain to its peculiar actions.

6. If the tetanus produced by strychnia has been strong, the muscles are less irritable, and pass much sooner into the state of cadaveric rigidity, which is very strongly marked, and seems to last longer than it generally does. The same early onset of rigidity may be observed in animals killed by tetanus excited by electricity.—*Medical Times and Gazette*, Sept. 13th, 1856.

Researches on Poisoning by Phosphorus.—M. Schuchardt has published a very complete memoir On Poisoning by Phosphorus and its Compounds. In many cases, especially when given in substance, phosphorus acts with energy on the stomach and intestines, producing extensive and deep ulceration, and even perforation. In other instances, when death has occurred more rapidly than usual, there is no severe structural lesion of these organs; this is chiefly observed in poisoning by the compounds of phosphorus. The lungs are often the seat of extensive sanguineous infiltration. The most constant characteristic of poisoning by phosphorus is fluidity of the blood, which is deep-coloured, coagulates imperfectly or not at all, and presents a thin layer of a peculiar rose or purple colour. Ecchymoses are also frequently observed on many organs, especially on the surface of the lung, pericardium, stomach, and skin. According to Siedbeck, the blood, on exposure to the air, does not become red, but darker than before. This condition of blood is not inflammatory, and therefore cannot be the result of the local lesions; moreover, it is often independent of these. There can be no doubt that change in the blood constitutes one of the most prominent features of poisoning by phosphorus, without neglecting the lesions of the stomach and intestines.

The acids of phosphorus have not the same action as phosphorus itself. Given in doses which would be poisonous with phosphorus, they produce no effect. The administration of alkalies simultaneously with, and again some time after, has in no way retarded or prevented death; but in these cases the local effects have been less, or completely absent. The local lesions, Schuchardt remarks, may be the result of the combustion of the phosphorus, and the acids thus formed may increase the destruction process, although experiments show these to be incapable of producing it themselves. But it must be observed that the acids have not been given in a state of complete concentration, and that, in spite of this circumstance, the phosphoric and phosphorous acids injured, though superficially, the mucous membrane of the stomach. Various researches, especially those of Mitscherlich, show that phosphoric acid has much analogy in its action with sulphuric acid. Now, in the combustion of phosphorus, the acid may well be supposed to be in such a state of concentration as to be capable of cauterizing the tissues.

It further appears that the acids of phosphorus are not transformed into phosphuretted hydrogen in the animal system.

Schuchardt has experimented with phosphuret of calcium. This substance is a compound of several combinations of phosphorus, lime, phosphate of lime, and lime in excess. In contact with water, it is decomposed into the three forms of phosphuretted hydrogen (principally the gaseous) and hypophosphite, phosphite, and phosphate of lime. Phosphuret of calcium does not reach the stomach if given in substance, and therefore must be first suspended in oil. In this form it diffuses a strong odour, and slowly disengages phosphuretted hydrogen. A grain and a half of this substance, rubbed up in a drachm and a half or three drachms of oil, is generally a fatal dose, producing the same symptoms and post-mortem appearances as those which arise from phosphorus. The erosions and ulcerations are, however, neither so extensive nor so deep. They were observed in the œsophagus in a case where the phosphuret was administered in substance by the mouth, and did not reach the stomach; and in the rectum, in a case where a piece of the phosphuret was introduced into that organ. Passing to the consideration of the antidotes for phosphorus, Schuchardt commences with magnesia, as proposed by Orfila. Dr. Landerer relates a case in which a child swallowed nearly a teaspoonful of phosphorous paste, prepared for killing rats. Magnesia, rubbed up in sugared water, was given in large quantities, and the child was out of danger in eight hours. On the other hand, seven rabbits which Dr. Schuchardt subjected to experiment, all died, although the dose of phosphorus did not in any case exceed a grain. But the local lesions were less marked. It must be observed that, in other experiments, the phosphorus was given in the form of a pill; that the magnesia was given in small doses, an hour or an hour and a half after the poison; and that the administration of it was not continued for a sufficient length of time. In this respect, the experiments of Schuchardt are incomplete. M. Duplos proposed as an antidote a mixture of one part of magnesia with eight of solution of chlorine. A portion of the magnesia is transformed into the hypochlorite, having a considerable oxidizing power. The phosphorous acid and phosphuretted hydrogen are changed—the first into phosphoric acid, and the second into hydrochloric acid and an acid of phosphorus; and all these acids are rapidly neutralized by the magnesia. Bechert has obtained favourable results from this method; others, including Schuchardt, have failed; and Holländer even found death to occur more rapidly from it. Experiments with the chloride of lime did not give any more favourable results. The discovery of an antidote for phosphorus as poison has therefore to be made.—*Zeitschrift für Rationelle Medizin*, Band viii. Heft 3; and *L'Union Médicale*, December 27th, 1856.

Poisoning by Turpentine.—Dr. John Maund, of Melbourne (Australia), relates the case of E. H., a woman, aged thirty, of healthy appearance, who had for some months been living with a man as his mistress and housekeeper, and who, on his signifying his intention of leaving her, became low-spirited, and indulged in liquor. On the day of her death, a neighbour who called to see her, noticed a soda-water bottle nearly full of turpentine. At the request of the deceased, she (the neighbour) went to fetch some meat from the butcher; on returning in a short time, not finding the deceased, and receiving no answer on calling, she put down the meat and left the house. Four hours afterwards the woman was found dead; the meat and other things remaining as the witness had left them. The position of the body, which had not been altered when Dr. Maund first saw it about forty hours after death, suggested at first that death had occurred from strychnia. The deceased had evidently sat, immediately before death, on the side of the bed; and seemed to have simply fallen backwards. The legs were rigid and stretched out, and the soles of the feet were concave; the arms were bent across the chest, and great force was required to move them from that position; the biceps muscles were contracted, and very hard. The body assumed the state of opisthotonos, and all parts were rigid, but the thighs least so. The eyes were open and prominent, and the pupils slightly dilated; the jaws were fixed, and could not be opened; the skin generally was pale, but livid in places, especially about the head. Death appeared to have occurred suddenly from tonic spasm; there was no derangement of the dress or of the bedclothes to indicate convulsion; nor were there any external marks of violence. An empty pail was found on the floor close to the deceased, as if it had been placed there in expectation of vomiting. On post-mortem examination, the membranes of the brain and spinal cord were found greatly distended with very dark sizy blood, which had no unusual smell; the brain was to a less extent congested with blood of the same character. The mucous membrane of the trachea was rendered arborescent by the ramifications of a network of distended vessels, but in the intervals the membrane was of the usual colour. The lungs were gorged with dark blood; the right cavities of the heart were distended with dark blood; the left contained a small quantity. The liver and kidneys were congested, but less than the organs above-mentioned. The bladder was empty and contracted, but healthy. When the stomach was opened, a powerful odour of turpentine, which had not before been perceived, became evident. The stomach contained a small quantity of a thick fluid, resembling an emulsion of turpentine with mucilage. The duodenum and upper part of the jejunum were much congested, and the smell of the turpentine was evident in all parts of the intestinal canal. The mucous membrane of the stomach was congested, and several very large vessels distended with blood were found passing from the cardiac to near the pyloric extremity, and close to these vessels were several small ecchymosed patches. The stomach contained three ounces of semi-fluid substance, in which were observed globules of what appeared to be oil of turpentine. The contents of the stomach were thoroughly mixed with distilled water; in three hours there was found on the surface a limpid fluid, which, on being removed by the pipette, was ascertained to consist of six drachms of the oil of turpentine. No strychnine could be discovered either in the contents or tissue of the stomach, or in the turpentine. The solid contents of the stomach consisted of wheat and potato-starch corpuscles.

The house was carefully searched for poison, but nothing was discovered. The bottle containing the turpentine was found on the shelf where it had been seen by the witness; the quantity removed was about six ounces, but it is impossible to say whether the whole of this had been swallowed. There was no smell of turpentine in the house, nor any suspicion of its having been taken until the stomach was opened. The deceased had several times hinted that she would destroy herself.

Dr. Maund states that he has been unable to find any other recorded case of poisoning by oil of turpentine. He observes that Christison and Taylor are not aware that turpentine has ever proved fatal; while Beck and Guy do not treat of it as a poison. Pereira and Christison state, on the authority of Professor Schubarth, that two drachms of the oil of turpentine given to a dog, produced immediate staggering, cries, tetanus, failure of the pulse and breathing, and death in three minutes. Being desirous to learn if these effects were constant, Dr. Maund, in conjunction with Dr. Youl, made the following experiments:

Experiment 1.—Half an ounce of turpentine was given to a moderate-sized dog. It immediately produced quickness of breathing, increased flow of saliva, frequent but ineffectual attempts to vomit, giddiness and apparent intoxication. These symptoms passed off, and the dog recovered.

Experiment 2.—To another dog two ounces of oil of turpentine were given. The symptoms were the same as in the preceding case, with the addition of shaking of the limbs and a greater amount of general prostration. Half an hour afterwards drowsiness was a prominent symptom, and the dog could with difficulty stand. The eyes were much congested and everted; the countenance presented the appearance of suffering and depression; the dog lay down on its side; frothy sputa ran from the mouth, and the animal seemed to be dying. Consciousness was never totally lost, for the animal could, though with difficulty, be roused by speaking loudly or by touching it. The following morning it seemed much better, and gradually recovered.

Experiment 3.—To a blind dog three ounces of the oil of turpentine were administered. For the following quarter of an hour the only effects manifested were quickness of breathing and increased flow of saliva. Giddiness and general intoxication then supervened; in half an hour the dog lay on his side with the legs stretched out, and, excepting that the eyes were not everted, presented much the same appearance as the second. On the following day it gradually recovered.

Experiment 4.—To another dog four ounces of turpentine were given. In two or three minutes there was great excitement; the respirations were 108 in a minute; clear saliva streamed from both sides of the mouth, but there was no giddiness or sleepiness, as in the other cases. Half an hour after the turpentine was given, the dog broke his cord and ran about as if greatly excited; but he would come when called, and appeared perfectly tractable and docile.—*Australian Medical Journal*, October, 1856.

[A long list of other cases of poisonings, recorded in the British Journals, lies before us. But in the limited space at command we have selected only such instances as offer some new or marked feature in pathology, physiology, or forensic practice. The instances of poisonings by stramonium, ammonia, and turpentine, are all novel in their way; and the last is of special interest, as leaving after death appearances which admitted easily of a turn in a false direction. Had a man of feeble observation and clumsy manipulative skill made the post-mortem in this case, or had it been handed over to a man who had prejudged, or whose craving for revenge was strong, or who was content to believe solemnly in every scrap of circumstantial evidence, or who had sold himself to the business of advocate, medical or otherwise, under such circumstances, by a movement imperceptibly slight, this case could have been transformed into a horrible tragedy, and unsuspected turpentine have become the efficient cause of two deaths instead of one.]

II. Insanity.

Detection of Feigned Insanity.—Dr. J. C. Bucknill has some excellent observations on that important forensic point—the diagnosis of insanity. He considers that in the great majority of cases feigned insanity may be 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 most 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 or harsh or clammy, pallid 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."

Speaking of chronic mania, Dr. Bucknill observes that it may be imitated; and that 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 he may deceive the most skilful alienist. Fortunately for the credit of psychologists, insanity is rarely feigned, except by ignorant and vulgar persons, who are quite unable to construct and act out a consistent system of disordered mind. It must be remembered that all the features of every case of insanity form a consistent whole, which requires as much intelligence to conceive and imitate as it does to conceive and imitate any dramatic character. The idea which the vulgar have of madness is of quite a different kind. They represent it as a monster, half man, half beast; the emotions they represent unchanged and human, the intellectual functions they represent entirely perverted, grovelling, and bestial. They think that madness utterly alters the character of a man's perceptions and destroys his judgment, so that he not only ploughs the shore and sows salt for seed, but that he cannot recognise his own son, or avoid the destruction of his life. In more homely cases it will be found that men feigning insanity pretend that they cannot read or write, or count ten correctly, or tell the day of the week, or how many children they have; they answer every question wrongly, which a real lunatic who could be made to understand the question and to answer it at all, would certainly answer right. In illustration of these facts, he subjoins the following case of simulated insanity reported by Dr. Snell, in the 'Allgemeine Zeitschrift für Psychiatrie,' December, 1855:—"In the House of Correction at Ebernach, a man attempted for some years to escape punishment by imitating insanity. He would not work, he danced round his cell, sang unconnected words and melodies, and made a peculiar booming sound. When any one went into his cell, he put on a forced stupid expression, he glanced at people sideways, but generally fixed his look on the floor or on the wall. To questions he gave either no answers, or answers altogether wrong; for instance, to the question, How many days were there in the week? he answered, ten. He would not recognise the people whom he constantly saw; he said he had never seen me, and did not know me. When I asked him if he knew who I was, he said a man. I placed before him a keeper with whom he daily had intercourse, and asked him if he knew who this man was; he said at first he did not know, and then he said he believed he was a soldier. There could be no doubt there was deception in this case. The unmasked deceiver tried to play his part for some time, and then gave it up."

In the detection of feigned insanity, much stress has been laid by writers upon the suddenness of the attack, as distinguishing it from real insanity, the invasion of which is gradual. This point of diagnosis must be accepted, however, with much caution. Dr. Bucknill has known real cases of mania manifest themselves with the utmost suddenness; he has known patients who went to bed apparently in good health, awake in a state of mania; he has known patients become suddenly maniacal under the influence of exciting and denunciatory preaching, and during other conditions of intense temporary excitement. Doubtless, in all these cases the brain was previously prepared for the sudden explosion, but the symptoms of latent disease had not been of a nature to attract any observation, and therefore in a diagnostic point of view the sudden outburst of real insanity must be accepted as possible.—*Asylum Journal*, January, 1857.

III. Hygiene.

The Measle of the Pig.—Dr. Alex. Flemming, of Dublin, has published a report on this subject. He was requested to undertake the task by the *Committee of Provision Merchants* of Cork. The questions submitted by the Committee were as follows; the answers are those of Dr. Flemming and his colleague, Professor Smith. We have epitomized the replies.

1. *Question.* What is the nature and origin of measle in the pig?

Answer. The measle of the pig is an animal parasite, the Cysticercus Cellulosæ, or bladder flesh-worm. It is highly probable, if not quite established, that measles orginate in the eggs of the tape-worm which infest the lower bowels of the dog. Each mature joint of the last parasite contains many thousand eggs. These, when voided by the dog, are resolved into a fine dust, are scattered by the wind, and thus mixing with the food or drink of the pig, enter its body, and are there converted into the measle or flesh-worm.

2. *Q.* Are all pigs measly?

A. All pork is not measly. In the specimens of healthy pork we found no trace whatever of the parasite in any stage of development.

3. *Q.* Can pork be measly, and that condition be invisible to the naked eye?

A. In the specimens of both slight and badly-measled pork submitted to us, the worms were all visible to the naked eye. All appeared to have reached the same degree of organic growth; and in none of the specimens, healthy or otherwise, could we find eggs, or the slightest trace of the parasite in an earlier stage of development.

4. *Q.* Is there any analogy between "measles" in the pig, and the disease known by that name in man?

A. None.

5. *Q.* Is fresh measly pork wholesome?

6. *Q.* Is cured measly pork wholesome?

A. We cannot regard bad measly pork, fresh or cured, as wholesome food for man. We see no valid reason for regarding "slightly measled" pork as unwholesome; but it must be well cooked. We believe that the life of the parasite is destroyed by the process of curing.

7. *Q.* What is the chemical composition of the measle?

A. Chemical analysis could not aid much in this inquiry.

The Reporters suggest that the rational guide to the treatment of the disease will consist in providing the pig with thoroughly clean food and drink, promoting its general health, and removing it from the neighbourhood of dogs affected with tape-worm.

In their further remarks, the authors follow the views of Küchenmeister and Leuchart as to the transmissions of the entozoa.—*On Measle in the Pig, and on the Wholesomeness as Food for Man of Measly Pork. By Alexander Flemming, M.D. Dublin,* 1857.

Sanitary Improvements in the Manfacture of Chemicals.—M. Kuhlmann, who has many large manufactories of artificial soda at Amiens and elsewhere, has completed a system for condensing acid vapours (chiefly hydrochloric), by making the condensation of the acid vapours the basis of a manufacture of salts of barium. He places after the ordinary condensation apparatus, earthen vessels, containing native carbonate of baryta in masses, moistened with water.

After passing five or six of these vessels, the whole of the hydrochloric acid, uncondensed by the first or ordinary process of subjecting them with an excess of steam to a gradual cooling before entering the chimney, is held in solution as chloride of barium. To complete this process in the Amiens factory, he makes the vapours passing from the baryta vessels go through a large earthenware cylinder, having within it a wheel with flat arms. As this plays round, it makes the vapours traverse a fine-rain solution of carbonate of baryta. By a further plan, very simple in its way, M. Kuhlmann forms from his chloride of barium artificial sulphate of baryta, and reproduces his hydrochloric acid in a free state.

Kuhlmann has also applied a system of condensation to the chemical products in the manufacture of lamp-black. The vapours, consisting of the ammonia mixed with the burnt air of the furnaces, are driven into a stone trough containing an iron mill with wings. This is covered by a semi-cylinder of metal. As the vapours pass through the cylinder, the mill throws out a shower of fine drops of solution of chloride of manganese, the residue of the manufacture of chlorine. All the ammonia is thus condensed into the muriate. The liquid condensed consists of carbonate and sulphuret of manganese and coal soot; it is easily used in making sal ammoniac, and can be applied to the manufacture of artificial manures. This plan diminishes the smoke of the furnaces; but the author has not found that the quantity of ammonia condensed is sufficient to repay the expense; mechanical power may therefore have to be employed to facilitate the chemical action of bodies which chemically react on each other.—*Journal de Pharmacie et de Chemie*, Nov. 1856.

Detection of Ergotized Rye.—To detect ergotized grains in corn, M. Payen gives the following instructions:—The ears affected are distinguishable by many of the grains in it being replaced by a violet-brown substance, almost black, of larger volume, and frequently twisted, brittle, having a grey mass inside. The ergot may be distinguished even when no larger than the healthy grain, or when broken into several pieces, not only by its external dark colour, but also by its lightness; it floats on water, whereas the healthy grains sink to the bottom.

One-eighth to one-tenth per cent. of ergot in bread may cause gangrene and loss of the limbs, and the poisonous effect is more powerful on animals than on man. In poultry, the phalanges fall off; even the beak is detached. In pigs, the nails fall off, and the animal dies.

The dangers of ergot may be avoided by properly cleansing the grain, by the hand, by sifting, or by fanning. These various processes would not be expensive, as the ergot would produce profit, sold for medicinal purposes.—*Journal de Chimie Médicale*, Dec. 1856, and *Chemist*, Jan. 1857.

In the 'Journal of Public Health' for Jan. 1857, Dr. Pickles publishes part of a classical paper On Vegetable Poisons, which we reserve for notice until its conclusion.

HALF-YEARLY REPORT ON MICROLOGY.

By John W. Ogle, M.B. Oxon., F.R.C.P.
Curator of St. George's Hospital Museum, and Physician to St. George and St. James's Dispensary.

Part I.—Physiological Micrology.

NERVOUS SYSTEM AND ORGANS OF SENSE.

Structure of the Nerve-Cell.—In a communication to the Academy of Sciences, Stilling stated that he found a decided envelope as well in the central nerve-cells as in the peripheric ones, which appears to be formed of innumerable very fine tubules, like those composing the network of the primitive nervous fibres. The parenchyma, likewise composed of a mass of such tubes, equal to those of the primitive fibres, but forming by their close union a sort of glandular tissue, is closely connected with the envelope externally and the nucleus internally. The nucleus itself has a constitution analogous to that of the parenchyma, having, like it, a double contour, interrupted by the small tubes passing externally towards the parenchyma of the cell, and internally towards the nucleolus. The nucleolus he states to be composed of three concentric layers, from which prolongations may be traced as far as the limits of the nucleus. All the central nervous cells are furnished with prolongations composed of tubes of the same nature as those constituting the parenchyma of the cells.

Erectile Apparatus of the Eye.—Dr. C. Rouget, in a paper read before the Academy of Sciences,* brings forward new facts regarding the elementary structures of these parts. He refers to the eyes of birds, the principal mammals, and man. At the adherent border of the ciliary processes he describes a layer of bundles, which is the layer of circular fibres of the ciliary muscle. This in birds is formed, in front, of transversely-striped fibres; behind, of regular fibres, rounded, refracting, and analogous to the fibres of yellow elastic tissue, but containing here and there fine and regular transverse stripes. In mammals the circular layer is formed of bundles of dartoid fibre, smooth, and containing elongated nuclei, in the midst of which run plexiform divisions of ciliary nerves. Outside the annular ciliary muscle, in a kind of stroma of the choroid at the posterior edge of the ciliary region, are bundles of radiated ciliary muscle. These bundles, formed in birds of striated fibres, compose at least two muscular plains; one, external and posterior, is inserted after a short course into the posterior region of the bony ring of the sclerotic; the other, covered at its origin in the choroid by the former, is prolonged anteriorly as far as the anterior margin of the bony ring where a portion of the fibres are inserted; whilst the remainder is attached to the membrane of Descemet, the true elastic tendon of the anterior plain of radiated muscular fibre. This is the muscle described by Crampton and others, who, according to Rouget, were deceived as to its origin and true meaning. The muscular apparatus of the iris is only the continuation of the deep plain of the ciliary muscle (circular fibres). In birds, the transversely-striated bundles enter the iris obliquely, and, keeping in the course of this membrane a generally circular direction, cross each other more or less obliquely. In man, and most mammalia, the same disposition obtains as in that of the iris of birds. At the anterior edge of the ciliary muscle the bundles of the deep layer of this muscle, continuing their direction obliquely transverse, penetrate the iris; and then, covering the external surface of this membrane, and entangling the vessels in their meshes, they cross each other more or less regularly, arrive at the edge of the pupil, and form the ring of circular fibres at the surface of the iris, the sphincters. The author then speaks at length of the mechanism of the parts, and of the functions they play in adapting the lens to vision, showing how the contractions of the circular bundles of ciliary muscle force the blood returning from the iris into the ciliary folds, which, by compression, affect the convexity of the lens.

VASCULAR GLANDS.

The Spleen.—Billroth† details his microscopical observations on the spleen of the amphibia, of fishes, birds, and mammals; and upon the anatomical data, enters at some length upon hypotheses as to the development of the red blood-

* Archives Générales, p. 112. No. 3, 1856.

