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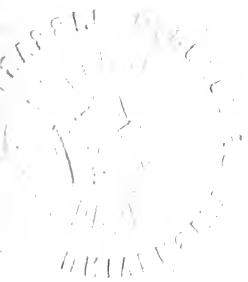

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Analytical and Critical Reviews.

I.—Railway Injuries.[1]

A CERTAIN very distinguished surgeon, whose tongue could at times be as sharp as his knife, is reported to have once said, "If you want to make a little money easily, manage to be in a railway accident; don't be killed; and then buy Erichsen's book on 'Railway Spine.' Such a sentiment, though in a very exaggerated form, gives an idea of the sort of views held by some so-called practical surgeons, often elderly, always opinionated, on the most difficult and perplexing sets of cases, whose anomalous symptoms are accounted for by the theory of concussion or wrench of the spine.

The less educated, and consequently more easily amused portion of the general public, including about nine hundred and ninety-nine in every thousand, find in the reports of trials in which sufferers claim damages from railway companies an unceasing fund of amusement and a provocative of innocent mirth; while the lawyers have not only a magnificent field for the exercise of their wits, but a most profitable excuse for frequent "refreshers" in the long examinations and cross-examinations of opposing medical evidence.

The proverb that "doctors differ," though applied originally to the disputants of the Sorbonne, is still useful to turn an epi-

[1] 1. *On Concussion of the Spine, Nervous Shock, and other obscure injuries of the nervous system in their clinical and medico-legal aspects.* By JOHN ERIC ERICHSEN, Fellow of the Royal College of Surgeons, Senior Surgeon to University College Hospital, and Professor of Clinical Surgery at University College, Consulting Surgeon to the Hospital for Diseases of the Nervous System, to the Hospital for Children, Chelsea, &c. &c. London, 1875.

2. *Legal Reports of 'Times' Newspaper, passim.*

gram regarding our profession, and in no cases are these differences so sadly prominent as in the reports of the trials to which we are now alluding.

The easiest and pleasantest, because wickedest and most uncharitable explanation of such differences of opinion, is the one which at once approves itself to the forensic intellect, and even draws a simper or a shrug of the shoulders from the tradesman and small shopkeepers who furnish the jury, namely, that the doctors are paid and will say whatever suits the side that pays them. The brutal ignorance and malice which can impute such motives to members, often most distinguished ones, of a profession, which more than any other signalises itself by its unpaid services and unrequited devotion, might almost raise a smile, were it not so frequently repeated, that the weaker vessels of the public, and even of the profession, seem almost beginning to believe it.

We hope to show in the following brief notice of the excellent and valuable work which heads this article some of the reasons which explain the remarkable differences of opinion which really eminent men hold, and are entitled to hold, regarding the diagnosis, prognosis, and treatment of cases of railway injury—differences, in many cases, which cannot be avoided and may never be reconciled.

If two engineers differ—and even they have their disputes—as to the power of a given arch or beam to stand a given test, they have the immense advantage of being able to experiment by models, by samples, and to settle what amount of strain the iron will bear before it yield, or what weight the arch will support.

Alas! our materials are sadly various; we cannot order patients to sample; we cannot say that if one man has stood a blow on the small of his back from an iron buffer, coming with a force equal to a weight of ten tons, that henceforward we may be sure that all such blows will be borne by all such men with equal impunity. We have no such fixed data; our sources of error are infinite—various—as the individuality of every member of the human race. A hereditary taint of scrofula may make the blow, which the muscles of the athlete scarcely seemed to feel, a source of lifelong disease; an inch of difference in direction may destroy the spinal cord, or merely bruise the muscles of the hip.

Again, to add to our difficulties, our materials are living beings with individual interests, and into our calculation must come the possibility of deception. There are some cases, fewer probably than either the profession or the public believe, in which the patient, shrewd, persevering, and unscrupulous, deceives not only the jury, the railway company, but even his medical atten-

dant; and such deception it is almost impossible to detect and expose.