† Beitrage zur Vergleich. Histologie der Milz: Müller's Archiv, Heft 1, p. 88. 1857.

globules in the red spleen-pulp, just as the lymphatic corpuscles are formed in the alveolar substance of lymphatic glands. He leaves quite undecided the question of the function of the spindle- or the star-shaped cells, and does not think that it is proved that the cavernous network possesses any contractility, though he allows it elasticity.

GENITO-URINARY SYSTEM.

On the Minute Anatomy of the Vas Deferens.—Ludwig Fick,* after referring at some length to the method of contraction, and the mechanism by which the semen is carried along this canal, and comparing the various descriptions of the vessel by Arnold and Kölliker, goes on to describe certain parts of the canal as he found it in man and in the dog. Examining that part which has the smallest calibre and the thickest wall, he describes the contractile parietes as consisting, not of three isolated fibrous layers, but rather of a fibrous groundwork, tearable into concentric circular shreds, in which is interweaved a network with elongated spaces. This trabecular work is developed chiefly on the outer and inner side, whilst the concentrically-splitting fibrous layer forms by far the largest mass of the wall of the tube, being placed in the middle. The trabecular texture appears not to arise from any preformed isolated fibre cells, or to have any preformed morphological existence, like the striped or the smooth muscular fibre, but is like elastic tissue, or that of the arterial fenestrated coat. In the dog, the entire wall is composed of two layers, of which the thicker outer part shows a very coarse texture, whilst that next to the mucous membrane shows the same kind of texture, only of very fine fibrous elements. In proportion as we approach the prostate, does the fibrous layer, which is parallel to the calibre, increase both on the exterior and the interior. The author by no means looks upon these structures forming the vas deferens as being muscular, and is strongly opposed to tissues being called muscular because they evidence phenomena of irritability, which so many tissues do that differ from each other, as well as from true muscle.

CARTILAGINOUS SYSTEM.

Cartilage Cells, their Development, &c.—F. Lachmann* brings forward a specimen of enchondroma, and describes it minutely, with special reference to, and with the intention of illustrating the question of, the cellular nature of the so-called areolar tissue-substance and the disputed cell-character of bone and cartilage corpuscles, whose membrane is looked on by some only as the innermost layer of the matrix surrounding the bone or cartilage cavities. On microscopical examination, most parts of the chondrine-giving matrix were found to contain hollow spaces (cartilage cavities), rounded or elliptical or ovate, surrounded by a thick membrane (the cartilage capsule of Virchow), which was distinctly distinguishable from the surrounding substance, with which it was most intimate, and contained round or oval bodies which did not fill them. These are the bodies looked on by Müller and others as nuclei of cartilage cells, and by Virchow as the special cartilage cells themselves. By the author they are called cartilage corpuscles, and contained chiefly granular material, and in many parts of the tumour fat drops. In many cartilage cavities, in the place of these rounded bodies, others with hollow radiating, and often branched, processes existed, which reached in general to the capsule of the cartilage cavity. Many were of a form intermediate between the round and the radiating. Both in the case of the round and the radiating they were chiefly single, but sometimes there were more in the cavities; and they were surrounded by fluid, not being imbedded in any fine material. Oftentimes on

* Müller's Archiv, p. 473. Aug. 1856. † Ibid., No. 1, p. 15. 1857.

trying to insulate them, the projections burst. The transition of the corpuscles (soon to be described) into cartilage bodies imbedded in the matrix, renders it improbable that they are only the coagulated contents of the cartilage cavities, but it is difficult to make out whether they are the equivalents of cells, or only of cell-nuclei. At times, a slender, pale contour was seen to surround the bodies, which one might look upon as the raised thin membrane of the cell. The author inclines to look upon these bodies as being cells, partly from the close fitting of the fine membrane to the coagulated contents, and from the fact that one or two rounded bodies are often seen within them, appearing like nuclei, with one or two nucleus corpuscles inside. The character of fresh cartilage, and the effects produced upon its corpuscles on the addition of a solution of chloride of sodium or sugar, corroborate this view. By the alternate use of these solutions and pure water, one can make the cartilage corpuscles shrink repeatedly and again swell out, the nucleus becoming obscured and again cleared, and so on. The author then seeks to show the identity of these bodies with the radiating cartilage and bone corpuscles imbedded in a homogeneous matrix. He points out and delineates cartilage corpuscles, in which exist radiating corpuscles, the cartilage cells, which do not at all limit themselves to the cavity of the cartilage capsule, but reach, as it were, through the limiting membrane into the matrix beyond. He says he had not been able, as Virchow did, to see a bulging out of the wall, but the processes appeared simply to pierce through and pass into the homogeneous or slightly granular matrix, in this way completely simulating the so-called bone cells; and also, as in their case, an anastomosis of the processes of different bodies existed. Nevertheless, unlike the case of bone where the space is filled by a homogeneous mass, and where no capsule exists, the body of the cartilage cell remained separated by fluid from the thick capsule membrane. This difference is, however, balanced, seeing that in the cartilage cavities a firm chondrine-yielding mass is deposited for the most part before the cartilage cell has burst through the wall of the capsule; and so cartilage corpuscles are often seen lying in a space filled with a slightly granular, more or less firm, material, which is surrounded by a thick capsule membrane. Every gradation of form exists between those cases in which the contents of the capsule have quite a different appearance to the surrounding parts, and those in which they are no longer to be distinguished from the matrix; and by the observation of these facts the author thinks he has established transitions from the round cartilage cells lying in a cavity with a thick capsular membrane, to that form which anatomically only differs from solid bone by the want of Haversian canals. He thinks he has proved that the radiating cartilage corpuscles of osteoid cartilage arise from changes in the round cartilage cells, and that therefore the membrane which can be detected and isolated on treating the cartilage with re-agents is the true membrane of bodies which must be looked upon as cells, and not as merely the innermost layer of the surrounding matrix. He proceeds to show how the process of ossification may be carried on according to the above plan. He supposes the cartilage cell to put forth hollow projections, which grow until they reach the capsule of the cavity in which they are, and even to pass into the surrounding matrix. During the growth of these projections, or it may be before, a space is formed between the cell and the capsule, either by an increase in the size of the cavity, or a shrinking of the cell; and this space, at first filled with fluid, becomes filled with a firm material, either deposited throughout at once or more gradually from the periphery to the centre. This material becomes very like the surrounding matrix, and the projections of one cell become united to those of another. In the mean time the matrix of the cartilage, at first yielding chondrin, changes into a glutin-yielding material containing lime salts, and along with this the firm material within the cartilage capsule becomes not a chondrin, but a gluten-yielding material, containing also lime salts. The calcification appears first at one time outside, at another time inside, the capsule. In cartilage engaged in formation, the cartilage capsule must be looked upon as the mother cell of the cartilage cell. Very often two or more capsules are seen aggregated and surrounded by a large outer and investing capsule-membrane, filled with fluid or firm material, and having the same relation to the capsule as to the included cartilage cells, and thus the capsule must be considered as an altered cell-membrane. Just as the contents of the inner capsule become gradually blended with the surrounding material, so also do those of the outer ones, becoming gradually less clear until they at last cannot be seen. The author considers the cartilage capsules to arise from the cartilage cells by endogenous cell-formation. In a very few cases radiating cartilage cells are to be seen in cavities which are surrounded by a membrane possessed of radiating projections, which membrane lies in a cavity surrounded by a capsular membrane; and as the projections of the outer membrane do not correspond to those of the cartilage cells, it cannot be looked upon simply as the membrane of these bodies raised up from them.

The developmental process of cartilage is described by the author much as follows:—In the original formation, cells of cartilage, new cells, cartilage cells, or corpuscles, are developed, and the membrane of the mother cells becomes the cartilage capsule; whilst the new cells, by endogenous cell-formation, equally pass into capsules. Their membranes become greatly thickened; and between them and the membrane of the mother cell, a firm material, containing lime, is found. The process advances, the cartilage cells grow by endogenous growth, and the whole cartilage increases with the deposit of firm material in the capsule. When cartilage ossifies, or assumes an osteoid development, the true cartilage cells no longer form subordinate cells, but grow out in radii, and the same changes as described above take place in them.

Cartilage, Intervertebral, Alterations in.—Luschka* has communicated a lengthy paper on the alterations which take place in the intervertebral cartilage, according to age. He gives special attention to the three elements entering into the formation of the intervertebral substance in earliest infancy—viz.: 1. The cavity and its contents; 2. The cartilaginous plates; and 3. The annular fibres. The cavity at this age seems to amount to half the size of the whole intervertebral substance, the inner surface being smooth and even, and to contain a clear, colourless, transparent, gelatine-like material, in which are seen with the unaided eye many whitish clumps, which, when examined microscopically, are found to possess great variety of form—being spherical, club-shaped, and pedunculated; also an irregular mesh-enclosing, trabecular tissue is seen. Most of these forms appear to be an aggregate of hyaline, sharply-defined, dark drops; but closer investigation shows them to be true cells, having a transverse breadth of 0·04 millimètre. Their walls for the most part have a double contour, and are in parts so united among each other, that the entire object appears as a fine network whose spaces contain a homogeneous substance. The elements of a majority of these clumps of cells are not united or covered by any special material, but in many, a structureless connective-like substance forms the bed for the cells. The contents of the cells are not uniform: one often finds a rounded granulated nucleus of 0·01 millimètre broad, even without the addition of any re-agent; but on adding acetic acid, almost every one is seen to contain a nucleus. A few only of the cells have finely-granulated contents, and even then they contain almost regularly one or more drops of a hyaline substance, which is sharply defined, and distinct from the surrounding molecular mass. Such free drops are also seen between the cells, arising either from exudation through the cell-wall, or melting down of the cells. Whilst the homogeneous transparent cells are predominant in the intervertebral gelatinous substance of subjects in earliest infancy, in that of the twelve weeks'

* Virchow's Archiv, 1856.

fœtus, which already possesses cavities filled with gelatinous substance, we find an excessive number of finely-granulated nucleus-holding flat cells, very like the epithelium of the mouth, and many of the forms undergoing increase. For not only are more nuclei found in a cell, but also larger mother cells, with numbers of offspring cells; and in many human fœtuses, the gradual transition from the quite granulated to the completely hyaline cell may be seen. Very often the cells, along with a nucleus, contain a small, clear, homogeneous drop, which becomes larger and larger until the whole cell-contents becomes homogeneous and fluid, and at last free by the destruction of the cell walls. This is seen in a fresh and young fœtus, even without the addition of water. The same clear drops are to be found in the interior of cells of the intervertebral substance of certain fishes, and the author looks upon this as a product of cell activity. He found in very young mammalia-embryos, no cavities between the already cartilaginous vertebræ, but in their place a small number of cells which had the greatest similarity to those of the chorda dorsalis of these animals, and which he thinks increase by endogenous formation, and suddenly pass into a fluid state, by which the formation of cavities is assisted by the dissolution of the surrounding tissues.

II. The cartilaginous plates are found to contain three kinds of substance. Near to the limits of ossification exists the well known pyramid-like cartilage-cell; next comes the hyaline cartilage, with elongated small cartilage cells, whose long axis runs parallel with the surface of the vertebræ; and in the third place, the layers next to the gelatine-holding cavities possess in part a quite homogeneous soft matrix, and partly fine fibres radiating towards the cavity; and in it also exist irregularly-placed cells containing nuclei with from one to two nucleoli. This matrix passes so gradually into the contents of the cavity, that it appears to be concerned in the process of fluidification.

III. The fibrous ring at this age, in proportion to that of the adult, has a very slight thickness, but exhibits, on section, a clearly laminated edge. The lamination extends to the limits of the gelatine mass, recognisable by the naked eye. When this is removed, the inner surface of the ring appears almost as smooth as the surface of the cartilage discs next to the cavity. In the finer structure of the fibrous ring of the newly born, one perceives, on a fine perpendicular section, fibrillated cellular bundles, circumscribing large interspaces of various forms. The greater number of these bundles are fibres, which, proceeding from the destruction of the matrix of the cartilage plates, are in direct continuity with them; and lying between the fibres as well as in the meshwork, are large and small cartilage cells, and also areolar-tissue corpuscles, in every possible stage of development to elastic fibres. The cellular-tissue bundles branch out in every direction, and the processes of the cells are united into a network, enclosing the round mesh spaces, the cell nuclei existing almost everywhere, and forming thicker, darker spots, like knots in the network. The very fine elements of the network which exist in the newly-born, are in the adult very thick. Towards the cavities, the substance of the fibrous ring is markedly softer; and often one sees numerous fine fibres ramifying in every direction. In old age, the intervertebral substance shows great numbers of anomalies, both in the cavities, the cartilage discs, and the fibrous ring. In old people, the cavities are sometimes extremely small, at other times very capacious, but generally only a small slit. Very often they are two in number, owing to the original gelatine-like nucleus being turned into a felty mass crossing the original cavity. In one case of a person twenty years old, the cavity in the lumbar region, like that in infants, was very large, the synovia-like fluid containing many fine cartilage cells and other material, the result of dissolution; and the wall of the cavity was not smooth, but velvety. In later life, sometimes the cavity is filled with a reddish succulent mass, which passes into the neighbouring tissues, destroying the corresponding cartilage discs; and this mass seems to arise from the spongy bone-substance of the vertebræ, not from any changes in the cartilage. The masses of elemental forms which are found are partly portions of cartilage plates and fibrous rings, containing colouring-matter, or blood, or pigment; and in part also new formations; and amongst the latter exist cells concerned in the development of cellular fibres, and also bloodvessels, which, being in connexion with those of the bone-substance, often form loops and enlargements. In decrepid people, the cartilage discs are almost always of a dirty-yellow colour, and very thickened, and frequently beset with fat, either free or enclosed in cartilage cells. Very often there is a partial dissolution of the bone; and often the destruction of the cartilage is ushered in by an aneurysmal dilatation of bloodvessels entering into the pores of the bone, and looking like apoplectic spots. The author here speaks of alterations in the bone tissue which are not comprehended in this part of the Report, as they are obviously pathological—such as the hardening and polishing of the bone, the ossification of the annular fibres, and exostoses from the vertebræ, &c.

Part II.—Pathological Micrology.

TUMOURS, MORBID DEPOSITS, EXCRESCENCES, ETC.

Cartilaginous Tumours. By H. Meckel.*—Apart from those forms which are of a mixed nature, the author divides these growths into five main varieties: I. Ordinary Enchondroma; II. Stellate-Cartilage Tumours; III. Fibro- and Reticular-Cartilage Tumours, so called on account of the disposition of the intercellular substance; IV. Granular-Cartilage Tumour; and V. Pouched-Cartilage Tumour (*schlauch-knorpelgeschwulst*). The two first he looks upon specially as enchondroma, and the third form as simply an hypertrophy of cartilage. In the fourth, the cartilage tissue retires, as it were, towards a mass of cellular tissue; whilst the fifth he considers should be only doubtfully looked upon as cartilage. All the varieties appear to be formed by bloodvessel-bearing cellular tissue and special cartilage-elements, the union of which is best seen in the ordinary cartilage tumours.

In the first variety, the cellular tissue, in which are the cartilage cells, is to be looked upon as their matrix, and is formed first, being divided into large and small meshes, inside which new material is formed into cartilage, either under the influence of neighbouring bone tissue, or from other causes. Seeing that other fluids and tissues of a different chemical composition may form in the cellular network, the cartilage formed within it may be looked upon as a secretion.

The amorphous material out of which the tumour arises seems to possess a twofold organization, and to be able to separate itself into colloid tissue and chondrin tissue; the one for the function of adhesive union, the other as an elastic support. The opposition of these two tissues and functions is common in enchondroma, as in healthy tissue-formation; the common capsule, and also the various septa of the mass, being of pure areolar tissue, and the compartments being variably filled with fine cartilage-tissue, or being more or less cyst-like, the cartilage appearing as a syrupy or gelatinous fluid. The firm cartilage masses lie as irregular angular masses, mosaic-like, and united by a stroma of cellular tissue, whilst the soft cartilage-masses appear as closely-thronged cyst-like spaces, semi-fluctuating. A transition of this soft cartilage-mass into gelatinous cancer seems never to occur, although from the alveolar structures some resemblance exists. Among this class, the author describes a spurious transition-form of cellular tissue and cartilage, which appears to be a form of enchondroma passing into fungus and medullary excrescence, such as affects the iliac bones, the sternum, and the bones of the hand in old persons.

Varieties containing the elements of pure cartilage are numerous, the carti-

* Annalen der Charitè-Krankenhaus. zu Berlin, p. 60. 1856.

lage elements appearing to arise out of the vascular cellular-tissue of the tumour, either at first as clumps of gelatinous material containing one or more nuclei, becoming rounded later on into membranous cells; or else without any obvious gelatinous stroma, small nuclei form directly into closely-packed cells. The small cartilage cells are sharply contoured, moderately firm, with firmly granular contents, and rounded, angular, caudate, or dentate in form. External to the cells is secreted a homogeneous material, which becomes the inter-cellular tissue. This generally is as clear as water, and gelatinous or syrupy in consistence, the cartilage corpuscles causing a slight cloudiness in it, in the quickly-growing cartilage; but in cachectic cases often containing a mixture of blood. Sometimes the entire tumour appears like an aggregate of cysts, with gelatinous contents like what is called cysto-sarcoma; but yet, on investigation, the whole mass may be considered as cartilage.

The next form which comes in succession presents a part of the extra-cellular material as forming regular laminæ around the cartilage cells, a form leading to great variety of nomenclature. The author then proceeds to show how very large cartilage-corpuscles arise from small cartilage-cells, and how the laminated condition arises out of the activity of the cells. He describes the increase of cells in the cartilage of enchondroma as arising partly by isolated productions of new cells in the matrix, partly by the formation of many-nucleated mother-cells, and partly and chiefly by the division of cells. Excepting calcification, changes inside the cartilage cells are not of importance. In old cartilage, deposits of fat and occasionally of the colouring matter of blood, occur, which have found their way by endosmosis, and assume a granular or crystalline form. Cartilage corpuscles in enchondroma seem to have but little tendency to calcification or ossification, which is of importance pathologically and therapeutically, for in old enchondromata, when a complete ossification does occur, the growth of the cartilage tumour becomes arrested, and a bony tumour remains. This slight tendency to ossification proper seems to depend on general cachexy, and partly on local causes. Irregular calcification seems to be unconnected with bloodvessels, and in the process, the cartilage cells themselves remain free from calcification, as in normal cartilage, the process taking place in the thickening laminæ of the cartilage cells. Sometimes the whole amorphous inter-cellular substance is finely or densely calcified, either before or at the same time with the calcification of the laminæ. This species of calcification comes the nearest to normal ossification, but the whole of it remains granular and crumbling, and the cartilage-corpuscles are never like the normal cartilage-corpuscles. Ossification with true bone-tissue-formation occasionally occurs in enchondroma, and may best be seen in the periosteal and fibrous investments of enchondroma of the finger, femur, &c., in which, at the limits of the ossification, transparent cartilage-tissue with stellate cells are seen, and further inwards, opacity of the inter-cellular and laminated substance.

The author holds the presence of cartilaginous growths to be an evidence of a scrofulous or rhachitic taint; and, after detailing several cases too lengthy to be here introduced, he alludes to the general course and locality of such growths, and also to their treatment. He then proceeds to consider the form of "Stellate-Cartilage" Tumour. This he describes as being, on section, like the former in general characters, but as having the following peculiarities:—In the first place, this form of tumour occurs at a later period of age, and affects generally soft parts, fibrous membranes, and, though seldom, bony tissue possessing a diploe. Occasionally several tumours exist at the same time, and in some cases they return after extirpation. When in soft parts, they grow to the size of an apple, are very elastic, and covered by a capsule formed by a smooth layer of cellular tissue, and are very moveable. The outer surface is tuberous, lobulated, and of a yellowish-brown or white colour; and on section, presents an obscure separation into lobes and lobules by means of fibrous and areolar tissue. On microscopical examination, a scaffolding of very fine areolar tissue, with collapsed cells and finely-fibrillated inter-cellular substance, is visible. The fibres coming between the cartilage elements are readily to be seen. Some cartilage cells lie distributed or closely pressed between the cellular-tissue fibres, or exist in nests up to ten or fifteen in number. In those parts which are remarkable for firmness, the cartilage cells are surrounded by inter-cellular substance and laminæ, as in the first form, before described; but the greatest part of the material giving the special character to the tumour contains a soft gelatinous, peculiar kind of cartilage. In the earliest developmental steps one sees inside the small meshes of cellular tissue a conglomeration of closely-adhering cartilage cells, which either fill the entire mesh, or are suspended free within a clear slimy fluid. In the last case, the conglomerate appears as large mother cells with a simple outline, and having no capsule, but only exhibiting a smooth streaky mass; they sometimes appear as membranes and cylinders, in the same manner that epithelium sometimes does. The next developmental step is, that the hitherto rounded cartilage-cells become stellate, whilst at the same time they become so separated by a clear intervening substance that the cells appear to remain fixed to each other by numerous projections. The intervening substance is very soft and tender, and so transparent that the cells oftentimes look like empty spaces, and require the addition of iodine solution to make them visible: very often it is nothing more than a tenacious fluid. Ossification seems never to take place in the stellate cartilage. Of this form the author relates two or three cases.

The next form, the "*fibro-cartilage* and *reticular*-cartilage tumour," appears to be rare. It generally forms as an outgrowth from permanent cartilage.

The "*granular-cartilage* tumours" comprehend those formations which evidently exhibit the lowest development of cartilage. In them a cellular tissue of various kinds always forms the matrix in which small cartilage-elements are strewed. They are the same as described by Lebert as epulis, sarcoma of the upper jaw, dura mater, and other places. According to Meckel, they almost always arise from bone, and exhibit slowly-growing rounded, tuberous, occasionally fungous, luxuriant masses, with somewhat plentiful blood-contents. They also have more or less the character of malignancy, returning three or four times after extirpation, but not extending further in the body.

On microscopic examination of this form of tumour, clear portions are seen between the fibres and spindle-shaped cells of the matrix, like rounded discs, or irregularly indented. These are finely granular, and occasionally have no nucleus or membrane. They appear to arise out of the primary matrix. Most of these bodies, which are gelatinous, have great numbers of nuclei—as many as twenty or forty—within them, which increase by division.

Of the last form—the "*pouched-cartilage*" tumour—the author has himself only seen four cases, all of which arose in the neighbourhood of bone or cartilage, and varied in size up to that of an apple. This variety consists either of the purely pouched-cartilage masses, or is combined with the granular cartilage-mass, is lobulated and capsulated, and may be united to the skin and other parts. The entire tumour consists of many very small lobules separated by areolar partitions, giving on section a bluish-white trabecular appearance; and between the partitions is a quantity of yellowish, semi-transparent, soft enchondroma-like substance. Occasionally, slight calcification exists. On minute examination, a fine and coarse network of normal-looking, and in places undeveloped, areolar-tissue fibres and spindle cells form the matrix, the spaces between the fibres being filled with the specific tumour-structure. This material is occasionally marrow-like and whitish, but generally yellow, like honey, or brownish, and in parts red, with effusion of blood. It is from the first almost of cartilaginous hardness, or gelatinous, with parts softened, and having blood extravasated within. It adheres firmly to the tissue, and is squeezed out entire. After slight putrefaction, it may be squeezed out like thick opaque mucus, in which clear crystalline globules exist. After these contents have been removed, the remaining scaffolding of the tumour is seen to be very finely or coarsely spongy, and by the aid of the microscope, the areolar-tissue network is seen to be occupied by mother cells. Of these cells, the smallest form is somewhat like ordinary round cartilage cells; the next is larger, and rounded or angular, and somewhat caudate, having a thick and at times concentric membrane, thickened by laminæ.

The contents are finely granular and cloudy, coagulating on the addition of acetic acid, and the contained nucleus is obvious. Such structures pass into mother cells, whilst numerous nuclei, as high as one hundred in number in some cases, form in the ever-increasing mother-capsule. The mothers cells consequently assume most multifarious forms, with rounded caudate projections like placenta cells, with the formation of buds or diverticula. Where the large capsules have burst, one finds a large number of small nucleated cells lying free in the water, which are indeed very like lymph corpuscles. Hitherto, the elements of the tumour strongly resemble the granule cartilage, but more ultimate forms deviate very much. Isolated nucleated cells, which lie imbedded inside the mother cell by the transformation of their contents and the assumption of a firm gelatinous, vitreous material, form very transparent globules, in which the primary nucleus is occasionally, but not always, seen adherent to the wall. These vitreous bodies then assume most wonderful and varied forms, having pouches branching out in every direction, the glass-like contents always becoming finely-granular and opaque, contracting on the application of acetic acid, and coagulating on that of an alkali, and becoming coarsely granular. These gelatinous masses appear to be, in fact, a peculiar degenerating cartilage-material. The various forms assumed by them are tear-like, carrot-like, biscuit-, club-, bottle-like, also cactus-like, and linear. It is difficult, however, to make out whether these varied forms result from a melting down, as it were, or a budding outgrowth. Sometimes a brood of young nuclei or complete cells, and even another vitreous body, is included, and in some cases under the structureless membrane lie fine fibrous lines in the larger pouches, and where blood had been seen by the naked eye, the mother cells are seen to contain blood-corpuscles lying between the vitreous body and the smallest cells, but never in the vitreous bodies.

Degeneration of this form of tumour appears to take place in various ways. The vitreous pouches may shrink to a yellowish-brown wrinkled structure, or there may remain simply a branched anastomosing linear or slightly-bent fine fibrous tissue; or fatty and calcareous matter may form in the vitreous pouches, the tissue becoming crumbly, and the calcification spreading over the entire mother cell. Softening and extravasation sometimes produces a bluish-red soft mass, and at times yellow and black pigment-granules are found, the result of old degeneration. Of this kind of tumour the prognosis is not good, and local malignancy and a return after extirpation may be expected.

In an appendix to the author's paper, it is stated that Billroth had given the name of Cylindraria to this species of tumour.

As to the exact character of the glass-like, club-shaped vegetations, Meckel himself looks upon them as structures analogous to cartilage; Busch takes them for vessels, whilst Billroth considers them to be structureless areolar tissue.

Heart, Fibrous and Fatty Formation in.—Albers, of Bonn,* relates a case of this. The patient, aged seventy-seven, died of disease of the sternum and thoracic vertebræ. At one part of the left ventricle, the pericardium was adherent to the surface of the heart, which at this part was knotted, showing

* Virchow's Archiv, p. 218. Sept. 1856.

a tumour of a yellow colour projecting over the surface, and occupying the thickness of the wall at the apex of the heart. It was of the size of a hen's egg. A very similar tumour existed also in the bulbus aortæ.

The larger tumour at the apex of the heart was quite separate from the neighbouring structures, no transition existing, but still there was no sac between the growth and the muscular tissue of the heart. Section of the growth showed glistening, wavy-striped tissue, enclosing spaces not altogether unlike the tissue of a fibrous growth. On microscopical examination, rounded capillary-fibres were seen, cleared by acetic acid, running in a tufted direction, and returning in an arched form, having about them here and there cells like nuclei. In the other parts, great numbers of fat cells were seen. Bloodvessels were also visible, passing into the tumour in every direction. No fatty or unhealthy muscular fibre of the heart surrounding was seen.

The second tumour—that in the bulbus aortæ—showed as to its outer wall more fat cells and less fibre than the other one. This outer wall formed a kind of shell around a softening mass of fat, cholesterine and granular material. Some parts, which were exceedingly hard, contained calcareous matter.

NERVOUS SYSTEM.

Collonema of the Brain.—E. Wagner* describes an instance of this formation, which was of about the size of a walnut, and situated in the neighbourhood of the corpora quadrigemina. It had a delicate, thin investment, and it was of a gelatinous consistency. On section it had a yellowish, shiny appearance. When examined microscopically, it showed a soft amorphous, finely-granulated appearance, mixing with water very slowly, and on the addition of acetic acid became thready and lumpy, whilst it became quite fluid on the addition of potash solution. It contained also large numbers of areolar tissue-like corpuscles, of which many possessed projections, whereby they anastomosed together, and also many colloid globules.

On the Texture of Neuroma.—Professor Schuh† adverts to the general view that only medullary sarcoma and neuroma (the latter showing the nature and structure of gelatinous sarcoma, as they have been termed) are developed in nerve as pseudo-plasmata, and relates a case of so-called neuroma of the ulnar nerve which proved to have all the characteristics of what he calls fasciculated cancer (*Bündel-Krebs*). He depicts the characteristic differences between gelatinous sarcoma and the fasciculated cancer, which in many points are undistinguishable. The case which he describes was that of a woman, aged forty-three, who had received a blow on the finger by the fall of a piece of wood nine years before: the effects of the blow passed off; but after seven years, she experienced attacks of pain in the two smallest fingers of the hand, lasting only a short time. In another year's time, intense pain in all the hand and fore-arm came on. This passed away; but in a few more months, along with pain, a small tumour on the inner side of the right upper arm, lying on the coraco-brachialis muscle, appeared. This grew very rapidly to the size of a large apple, and at the extreme ends of it a string-like body could be perceived connected with it, which was taken for a nerve. Fluctuation was doubtful, and very gentle pressure gave great pain, extending towards the hand, and specially the two little fingers. No pain existed without pressure in the tumour, though often in the hand. The two smaller fingers were kept extended, having a very high temperature and deficiency in sensation. The tumour was removed, and the wound healed well; the sensation and state of the temperature becoming normal in a short time. It proved to be soft, easily lacerable, full of blood,

* Virchow's Archiv, Band viii. p. 4.

† Zeitschrift der k. k. Gesellsch. der Aerzte zu Wien, p. 10. Dec. 1856.

and of a greyish-red colour. When torn, the surface presented a fasciculated appearance, as far as could be judged in a structure so softened. The microscope showed round and oval nuclei, with glittering nucleoli and cells of various sizes and shapes, besides many cells drawn out into fibres at both ends, having an obviously fasciculated arrangement. This constitution Schuh distinguishes from that of gelatinous sarcoma, in which, besides a fibrous or areolar structure, nuclei and cells exist, but do not observe any fasciculated direction.

SECRETING GLANDS.

Liver, Ulcerating Echinococcous Growths of.—Entozoa in the Portal Veins, &c.—H. Luschka* details a case, with minute microscopical observations. The subject was a man, aged twenty-four. The left lobe of his liver formed a cyst the size of a man's head, containing yellowish-green flocculent and purulent fluid. The inner surface, which was of a dirty green fatty nature, was roughened by large and small projections, and presented numbers of rounded openings. Its walls were of various thickness, and consisted of the thickened peritoneal covering, and a fibro-cartilaginous-looking, light-yellow, and firm material towards the cavity, having in it a vast multitude of small apertures, looking like the openings of so many canals. At the under surface of the liver, several large prominences existed, and deeply-extending knotted cords, as it were, in the parts where the lymphatic vessels exist, having the same appearance as the multilocular substance of walls of the cyst. The gelatinous material for the most part showed under the microscope slightly-plaited lamellæ of various thickness, and quite transparent. Moreover, great numbers of large and small spherical and branching hollow structures, with walls having the appearance of the lamellæ, could be extracted uninjured from the canals. The contents of these hollow structures, which were nothing else than echinococci possessed of projections, varied greatly, the lamellæ being remnants of the same. The contents were mostly granular, becoming fatty, and particles of biliary material or hæmatoid crystals. In very few cases the echinococci embryo, with a complete circle of hooklets, was to be seen. Moreover, here and there in the twigs of the portal vein, bodies of the size of a hemp-seed were to be seen, with a thick concentrically-laminated wall, from the inner side of which, in many places, small elevations rose, consisting of pedicles, elongated, and passing into variously-branched projections, of which the smallest were club-shaped. These could be seen to contain a cavity with finely-granular contents and a structureless wall. Some were bifurcated, and some were, as it were, only hung on by a single thread. No trace of hooklets was anywhere to be seen. These bodies doubtless showed the method of multiplication of the echinococci by budding.

GENITO-URINARY SYSTEM.

Carcinoma of the Bladder.—Dr. Lambl* gives ten cases of papillary cancer, scirrhus, medullary cancer, &c., of the bladder, much too long to be here detailed, which were diagnosed by a microscopical examination of the urine. He observes, that not only can the microscope discover organic and inorganic bodies in the urine, but that it is the only competent guide in doubtful cases of diagnosis. He passes in review those new formations of the bladder with which the practical physician is concerned. Excepting the ordinary hypertrophy of the mucous membrane, arising from the inflammation caused by calculus, or an obstructed urethra, &c., he describes the villous hypertrophy as being most

* Virchow's Archiv, p. 206. Sept. 1856.

† Vierteljahrsschrift für der Praktische Heilkunde, Band i. p. 1. 1856.

common, which exists in the form of soft papillæ and folds, like those in the intestine; after which comes the mucous polypus in the form of a pedunculated tumour. The papilloma or papillary tumour forms on the one side a natural transition to the villous hypertrophy of the vesical mucous membrane, as it is on the other side with difficulty separable from the papillary cancer in many cases. By Rokitansky and Schuh these two are classed together. Virchow and Förster distinguish them the one from the other; and the author agrees in opinion with the two last, considering that they have nothing in common but the outer form. Papilloma exists in the mucous as well as the serous surfaces of organs, and to a certain extent may be seen on the endocardium as tufted or pencil-shaped growths from the aortic valve-flaps. A malignant aspect, according to the author, may exist as regards these tumours, without their being really considered malignant, and this is to be found rather in the want of assimilation of the textural type to the original tissue, and the proportionate production of cells deficient in persistent or higher textural capacity, which may be the case with papilloma as well as papillary cancer. The author suggests that the physician would regard rather the uræmia and other hæmaturia, which are the most malignant symptoms of all the new formations. In medullary cancer we have an abundant reproduction and dissemination, and also the projection through neighbouring organs. The author then alludes to tuberculous formations and serous cysts, which he had never seen himself; as also colloid cysts, with yellow honey-like contents. He speaks of five distinct forms of prostatic hypertrophy.

MISCELLANEOUS SUBJECTS.

Corpora Amylacea, and also peculiar Blue-coloured Bodies in the Lung. By M. Friedreich, of Wurzburg.*—These bodies were chiefly oval or round, but in places angular in form, and in many cases showed a central nucleus, having the concentric layer disposed around it. Occasionally, the central nucleus was of the form of round granular aggregations, of various sizes, and differed from the surrounding layers, not only by reason of its sharp outline, but also in its chemical nature. Very often the central nucleus was formed of a pigment mass, black, or of a lighter or darker brownish-red, which was in some places amorphous, and in others existed as a crystalline body of large size. The great variety in appearance depends doubtless upon the age of these bodies. Those most clearly concentric in arrangement were obviously the most recent. Those of greater age had lost their concentric appearance, and gradually assumed a yellowish-white or aqueous-looking, wax-like, glittering appearance. This change begins towards the centre, and passes peripherically. The next change which these bodies undergo is the assumption of a punctate granular character, beginning at the middle and gradually encroaching over the entire body. Very often a radiating splitting or fracture of the central part indicates great age, which may be so great that the whole body falls to pieces. Nevertheless, new laminated formations round the old and decrepid bodies may take place. The size of the bodies varied from 0·06 to 0·08 or 0·01 millimètre as a rule, but here and there exceptional oval ones were seen, varying in diameter from 0·11 to 0·12 millimètre. The author then enumerates the various re-actions produced in these bodies by the addition of iodine, iodide of potassium, chloride of zinc, sulphuric acid, &c. Acetic acid made them clearer and more transparent, and more obviously concentric. Ether and alcohol made them swell up, and brought out the concentric rings remarkably, favouring also the tendency to splitting before spoken of. Alkalies did not materially alter these bodies, but concentrated sulphuric acid dissolved them. In one case, the attempt to convert the starch granules into sugar artificially was unsuccessful.

* Virchow's Archiv für Path. Anat. und Phys., p. 613. June, 1856.

The presence of corpora amylacea in the lungs was first noticed by the author in July, 1855, in a man, aged sixty-two, who died of pericarditis, with secondary hæmorrhage into the lung tissue, and pneumonia. In the mass of pneumonic infiltration, numbers of these bodies were found, six or eight being seen at a time under the microscope. They were strictly confined to this part of the lung. In another case—that of a man, aged seventy-five, who died of diseased heart, hydrothorax, and compression of the lungs—these bodies were found existing throughout the entire lung: so numerous were they, that as many as ten were found in the field of the microscope at the same time. They were also found in the mucous secretion of the bronchial tubes, so that they would no doubt, if looked for, have been found in the sputum during life. The bronchial glands were natural. The author imagines that these bodies may be connected with the elements of the blood, and possibly with the extravasation of blood.

Friedreich has also more recently* communicated another instance in which these bodies were found in the lungs of a woman, aged forty-three, subject to epilepsy, who had diseased brain and heart. The lungs were in a state of induration, and in many places were of a brownish-red colour, showing what is generally called pulmonary apoplexy. The upper parts only of the lungs contained air; and some of the branches of the pulmonary artery, in the indurated and apoplectic parts, were plugged up by adhering coagula. The epithelium of the air cells was almost all occupied by yellow or yellowish-brown diffused pigment-granules; and the bloodvessels between the air cells, &c., were beset by brown granular pigment. In the infarcted parts the corpora amylacea were found. They differed in form and size from those mentioned in the former case. Some were oval, measuring 0·15 millimètre, and had large nuclei. In one of these formations the central body was granulated, long, and twisted in a serpentine form; in another it was large and clumpy, with a sharp outline, and becoming yellow on the addition of iodine, whilst the surrounding laminæ became blue. In some cases the central bodies were many in number, but yielded the same reaction as the peripheric parts. In one instance, the central body was perforated by a smooth opening. But besides the above variety, another, totally different chemically from the corpora amylacea, was found, but exhibiting a like form to them. They were mainly round, and of various sizes, with central depressions, from which passed out radii towards the circumference—some of these being distinct clefts, causing the bodies at times to split into segments. Some had the appearance of a number of pins joined, and squeezed tightly together at their centres. The peculiarity of most of them was that, after being a little time under the microscope, they assumed by degrees an evident blue colour, which became gradually intensified, so that in a quarter or half an hour they were of a bright and deep blue colour. After much examination, it was obvious that the blue colour was obtained by the oxidation of the bodies, owing to contact with atmospheric air. By Professor Scherer, these bodies are supposed to be a form of phosphate of iron. On the addition of sulphide of ammonium they became black, and yellowish-brown on the addition of soda and potash. No change was produced by sulphocyanide of potassium or acetic acid, but they were dissolved by sulphuric acid and nitric acid, a pale outline being left. Natural iron-blue (vivianite) gave the same results when tested, and to a certain extent showed similar forms as these bodies, but was wanting in the pale organic framework left after dissolution. These same bodies were seen to beset the minute bloodvessels of the interstitial tissue of the lungs, in heaps of fifteen or twenty; and in all cases, the longer the lungs were kept, the rarer the bodies became; whilst, on the other hand, the triple ammoniaco-magnesian-phosphate crystals increased in number. The author supposes that the bodies described by Black as being found in some cases of tubercular sputum,† were of the same nature.

* Virchow: Arch. f. Path. Anat. u. Phys., p. 201. Sept. 1856. † Ed. Jour., part ii. p. 129.

HALF-YEARLY REPORT ON PATHOLOGY AND MEDICINE.

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I. *On Cerebral Abscesses.* By Professor LEBERT. (Virchow's Archiv für Pathologische Anatomie und Physiologie. Band x. Hefte 1, 2, 3.)

PROFESSOR LEBERT observes on the importance of a minute investigation of each form of disease that affects the brain, and draws attention to the fact, that abscesses of that organ have not as yet met with that consideration which they merit. He has observed five cases himself, and has collected a large number reported by various authors, the analysis of which yields the following results:

Of the 80 instances collected, 22, or above a quarter, presented scattered abscesses in various parts of the brain; the remaining 58 were cases in which solitary abscesses were found in some part of the encephalon. These were distributed as follows:

Left hemisphere . .	23 cases.	Cerebellum	12 cases.
Right hemisphere . .	18 „	Pituitary body . . .	2 „
Corpora striata . .	2 „	Medulla oblongata . .	1 „

In the cases of multiple abscesses, there were never more than five. In 11 there were two; in 6, three; in 3, five; in 2 cases the number was not specified. The abscesses generally occupy the white substance, only affecting the grey matter by extension from the former. The author points out the peculiarity of the fact that suppuration prevails in the less vascular white matter of the brain, while the morbid condition most prevalent in the grey matter is softening. The form of cerebral abscesses is generally oval; they vary in size from that of a pea to that of a hen's egg and more. An entire hemisphere is at times found converted into a pouch filled with pus. When a communication is established with one of the cerebral ventricles, the form becomes very irregular. The contents are generally a greenish pus of considerable density, rarely containing blood. In 18 cases the pus is reported as having been very fetid. The microscope shows the pus to be very granular, and not containing many well-formed pus-corpuscles; the older the abscess, the more they seemed to be retrograding. The abscess is at first surrounded by cerebral tissue infiltrated with pus, beyond which the cerebral tissue is softened, and, if the abscess is very recent, presents a red zone of vascular injection. Plastic exudation soon forms a membranous sheath, which may attain a thickness of from one to four millimètres. The cyst itself is supplied with bloodvessels, and thus helps to promote suppuration. It does not appear that these encysted abscesses can be cured; at least, no evidence can as yet be offered to prove it.

With regard to the duration of the affection, it appears, from an analysis of 18 cases, in which the period was noted, to have been as follows:

From 10 to 20 days . .	1	From 40 to 50 days . .	3
„ 20 to 30 „ . .	2	„ 50 to 60 „ . .	5
„ 30 to 40 „ . .	4	„ 60 to 90 „ . .	2
From 90 to 120 days . .	1		

In many cases there was no indication as to the duration of the disease; in others, only the acute symptoms which closed the scene were considered, though a chronic stage had evidently preceded their outbreak for a longer or shorter period.

Professor Lebert next considers the question of the rupture of an abscess, and its communication with other parts. Perforation or abnormal communications were found to have occurred in 12 cases. The lateral ventricles are the parts into which perforations most frequently occur; the presence of pus causes inflammatory thickening of the ependyma, and scattered spots of inflammatory softening in the adjoining cerebral tissue. In 5 cases the perforation was effected through the ear or the orbit, and the abscess was discharged externally. One of these cases is related by Itard, in which the internal ear is said to have remained healthy, though the cerebral discharge made its way outwards through the petrous portion of the temporal bone.

Professor Lebert analyses the histories also, with a view to determining whether any uniform lesion of other organs accompanies abscess of the brain. This does not seem to be the case; the general conclusion appears to be, that what debilitates the individual causes a predisposition to this affection. In 6 cases, pyæmia supervened distinctly; 3 cases in which it is suspected to have occurred, are not accompanied by sufficiently detailed necropsies to justify a positive statement. A complication with tubercular disease was only noted three times, in one case affecting the cervical, bronchial, and mesenteric glands, in another the mesenteric glands only, and in a third the bronchial glands were tubercular, while the lungs were full of miliary tubercle.

The most frequent cause of cerebral abscesses is internal otitis; this in its turn often resulting from scarlet fever, angina, or scrofula. Cerebral abscesses also occur as sequelæ of inflammations of distant parts—as of pneumonia, pericarditis, enteritis, or of measles. They also occur in the form of metastatic abscesses, associated with chronic diseases which appeared to exercise no definite influence in their production, and as a result of traumatic injury.

The latent character of the disease is important in regard to diagnosis. Sudden headache is the symptom which most frequently first excites attention; it is generally accompanied by febrile symptoms; vomiting, difficult articulation, and convulsive attacks may supervene; the patients become heavy and morose, and show delirium, contraction of pupils, photophobia; numbness and formication may supervene, and apoplectic symptoms may occur; but all these symptoms vary much in different cases. The intellect suffers comparatively little; sensibility suffers more frequently: the headache is more or less intense, generally diffuse at first, and subsequently unilateral. Coma occurs frequently, but often only temporarily. Paralytic states were observed in about one-half of the cases; they were generally local, but showed themselves also in the form of general muscular debility. Diminished articulating power was observed in 10 cases. In regard to the special senses, only the affection of the ears presents any points of importance. No special symptoms are observed in reference to the vascular or respiratory system. Disturbance of the digestive organs showed itself in the form of vomiting in 20 cases; involuntary defæcation occurred towards the fatal termination of 11 cases. The duration of the disease appears to fluctuate from two or three weeks to two months; there is necessarily a difficulty in determining the point, as the commencement can only be approximately fixed. It occurs at all ages; but the greatest frequency prevails between the sixteenth and thirtieth years.

On the subject of treatment nothing is suggested, as no case of cure is known. The author especially protests against adoption of any surgical proceeding for the purpose of removing the contents of the abscess.