Some years ago we were engaged professionally in the trial of a railway case in which the claim was a very large one. The plaintiff, who had been attended by two eminent medical men, had varied and anomalous symptoms, chiefly subjective, the recital of which fairly and temperately by his medical men evidently impressed the jury and somewhat puzzled the witnesses for the railway company. But what impressed the court, the public, the jury, and eventually caused the withdrawal of the case by the railway company and made them tender substantial though modified damages, was the appearance in court of the plaintiff himself. Pale, worn, and nervous, he was supported into the box by two stalwart friends; his crutches seemed too heavy for the nerveless arms, and after a mute appeal to the compassion of the judge, he was permitted to falter out his evidence in jerks and spasms from a comfortable arm-chair. He had been a strong, ruddy, hard-working man, now he was a wreck; he could not walk, hardly even stand without support; his occupation was gone, he did not think he would long survive it. The compromise made, the verdict recorded in his favour, the sufferer was again helped out of court, but next day we saw him again. What a change! the pallor of paint washed from his cheeks, crutches thrown aside; he was stepping jauntily along at four miles an hour on his way to the railway station, wondering perhaps if he would have again the good luck to meet with such a useful and profitable accident. With such a patient to deal with no wonder anomalous symptoms perplex the faculty, mystify juries, and excite the derision of the legal profession.

No class of surgical maladies or injuries are so perplexing to diagnose, prognose regarding, or treat, as are those nervous ailments which follow concussions, blows, wrenches or fractures of the spine. Protean in their forms, the severity of the symptoms bears no relation to the severity of the lesion; or if there is any sequence it would almost appear to be the most perplexing and contradictory one, that the more trifling the injury appears to be, the more severe and dangerous are the resulting phenomena.

As an example of this, the following cases may suffice, which have come under our own observation lately, and are unpublished.

A tall strong man was brought into the out-patient room of the hospital leaning on the arm of a friend, but walking well. He announced that he had been crushed by a runaway horse, between a heavy laden cart and the wall, and that his back was

broken. On this being doubted, he said he knew it was because he was now both bent and shortened. After he was stripped it was seen at once that a fracture with extensive angular displacement had taken place about the sixth to eighth dorsal vertebræ, the bodies of which had been crushed together. He was kept in bed for some ten weeks, made a perfect recovery, and during the whole progress of his case had not a single symptom which would imply that the spinal cord was one bit the worse for the accident. He walked with a remarkable stoop forwards, and he had lost then and there about 1½ inches of stature.

Contrast with this the case of a young lady who, in a comparatively slight railway collision, sustained a backwards and forwards movement of her head at the junction of the cervical and dorsal vertebræ; no blow or bruise was felt and very little uneasiness at the time. For more than six months the symptoms of spinal wrench or jar succeeded by inflammation have been steadily developing; stiffness of whole column with intense pain, and difficulty of moving the head, have now changed into complete paraplegia, and a fatal issue may be expected.

In the scientific, cautious, and philosophical work which heads this article Mr. Erichsen tries to classify and arrange these anomalous cases, and endeavours with very considerable success to group the various phenomena in something like a natural order or sequence.

The real practical difficulty in the whole question seems to be the explanation of the primary symptoms of concussion or wrench of the spine in those cases in which we have no evidence either of fracture or dislocation of vertebræ, of separation or laceration of ligaments, of rupture of muscle, or hæmorrhage into its substance, or even of hæmorrhage into the membranes or substance of the spinal cord. By exclusion we can get rid of each and all of these symptoms, and yet there are patients to whom something has happened, very difficult to explain, and yet evident in its results.

What this something is we do not know. Mr. Erichsen tries to give us an idea of it by an analogy. When a magnet is struck a heavy blow with a hammer, the magnetic force is found shaken or concussed out of the horseshoe; we do not know how this is done, but we know that it is so, and that the iron has lost its magnetic power. So if the spine is badly grazed, shaken, or concussed by a blow or shock of any kind communicated to the body, we find that the nervous force is to a certain extent shaken out of the man, and that he has in some way lost nerve power.

What immediate change, if any, has taken place in the nervous structure to occasion this effect, we no more know than

that change happens to a magnet when struck. But we know that a change has taken place in the action of the nervous system, just as we know that a change has taken place in the action of the iron by the loss of its magnetic force.