II. *On the Hæmatoma of the Dura Mater.* By Rudolph Virchow. (Verhandl. der Physikalisch-Medicinisch. Gesellssch. zu Würzburg. Band vii. Heft 1.)

Virchow is of opinion* that the sanguineous cysts found at the inner surface of the dura mater, and interpreted either as effusions on the free surface of the

* The view propounded by Prof. Virchow is not entirely new. It was already suggested by Bayle; cf. Traité des Maladies du Cerveau et de ses Membranes, p. 256. 1826.

dura mater, or as effusions between its layers, or between it and the arachnoid, should be attributed to chronic inflammation of the dura mater. The author recommends the adoption of the term pachymeningitis to designate inflammation of this membrane. In the variety affecting the free surface, which he does not regard as being invested by a serous covering, we find in recent cases very fine layers of fibrin spread to a greater or less extent over the dura mater; and these are generally accompanied by small extravasations, which are converted into pigment. By the repetition of the inflammatory process, numerous layers of fibrin become deposited one upon the other, and much more numerous and larger vessels form in these layers than are to be met with in the dura mater itself. To these new-formed vessels, Virchow attributes the hæmorrhage which gives rise to the formation of the hæmatoma, the cyst being formed by the extravasation taking place between the layers of the false membrane. Virchow observes, that an examination proves the cyst to consist of a new formation, and that neither the dura mater itself nor the arachnoid forms one of the parietes; moreover, the exudation contained in the cyst is always more recent than the surrounding membrane; the blood being partly coagulated and partly fluid, with well-formed corpuscles. The disease itself is very chronic, but terminating after continued cephalic suffering, suddenly, with symptoms of apoplexy.

The hæmatoma may attain a considerable size; it may be from four to five inches long, by two and a half inches broad, and one-half to three-quarters of an inch thick. It is of a flattened, circular form, with a central elevation. The long diameter is parallel to the falciform process; it generally occurs on one side only, or if bilateral, one is more developed than the other. The affection appears to occur only in the adult, and generally after the age of fifty. Inflammation of the dura mater occurs very frequently in idiots, and often gives rise to hæmatoma. A curious case is mentioned by Virchow, in which a large hæmatoma of the right side was accompanied by hemiplegia of the same side, in a man aged thirty-nine, who died apoplectic.

III. *On the Sounds Perceptible about the Head and at the Upper Portion of the Spinal Column in Children.* By Dr. Hennig, Director of the Policlinique for Children in Leipsic. (Vierordt's Archiv für Physiologische Heilkunde. Jahrg. 1856. Heft 3.)

At the suggestion of Dr. Hennig, Dr. Wirthgen,* in 1855, wrote his inaugural dissertation on the subject of the present paper, which contains the results obtained by both observers. They are shortly as follows:

1. If the ear be applied directly, or with the intervention of a stethoscope, to the anterior fontanelle of a child, taking care not to exercise a painful pressure, sounds are heard. They may sometimes be heard at the posterior fontanelle, along a line drawn from the anterior fontanelle to the temporal fossa, and, though less frequently, along the sagittal suture. Occasionally sounds are audible over the entire cranium.

2. The sounds are perceived from the twentieth week of life to the sixth year, at all times of the day, in all positions, and whether the child be asleep or awake.

3. There are two classes of sounds: those that are heard during the first period, up to the third or fourth year, are always of a blowing character, and always intermittent; those heard later are more limited, and have a double stroke, analogous to the sounds of the heart. In addition to these sounds, the respiratory murmur may be recognised in auscultating the head; it is less frequent than the blowing sound above mentioned, but is more enduring, the ratio of frequency being in health, in the infant, as 4 : 1.

* G. Wirthgen: De strepitu qui in Capite auscultando auditur. Lips. 1855.

4. Dr. Hennig attributed the intermittent blowing sound heard at the large fontanelle up to the time of its closure, to the pressure exercised by the arteries upon the cerebral sinuses.

5. The blowing is the louder the more advanced the process of ossification while the fontanelle continues open; the more the muscular system of the body is developed, the stronger the cardiac impulse, the more elastic the vascular parietes, and the more the blood resembles that of chlorotic subjects.

There is an apparent contradiction in the latter part of the last sentence, which is not cleared up by what follows; but we prefer giving the author's words to making any substitution, which would fail to convey what he actually says. He continues:—The blowing murmur diminishes if the child is reduced in strength, if the cranial bones are soft and thin, while the fontanelle continues open; it diminishes in healthy children when the orifice is actually closing; the same is the case when the child recovers from hydræmia, and at the commencement of conditions which cause an abnormal elevation of the cranium. The sound entirely disappears when the cranium is perfectly closed; it does so in very feeble atrophic children; in acute hyperæmia and extravasation within the cranium, or if the longitudinal sinus is choked up with coagula.

Dr. Hennig observes, with regard to the application of auscultation to the diagnosis of cerebral disease, that although it does not serve to distinguish the individual morbid condition, it assists in defining the general character of the disease. If the child is vigorous, the cerebral murmur will disappear at the proper period of development; it will be absent in hyperæmia of the brain and the meninges, in sanguineous extravasation, in acute hydrocephalus, in extensive encephalitis, and in cerebral and meningeal tuberculosis, especially if accompanied by meningitis or serous effusion, as is usually the case.

IV. *Narrowing of the Foramen Magnum Occipitis and of the Spinal Cord, by the formation of Bone.* By Dr. Arthur Willigk. (Prager Vierteljahrsschrift, 1856. Jahrg. xiii. Band 3.)

Dr. Willigk remarks on the extreme rarity of osteophytic growth in the vicinity of the foramen magnum, which he attributes to the fact, that diseases of this part generally run so rapid a course as to prevent the formation of bone in a deposit. Among several thousand autopsies, he has not seen a single case of the kind. In cases of extreme osteophytic growth affecting the whole cranium, the margins of the foramen have been found to remain perfectly smooth, showing an apparent immunity of the part. The cranium which Professor Willigk describes, was found by him in the Museum of Olmütz. A delicate osteophytic growth existed on the inner surface of the frontal, sphenoid, and right temporal bones. The anterior part of the foramen magnum was much reduced. A space included between the anterior part of the condyles and the front margin of the foramen was filled with new bone; this extended along the left margin of the foramen backwards to the extent of three centimètres (about an inch), and sent down a styloid process slantingly into the spinal canal. The surface was compact, here and there exhibiting large pores. The growth was of that kind, that movement must have been impossible.

V. *The Production of Reflex Action a Means of Diagnosis.* By Dr. A. Stich. (Annalen des Charitékrankenhauses. Jahrg. vii. Heft 1.)

Assuming the correctness of the doctrine, that irritation of a nerve of sensation produces motion only if the spinal cord co-operates, and that the excitability of the central organ is increased if the communication between the nerve of sensation and the brain is interrupted, Dr. Stich applies the doctrine in cases of paralysis, to determine whether the lesion be one of peripheral or centric origin; the presence or absence of reflex action on the application of chemical or mechanical stimuli being the best. Thus, in the case of a woman who was delivered with the forceps, paralysis, both of motion and sensation, affected her left leg, and the affection was attributed to the pressure exerted upon the nerves during her confinement. But when, according to Dr. Stich's proposition, the foot was placed in hot water, the patient felt no heat, but some spasm was visible in the limb, showing that the impediment to conduction was not peripheral, but centric, as the communication between the nerve of sensation and the cord was evidently uninterrupted. That the latter view was correct was shown by the course the disease took, as it ended in complete anæsthesia of the entire left side, and deranged mobility of both sides.

Dr. Stich applies the method to determining, in anæsthesia of mixed nerves, whether the sensory root is diseased, or the centric origin of the nerve. If in such cases, on applying irritation, reflex action results, we may safely conclude that the sensory root is not the part diseased.

The author also proposed to employ the method when, in the case of disease of the spinal cord, we desire to determine the extent of the lesion which prevents conduction; inasmuch as hitherto we have been able only to define its boundary in the direction of the cerebrum, we may now have it in our power to determine the lower boundary towards the cauda equina. Dr. Stich is of opinion, that by this mode of procedure we may be enabled to determine the existence of several detached diseased spots in the cord, though he has not as yet satisfied himself of this experimentally.

The interruption of the conduction from the spinal cord to the brain is a circumstance which deserves our consideration in reference to most forms of spasmodic disease; many of these run their course without being productive of pain; an impression is conducted by a sensory nerve to the cord, but is not propagated to the cerebrum; hence the absence of pain. The author argues that these affections may be regarded as affording support to his views. He appears here to enter upon very debatable ground.

VI. *Report upon Two Hundred and Eighty Cadaveric Inspections.* By Professor Buhl, M.D. in Munich. (Henle und Pfeuffer's Zeitschr., Neue Folge. Band viii. Heft 1.)

The post-mortems upon which Dr. Buhl reports, were, with the exception of three, performed during the period included between the 12th November, 1854, and 12th August, 1855; 209 were bodies of patients who had died in the General Hospital at Munich, 19 belonged to the Polyclinique, and 49 were private patients of Munich physicians.

We extract from this very interesting and able Report some of the author's remarks on the subject of pneumonia. He describes three forms of this disease: croupous pneumonia, tubercular diphtheritic pneumonia, and a third, to which he applies the name of desquamative pneumonia. Croupous pneumonia is universally described as presenting the two stages of red and of grey hepatization; in the former stage, the lobes, or part of them, are uniformly enlarged, increased in size and weight, are deprived of all air, more friable, and, when cut, granular and dark red. The colour is due to hæmorrhage into the pulmonary vesicles and bronchules. This stage is preceded by disease of the coats of the vessels and vesicles which induces their rupture, as well as an exudation of fibrin, and which the author defines as an acute derangement of their nutrition. During the second stage the colour is converted into a reddish-grey, or grey; the friability increases; the microscope exhibits a large number of pus corpuscles, the epithelium is converted into a molecular detritus, and suppurative destruction or chronic induration may result.

The tubercular diphtheritic variety differs from the former in not affecting a portion of pulmonary parenchyma continuously, but in lobules only; it is analogous to the second stage of croupous pneumonia, but the parts affected are absolutely deprived of blood, and exhibit increasing dryness; while in grey hepatization the parts are only partly deprived of blood, and manifest increasing moisture or succulence. Molecular infiltration, molecular disintegration, with necrotic destruction of the pulmonary vesicles and bronchules, characterize the former; while in the latter we meet coagula containing pus corpuscles, liquid pus, and even abnormal contents in the vesicles. The author regards the morbid process in the diphtheritic variety as based upon an *arrest* of all nutrition; while the croupous form he attributes to a *disturbance* of nutrition.

The third form of pneumonia, the author states, occurs only as red hepatization. The affected lobules are less increased in weight and size than they would be in the croupous variety; the cut surface is of a uniform red colour, and presents shades of a livid, brownish, or, less frequently, blackish-red colour; it is smooth, the tissue deprived of air, and of elasticity; it is tough, and but slightly friable. There is an almost entire absence of extravasation and fibrinous exudations; the air is expelled from the vesicles by loose granular epithelium, which is surrounded by an albuminous exudation. As the disease advances, the tissue, without recovering its elasticity, becomes turgescent and filled with blood; the epithelium undergoes molecular disintegration, and discharges its nuclei, or is converted into fat granules. The residue of the epithelium and the fluid in which it lay is gradually absorbed, and the vesicles either collapse or again expand to the pressure of the air. The lining epithelium is regenerated, probably after repeated desquamation, but the tissue does not recover its elasticity for a long time. During the later periods of this variety, or during protracted convalescence (and sometimes earlier, if the pneumonia was very severe), the epithelium presents fatty degeneration, which may be even recognised by the naked eye.

Dr. Buhl denies that these varieties of pneumonia are stages of the same disease, but admits that they may co-exist in the same individual. He is of opinion that the desquamative form is frequent, and from its greater tendency to recovery, less liable to present itself to us in the dead body.

VII. *A new Case of Sarcinæ in the Lungs.* By RUDOLPH VIRCHOW. (Archiv. für. Pathol. Anat. und Physiol., Band x. Heft. 3.)*

In the lungs of a shoemaker, aged thirty-three, who died of phthisis in 1856, Professor Virchow found extensive cavities in the right lung, not confined by a false membrane, but surrounded by the lung tissues in a state of pulpy degeneration. This tissue, when examined by the microscope, was found to consist almost entirely of sarcinæ, with a small admixture of portions of the parenchyma and a small quantity of extravasated blood. If a portion of the lung tissue, which retained its consistency, was spread out under the microscope without being washed, it was found thickly beset with sarcinæ. The reaction was alkaline.

VIII. *On Œdema Glottidis, resulting from Typhus Fever.* By THOMAS ADDIS EMMET, M.D. (American Journal of the Medical Sciences, July, 1856.)

Dr. Emmet draws attention to two forms in which œdema glottidis occurs as a secondary affection of typhus, either as a result of a reactive ulceration of the mucous membrane of the air passages, in consequence of typhous deposit,

* A similar case was published by Prof. Virchow, in the Archiv, Band ix. p. 574. He terms it, pleonastically, pneumonomycosis sarcinica, from μύκος, a fungus.

or as a result of the debilitated condition of the patient alone remaining after the subsidence of the primary disease; in the former variety the infiltration takes place slowly; in the latter, with great rapidity, so as to cause almost instantaneous death. Dr. Emmet supports his views by cases. He is of opinion than œdema glottidis is more frequently the cause of the fatal issue of diseases than is commonly supposed. With regard to the performance of tracheotomy or laryngotomy, it is not advisable at all in the cases of laryngo-typhus, since in every fatal case of this affection, bronchitis was found to co-exist; a more favourable issue may be expected to follow where it is performed for simple œdema. The relative frequency of œdema of the glottis as a sequel of typhus, may be gathered from the fact, that out of 1931 cases of typhus, 23 presented the laryngo-typhous, 7 the simple, form of œdema glottidis.

IX. *Two Cases of Thoracentesis, performed for Acute and very considerable Pleuritic Effusion, and followed by Recovery.* (L'Union Médicale, tome x. Nos. 147, 148.)

These cases occurred at the Hôpital St. Antoine, under the care of M. Aran. The patients were men respectively of the ages of twenty-six and thirty-nine years. In the first, the pleurisy affected the left side, and the effusion was so considerable as to force the heart above an inch (three centim.) beyond the right margin of the sternum; in the second, the right side was affected, and the heart was pushed over to the left, so that the heart-dulness only commenced at the left edge of the sternum. The dislocation of the heart forms one of the chief sources of the danger accompanying pleuritic effusions, and may therefore be regarded as an argument in favour of paracentesis. Paracentesis was accomplished in the former case a few days after the patient's admission to the hospital, when he had been about four weeks ill. One thousand two hundred grammes (above twenty-six ounces) were evacuated; the immediate relief was great, and an entire recovery followed, so that he was discharged cured three weeks after. In the second case, the operation was performed four weeks after the commencement of the illness, and a week after the patient's admission. The amount of fluid evacuated was 2500 grammes (about fifty-five ounces). A fortnight after, the patient is reported to be doing perfectly well, being retained in the hospital simply as a matter of precaution.

In neither of the cases was there much fever on the day on which the puncture was made; the pulse was eighty-four in the first, sixty in the second patient; the former presented twenty-four, the latter twenty, respirations in the minute. They had some appetite, and probably neither patient considered himself dangerously ill; still, the extent of the effusion left no doubt that their malady was a very serious one. The recovery was the most rapid and complete, as regards the expansion of the compressed lung, in the second case—still, in both the lung that had been affected, was restored nearly to the normal condition. The first at his discharge is reported to have retained only a slight dulness, with a somewhat feeble respiratory murmur on the left side; while the second, eight days after the operation, presented nothing but a slight diminution of the respiratory murmur at a circumscribed spot at the lower and outer part of the affected side.

X. *On the Diagnostic Value of the Symptoms commonly regarded as Indicative of Pulmonary Cavities.* By Dr. N. Friedreich. (Verhandlungen der Physikalisch. Medicinischer Gesellschaft in Würzburg. Siebenter Band, Heft 1.)

The cracked-pot sound, the tympanitic percussion-sound, the amphoric and metallic respiratory sounds, are in this paper examined in relation to the diagnosis of pulmonary cavities. We recently drew attention to Professor Bennett's* observations on the occurrence of the cracked-pot sound in various conditions unconnected with cavities. Dr. Cockle has also† shown that it may occur in cases of simple bronchitis. Dr. Friedreich gives three cases of pleurisy in which this sound was met with. In the first (a man, aged twenty-two) it occurred in the left infra-clavicular region, at the time when the effusion on the same side was receding, and it lasted until its complete absorption. In the second (a man, aged twenty-two), the sound occurred from the commencement of the affection, and whether the nose and mouth were open or closed, in the left infra-clavicular space, as far as the third rib, to which the pleuritic effusion reached. It disappeared before any change in the exudation was perceived. In the third case (a man, aged twenty-three), the *bruit de pot-fêlé* was produced, the mouth and nose being open, at the upper left side, down to the third rib, at which point the effusion commenced. The patient was still under observation when the paper was written. With regard to the occurrence of the sound in healthy subjects, Dr. Friedreich has failed to discover it in the adult, but on examining forty-six children under fourteen years of age, he met with it twenty-six times—fourteen times audible on both sides anteriorly, but only in five equally loud—in the other cases, generally louder on the left than the right side, and only twice louder on the right than the left. In explaining the production of the cracked-pot sound, Dr. Friedreich opposes the theory that it is due to air being forcibly expelled through the glottis, because on applying the stethoscope to the larynx, while another person produces the sound, no indication of its formation at the glottis is obtained. In bronchitis and early infancy he believes the production of the sound to be due to the compression of the smaller bronchi during the act of percussion. He adopts Skoda's theory of its production in phthisis, while in pleurisy he attributes it to compression of the pulmonary tissue by the exudation, and the forcible expulsion through the smaller bronchi of the air contained in them, when percussion is employed.

XI. *A Case of Paracentesis of the Pericardium.* By Professor Trousseau. (L'Union Médicale, Oct. 7, 1856.)

A young man, aged twenty-seven, was admitted in the Hôtel Dieu on the 2nd of June, 1856, under the care of Prof. Trousseau, having been taken ill a few days previously with violent fever, and all the symptoms of capillary bronchitis. A few days later, a blowing murmur was heard at the apex of the heart, and at the end of a week there was a double murmur at this point, while a few days later still, the second sound of the heart was redoubled, so as to produce a triple sound, *the bruit de rappel* or *de galop* (– ⏑ ⏑́). The catarrhal symptoms diminished while the cardiac dulness extended, so as soon to leave no doubt of the existence of pericardiac effusion. The cardiac bruits became less distinct, and at last disappeared; the anxiety of the patient increased in the exact ratio of the increased effusion. The symptoms at last became so urgent, that, after a consultation, the operation of paracentesis of the pericardium was decided upon. The dulness extended upwards to the third rib, downwards somewhat below the base of the thorax, and laterally from about three-quarters of an inch to the right of the mesian line of the sternum, to about four inches to the left of the left nipple.

An incision was made with a bistoury in the centre of the circle of dulness, below the nipple, in the nearest intercostal space. The parts were successively divided with great care, until the operator reached the pleura. This membrane was then divided, and on introducing the finger, the distended pericardium was

* British and Foreign Medico-Chirurgical Review, p. 447. Oct. 1856.
† Association Medical Journal, July, 1856.

distinctly felt. This was first pricked with the point of the bistoury, and the opening gradually widened by a grooved probe. A large quantity of serosity, slightly reddened, escaped. About three ounces of this was collected in a porringer, and immediately set, like "gooseberry jelly." The fluid having ceased to run, the patient was placed on his left side, and about six ounces of a bright yellow liquid, quite distinct from the former, was discharged. It differed in colour, as well as in the more imperfect coagulation. The autopsy subsequently showed that the latter came from the pleural, while the former was derived from the pericardial, cavity. The total quantity of fluid discharged amounted to about four hundred grammes, or about twelve ounces. It was supposed that the existence of false adhesions prevented a more complete discharge. The patient was temporarily relieved, but in the evening eclampsia supervened, followed by convulsions, affecting the right side of the body only. On the following morning, the right side of the body was completely paralysed, as well as the tongue; still intelligence seemed to be preserved. The pulse was 160, but the respiration was not more difficult. Death ensued on the fifth day after the operation.

The post-mortem was performed with great care. The left pleura contained a citron-coloured liquid resembling that which had run out at the second stage of the operation. It contained no false membranes or adhesions. The pericardium resembled an enormous ball, of the size of a man's head; it was not adherent to the ribs. A purple spot marked the point where the opening had been effected in the pericardium. It was lined on a level with this spot with false membranes, coloured red. On opening the pericardium, one thousand grammes (about thirty ounces) of pale reddish serum flowed out, like that first obtained on making the puncture. It contained very few fibrinous flocculi. The heart, as well as the interior of the sac, was covered with a thick reticulated false membrane. The heart was somewhat increased in size, and the cavities were rather smaller than normally. The orifices were rather small, but otherwise healthy. Tubercles were found in the lungs, the pulmonary and abdominal glands.

XII. *Clinical Observations in the Franz-Joseph Hospital for Children in Prague.* By Professor Löschner. (Vierteljarsschrift für Praktische Heilkunde. xiii. Jahrgang, 1856.)