With reference to the effect supposed to be produced on the nervous system of the cord by the vaso-motor system producing contraction of the blood-vessels, the following interesting and suggestive passages may be quoted from Dr. M'Donnell.[1] "When we see a large snake struck across the tail with a rod, and instantly, in the twinkling of an eye, seized with a paralysis as complete as death, which yet after a time passes off again, can we attribute such a condition to the constriction of the blood-vessels of the cerebro-spinal axis? I think not; because we know that among these animals the cerebro-spinal system does not (even after the evacuation of the blood of the body) speedily cease to exercise certain functions and perform movements; and moreover, I have proved experimentally that even after decapitation a blow across the tail stops for a while all the movements which, under ordinary circumstances, persist for a considerable time. The effect, therefore, cannot be attributed to disturbance through the blood-vessels; it appears rather to be due to an altered molecular condition of the nervous centre resulting from the blow, and more or less persistent. That a shock conveyed to the central nervous system through its peripheral nerves should, with the instantaneousness of a lightning flash, lock in insensibility and motionlessness the entire frame of the creature, cannot, to my satisfaction, be accounted for through the action of the vaso-motor trunk upon the blood-vessels."

If this is not a theory it is an analogy, and a good one, and may be a comfort to us, but still it does not explain why it is that this extraordinary change does not occur in cases where the stroke or jar has been of severity sufficient to crush the spine or fracture a thigh or the base of the skull.

A stable-boy in love let himself down from the parapet of a very high bridge, and fell on his feet, breaking both his thighs; he never had a spinal symptom. A drunken workman leapt clean out of a fifth-story window, and fell on his heels with such force that the bones of the heel of both feet were pulverised, yet he had not a spinal symptom.

Mr. Erichsen comforts himself again with an analogy, which for the same circumstances was a great favourite of the late Mr. Syme. If a watch falls to the ground, and the glass is broken, the mechanism rarely suffers injury, and the watch

[1] 'Physiology and Pathology of the Spinal Cord,' 1875, pp. 204, 205.

may not even stop; if the glass escapes, the jar will stop the watch, perhaps even be sufficient to spoil its regularity for ever. In the case of the spine, the violence of the shock may have expended itself in producing the fracture or the dislocation, so that the nervous system escapes the jar.

There is in all probability some obscure change in the mutual relation to each other of the cells or ultimate structures of the nerve-tissue, a change which takes time to develop itself into what we can see by the microscope or trace by symptoms.

What, then, is the series of phenomena destined to end in such serious results? How are we to recognise such a case so as to be able to prognose it? Could we prevent the serious results did we recognise the nature of the case in time?

Unfortunately at the time of the accident the sufferer is usually quite unaware that anything out of the common has happened to *him*. His attention has been taken up entirely with the circumstances of the collision. He has felt the jolting of the carriage before it left the rails, or has found himself on an embankment, or under a mass of *débris*, almost without knowing what has happened. He is perhaps so delighted at having escaped with his life, and without broken bones, that in all probability he is in a preternaturally excited, even cheerful condition; runs about assisting every one, telegraphs that he has made a wonderful escape, shows no evidence of paralysis, concussion, shock, or strain, and may even take a long journey to reach his home. Then, Mr. Erichsen tells us, "a revulsion of feeling takes place, he bursts into tears, becomes unusually talkative and is excited. He cannot sleep, or if he does he wakes up suddenly with a vague sense of alarm. The next day he complains of feeling shaken or bruised all over, as if he had been beaten, or had violently strained himself by exertion of an unusual kind. This stiff and strained feeling chiefly affects the muscles of the neck and loins; sometimes extending to those of the shoulders and thighs. After a time, which varies much in different cases, from a day or two to a week or more, he finds he is unfit for exertion and unable to attend to business. He now lays up, and perhaps for the first time seeks surgical assistance." Pp. 157, 158.

It is difficult to improve upon, or to correct this excellent description; but in our experience we have occasionally found the stage of exaltation and excitement last for a longer time before the patient begins to discover that he has been injured, and it is quite possible that for the first two or three nights after the accident the sleep may be sound and refreshing. Still the patient is not himself, he is what the Scotch call *fey*, excited, absurdly cheerful; in an altogether exalted frame of

mind, and the longer this stage lasts, the more complete and prolonged is the subsequent condition of depression and gloom.

The one great pathognomonic sign which Mr. Erichsen describes, and all who have watched these cases will homologate his opinion, is the continuity of the chain of symptoms. However long the stage of excitement, and deep the stage of reaction, when once the secondary symptoms begin to show themselves the patient and his friends and his surgeon will all be certain of this, that the sufferer's condition has been materially altered by the accident, and that he has *never* been quite himself, *never* in his usual condition of placid, unconscious health, since the moment he received the injury.