We extract from the above lengthy Report the following cases of rupture of the septum ventriculorum, as a result of endocarditis. The perforation took place at the point at which, as we have shown on a former occasion,* there is normally an absence of muscular tissue:

1. Simunek Carolina, a girl, aged four, had enjoyed good health till four days before admission to the hospital, when she was suddenly seized with rigors, loss of strength, and thirst. The skin was burning hot; on the following day was covered with diffuse redness, which disappeared after three days. The child did not improve; but as dyspnœa, swelling of the face and feet, supervened, the parents brought her to the hospital. Admitted on the 16th October, 1855, she presented traces of rhachitism, though generally well developed. The skin generally was œdematous, hot, and slightly desquamating; the tongue furred; some diphtheritic exudation in the fauces. Percussion was dull on the left side, from the third rib to the seventh vertically, and from the edge of the sternum to the axillary line laterally. The impulse of the heart was visible and broad, a distinct whirr being communicated to the hand when applied to the thorax. A prolonged blowing murmur intervened between the first and second sound, so that both sounds ran into one another; there was also extensive friction-sound. At the inferior parts of the lungs there were ægophony and

* Jones and Sieveking's Pathological Anatomy, p. 315.

very feeble respiration. Pulse 132, very small; urine scanty, not albuminous; bowels costive; dyspnœa. On the 18th, the dyspnœa continued very urgent, the œdema had increased, the blowing murmur continued, and the friction-sound was increased; pulse 140. On the 19th, the serous accumulation in the thorax had manifestly increased, as well as the dyspnœa; the urine contained a small quantity of albumen; cyanosis supervened; and in the morning of 21st, death relieved the patient. *Post-mortem:* The thorax was normal, without a trace of ricketty distortion. Both pleura contained several pints of serum; the lungs not adherent, excepting the upper lobe of the left lung; otherwise compressed and containing no air, but presenting no structural alterations. The pericardium much distended with serum; the heart enlarged in all its diameters, so as to resemble the heart of an adult; a yellow layer of recent exudation invested the base of the heart. All the cavities were much dilated, and the parietes considerably thickened, the right ventricle presenting a diameter of two and a half to three, the left of nearly six, lines. The curtains of the mitral valve were much thickened, but the auriculo-ventricular orifice was not diminished in size. A few milk spots were seen on the endocardium, and in the vicinity of the mitral valve a few spots of fresh exudation. At the upper part of the septum ventriculorum, where the muscular tissue terminates and the two endocardia join to form the entire septum, underneath the right and posterior valves of the aorta, there was an irregular orifice of the size of a bean, surrounded by an elevated margin: the inner angle of the tricuspid, with an additional exudation of fibrin, had almost converted it into a sac. The semilunar aortic valves were slightly roughened; the corpora aurantii much developed; the aortic bulb dilated. The other features of the post-mortem have no special bearing upon the main disease, so we omit them.

Professor Löschner points out that the perforation must have taken place some time before death, and yet did not produce the symptoms one would have anticipated from so serious a lesion; the immediate cause of the fatal issue being the scarlet fever, with its sequelæ. The author is of opinion that, previous to the fatal attack of pericarditis, accompanied by recent endocarditis, there must have been several attacks of cardiac inflammation. It certainly is difficult to believe such effects to have been produced, and the child to have remained sufficiently free from symptoms without attracting the attention of even superficial observers. It suggests itself whether the opening in the fibrous inter-ventricular triangle may not have been congenital, and whether the subsequent deposit of lymph upon its edges may not have induced the appearance of perforation.

2. Theresia Lokay, aged four, had never enjoyed sound health. When admitted into the hospital, Jan 1st, 1855, she presented a feeble habit, with a slight cyanotic complexion, a pigeon breast, feeble respiratory murmur, with frequent large and small mucous râles. The impulse of the beat was distinctly visible, and much increased in extent and force; a purring was perceptible to the touch. A blowing murmur was heard with the first sound over the ventricles; the second sound of the pulmonary artery was increased; the radial pulse very small—96. There was great restlessness; the nights sleepless. The cardiac action became more violent; the pulse more and more reduced. On the seventh day after admission, consciousness left her; convulsions supervened; the cyanosis became very intense; and on the ninth day, death ensued. The autopsy showed the cerebral ventricles much distended with clear serum, their parietes and the fornix pulpy. The ricketty thorax was completely filled with the lungs; the heart lay almost horizontally. The right lung contained numerous solitary and agglomerated nodules of yellow tubercle; but a few tubercular deposits were found in the left lung. The pericardium was universally adherent to the pleura; the heart also adherent to the pericardium, and much enlarged in the longitudinal and transverse diameters. The foramen ovale was patulous; the left ventricle hypertrophied; the free margin

of the mitral valve thickened, its tendons partially adherent; the fibrous interventricular triangle exhibited a perforation sufficiently large to permit the passage of the tip of the little finger: the perforation was covered in the right ventricle by the inner curtain of the tricuspid, which was attached to the margin of the orifice by exudation matter. The other curtains of the tricuspid were thickened. The liver was somewhat enlarged, and exhibited slight fatty degeneration; the spleen was tumid, and its capsule studded with tubercles which penetrated into the splenic tissue; the mesenteric glands swollen; the kidneys healthy.

Well-defined cases of inflammation of the muscular tissue of the heart belong to the curiosities of medical literature; we therefore add a brief abstract of the following case:

A girl, aged eleven, said to have been indisposed only for twenty days, was admitted on the 3rd March, 1856. She had been seized with loss of consciousness, great debility and apathy. She was well built, but very pale, with glassy eyes, yellow tongue, and a cyanotic appearance of the mucous membranes. Respiration irregular, but no marked derangement perceptible in the lungs by percussion and auscultation. The heart occupied its proper position; impulse feeble; the heart-sounds sharply defined; pulse irregular, varying from 60 to 100. The spleen enlarged. The vegetative functions impaired. The girl was almost unconscious, and very apathetic. During the night she became restless; spoke incoherently; the pulse became intermittent and almost imperceptible; and on the following morning, at eight o'clock, death took place. The main feature presented by the post-mortem was the condition of the heart: this organ was large—exhibited numerous ecchymosed swellings on the surface; towards the apex there was a spot of the size of a pea, filled with yellow exudation, and occupying the parenchyma of the heart; there were several other smaller spots at different parts of the heart, showing manifest myocarditis. The whole left ventricle was hypertrophic; the valves perfectly healthy, as well as the endocardium.

XIII. *Case of Dislocation of the Spleen, occurring suddenly, and running a rapid course.* By Professor HELM. With Remarks by Dr. KLOB. (Wochenbl. der Zeitschr. der k. k. Gesell. d. Aerzte. No. 37. 1856.)

B. G., a needlewoman, aged twenty-one, who had had ague two years previously, was seized on the 7th March with violent pain in the left abdomen: this increased in intensity; and on the following day she was admitted into the hospital at Vienna, where she stated that, over-night, a tumour had formed in her abdomen, which could be felt between the ribs and the left ilium, of the size of a child's head. The splenic region was sonorous. The slightest contact produced intense suffering, and the rapidly-increasing tympanitis soon prevented the possibility of feeling the tumour. There was great dyspnœa, but no apparent pulmonary or cardiac disease; the pulse very small; constant emesis. Death the same evening at seven, P.M. *Post-mortem:* Nothing of consequence was observed in the cranial and thoracic cavities. The distended abdominal cavity contained about ten pounds of a chocolate-coloured acid liquid, mixed with undigested food. The liver was pushed up, and anæmic. The spleen, quadruple the normal size, lay on the inner surface of the left ilium, its hilum directed upwards: it was torn from its connexions with the stomach and diaphragm, and hung by a pedicle which was formed by the vessels and the cellular tissue accompanying them, the pancreas and the ligamentum pancreatico-lienale. The spleen was twice rotated upon its axis in such a manner that the pancreas was turned spirally round the pedicle. The stomach was pushed up into the left hypochondrium, so that its posterior wall was directed forwards. Its coats were converted into a gelatinous, dark, reddish-brown, friable mass; and a space of the size of a dessert-plate, at the fundus, was completely diffluent.

In the observations on the case, it is stated that Professor Dietl details a similar case observed by himself, in the 'Med. Wochenschrift,' 1854; and quotes three cases in another paper contained in the same journal for 1856. All the cases hitherto observed have occurred in females. It is due to an increase of the volume of the spleen, when there is not a coincident increase in the strength of its ligaments.

XIV. *A Case of Varix Anastomoticus between the Splenic and Azygos Veins, accompanying partial Obturation and Ossification of the Vena Portæ; and severe Icterus caused by Gall-stones.* By Rudolph Virchow. (Verhandlungen der Physikalish. Medicinischen Gesellssch. Band vii. Heft 1.)

The case occurred in a man, aged sixty-six, of intemperate habits, who for about five months before his death suffered from icterus, with general prostration, but without symptoms indicative of any definite lesion.

At the autopsy, the ductus choledochus was found choked up with a biliary calculus. Several small ones filled the gall-bladder. The liver was reduced in size, granular and green throughout; there was general dropsy and an enlarged spleen. The vena portæ was almost entirely obliterated; close to the liver, at the main division of the vein, a hard mass of the thickness of the thumb was felt in it, and on dividing the vessel this appeared to be a plug of calcareous matter, which terminated in thin prolongations. On close examination, the mass proved to be pervious, and to be composed of the thickened internal coat of the vessel. As a result of this obturation, the vessels composing the vena portæ were much enlarged, and varicose. A collateral circulation was established by means of three communications between the splenic and azygos veins, so that a large portion of the portal blood went directly to the heart; the azygos was converted into a series of large sacs, and appeared to be twisted on its axis. Some of the sacs were 1·5 centimètre (nearly half an inch) in diameter. There was much calcareous deposit in the coats of the different veins.

Professor Virchow enters into an interesting disquisition regarding the nature and causes of the different appearances found in this case, for which, as well as for the details of the case itself, we refer the reader to the original. We merely add that the author regards the diseased condition of the vena portæ as the result of pressure exercised by the gall-stones occupying the ductus choledochus.

XV. 1. *On Addison's Disease.* By M. Trousseau. (Bulletin de l'Académie Impériale de Médecine. Tome xxi. No. 23, September 15th, 1856.

2. *On the Present State of our Knowledge regarding the Bronzed Skin-Disease of Addison.* By L. Danner. (Archives Générales de Médecine, Janvier, 1857.)

It is gratifying to find Dr. Addison's merits recognised by his distinguished *confrère*, M. Trousseau, who justly proposes that the browned skin-disease should be named after the discoverer. We willingly adopt the suggestion, and shall adhere to the term introduced into medical science by M. Trousseau.

After quoting the experiments of Brown-Séquard on the physiology of the capsules, M. Trousseau gives the details of a case which fell under his own observation. It occurred in a man, aged thirty, presenting the traces of early rhachitism, but who, with the exception of two hæmorrhagic attacks, had always enjoyed good health. Five months before admission he began to fall off in flesh, and found his skin turning brown. No washing would remove the colour. On admission on the 30th of July, his face and hands presented a much more browned appearance than the French soldiers show on their return from Africa; it had a slight tinge of blue. The remainder of the body was tattooed black, but the nipples, the arm-pits, and the genitals were darkest. The lips and gums were as black as those of a dog. The nails alone formed a contrast with the remainder of the body by their whiteness. A blowing murmur was heard in the large vessels; the blood (according to M. Robin) was that of a person slightly anæmic, and the urine contained a few pus-globules. There was slight pain in the renal region. Under the use of tonics he seemed to improve, but a state of general malaise and diarrhœa supervened about the middle of August, followed by extreme prostration, a loss of intellectual power, and incoherence. Death ensued on the 17th of August. The brain was found normal. The lungs were gorged with black blood, and contained a few tubercles at the apices. The liver was of normal size, but softened and of slate colour; the spleen enlarged and softened, of a reddish-black colour, and studded with minute white points; the kidneys apparently healthy, but the supra-renal capsules were transformed into large tumours nearly the size of hen's eggs, and studded with yellowish-white nodules resembling tubercle. Some are softened; they are united by a greyish fibrous tissue. M. Brown-Séquard, who made a microscopic examination of the morbid deposit, found it to be tubercle, some of which was cretaceous. A whitish liquid found under the envelope contained detritus of the capsule, with some pus.

A second case of Addison's disease similar to the last is detailed in the same place by M. Sécond-Féréol. The 'Gazette Médicale de Paris' (Oct. 4, 1856), also contains a case of Addison's disease, communicated by M. Seux, of Marseilles, in a girl, aged twenty-four, but in whom the correctness of the diagnosis was not confirmed by an autopsy.

In a careful memoir on the subject of Addison's disease, M. Danner passes in review the evidence brought to bear on the subject. He arrives at the conclusions, that a relation has been satisfactorily proved to exist between bronzed skin and disease of the supra-renal capsules. To the cases just mentioned, M. Danner adds the *résumé* of one observed by M. Malherbe. These cases confirm the results obtained by English observers, and as, with the exception of two cases presented to the Pathological Society by Drs. Peacock and Hughes Bennett, none have yet been published either in England or France which demonstrate the existence of bronzed skin without a co-existent lesion of the supra-renal capsules, the evidence in favour of Addison's disease appears sufficiently strong to ensure its permanence in nosology.*

XVI. *On a Case of Multiple Abscesses in the Liver, originating in Inflammation of the Biliary Ducts distended with Bile.* By M. CRUVEILHIER. (Archives Générales de Médecine, Janvier, 1857.)

A washerwoman, aged thirty-two, was admitted under Professor Cruveilhier, with intense icterus, accompanied by severe pain in the region of the liver, extending into the right iliac fossa. No tumour was perceptible in the region of the gall-bladder. The affection had commenced a fortnight previously with pain in the right iliac fossa, and was followed a week later by icterus. On admission, the patient was extremely prostrated, the voice faint, the tongue very dry, with a fuliginous fur, and micturition and defæcation were involuntary. The typhoid symptoms became more urgent, and death ensued a week after admission. The entire peritoneum was found to be invested with a purulent false membrane of icteric hue; several spoonfuls of bile were found between the left lobe of the liver and the stomach, which did not proceed from a ruptured

* At the time of our going to press, Mr. Hutchinson has brought forward a case at the Pathological Society, analogous to the one by Dr. Bennett. It is also right to state that we have notes of four cases published at different times, in which absence or disease of the supra-renal capsules were not accompanied by bronzing.

gall-bladder, of which organ there was scarcely a vestige, but from the rupture of biliary abscesses which occupied the convex surface of the left lobe of the liver. This organ showed on section, the biliary ducts enormously dilated, and filled with yellow bile. The ducts presented here and there ampullæ, with circular valve-like folds, but were otherwise healthy. There were also scattered through the liver about ten abscesses; the smaller ones were manifestly formed within the bile ducts; in the more advanced abscesses the inflammation had passed the limits of the biliary canals, and involved the hepatic tissue itself. The gall-bladder was shrunk up to a minute cavity capable of holding a raisin stone; it contained pultaceous matter and a biliary calculus. The latter was so placed as entirely to intercept all communication between the ductus hepaticus and the ductus choledochus. The author refers the distension and suppuration of the gall-ducts to the diseased state of the gall-bladder and the presence of the calculus. The main point to which he draws attention is the fact demonstrated by the case, that multiple abscesses of the liver may result from inflammation of the biliary passages, as well as from inflammation of the portal veins, and that they may be independent of purulent infection.

XVII. *Cases of Œdema of a Single Lower Extremity.* By F. W. Lewis, M.D. (Hollingsworth's Medical Examiner, Nov., 1856.)

Dr. Lewis details eight cases in which œdema occurred in one or the other lower extremity alone. Four he observed in Paris; the others occurred in his own practice in America. The conclusions to be drawn from these cases will be more palpable if we tabulate them:

No.	Sex.	Age.	Disease.	Limb affected.	Post-mortem appearances.
1.	F.	50	Phthisis	Left leg	Left common iliac vein compressed by both iliac arteries; immediately below compression a firm coagulum extending to femoral vein; inner coat rough.
2.	F.	(?)	Phthisis	Left leg	Left common iliac compressed by left common iliac artery; firm coagulum in iliac vein, extending into femoral; with traces of inflammation.
3.	M.	88	Chronic dysentery.	Left leg	Indurated inguinal glands, pressing on femoral vein.
4.	M.	48	Dysentery, diabetes, phthisis.	Left leg	Obliteration of iliac vein by enlarged glands; no coagula below the point of obliteration.
5.	F.	16	Phthisis	Left leg	Pressure upon iliac vein by iliac artery; coagulum below the point of compression.
6.	M.	40	Phthisis	Right leg	Indurated inguinal glands surrounding origin of right femoral vein; coagula; internal surface rough.
7.	F.	37	Phthisis	Right leg, shortly before death also the left	Coagulum in right iliac vein, apparently independent of any compression; the left iliac vein crossed by both iliac arteries.
8.	F.	23	Phthisis	Right leg	No appearance of compression; obliteration of right iliac and femoral by coagula; the femoral much thickened.

Dr. Lewis does not appear to be acquainted with the cases published by Dr. Bright, in his Medical Reports, showing the occurrence of obliteration of the veins by coagula as a result of wasting and debilitating disease, apart from any disease of the vessels themselves. His main object being to support the view which he states M. Piedagnel first to have promulgated, that these cases of unilateral œdema are due to compression exerted upon the iliac vein by the corresponding artery. His own cases, however, tend to show that, although this may often be the exciting cause of the intra-vascular coagulation of the blood, this explanation does not hold good universally. Only three of the above cases are attributed to the compression exerted by the artery (Nos. 1, 2, and 5). Three were manifestly produced by the pressure exerted by a glandular mass (Nos. 3, 4, and 6), and two were independent of any kind of compression (Nos. 7 and 8). On the other hand, in all there was a similar predisposing cause—viz., an exhausting disease; and it is probable that the influence thus exerted in the production of the symptom deserves more consideration than the author has bestowed upon it. It is well, however, in point of diagnosis, to remember that unilateral œdema may be brought about by direct pressure upon the abdominal vessels, as was shown in a case of abdominal cancer published by the Reporter not long since.* He occasionally attends a lady who for many years past has had a permanent and very marked œdema of the left arm, which can be traced to nothing beyond a cold; it in no way interferes with her general health, which is very good; however, when indisposed, there is generally a temporary increase in the œdema of the arm.

XVIII. *On the Sequelæ of Diphtheritis.* By Dr. Faure. (L'Union Médicale, Tome xi. Nos. 15 and 16.)

The extension of the diphtheritic or croupy exudation to the cavity of the mouth and nostrils, so constant in France, is almost unknown in England; nor are we in the habit of regarding the disease as one of a contagious character. It appears that the affection altogether puts on a more virulent form with our neighbours than it assumes on this side of the Channel. In the paper before us, Dr. Faure draws attention to a paralytic condition which is found to follow diphtheritis, but which, though previously observed by Bretonneau, Trousseau, and others, has not yet been described.

In certain cases, after the false membrane has entirely disappeared, and some time having elapsed, without apparent reason, the integuments become discoloured, livid, the joints painful, the limbs are deprived of all power, and the patient falls into a state of utter prostration. Generally the lower extremities refuse to support the body, and the arms no longer respond to volition; the movements are irregular; the velum palati, completely paralysed, is flabby, and floats about so as to be an obstacle both to deglutition and to articulation. All the muscles of mastication and those of the neck and chest are more or less paralysed; hence the food often lies in the mouth unswallowed, or causes regurgitation or respiratory spasm. Vision also becomes impaired, and the sensibility of the skin is much diminished. At times there is mental aberration. There is no reaction; fever rarely occurs, the prostration becomes extreme, and death ensues. The issue is not, however, necessarily fatal. Dr. Faure himself, being attacked by diphtheritis, taken by contact with the croupy matter of a child whom he was cauterizing, suffered some of the symptoms above described, and states them to have been very terrifying. He admits himself to be ignorant of the exact nature of the paralytic symptoms above detailed, but seems to incline to the view that they may be caused by a diphtheritic exudation within the ventricles.

The treatment consists in the administration of quina, steel, and other tonics, with a nutritious diet. Two cases are given in which, these remedies having failed, the immersion in cold water secured the recovery of the patient.

XIX. *Note on Glycohæmic Gangrene.* Communicated to the Academy of Sciences by Marchal de Calvi. (L'Union Médicale, Tome x. No. 144.)

Some years ago, M. Marchal de Calvi published a case of gangrene occurring in a diabetic patient. The man had suffered from a succession of furuncles, and one of his large toes then became sphacelated. His urine

* Association Journal. 1856.

contained 90 grammes of sugar to the litre (19½ drachms in 2·11 pints!). The patient improved after losing his toe; but two years later, having neglected himself, the gangrene returned and death ensued. Dr. Landouzy, of Rheims, soon after published a similar case. A third then occurred to M. de Calvi, in which a large gangrenous spot on one thigh was accompanied by diabetes mellitus; and a fourth is now given, in which the latter disease was associated with a large carbuncle at the back of the neck. The ages of the first two patients are not stated; those of the last two were respectively sixty and sixty-five. M. de Calvi suggests that the presence of sugar in the blood creates an inflammatory diathesis in the lining membrane of the vessels, and that, as the vital power is diminished in diabetes (as shown by the general depression), the inflammatory irritation thus produced is accompanied by a tendency to necrosis.

The author observes that the lithic-acid diathesis possesses a similar tendency of exciting gangrenous inflammation, and thus explains the origin of gangrena senilis, which, he says, is particularly common in England, on account of the large amount of animal food consumed; or, to use his own words, "where the mode of living places in contact with the lining membrane of the arteries the elements of excessive stimulation."

XX. *On Circumscribed Atrophy of the Skin.* By Dr. Reuss. (Vierordt's Archiv für Physiologische Heilkunde. 1856. Heft 4.)

Dr. Reuss reports two cases of a disease of which he states he has found no description in authors, and which appears to be almost identical in its characters with what we ourselves witnessed in April, 1856, in a young woman.

A lad, aged fifteen, at the end of 1855 had typhus, and while at its acme several parts of the skin were observed to undergo a peculiar change. They assumed a reddish-blue or reddish-brown colour; under a slanting light appeared whitish, of an asbestine or satiny gloss, and sharply cut off from the surrounding skin. They formed elongated streaks of half an inch to three inches in length, and were from one to four lines broad, and were all directed vertically or obliquely to the axis of the body. They were symmetrically arranged in both lower extremities below the trochanter major, above the patella, above the internal condyle of the femur, and across the outer side of the leg; altogether there were from twenty to thirty such streaks on each leg. The affected parts were sunk below the level of the surrounding skin; and when pressed, the bluish colour disappeared, and one could see the blood return into the subjacent dilated capillaries. The sensibility of the parts was diminished. Three months later, the appearances had somewhat faded, but were essentially the same. The second case resembled the last, but was not so well marked: it occurred in a young woman, aged twenty-eight. The one we ourselves observed occurred in a servant-girl, aged twenty-nine, who, after suffering from some severe abscesses, found that small white spots formed on the left side of the neck, extending from the sternum over the clavicle towards the spine—like zoster. The spots were sharply defined, very smooth, and bloodless; and looked as if the sub-epidermic tissue had been punched out. There had never been any elevation of the tissues or secretion. The outline was generally circular; or where two or more spots had coalesced, the outline became oval. They varied in size from the point of a pin to a split pea. There was a small patch of similar white spots on the right hypochondrium. Her general health, at the time we saw her, was good.

Like Dr. Reuss, we failed at the time in meeting with anything analogous in works on skin diseases. In the fourth edition of Mr. Wilson's work 'On Diseases of the Skin' (p. 378), which has just appeared, the affection is described under the name of Morphæa Alba.

XXI. *Case of Sclerema, or Pachydermatous Disease. A Disease consisting in a Peculiar Induration of the Skin over a great part of the Body.* By ROBERT M'DONNELL, M.B. (Dublin Hospital Gazette, Nov. 1, 1856.)

We quote the following case almost literally as it appears in the original paper:

Catherine Carr, aged twenty-four years, was admitted into the Richmond Hospital under Mr. Adams's care, June 18th, 1854.

She continued for some months in the hospital as a patient. Her complaint did not render her unfit for useful occupation—she accordingly received employment in the institution: she has therefore been now under observation for a period of rather more than two years. As to her present condition, the integument covering the face, fore part of the chest, and arms, presents in a very marked degree that induration which forms the most striking feature of her disease. On the face the skin is tense and shining; around the mouth, on the forehead, and more particularly across the nose, it seems as if tightened from contraction, and its rigidity interferes with the natural play of the features. Across the chest the skin is so tightly drawn as to produce a feeling of constriction. The hardness and stiffness are nowhere so great as in that covering the arms and hands. It is with difficulty moveable over the deeper structures; it has altogether lost its pliancy and softness; it feels like brawn; one might as easily pinch up between the finger and thumb the skin on the back of a pig as the skin over these parts. The free movement of the fingers is in a great degree impaired; the patient cannot perform any delicate handiwork; her former occupation of dressmaking she has been obliged to abandon, from her inability to handle needles, &c. The contraction of the skin in the bend of the elbow prevents the possibility of straightening the arm: in attempting to lift heavy weights, the skin in this locality has actually torn and become fissured, and in the bend of each elbow scars, the result of this, remain.

The tension of the skin over the knuckles, and the prominence of the lower extremity of the ulna, cause these points to ulcerate readily if exposed to friction; the power of feeling is slightly, if at all, impaired. The skin on the back, on the lower part of the body, and lower limbs, is in a perfectly normal state.

The patient complains of pain in the hands, like the stinging of nettles. This pain is made worse by exercise, is much relieved by bathing the hands in warm water, and is most troublesome after going to bed at night. She suffers from dyspepsia, and has had at irregular intervals violent attacks of bilious vomiting, after which she observes a temporary improvement in the condition of the skin; in other respects her general health is good. There is no derangement of the menstrual functions.

Cold seems to have been the starting-point of the disease, as it followed a wetting she got four years ago while recovering from an attack on the chest.

The rigidity of the skin commenced in the right arm, and passed across the chest to the other; the face was attacked later.

In the case of this patient, only temporary benefit has been derived from the various modes of treatment which have been resorted to: from nothing did she derive so much advantage and relief as from frequent warm baths and the use of cod-liver oil, which, besides being administered internally, was rubbed in over the indurated integuments after each bath.

XXII. *A Rigid, Anchylosed Human Skeleton, the Result of Rheumatism.* By H. P. C. WILSON. (Hollingsworth's Medical Examiner, June, 1856.)

E. E., a German woman, was admitted into the Baltimore Almshouse, of which Dr. Wilson is the physician, in 1846, on account of her being incapacitated from labour by chronic rheumatism. She was twenty-one years old, and already experienced great difficulty in using her limbs. She became gradually worse, and in March, 1855, she is described as lying on her back, unable to move a single joint, save a very partial motion of the lower jaw and the costo-vertebral articulations. Her hands were pronated, and her fore-arms flexed upon the arms, and resting upon the upper part of the abdomen. The soles of her feet were applied one to another, her legs were flexed upon her thighs, and her thighs upon the pelvis. She had lain in this condition for nearly nine years. Still the vegetative functions were well performed. She died of typhoid fever, supervening on scurvy, July 9th, 1856.

The interarticular cartilages of the upper and lower jaw were found completely ossified, with such an amount of bony matter thrown out in and around the joint as to allow but very partial motion. The occipito-atloid articulation was so anchylosed, that no traces of a joint existed. The intervertebral substance was converted into bone, so as to render the spinal column an inflexible pillar. The sterno- and scapulo- clavicular articulations were obliterated, the xiphoid and costal cartilages ossified, the humeri anchylosed to their respective scapuli; the same was the case with every joint of the upper and of the lower extremities. No joint in the body, save the two above mentioned, presented any movement. The bones were exceedingly light, not weighing one-third as much as their counterparts in the healthy subject. The earthy matter was apparently wholly removed from the cancellated structure.

XXIII. *On the Period of Life at which Hysterical Affections are most liable to be developed.* By Dr. BRIQUET. (L'Union Médicale, Sept. 4th and 20th, 1856.)

Dr. Briquet passes in review the doctrines taught by various writers on the subject of the occurrence of hysteria, and then analyses a series of 467 cases occurring in his own practice in the course of ten years, in which the commencement of the affection was carefully noted. Some of his inferences would probably not be universally adopted, but his numbers are important, the more so as they are in the main corroborated by the analysis of numerous cases collected by Dr. Landouzy, whose results are also given in the following table:

	Landouzy.	Briquet.
From birth to 10 years ...	0 cases ...	61 cases.
" 10 " 15 " ...	48 " ...	104 "
" 15 " 20 " ...	105 " ...	162 "
" 20 " 25 " ...	80 " ...	73 "
" 25 " 30 " ...	40 " ...	28 "
" 30 " 35 " ...	38 " ...	13 "
" 35 " 40 " ...	15 " ...	12 "
" 40 " 45 " ...	7 " ...	3 "
" 45 " 50 " ...	8 " ...	1 "
" 50 " 55 " ...	4 " ...	2 "
" 55 " 60 " ...	4 " ...	1 "

Dr. Briquet attributes the differences that are manifest between his table and the numbers given by Dr. Landouzy to the circumstance of his having exercised great care in determining the exact commencement of the disease. The following are his chief conclusions:

1. A considerable number of cases of hysteria occur while the sexual organs are yet in a rudimentary state.

2. The development of hysteria does not bear a direct ratio to the period of activity of the sexual organs, as this period commences at eleven or twelve years, and does not cease till the fortieth or forty-fifth year. On the other hand, hysteria progressively advances up to the age of twenty, and very rapidly

diminishes from the twentieth to the forty-fifth year. Consequently, of thirty-four years of sexual activity, there are only from nine to ten during which hysteria prevails, while it becomes less frequent during the remaining twenty-four; and yet the sexual activity is greater from twenty to forty-five years of age.

QUARTERLY REPORT ON SURGERY.

By JOHN CHATTO, Esq., M.R.C.S.E., London.

I. *On the Duration of the Incubation of Syphilis.* By Prof. SIGMUND. (Wiener Wochenschrift, 1856. Nos. 32 and 45.)

WHILE the space of time within which symptoms of *secondary* syphilis manifest themselves may be determined with exactitude,* those which have been termed *tertiary* symptoms present in this respect greater difficulties. The question whether tertiary are always preceded by secondary symptoms, is to be answered in the affirmative; and any doubts that may prevail upon the subject arise from the latter not having been sought for with sufficient care. The cases are frequent enough in which the patient first presents himself to the practitioner with tertiary symptoms, the secondary having been overlooked, and in the meantime having quite or partially disappeared. In other cases, affections of the skin, the glands, or the mucous membrane, do exist, showing plainly enough the presence of secondary symptoms, but which have either excited no attention or have been referred to some erroneous origin. These are the cases in which patients are said to have continued well in the interval between the manifestation of primary and tertiary symptoms; but Prof. Sigmund has never met with a single example of this, having been usually able to point out even several of these appearances, or remains of them, which, conjoined with the history furnished by the patient, were amply convincing.

Numerous cases which the author has observed step by step during several years have taught him that the occurrence of tertiary symptoms takes place very rarely so soon as to give them the appearance of immediately following the primary; and in those rare instances in which they have been observed within two to four months after the chancre, the existence of a yet older chancre has been almost always visible or acknowledged—to which, in fact, the existence of such symptoms is due.

In order to elucidate the period of incubation of tertiary symptoms, Prof. Sigmund has selected from his notes 1741 cases which presented them in marked forms, and in which the periods of their appearance and duration were accurately observed. In the following table, the shortest, the medium, and the longest periods are indicated within which they exhibited themselves after the appearance of the primary ulcer:—

		Months.	Months.	Years.
568	Inflammation of bone, or cartilage, or periosteum, or necrosis of bone or cartilage	3	24	41
547	Papular or pustular cutaneous affections	6	11	7
292	Scaly syphilides	9	16	13
183	Cutaneous ulcers	17	22	20
85	Perforation or loss of soft palate	21	32	19
19	Tuberculosis of cellular tissue	43	59	40
27	Disease of the nails	37	48	22
20	Contraction of tendons	41	71	7
1741				

* See British and Foreign Medico-Chirurgical Review, Jan. 1857, p. 266.

II. *On the Ligature of Arteries in Suppurating Wounds.* By M. NÉLATON. (Gaz. des Hôpitaux, 1857. No. 1.)

In one of his recent clinical lectures, M. Nélaton made the following observations, the occasion being a secondary hæmorrhage in the palm of the hand. Nothing is more difficult, he observed, than to arrest a hæmorrhage of the hand, especially when this is consecutive—that is, when the wound is covered by pyogenic granulations. If not previously instructed as to the proper management of these secondary hæmorrhages, you will be extremely embarrassed. The blood flows, you employ compression, and it ceases; but the hæmorrhage will not be long before it returns, and will then be uninfluenced by compression. If compression be made above the wound, œdema takes place in all the subjacent parts, and the hæmorrhage soon returns. The radial, or the ulnar, or the brachial may be tried, and yet the bleeding does not stop. Meeting such a case, M. Nélaton formerly was quite at a loss to know what to do, impressed as he was with Dupuytren's *dictum*, that arteries in a suppurating wound will not bear the ligature, the premature fall of this infallibly giving rise to a return of the hæmorrhage. Nevertheless, he ventured to tie the two ends of the bleeding vessel of the palmar arch; and although the ligature fell sooner than usual, no hæmorrhage followed. He has frequently since then tied vessels under analogous circumstances, and has never seen hæmorrhage as a result of the fall of the ligature. Although, therefore, this fall takes place earlier (usually about the third or fourth day) than is the case with a ligature applied to a healthy artery, it is not premature, for bleeding does not follow. Examining the matter experimentally upon the dead body, M. Nélaton has found that ligatures applied to arteries in a state of suppuration (as in patients who have died after amputation) produce identically the same effects upon the coats of these vessels as upon arteries remote from the seat of inflammation; the same division of the inner coats and preservation of the outer taking place in the two cases. He feels, therefore, perfect confidence in the soundness of the practice, supported as it is by numerous cases that have occurred to him, both in private and hospital practice.

III. *On the Valvular Nature of Strangulated Hernia.* By Prof. ROSER. (Vierordt's Archiv, 1856, pp. 355–368.)

Incarcerated hernia, in Prof. Roser's opinion, essentially depends upon a valvular mechanism. The obstruction of the contents of the intestine in the incarcerated portion arises from the folds of the mucous membrane lying valve-like against each other, and preventing the passage of gas, fluids, &c. Looking at the complete obstruction which takes place in the hernia, one might suppose that the parts concerned are compressed as closely as is an artery when tied. But all observation teaches us that no such pressure is here exerted; for while the venous circulation is only partially arrested, the arterial remains uninterrupted. Were it otherwise, indeed, the intestinal fold would become rapidly gangrenous. The question is why, if there is space enough to allow of the circulation in the part to continue, cannot we by pressure return the contents of the intestine.

The nature of the obstruction may be shown by a simple experiment. If a noose of intestine, containing some fluid or air, be brought within a ring about the size of the finger, and then pressure be made upon the apex of the noose so as to force the contents against the compressing body, complete obstruction to their passage will be found to prevail. And yet a catheter may be passed beside the intestine, and, by drawing the latter a little to one side, a considerable space will be perceived. If pressure be made in front of the encircling ring, the contents of the intestine are forced back; but if we press at the end of the noose, the portion that lies next to the ring is forced against the latter, and the canal is closed. If we open the noose on its convex side, and fill it with water, we may observe the valvular disproportion of the intestinal folds, which resemble the valves of the aorta when acting under water.

Deferring to another occasion the exposition of his theory of the taxis deducible from these views, Prof. Roser now points out the support they give to the operation for hernia, without opening the sac—a procedure he regards as one of the greatest improvements in surgery since the days of Paré. He believes it has made little progress in Germany and France, as compared with England, in consequence of the prevalence of a false theory of strangulation of hernia and erroneous ideas on the surgical anatomy of hernia. In respect to the first of these, too exaggerated an idea of the constriction that takes place has been entertained, leading to a belief that the mere dilatation of the tendinous margins could not suffice for the return of the distended and indurated hernia. The above experiment, which proves the valvular nature of the obstruction, must surely give more confidence in the efficacy of the external incision. We have not space to follow the author in his description of the anatomy of femoral hernia, and which, indeed, essentially resembles that furnished by Cooper.

IV. *On the Diagnosis of Obscure Forms of Ocular Congestion.* By Dr. QUADRI. (Annales d'Oculistique, tome xxxvii. p. 25.)

The diagnosis in obscure internal ophthalmias is often very difficult, and may lead to their being mistaken for purely nervous affections. Dr. Quadri cites a case in point in which the ophthalmoscope exhibited the retina in a normal state, and in which the absence of all symptoms of inflammation would have justified him in pronouncing a simple neurosis, when, happening to see the patient as soon as he awoke, he observed a very marked pericorneal injection, like that seen in iritis, and which at the end of an hour had disappeared. The same thing occurred the next day, and, in fact, the case proved an example of iritis, which was cured in the usual way, but the existence of which was indicated by no other symptom. In the normal state of the eye we observe only congestion of the superficial network of the conjunctiva on awakening; but the deeper-seated vessels are not engorged, unless they have become dilated as a consequence of disease. The observation of the eye at this hour may thus be of great utility in obscure cases.

V. *On Apoplectic Ophthalmia.* By Dr. QUADRI. (Ibid., p. 26.)

The inflammation of the membranes of the eye occurring in persons advanced in age and disposed to apoplexy, assumes special forms, calling for extraordinary treatment, and justifying, in Dr. Quadri's opinion, the appellation of apoplectic ophthalmia. It is usually manifested under the form of palpebral ophthalmia, the chief characteristic of which is the remarkable tendency it possesses of resuming an acute form without any obvious cause, and the complete resistance it offers to the ordinary means of relief. It is observed in persons of a certain age, whose configuration is that usually termed apoplectic, and is often preceded or accompanied by the various precursory symptoms of apoplexy. In other cases, however, it has been the first symptom of this affection, so that palpebral congestion, remarked in persons with disposition to apoplexy, should be well observed, as it may indicate to us the means of warding off a threatened attack. We usually may observe in these persons the puriform palpebral flux of Scarpa come on slowly, and continue for months or years, without deriving any advantage from the local remedies that are usually so useful. There seems, indeed, in these cases, an intolerance of astringents, half the usual doses being scarcely borne. If larger ones produce a notable amelioration, this is soon followed by a far more important affection of the deeper parts. Under mild doses of these astringents the affection seems to be yielding, when suddenly the acute stage returns, to again become relieved, and again to be followed by relapse, until the attack of apoplexy itself comes on. After several of such relapses, ectropion supervenes—the *ectropion senile* of the ancients.

In another form, a kind of purulent ophthalmia is observed, being accompanied by much photophobia, and an abundant secretion of yellowish, viscous mucus at the edge of the eyelids. There is here the same intolerance of astringents and the same disposition to relapse; and affection of the cornea, leading to ulcers, pannus, and blindness, may be the result.

This ophthalmia may long precede the attack of apoplexy, the period in Dr. Quadri's experience never having been less than one or more than three years. The prognosis is not a favourable one. Left to itself, the disease gets worse and worse, and ends in apoplexy; and although by the aid of art it may be relieved, and the intervals of relapse rendered longer, a radical cure never is produced. The palpebral flux, capable of much amelioration, is rarely cured, and the blennorrhœal ophthalmia is almost always followed by a pannus, which the frequent returns of the disease render incurable. In treating the disease, the prophylaxis is of the utmost importance, the slightest error of regimen leading to relapse. The medical treatment of the apoplexy requires no description; but Dr. Quadri states that he has derived great advantage from the red sulphuret of mercury combined with a little aloes. Collyria are ill borne, and ointments, such as Janin's or the Edinburgh citrine, both used diluted, are preferable. The blennorrhœal form requires caustic substances to be used with the extremest caution. Local and general bleeding is here of great value, and may often arrest the progress of a relapse. Blistering the nape must be rigorously prohibited, as rather disposing to than preventive of apoplexy.

VI. *Case of Myopathic Luxation.* By Dr. Friedberg.
(Osterreichische Zeitschrift, 1857, No. 1.)

O. H., when fifteen months old, fell with his left hand stretched out, and some hours after complained of pain in the arm. Two days later, swelling was observed at the wrist, as well as at the shoulder and left cervical region; that of the latter parts not disappearing for five or six weeks. It was found then, that although the child could use its arm, it forbore as far as possible, occasionally complaining of pain in it. At a still later period, frequent fugitive convulsive movements of the limb appeared, as well as progressive emaciation.

Various means having been uselessly tried, the boy, two years after the accident, was brought to the author's Klinik. The left arm was then found to be an inch longer than the right. The upper arm had lost a sixth of its circumference, but the other parts of the extremity were less emaciated. The emaciation affected the *pectoralis major*, the *latissimus dorsi*, the anterior superior portion of the *trapezius*, and all the muscles proceeding from the shoulder-blade to the arm. The middle portion of the deltoid had almost disappeared, as had the *supra-spinatus* at its external two-thirds. The scapula and humerus were normally developed. The acetabulum was empty, but its capsule was not thickened. The head of the humerus, which had sunk downwards an inch, could be easily restored to its place, from which, however, it immediately fell down again. Slight fibrillary contractions were observed in the muscles about the shoulder, the force of which was somewhat increased on the application of cold. The child could not execute any movement of the upper arm; while electricity only excited feeble contractions in the *pectoralis, latissimus*, and a portion of the *deltoid*, the middle of this last and all the muscles of the humerus being insensible to its action.

The child's health and development were good, and the author saw him yet a year later. The affection had continued to make progress, so that the left arm was a fourth less in circumference than the right, and hung motionless by the side; while the elasticity of the muscles of the fore-arm had diminished, as had the power of using the hand. The muscles attached to the humerus seemed like mere thin relaxed cords, and its development, as well as that of the scapula, had been remarkably retarded. The disappearance of the *deltoid* was almost complete, and the glenoid cavity seemed to have become more superficial. The lower half of the *trapezius* and the *rhomboideus* had also remarkably diminished.

Thus it appears that all the symptoms which have been assigned to the so-called *progressive muscular atrophy* may arise from traumatic inflammation of muscles. At the time of the accident in this case, the muscles surrounding the shoulder-joint underwent violent traction through the sudden pushing upwards of the head of the humerus. This was followed by pain and increased sensibility, and afterwards by the relaxed state of the arm. The inflammation not being dissipated, led to degenerative atrophy, which extended from the muscles originally involved to the others in their vicinity.

In explanation of the direction taken by the luxated humerus, Dr. Freidberg observes, that the disturbance of the nutrition of muscles, which leading to their relaxation, may permit dislocation, may occur in various ways, whether from the operation of violence, the propagation of inflammation from a joint, the poisoning the blood as by lead, or scarlatina contagion, through a continued interruption of innervation, or a diminution of the supply of arterial blood, &c. This deprivation of their elasticity renders them unable to oppose the action of their antagonists. This myopathic luxation may occur in different joints under more or less complicated conditions; and it is met with in its simplest form in the shoulder. Here it might indeed *à priori* be especially expected to occur, owing to the extensibility of the capsule and ligaments, rendering the retention of the head of the bone within the cavity exclusively a muscular action. And thus it is, while in the shoulder-joint a complete luxation may be the immediate effect of a myopathic paralysis, in other joints such luxation is at first only incomplete, its completion depending upon other secondary circumstances. For the production of a dislocation perpendicularly downwards, as observed in the present case, it is essential that the *supra-spinatus* muscle be either torn, or have lost its elasticity in consequence of the disturbance that has been produced in its nutrition.

VII. *Rupture of the Right Rectus Abdominis Muscle.* By Dr. Richardson. (American Journal of the Medical Sciences, Jan. 1857, p. 41.)

On Feb. 29th, a healthy athletic young man, aged twenty-eight, immediately after hopping over a narrow ditch, was seized with acute, persistent pain about two inches below, and to the right of, the umbilicus. He heard a distinct snap; and nausea, together with unavailing desire to evacuate the bowels, although he had recently had a stool, came on on March 1st. There was an increase of pain, frequent vomiting, and constipation, together with marked rigidity and increased sensibility all over the abdomen, while at the seat of injury a flat intumescence was observed. Leeches and then ice were applied to this, and chloroform was administered occasionally. Next day, as the patient's condition was aggravated, it was determined to operate, under the idea that a strangulated ventral hernia was present. The *fascia profunda* was exceedingly tense, and upon dividing it, a large coagulum of black blood was found occupying an irregular cavity, very like that of a diffused false aneurism. It was supposed to weigh about half a pound, and pressed deeply upon the peritoneum and bowels. The peritoneum was found uninjured, and advanced forwards as the coagulum was removed. When this was effected, the nature of the accident became apparent. The right rectus abdominis, with the corresponding epigastric artery and its accompanying vessels and nerves, as well as the sheath of the rectus, were torn completely across, the ends of the muscle having retracted, and being one inch and a half to two inches apart. "The precise point of rupture was a central one between the right linea alba and linea semilunaris, and the linea transversa which intersects the umbilicus, and the linea transversa next below it." No ligature was employed, torsion being applied to one artery, and warm-water dressing to the wound. On the 4th, a considerable coagulum was found again occupying the cavity of the wound, and resting on the peritoneum; but there was not much tenderness of the abdomen. Sleep had been procured by morphia, and as no stool had occurred since the accident, aperients were ordered. From this time the patient went on well, the wound healing kindly, leaving a depressed surface.

In relation to this case, we may furnish a short account of a paper recently contributed by Virchow to the Würzburg 'Verhandlungen' (band vii. p. 213), bearing the title, On Inflammation and Rupture of the Rectus Abdominis.

During the last few years, several cases of inflammation and rupture of this muscle have come under his notice, which have proved interesting from the analogy they bear to examples of myocarditis and rupture of the heart. In most of the cases, the rupture has occurred about midway between the umbilicus and the pubes, usually being confined to one side. In some cases in which he has found parenchymatous changes without rupture, these have always been found towards the lower part of the muscle.