"The friends remark, and the patient feels, that he is not the man he was. He has lost bodily energy, mental capacity, business aptitude. He looks ill and worn, often becomes irritable and easily fatigued. He still believes he has sustained no serious or permanent hurt, tries to return to his business, finds that he cannot apply himself to it, takes rest, seeks change of air and scene, undergoes medical treatment of various kinds, but finds all of no avail. His symptoms become progressively more and more confirmed, and at last he resigns himself to the conviction that he has sustained a more serious bodily injury than he had at first believed, and one that has in some way or other broken down his nervous power, and has wrought the change of converting a man of mental energy and of active business habits into a valetudinarian, a hypochondriac, or an hysterical paralytic, utterly unable to attend to the ordinary duties of life." P. 159.

A very little reflection will at once convince any thoughtful person how liable such a series of symptoms is, on the one hand, to be exaggerated or simulated by an artful patient, and, on the other, to be misunderstood, ridiculed, and even discredited in *bonâ fide* cases, under the cross-examination of an ingenious or unscrupulous counsel. The exalted, excited condition of the first few days will give the counsel a peg on which to hang a cross-examination which will show the jury that the patient had been quite uninjured, that he had been put up to simulation by his agent or medical adviser. The change of air and scene are described as idleness at the expense of the shareholders; the hysterical manner, perhaps the infirmities of temper, displayed by the patient in the witness-box, possibly the complete break-down into passionate weeping or a look of hopeless imbecility, will all be used against him by an ingenious pleader, and it will be very difficult to say him nay. While on the other supposition the clever rogue can easily learn fluently to describe and plausibly

to simulate many of these symptoms in a manner extremely difficult to detect or expose.

Again, the case may be rendered still more perplexing by a mixture in the case of real injury and intentional exaggeration. We were lately a witness in such a case, in which the conflict of medical evidence was very marked, and yet in nearly every particular such discrepancies were consistent with high scientific attainment and professional honour.

The patient had been injured in a most severe and fatal accident some six months previously. Many had been killed, and more maimed. The railway company had acknowledged liability. Public attention had been highly roused, and public sympathy was with the sufferers. Many of the claims had been settled liberally, and out of court; this one was contested on account of the very large sum demanded.

The patient was a Polish Jew, powerful, bulky, well nourished, with a crafty, almost hangdog, look, and perhaps the most repulsive voice, accent, and manner that ever were heard or seen in a witness-box. Alternately sullen and fawning, with a voice at times harsh enough to have suited the cursings of the impenitent thief, at other times whining and garrulous. He told his symptoms with an exaggeration of piling up the agony, and yet with a watchfulness and caution which showed he had been coached. He asked a very large sum. He was proved to have lied about his earnings prior to the accident, and his power of walking subsequent to it. He had very little to show for his symptoms. His appetite had been good, and he was fat and well nourished. The medical witnesses differed as to his credibility, as to his subjective symptoms, and no wonder. The real evidence of paraplegia he had so masked and distorted by varied and abnormal inventions of his own that truth and falsehood were inextricably mixed. His nationality, his tongue, his voice, his habit of lying, all went against him. He got a verdict for a sum little more than one tenth of what he asked, and yet that man was seriously, probably fatally, injured, and had he been an honest, honest-looking, manly Englishman he would have got, and deserved to get, a very large sum.

Passing on to the pathological changes in the cord which lead to, or end in, more acute symptoms, Mr. Erichsen is inclined to distinguish two distinct and, indeed, widely opposed conditions, viz. spinal anæmia and spinal inflammation. Though apparently so different in character many of the symptoms of these conditions are alike, the *anæmia*, however, being in all probability a functional, and thus possibly a temporary, condition, the inflammation having organic and permanent changes in the cord substance underlying it.

Inflammatory changes of a chronic or, at least, subacute character may come on gradually and insidiously, probably within a year from the concussion. In all probability both cord and membranes will be involved in the mischief, though Mr. Erichsen, following Ollivier, tries to differentiate the symptoms of spinal meningitis from those of myelitis. As a result of both, the change in the cord is most commonly a softening, perhaps, of the white matter only, but sometimes one of induration and enlargement.

For various reasons, well given by Mr. Erichsen, there is a great lack of records of examination of the cords of those dying after spinal concussion from railway injury. One only is to be found, and this is a valuable one, from the care with which the case was observed by Mr. Gore, of Bath, and the detailed report of the examination of the cord by Dr. Lockhart Clarke.