Isolated rupture of muscles from external violence are of very rare occurrence; and where great traction has been exerted, it is rather the tendon than the muscle that gives way. So, too, the ruptures which sometimes take place from excessive action, as in tetanus, must be very rare, and of a microscopic character. Those resulting from excessive rigor mortis, described by Rokitansky, M. Virchow has never seen. The comparatively frequent parenchymatous or spontaneous rupture presupposes organic changes of the muscle, inducing a pathological fragility. It is characterized by the slight amount of force necessary for its production, and the existence of changes in the immediate vicinity of the rupture, which are not infrequently, to greater or less extent, in other parts of the muscle. These changes Virchow has exactly described in his account of muscular inflammation, in the 'Archiv' (band iv. p. 266). They may consist in either a true fatty metamorphosis of the interior of the primary bundles, or of peculiar softenings which at last lead to a granular degeneration of the muscular substance. This last form it is that often excites inflammatory appearances, which are especially seen during metastatic processes, but which may be produced in the same way as in the heart when its supplying arteries are obstructed. The first form takes place more slowly, as in aged persons, in paralytic parts, and after protracted muscular inactivity.

So far as Virchow has observed, rupture of the rectus has always been preceded by this organic metamorphosis of its substance; and in quite recent cases, the same changes have been found at the circumference of the ruptured parts which have been found in diseased but unruptured muscle. After describing the microscopic appearances at the various stages of the affection, he goes on to say that he has always found rupture commencing at the posterior or peritoneal surface of the diseased muscle. The ruptured part becomes at once filled with blood, which coagulates and may be infiltrated, or projecting and visible through the peritoneum. Of the 7 cases Professor Virchow has met with in three years, 4 occurred in women aged nineteen, thirty-five, fifty-nine, and sixty-five years; and 3 in lads of fifteen, twenty-two, and twenty-three years. Four of the cases were examples of typhus, most having entered the ulcerative stage; in 2 others there was marked tuberculosis, and in another scorbutus. In most of the cases the proximate cause of the rupture was violent coughing. Virchow believes that some of the cases described by authors as "neuralgia epigastrica," "peritonitis musculosus," "rheumatismus muscul. abdom.," may be examples of such rupture, going on in certain instances to suppuration.

VIII. *A Contribution to the Statistics of Fractures and Dislocations.* By Dr. E. Gurlt. (Deutsche Klinik, 1857; Beilage, No. 1.)

In this paper, Dr. E. Gurlt furnishes statistical particulars of 1631 fractures (occurring in 1541 individuals) treated in the civil hospitals of Berlin during five years (1851–6). These he compares with the other statistical accounts that have been published. We are only able to notice some of his tables and conclusions, and first we may give an abstract of the comparison he makes between his own figures and those of other observers:

	1. Malgaigne.	2. Wallace.	3. Norris.	4. Lente.	5. Matiejowski.	6. Lonsdale.	7. Gurlt.	8. Middeldorpf	9. Mebes.
Head & face	95	127	128	193	44	94	89	10	0
Trunk	288	146	105	52	137	392	158	73	13
Clavicle	225	148	118	158	58	273	123	32	51
Humerus	320	442 (Humerus and Fore-arm)	377 (Humerus and Fore-arm)	161	134	118	216	27	46
Fore-arm	318			269	156	386	309	60	54
Hand	58	22	62	0	32	116	157	25	0
Femur	308	291	195	280	199	181	232	25	64
Patella	45	28	28	30	15	38	22	3	0
Leg	652	589	380	579	293	289	283	59	93
Foot	19	7	47	0	18	14	32	11	0
Unspecified	19	10	1	0	0	0	10	0	0
Totals	2347	1810	1441	1722	1086	1901	1631	325	321

We may notice the following of the author's observations:

1. *The Relative Frequency of certain Fractures.*—By the above table, it will be seen that, while in Nos. 1, 2, 3, 4, 5, and 9, the number of fractures of the lower extremity far exceeds that of the upper, the reverse of this is the case in Nos. 6, 7, and 8. This probably arises from the former series not embracing out-patients treated at their own homes, as is the case with many persons suffering from fractures of the upper extremities.

2. *Sex and Age.*—Although the great predominance of male subjects is well known, yet this is stated in very different proportions by different authors. Thus, Malgaigne states it at $2\frac{1}{2}$ to 1, Lente at about 8 to 1, Matiejowski at 2 to 1, Middeldorpf at 3 to 1, Mebes at $3\frac{1}{6}$ to 1, and Gurlt at $3\frac{1}{2}$ to 1. These proportions are quite destroyed in particular fractures, as, for example, in that of the neck of the femur, which in the author's cases presented itself forty-five times in women to thirty-one times in men—this depending upon the influence exerted by age in determining the proportion of fractures. From another table, given by Dr. Gurlt, it appears that, while little difference exists between the sexes early in life, as this advances females are more and more favoured until after forty, when the male predominance declines, and after seventy is replaced by that of females. The following is another comparative table:

Ages.	Malgaigne.			Lente.	Matiejowski.			Gurlt.			Mebes.		
	M.	F.	Tot.	Totals.	M.	F.	Tot.	M.	F.	Tot.	M.	F.	Tot.
1 to 10	45	18	63	95	64	27	91	177	88	265	18	9	27
11 to 20	179	25	204	221	118	43	161	167	26	193	55	6	59
21 to 30	269	87	356	454	114	38	152	249	25	274	54	13	67
31 to 40	345	87	432	413	153	36	189	206	18	224	36	7	43
41 to 50	316	94	410	217	116	42	158	136	18	154	41	16	57
51 to 60	268	158	426	85	116	53	169	105	50	155	20	14	34
61 to 70	183	133	316	40	47	47	94	41	31	72	12	11	23
Above 70	75	95	170	8	27	45	72	12	34	46	10	1	11
Totals	1680	697	2377	1533	755	331	1086	1093	290	1383	244	77	321

Dr. Gurlt explains the larger proportion of children contained in his own portions of the above table by the fact that he has embraced in it cases of that age that are usually treated as out-patients, or at special hospitals. Another table he gives shows the influence exerted by age on special fractures. Thus, below fifteen in his cases there is no example of fracture of the spine, ribs, olecranon, patella, or malleoli; while there are more fractures of the upper extremity than at any other period of life. Thus, among the 1631 fractures, there occurred between the ages of one and ten 49 fractures of the clavicle, 44 of the condyles of the humerus, and 46 of the bones of the fore-arm. The preponderance of fractures of the upper extremity is considerably greater during the first decennial period than later. Thus, while in the period one to ten years the fractures of the upper extremity amounted to 196, and those of the lower to 62, in the period twenty-one to thirty years the former were 125, and the latter 84. The proportion which fracture of the thigh bears to that of the leg at different ages is also seen in the following table:

	1 to 10.	11 to 20.	21 to 30.	31 to 40.	41 to 50.	51 to 60.	61 to 70.	Above 70.
Body of femur	51	18 } 22	11 } 14	9 } 12	6 } 17	7 } 23	13 } 25	9 } 25
Neck of femur	—	4 }	3 }	3 }	11 }	16 }	12 }	16 }
Leg	11	30	52	64	29	26	7	5

3. *Proportion to Population.*—Comparing the numbers of his cases with that of the civil inhabitants of Berlin, the author finds remarkable differences as to age and sex. Thus:

From	6 to 14,	about 1	fracture in	256	males and	895	females.
,,	1 to 14,	,, 1	,,	252	,,	568	,,
,,	15 to 60,	,, 1	,,	167	,,	1051	,,
Above	60,	,, 1	,,	134	,,	160	,,

4. *Time of the Year.*—According to a table given by the author, in which the fractures are distributed according to the months in which they have occurred, the greatest number happen in January, February, and March; then in June, July, and August; next in September, October, and November; the fewest occurring in December, May, and April. This proportion is, however, not constant, for there is no single month during the five years that has not varied in this respect.

The author makes other observations upon the side of the body in which the fractures preferentially occur, the proportion of compound to simple fractures (in his cases fifteen per cent.), &c.; but these do not seem of sufficient interest to call for notice.

Dislocations.—These, observed over the same space of time, are comparatively few in number, and are compared by the author with the figures furnished by Malgaigne and Norris.

Dislocations.	Malgaigne. Total.	Norris. Total.	Gurlt. M.	Gurlt. W.	Gurlt. Total.
Jaw	8	22	—	2	2
Spine	5	1	—	—	—
Sacro-iliac symphysis	1	—	—	—	—
Clavicle	42	12	6	—	6
Shoulder	370	101	48	17	65
Elbow	52	19	12	6	18
Wrist	16	4	1	1	2
Thumb	21	5	6	—	6
Fingers	10	2	1	1	2
Hip	40	21	13	2	15
Knee	9	1	3	—	3
Patella	2	—	2	1	3
Ankle	31	2	—	—	—
Metatarsus	2	3	2	—	2
Toes	—	1	—	—	—
Totals	609	174	94	30	124

In relation to sex, the author gives the following comparison at different ages, taking all the dislocations together:

Ages.	Malgaigne. M.	Malgaigne. F.	Gurlt. M.	Gurlt. F.
2 to 10	6	2	5	1
11 to 15	12	1	5	—
16 to 25	68	11	22	4
26 to 45	168	45	35	9
46 to 60	145	40	20	6
61 to 70	68	37	1	4
Above 70	22	18	2	5
Not given	—	—	5	—
Totals	489	154	95	29

Combination of Fractures and Dislocations.—In the 1631 fractures reported by Dr. Gurlt, there were 24 examples of a combination of dislocations; in 10 of these the dislocations occurring in other parts of the body, and in 14 complicating the same extremity. Of these, 7 were examples of fracture of the neck of the humerus with dislocation; 2 fracture of the scapula with dislocation of the humerus; 4 fracture of the internal condyle of the humerus with dislocation of the elbow; and 1 fracture with dislocation of the metatarsi. Malgaigne states that among 2358 fractures observed at the Hôtel Dieu, only 4 were complicated with dislocation; while in 1054 observed at the St. Louis, 13 dislocations also occurred.

IX. *The Results of* 100 *Lithotrity Operations.* By Dr. Victor v. Ivanchich. (Wien Wochenschrift, 1856, Beilage to 51.)

In this paper Dr. v. Ivanchich, of Vienna, furnishes a chronological list of 100 cases of lithotrity that have occurred to him, giving the name of each patient, and a very short summary of the particulars of his case. The following are the conclusions he arrives at from a general view of the whole number.

1. The *ages* were as follows:

18 and under 20	. . . .	4
20 „ 30	. . . .	1
30 „ 40	. . . .	3
40 „ 50	. . . .	9
50 „ 60	. . . .	31
60 „ 70	. . . .	35
70 „ 76	. . . .	17

2. There were 3 females and 97 males. 3. Of the 100 patients, 87 recovered, and 13 died. Six of the recoveries, owing to the presence of other important complications, were incomplete; complete recovery, therefore, taking place in 81. Eight of the 13 deaths were due to fatal causes foreign to the operation, so that but 5 of these actually ensued from the operation itself. 4. The calculi in 5 cases were composed of oxalate of lime; in 4, the nuclei were of uric acid, and the exterior phosphatic; in 31 there were phosphatic, and in 60 uric acid, calculi. 5. The *séances* averaged about 8. 6. The mean duration of the lithotriptic treatment was thirty-six to thirty-seven days. 7. The greatest weight of the calculous mass that was removed exceeded 32 drachms (8 Loth); the least amounted to a drachm. 8. Among the 100 cases there were 10 single and 2 double relapses. 9. Fifteen of the operations were performed under partial or complete narcosis, verifying the correctness of the conclusions drawn by the author in his essay upon the advantage of inducing narcosis in lithotripsy. 10. The patients were natives of the following countries:—32 Austria, 32 Hungary, 5 Bohemia, 4 Moravia, 4 Servia, 4 Hamburg, 3 Bavaria, 3 Russia, 2 Croatia, 2 Saxony, 2 Prussia, 1 Gallicia, 1 Italy, 1 Sclavonia, 1 Dalmatia, 2 Hanover, and 1 Moldavia.

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D. (Lond.)

LETTSOMIAN LECTURER ON MIDWIFERY, ETC. ETC.

I. Physiology and Pathology of the Unimpregnated State.

1. *A Few Words on Sterility.* By Dr. Carl Mayer. (Virchow's Arch., Sept. 1856.)
2. *The Employment of Carbonic Acid Gas as a Local Anæsthetic in Painful Affections of the Womb.* By M. Follin. (L'Union Médicale, Dec. 30th, 1856.)
3. *The History and Statistics of Ocariotomy, and the circumstances under which the Operation may be regarded as safe and expedient.* By George H. Lyman, M.D., Medical Examiner, Philadelphia. Dec. 1856.
4. *Involuntary Production of the Lacteal Secretion by Electricity.* By Dr. A. Auber, of Macon. (L'Union Médicale, Jan. 20th, 1857.)
5. *Cure of Vesico-Vaginal Fistula by Pinching and Bruising the Vaginal Mucous Membrane.* By Dr. Bertel. (L'Union Médicale, Feb. 1857.)

1. Dr. Carl Mayer's "few words" ostensibly extend over thirty pages. At the conclusion, he gives the following statement of the pathological examination of 272 sterile women.

In 2, no uterus.
„ 60 } 97 { anteflexions.
„ 37 } { retroflexions.
„ 35 } 38 { anteversions.
„ 3 } { retroversions.
„ 42 inflammatory irritations of the outer genital organs and os vaginæ; and amongst these, in fourteen women long married, an unbroken hymen.

In 51 chronic endometritis.
" 25 chronic oophoritis.
" 23 ovarian tumours.
" 12 uterine polypi.
" 6 fibroid tumours of uterus.
" 9 hypertrophy of uterus.
" 1 elephantiasis of outer genitals.
" 6 women, no pathological condition of genitals was found.
" 16 anteflexions—1, irritation of pudenda; 4, endometritis chronica; 5, oophoritis chronica; 3, ovarian tumours; 1, polypus; 2, hypertrophy of uterus.
" 13 retroflexions—1, irritation of pudenda; 6, endometritis chronica; 2, oophoritis chronica; 2, ovarian tumours; 1, fibroid tumour; 1, elephantiasis of pudendi.
" 10 anteversions—2, irritation of pudendi; 3, endometritis; 2, ovarian tumour; 1, polypus utero; 2, hypertrophy of uterus.
" 1 retroversion there was chronic oophoritis.

2. M. Follin describes a method of applying carbonic acid gas to the uterus. He confirms by his experience the utility of Dr. Simpson's mode of treating painful conditions of the womb by means of this agent.

3. From time to time we have had several statistical analyses of the cases in which ovariotomy has been attempted or performed. Dr. Lyman's—the most recent—embodies additional facts, some of which we extract. Dr. Lyman's researches embrace 300 cases. Of these, 33 were performed by Dr. W. L. Atlee, 32 by Dr. F. Bird, and 50 by Dr. Clay. Of Dr. Atlee's cases, the operation was completed in 19; not completed in 4, on account of adhesions, &c.: of the complete operations, 11 died, or 58 per cent.; of the 4 incomplete cases, 1 died. Of Dr. Bird's cases, 12 were complete and 20 incomplete operations. Of the complete, 4 died, or 33 per cent. Of Dr. Clay's cases, 40 were complete and 9 incomplete. Of the complete, 14 died, or 34 per cent.; of the incomplete, 2 died. (This account of Dr. Clay's operations does not appear to be the latest of that author. In Dr. Clay's 'Obstetric Surgery,' London, 1856, we find the following summary:—71 operations, of which 49 recovered, and 22 died.)

Dr. Lyman's analysis shows that:

In three-tenths of the cases, the operation could not be completed.

The rate of mortality in all the operations was 40·13 per cent.

In seven-tenths, the operation was completed, with a resulting mortality of 42·78 per cent.

In the unfinished operation, the mortality was 30·68 per cent.

The proportion between the whole number of recoveries *after the removal of the tumour*, and the whole number of operations undertaken in hope of such a result, we find to be as 39·66 to 100, or less than two-fifths!

Adhesions caused the abandonment of the operation in 22·06 per cent. of the whole number, or caused 77·27 per cent. of the failures.

(Dr. Clay states that adhesions constitute no contra-indication to the operation—except pelvic adhesions.)

No tumour was found in nearly three per cent. of the whole.

When adhesions complicated the removal, 47·82 per cent. died; when no adhesions complicated the removal, 32 per cent. only died.

Of the whole number of short incisions, 30·76 per cent. died; of those completed, 38·33 per cent. died; of those not completed, 22·80 per cent. only died.

Of the whole number of long incisions, 41·95 per cent. died; of those completed, 41·46 died; of those not completed, 45 per cent. died.

Previous tapping does not always cause adhesions.

As far as these cases go, the mortality is least between the ages of fifty and sixty, and greatest under twenty.

The mortality is least when the disease is of between three and four years' duration.

There is but little difference in the mortality between the married and single.

The right ovary is more often diseased than the left, though less so than often stated.

Of the above fatal cases, 42·35 per cent. were from peritonitis, 25·52 per cent. from hæmorrhage.

Death ensued, upon an average, on the eighth day, the average of deaths from peritonitis being also the eighth day; and of those from hæmorrhage, twenty-two hours.

In more than ten per cent. of the cases, important errors of diagnosis occurred.

Dr. Lyman submits the following deductions:

(1.) The mortality attendant upon ovariotomy is no greater than it is after other capital operations.

(2.) The mortality resulting from extensive incisions of the peritoneum is generally over-estimated.

(3.) Fully-developed cystic disease tends rapidly to a fatal result.

(4.) No method of treatment heretofore devised for it is so successful as extirpation—excepting, possibly, that by injection with iodine, of the results of which we have as yet insufficient statistics.

(5.) The operation is unjustifiable in the early stages of the disease.

(6.) After active development has commenced, with the supervention of constitutional symptoms, the sooner the operation is performed, the greater the chance of recovery.

(7.) No rule can be laid down as to the length of the incision, other than the general one—that the shorter it is, the less the mortality; and that therefore the primary incision should always be small, and extended afterwards as may be necessary, according to the exigencies of each particular case.

(8.) If, after the operation is commenced, extensive adhesions should be discovered, either the complete abandonment of the intended extirpation, or the attempt to cause suppuration and gradual contraction of the cyst by means of a permanent external opening, are to be preferred to the division of the adhesions and completion of the operation, as originally designed.

4. The observation of Dr. Auber is exceedingly interesting. He was applying the volta-faradic apparatus of Duchenne on the right breast of a woman who had been delivered seven months, who had not suckled. The object of applying it was to remove an anæsthesia of the skin. After the third application, the patient complained of being *as she was after her milk fever*, and obliged to cover her breasts, *both* of which moistened her dress. On the fifth application, some milk, of which a spoonful was collected, was examined by microscope. It seemed quite similar to that of a woman newly delivered.

Dr. Auber refers to a case in which the lacteal secretion was in like manner produced by M. Becquerel. He anticipates the possibility of thus making any woman fit to suckle.

5. Dr. Bertel records a case of cure of vesico-vaginal fistula by a method which consists in pinching and crushing the vaginal mucous membrane. A woman, aged fifty, had suffered from a fistula for fourteen years. It was deeply-seated, and engaged the body of the bladder on a level with the os tincæ. It was capable of admitting the tip of the finger through the vagina into the bladder. It was slightly oval; its larger extremity was directed towards the fundus of the bladder; its edges are somewhat thickened and hard; it was not funnel-shaped. The lesion followed a laborious delivery. M. Bertel applied a pinching instrument to nip the edges of the fistula together, which he

promises to describe hereafter when made more presentable and scientific. On the third day it was found that no urine escaped into the vagina. On removing the instrument, the opening was found closed. In its place was a ridge of a reddish-brown colour, easily bleeding, half the size of a cherry. Henceforth all urine passed by the urethra—no opening could be detected. The cure Dr. Bertel describes as perfect.

II. Physiology and Pathology of Pregnancy.

1. *On the Aptitude for Conception and the Duration of Pregnancy in Women.* By Prof. W. L. Grenser. (Schmidt's Jahrb., No. 12. 1856.)
2. *On the Insanity of Pregnant Women.* By Ideler. (Annalen des Charité-Krankenhauses zu Berlin. 1 Heft, 1856.)
3. *The Physiological Glycosuria of Lying-in, Suckling, and some Pregnant Women.* By H. Blot, M.D. (Gaz. Hebd. 1856.)
4. *Pregnancy in a Two-horned Uterus.* By Dr. Lumpe. (Wochenbl. Zeitschr. der k. k. Gesellssch. d. Aerzte zu Wien. August, 1856.)

1. Professor Grenser's interesting communication is in the form of an invitation to German physicians to collect observations on the aptitude for conception and the duration of pregnancy. He refers to the ovular theory and the observations of Bischoff, and lays down the following points as those which call for investigation:

(1.) The duration of menstruation.

(2.) The moment when the rupture of the Graafian follicle and the escape of the ovulum take place.

(3.) The duration of the aptitude for fructification of the escaped ovulum.

(4.) The life-duration of the ovule within the female genitals; and lastly,

(5.) The menstruation type.

(6.) Whether there really exist in woman an interval between the menstruation periods, during which complete coitus cannot be fruitful; whether this can only be the case in the four-weekly menstruation type and in deferred menstruation, or also in the three-weekly menstruation type, and so forth.

2. The memoir of Ideler is an elaborate argumentative essay, written in support of the proposition that the insanity of pregnant women proceeds from pure mental disorders abstracted from physical lesions. It is an example of the emotional theory of the etiology of insanity carried to excess. Ideler, it is well known, holds a foremost position amongst representatives of the spiritual, as contra-distinguished from the materialist psychologists. He cites two cases in illustration of his doctrine; and since it is useful to look at the important subject of insanity in women as associated with the functional activity or disorder of the generative system from the so-called spiritual point of view, we extract the key-passages of Ideler's commentary, and a summary of the cases. He observes, that a certain dread that death will happen at the period of delivery is so constant in pregnant women, that it may be looked upon as natural. It will not, he says, be denied that the fear of death of pregnant women is frequently the cause of severe nervous attacks, abortions, and the unfortunate course of delivery and puerperal state. He does not, however, undertake to follow out the etiological relation of the death-fright to insanity. He says, that in many cases the entire physiognomy, and every appearance and act of the insane, bear the complete expression of fear. In the full development of despair, the patients lament that the devil, murderers, wild beasts, destroying forces of nature, flames, deluges, rush upon them. The two following cases he relates as unquestionable instances of the origin of mental disorder in death-fright.

Frau H., thirty-six years old, began to menstruate early in life, and continued to do so regularly. She married at twenty; had had four deliveries; the first labour was accomplished by the forceps, through which she was so severely affected, that her child had to be given over to a nurse. An abortion of a four or five months' fœtus followed, after which she went abroad too soon, and brought on uterine hæmorrhage and such prostration that she expected to die. The third delivery brought a full-grown child into the world. In 1854, being for the fourth time pregnant, the recollection of the severe forceps-delivery and the nearly fatal flooding was constantly present, and wrought the conviction that she would not survive the next labour. In consequence of these depressing cares, she felt herself also bodily weak, and lost her sleep. She often felt herself unequal to her work, especially when pains in the right side of the abdomen came on. Having gone out one day in a short gown, she fancied that all the people in the streets looked at her, and on her return home that they came through the walls to mock her. From this time she had the vision of a grave before her, even during waking. Suddenly she conceived aversion for her husband; fancying he wanted to murder her, she tried to jump out of the window. This anguish increased, so that at last a true outbreak of mania took place. No furor. She was admitted into the insane division of the Charité, where the described condition persisted for several days. *On account of her pregnancy, all curative treatment was postponed;* and, for precaution's sake, the strait-waistcoat was put on. She was delivered of a healthy child. The puerperal period passed well, but without change in her mental state. She remained for months afterwards stupid, inactive, buried in herself. A persevering use of the douche first aroused her from this apathy. She ultimately recovered.

The other case brought forward as an instance of pure primordial psychical disturbance, is equally characterized by evident somatic disorder. The state of gestation is itself a somatic complication largely, if not primarily, concerned in the etiology of these cases of mental alienation. It must be obvious that the author has selected a theme beyond the power of human reason to demonstrate; for who can hope to disentangle mind from matter, and study the essential properties of each apart from the other?

3. For the particulars of Mr. Blot's observations, we refer the reader to the last Report on Physiology in this Review, Jan. 1857, p. 250.

4. Dr. Lumpe refers to a description, published by him in 1843, in the Oesterr. Med. Wochenschr., of the autopsy of a woman who died of metroperitonitis four days after delivery. In this case the uterus was found divided longitudinally as far as the os internum. The os internum and cervix were simple. The right cavity, which contained no ovum, was enlarged, and lined with a thin vascular decidua.

The following case is a further illustration of this remarkable anomaly. A woman, aged thirty, who had lived in barren wedlock for ten years, sought advice on account of menorrhagia and leucorrhœa. Dr. Lumpe found the external genitals quite normal; the vagina, simple for the first third from outlet, was apparently divided into two equal canals throughout the upper two-thirds. The septum presented a complete duplicature of the vaginal mucous membrane, which was attached in front along the urethra, and hung loose, flapping like a sail behind; so that during exploration by the finger, it sometimes covered the right and sometimes the left cervix in such a manner, that on superficial examination, the bicornute condition of the uterus might have been overlooked. An unequivocal solution was only obtained by using two fingers. From each half-vagina was felt a completely developed cervix. Both cervices were of equal size, and on same level; they diverged from the point of union to either side, nearly at a right angle; they were quite symmetrical, and provided with a small cross-fissured os, which admitted a sound. No clear examination of the bodies of the uterus could be made by palpation;

but the fundus appeared to be bent right and left, exactly as was the case with the two cervices.

Dr. Lumpe had no opportunity of seeing this case again till some time after, when he was suddenly called to separate an adherent placenta. In performing this, Dr. Lumpe found a complete cavity, bent towards the left like a retort; the placenta was adherent to the fundus; this had a remarkably long oval form. The right non-pregnant uterine-horn had been so much hypertrophied, that it reached nearly half the size of the other. The vaginal portion of the right horn was quite effaced, and its orifice only marked by a soft, cushion-like ring.

Labour had come on at the beginning of the ninth month without obvious cause, and had proceeded naturally and easily under tolerably strong pains. The child was delicate, but lively. The puerperal period was passed favourably. The involution of the uterus proceeded regularly as in the undivided uterus.

III. Labour.

1. *Inversion of the Uterus replaced on the Third Day.* By J. G. Porter, M.D., of New London, Conn. (American Journal of Medical Science, July, 1856.)
2. *Complete Inversion of the Uterus at the time of Labour, with remarkable Absence of the Ordinary Symptoms.* By W. F. Montgomery, M.D. (Dublin Hospital Gazette, April, 1856.)
3. *A Repelling Ring in Shoulder Presentations.* By Dr. Camille Bernard (d'Apt). (L'Union Méd., Dec. 30th, 1856.)
4. *Rupture of the Uterus; being a Sequel to a Monograph on this Subject of* 1848. By James D. Trask, M.D. (American Journal of Medical Sciences, July, 1856.)

1. Dr. Porter's case of inversion of the uterus is an illustration of the possibility of replacement on the third day. A lady, aged thirty, had a not severe labour on the 18th of March. The delivery of the placenta was delayed two hours, owing doubtless to atony of the uterus. It came away somewhat torn, but undue interference was disclaimed. Previous and subsequent to its delivery there was much flooding and great prostration. On getting up in bed some hours after to urinate, the uterus made a complete descent through the external parts. Increased prostration followed; micturition became impossible until after the re-position of the uterus. She was seen by Dr. Porter three days after the accident. Flooding not severe, but great prostration. The uterus filled the vagina. Gentle but gradually increasing force with the back of the flexed fingers caused the mass to diminish in size. As it grew less in dimensions it was more easily grasped, and ultimately the uterus, with the hand encircling and compressing it, was used as a stem, with which upward pressure was exerted. The restoration then became much more rapid. Complete relief followed the re-position. A mild attack of phlegmasia dolens followed, but she eventually recovered.

2. Dr. Montgomery's case of inversion of the uterus illustrates another variation from the ordinary pathological history of these cases. Mrs. M. was delivered of her fourth child, head presenting. The labour had been lingering, and two half-drachm doses of ergot had been given, with little apparent effect. As the placenta did not seem likely to come away, the womb being sluggish, the nurse was directed to make pressure over the uterus, while the doctor drew upon the cord. In about ten or fifteen minutes the placenta came away, followed on the instant by a *large round tumour*, which passed completely out of the vagina. This was ascertained to be the uterus completely inverted. It was returned within the vagina without much difficulty, but pressure on the fundus failed to restore it to its proper place. There was some hæmorrhage, *but not much.* There was a pressing desire to make water, but scarcely any

other symptom. This was her condition when Dr. Montgomery was called in, an hour and a half after delivery. The doctor found the whole pelvic cavity filled up with a firm fleshy tumour, *perfectly insensible;* and in passing the finger along it upwards, it was found to terminate in a cul-de-sac all around. The patient was placed under chloroform; the tumour grasped, it was compressed as strongly as possible from the lateral circumference towards the centre, and at the same time pushed upwards and forwards towards the umbilicus; for several minutes this seemed without effect, but at length the tumour began to yield, receding and gliding by a spontaneous movement *of the whole tumour* upwards, and not of the lowest part of the fundus, re-entering itself; and then all at once it almost sprang away from the hand, and was restored to its place. The resistance to the re-position was so great, says Dr. Montgomery, that he does not think he could have succeeded had not the patient been under the influence of chloroform. She did well.

3. Dr. Camille Bernard has contrived a new instrument for the purpose of pushing back the shoulder when it presents, so as to admit of bringing down the head or feet in its place. The instrument consists of a ring about two inches diameter, moveable on two stems sixteen inches long. It offers the following advantages: it embraces firmly the arm by its base; it pushes back the shoulder without danger in any direction, and keeps it up as long as may be desired, whilst you are either proceeding to seize it by the hand, or to effect turning by the feet. Dr. B. relates a case in which he used his instrument. The left shoulder presented the sternum forwards. The arm was outside the vagina, and livid. Child dead. Had the child been born living, he would have preferred turning by the head; but as it was, he determined on podalic version. He passed his ring up over the arm, and fixed it in the armpit by fixing the two stems one against the other; then supporting the uterus externally with the left hand, he gently pushed back the shoulder; it gradually receded, and was soon enclosed within the vagina. He then passed his left hand into the uterus, which was not obstructed by the instrument, and having seized the right leg, he imparted a movement of evolution by the combined operation of traction on the leg, and pushing by the instrument. The woman did well.

4. Dr. Trask's paper on the Statistics of Rupture of the Womb is a valuable continuation of his former well-known contribution on this subject. This first paper was published in the 'American Journal of Medical Sciences' in 1848. It contained an analysis of 303 cases. He now adds more than 100. We can only reproduce some of the more striking deductions. The cases now presented, he says, afford still further confirmation of the views urged more especially by Dr. Murphy, that a diseased condition of the womb is frequently met with in cases of this accident. Thus, the uterus was *thin and brittle* in 1 case; *softened* in 7; in 1 the *peritoneum was* extensively *detached;* in 2 the uterus was in a *scirrhous* condition; in 1 the uterus had long been *diseased;* in 2, deeply *ecchymosed;* in 1 the walls were *flabby;* in 1 there was *great development of muscle.* Of 22 cases in which the point is distinctly stated, in 19 there was positive disease, and in 3 no appreciable disease. Dr. Trask observes, that his second collection of cases exhibits a larger proportion of cases in which the womb is reported to have been diseased than does his first. He attributes this to the greater attention directed to this point since 1848. (It is especially desirable, in future investigations, to examine the structure of the womb in the neighbourhood of the rent, by the aid of the microscope, and to note if the muscular fibres are advanced in the process of fatty metamorphosis which takes place normally after labour.)

Dr. Trask observes, that the cases in which disease was found were cases of *spontaneous* rupture.

In 11 cases the pelvis was more or less contracted. The head was impacted from disproportion in 3 cases. There was obliquity of the os in 2 cases, the pains being directed against the pubes. In 1 case the descent was prevented by an enlarged ovary.

Rigidity of the Os.—In 3 cases obstinate rigidity of the os appeared to be the cause of rupture.

Obstructing Bands in the Vagina.—The resistance of these apparently caused rupture in 4 cases.

Time from Beginning of Labour to Rupture.—Taking the whole of the cases (i.e., of both series) in which this is specified, it is found that

Rupture occurred	in	6 hours and less .		in 38	cases.
,,	,,	12 ,,	over 6	in 36	,,
,,	,,	18 ,,	over 12	in 10	,,
,,	,,	24 ,,	over 18	in 20	,,
,,	,,	36 ,,		in 16	,,
,,	,,	48 ,,		in 14	,,
,,	,,	Three days and less		in 11	,,
,,	,,	Four ,,		in 2	,,

Comparing these with the duration of labour in the 15,850 cases reported by Dr. Collins, we find that 80 per cent. terminated within six hours, 16 per cent. in from six to twelve hours.

In 5 cases ergot was given.

Situation of the Rupture.—In rupture during pregnancy, 4 involved the fundus. During labour: Of the entire number of cases, 110 are distinctly spoken of as involving the cervix, 17 the fundus, 71 the body of the womb. Of these 71, by far the larger part are reported as ruptures of the anterior or posterior part, or of the right or left side. In 4 cases the peritoneum was not involved.

The largest number of cases occurred at the age of thirty years.

It was very frequent in first pregnancies.

In 3 cases the rent took place with a cracking noise, heard by the patient or bystanders.

Influence of Delivery on Mortality.—Total of all cases delivered, 207. Of these, 77 recovered, or 37 per cent. Total of all cases undelivered, 115. Of these, 27 recovered, or 23·5 per cent.

IV. Physiology and Pathology of the Fœtus.

Case of Malformation of the Extremities through Stricture, as a Contribution to the History of Spontaneous Amputation. By Dr. Frickhoffer, of Idstein. (Virchow's Arch. für Pathol. Anat., Sept. 1856.)

Instances of spontaneous amputation of the fœtal limbs in utero, especially where the *modus operandi* seems clear, are of interest, as serving to illustrate the theory and conclusions upon this question so ably set forth by Dr. Montgomery. The subject of Dr. Frickhoffer's case was the tenth child of healthy parents; itself was a well-nourished, strong boy. The following particulars were observed the day after birth:

Above the left elbow-joint there was a deep stricture of the skin and other soft parts down to the bone; the parts below this stricture were atrophied, but œdematous; the hand-joint on the radial side bent, and the fingers of this hand in permanent contraction.

Below the left knee-joint there was a still sharper stricture, as if one had divided the soft parts by the circular incision down to the bone; still the cutis was continued from one cushion to the other; the whole lower leg was atrophied, but œdematous, and upon its fore-part were three parallel and cross-running depressions; the foot itself was twisted inwards, as in *talipes varus.*

The middle and ring-fingers of the right hand were united by skin from the second joint; on the back of the ring-finger, directly over the joint between the fore and middle phalanx, there was a protuberance the size of a pea, caused by a double stricture before and behind it, like those on the left arm and leg.

The most remarkable appearance was a strong ligament, which appeared to be a continuation of the skin running from the radial aspect of the foremost joint of the index finger of the right hand; this was an inch and a half long. A similar, but small, wart-like continuation, was found at the extremity of the foremost joint of the right little finger.

In other respects, the child was normally made, strong, and well nourished.

Unfortunately, the placenta and remains of the ovum had been destroyed at time of labour.

According to the midwife, there was great difficulty in the delivery of the shoulders, and she had aided the expulsion by hooking her finger in the armpit, whilst during a pain a distinct cracking was heard. No twisting of the cord was observed. The author is convinced that all the strictures were caused by the cutting of the strong ligamentous continuations such as described.

The first thought was to complete the amputation of the leg, to rid the child of what was presumed would soon be a burden. But after ten weeks, it was observed that the strictured spots had risen up considerably, and the limbs had acquired a more perfect nutrition.

BOOKS RECEIVED FOR REVIEW.

Nederlandsch Tijdschrift voor Geneeskunde. Amsterdam, 1857.

North American Medico-Chirurgical Review. Jan. 1857.

Report of the Water and Sewers Committee on the Sewerage of the Mysydd District, and other parts of Swansea. 1856.

First, Second, Third, Fourth Quarterly Reports of the Medical Officer of the Borough of Swansea. 1854.

The Medical Report on the Sickness and Mortality of the Borough of Swansea for the Year 1854. By W. H. Michael. Swansea, 1856. pp. 28.

The Morningside Mirror. Vol. XI. No. 1—12. Vol. XII. Nos. 1 and 2.

The Medical Profession in Ancient Times. By John Watson, M.D. New York, 1856. pp. 222.

Defects of Sight: Their Nature, Causes, Prevention, and General Management. By F. Wharton Jones, F.R.S. London, 1856.

A Compendium of Domestic Medicine, and Companion to the Medicine Chest. By John Savory. Fifth Edition. London, 1856. pp. 364.

Pathological Chemistry, in its Application to the Practice of Medicine. Translated from the French by MM. Becquerel and Rodier. By S. T. Speer, M.D. London, 1857. pp. 566.

On Rheumatism, Rheumatic Gout, and Sciatica. By H. W. Fuller, M.D. Second Edition. London, 1856. pp. 454.

The Change of Life in Health and Disease. By E. J. Tilt, M.D. Second Edition. London, 1857. pp. 307.

Wandsworth Asylum. A Letter to the Committee of Visitors of the Surrey Lunatic Asylum. By C. Snape, Medical Superintendent, &c. London, 1856.

The Stomach and its Difficulties. By Sir J. Eyre, M.D. Fourth Edition. pp. 162.

Lehrbuch der Pathologischen Anatomie. Von Carl Rokitansky. 3te Auflage. Zweiter Band. Zweite Lieferung. Wien, 1856.

Military Sanatoria. Letter, &c. By P. Pincoffs, M.D. Second Edit. London, 1856.

The Prostate Gland and its Enlargement in Old Age. By Decimus Hodgson, M.D., Edinburgh. London, 1854. pp. 84.

Clinical Lectures on Cancer and Cancroid Growths. By Maurice H. Collis, M.B. Dublin, 1856. (Reprint.)

Norsk Magazin for Lægevidenskaben. Band x., Hefte 9 and 10.

The Physiological Anatomy and Physiology of Man. By R. B. Todd, M.D., F.R S., and W. Bowman, F.R.C.S., &c. Part IV. Section 2. London, 1857.

On the Physiological Action of Atropine. By Geo. Harley, M.D. (Reprint.)

First Report of the Clinical Hospital for Diseases of Children. By A. Schoepff-Merei, M.D., and J. Whitehead, M.D. Manchester, 1856.

On Tubercle. By Dr. M'Cormac. (Reprint.)

First Lines in Chemistry. A Manual for Students. By A. J. Bernays, Ph.D., F.C.S. London, 1855. pp. 484.

Ranking and Radcliffe's Half-yearly Abstract of the Medical Sciences. Edited by W. H. Ranking, M.D., and C. B. Radcliffe, M.D. Vol. XXIV.

Hints on the Pathology, Diagnosis, and Prevention and Treatment of Thoracic Consumption. By J. C. Hall, M.D. Third Edition. London, 1856. pp. 148.

On the Drainage and Sewage of London and of Large Towns. By Jas. Copland, M.D., F.R.S. London, 1857. pp. 30.

On the Theory of Final Causes. By Dr. M'Cormac. Belfast, 1857.

The Liverpool Medico-Chirurgical Journal. No. I. Jan. 1857.

Braithwaite's Retrospect of Medicine. Vol. XXXIV.

The Medical List; or, English Medical Directory for 1857. London.

Annual Medical Report of the Kent Lunatic Asylum at Barming Heath, Maidstone. Maidstone, 1856.

A Descriptive Catalogue of Preparations illustrative of Diseases of the Ear, in the Museum of Jos. Toynbee, F.R.S. London, 1857. pp. 127.

Outlines of Ophthalmology. Third Edition. By W. Mackenzie, M.D. London, 1856. pp. 72.

The Assurance Magazine. Jan. 1857.

Tables for the Chemical and Microscopical Examination of Urine. By Dr. Lionel Beale. London, 1856. pp. 20.

An Essay on the Therapeutical Effect of Purgatives on the Horse. By John Field, M.R.C.V.S. London, 1856.

On the Treatment of Iritis without Mercury. By H. W. Williams, M.D. Boston, 1856.

First Quarterly Report on the Sanitary Condition of the Whitechapel District. By John Liddle, Med. Officer of Health. 1856.

Adulterations Detected; or, Plain Instructions for the Discovery of Frauds in Food and Medicine. By A. H. Hassall, M.D. London, 1857. pp. 712.

Madeira: its Climate and Scenery. A Handbook, &c. By R. White. Second Edition. Edited by James Y. Johnson. Edin. 1857. pp. 338.

On Certain Painful Muscular Affections. By Thomas Inman, M.D. Liverpool, 1856. pp. 49.

Statistical Notes on some Diseases of India. By John Waring, Esq. pp. 63. (Reprint.)

Annual Report on the Sanitary Condition of the Belgrave Sub-District. By L. J. B. Aldis, M.D., Med. Officer of Health. 1857.

Some Observations on the Ova of the Salmon in relation to the Distribution of Species. By J. Davy, M.D., F.R.S. 1855.

Discovery of the Cause, Nature, Cure, and Prevention of Epidemic Cholera. By M. L. Knapp, M.D. Cincinnati, 1855. (Reprint.)

The Constitution of Women, as illustrated by Abdominal Cellulitis. By C. Bell, M.D. Edin. 1856. (Reprint.)

On the Protection and Modifying Powers of Vaccination. By C. G. Seaton, M.D. London, 1857. (Reprint.)

Cranial Presentations and Cranial Positions. By R. N. West, M.D. Glasgow, 1857. pp. 60. (Reprint.)

On True Parthenogenesis in Moths and Bees. By C. T. E. V. Siebold. Translated by W. S. Dallas. London, 1857. pp. 110.

Epidemic Cholera, Diarrhœa, and Dysentery. By H. Jeanneret, M.D. London, 1857. pp. 34.

Torquay in its Medical Aspect. By C. R. Hall, M.D. London, 1857. pp. 165.

Société Impériale de Médecine de Constantinople. Discussion sur le Typhus. Constantinople, 1856. pp. 200.

Paddington Sanitary Report for the Year 1856. By J. B. Sanderson, M.D., Medical Officer of Health. pp. 24.

The Constitution of the Animal Creation as expressed in Structural Appendages. By G. L. Holland, M.D. London, 1857. pp. 310.

American Monthly. Jan. 1857. Edited by C. H. Parker, M.D., and J. H. Douglas, M.D. New York.

Edinburgh Essays by Members of the University, 1856. Edinburgh, 1856. pp. 350.

Excelsior; or, Murray's Royal Asylum Literary Gazette. No. I. Jan. 1857.

Of Nature and Art in the Cure of Disease. By Sir J. Forbes, M.D. London, 1857. pp. 264.

On the Pathology, Symptoms, and Treatment of Ulcer of the Stomach. By W. Brinton, M.D. London, 1857. pp. 227.

The Anatomical Remembrancer; or, Complete Pocket Anatomist. Fifth Edition, London, 1857. pp. 248.

Medical Notes on the Militia. By J. Ingham Ikin, F.R.C.S. London, 1857. (Reprint.)

The Transactions of the Medical American Association. Vol. IX. Philadelphia, 1856.

Compressed Air as a Therapeutic Agent in the Treatment of Consumption, &c. By Arch. Simpson, M.D. Edinburgh, 1857.

Clinical Lectures on Certain Diseases of the Urinary Organs and Dropsies. By R. B. Todd, M.D., F.R.S. London, 1857. pp. 435.

On Diseases of the Skin. By Erasmus Wilson, F.R.S. Fourth Edition. London, 1857. pp. 747.

Twenty Aphorisms. By H. M'Cormac, M.D. London, 1857.

On Diseases of the Bladder and Prostate Gland. By W. Coulson, Surgeon to St. Mary's Hospital, &c. Fifth Edition. London, 1857. pp. 640.

First Annual Report of the Medical Officer of Health, made to the Vestry of St. James's, Westminster. 1857.

Cases and Observations of Spinal Deformity. By Samuel Hare, F.R.C.S. London, 1857. pp. 88.

Address to the Medico-Chirurgical Society of Edinburgh. By J. Miller, F.R.S.E., &c. Edinburgh, 1857.

Statistical Society of London. First Report of the Committee on Beneficent Institutions. 1. The Medical Charities of the Metropolis. London, 1857. pp. 68.

Human Physiology, Statical and Dynamical. By J. W. Draper, M.D. London and New York, 1856. pp. 649.

Neue Orthopädische Behandlungsweise, veralteter Spontanen Luxationen im Hüftgelenke. Von Johannes Wildberger. Würzburg, 1855. pp. 112.

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