The essential points in his report are—partial thickening of the membranes and occasional adhesions to the white columns; a diminution, to a most striking extent, of its antero-posterior diameter; disease of the posterior white columns, the nerve-fibres of which had been to a certain extent replaced by compound granule-corpuscles and wavy fibrous tissue. The case, in many respects, resembled one of locomotor ataxy.

In the description given by Erichsen of the symptoms of spinal meningitis and myelitis he draws chiefly from the works of Abercrombie and Ollivier, and compares his own cases with those recorded by these distinguished authors; the railway cases, however, are all of a much more chronic or, at least, subacute character. It must be remembered, also, that in all such chronic cases the special symptoms of each variety are much confused and mingled, so that it is almost impossible to distinguish those referable to the inflammation of the *membranes* from those caused by destruction or softening of the *substance* of the cord. In spinal meningitis of an acute character, especially accompanied by suppuration, cerebral symptoms are almost invariably present, and on post-mortem examination there is pretty certainly to be found effusion of lymph or pus on the membranes of the brain, especially about the base.

In a case lately observed by the writer, in which an exceedingly rapid attack of spinal meningitis resulted from a punctured wound which penetrated the membranes, but not the substance of the cord in the neck, and allowed the cerebro-spinal fluid to escape in large quantity, the symptoms were such as first to be mistaken for hysteria, and then for tetanus, till the diagnosis of the more serious lesion was confirmed by the extraordinary and rapid increase in temperature, repeated rigors, muscular spasms, delirium, ending in coma and rapid death. The symptoms of

inflammation, as a result of spinal wrench or concussion, are chiefly those of pain and consequent stiffness. The pain varies in amount, is generally worst opposite the seat of lesion, and is the cause of the peculiar stiffness of gait, anxiety on movement, and awkwardness in stooping, so often seen after railway injuries.

If the inflammation of the membranes is at all severe or extensive it is sure to affect the spinal nerves, either by causing undue pressure on them at their exit from the sheath, or, less likely, by extending down the neurilemma. These pains are connected with rigidity, contraction, or painful movement of the muscles of the limb.

In endeavouring to distinguish the symptoms of spinal myelitis from those of meningitis, which, as we have said, is only possible in early stages, and is not at all certain in any stage, we may, perhaps, expect cerebral symptoms to occur more rarely and pressure to elicit more suffering in the myelitis than in the meningitis. We may quote here from Erichsen the more characteristic symptoms of myelitis:

"The sensibility is first augmented, but after a time becomes lessened, and gives way to various uneasy sensations in the limbs, limbs, such as formications—a feeling as if the limbs were asleep (*engourdissement*). These sensations are first experienced in the fingers and toes, and thence extend upwards along the limbs. These sensations are most complained of in the morning, soon after leaving bed. They intermit at times, fluctuating in intensity, and in the early stages are lessened after exercise, when the patient feels better and stronger for a time, but these attempts are followed by an aggravation of the symptoms. Some degree of paralysis of movement, of loss of motor power, occurs in certain sets of muscles or in one limb. Thus the lower limbs may be singly or successively affected before the upper extremities, or *vice versâ*. Occasionally the loss of power assumes a hemiplegic form. All this will vary according to the seat and extent of the myelitis. There is usually constipation in consequence of loss of power in the lower bowel. It is very rare that the bladder is early affected, the patient having voluntary control over that organ until the most advanced stages of the disease, towards the close of life, when the softening of the cord is complete. Ollivier remarks that in chronic myelitis patients often complain of a sensation as of a cord tied tightly round the body. The gait of patients affected with chronic myelitis is peculiar. It is unsteady, rolling, like that of a partially intoxicated man. The foot is raised with difficulty, the toes are sometimes depressed, and at others they are raised, and the heel drags in walking. The body is kept erect and carried somewhat backward."—Pp. 186, 187.

So far, then, we can recognise, with some distinctness, the symptoms of the second stage in those cases of concussion of the

spine which result in a chronic inflammation, either of the cord or its membranes or both combined. A careful examination into the history and the objective phenomena, of temperature, pulse, contractility, and firmness of muscle, will generally enable the surgeon to come to a pretty accurate diagnosis, and thus to give a tolerably certain opinion and prognosis, which latter is generally an unfavorable one.

We must now attempt a much more difficult task, to follow our author in his description of the symptoms of spinal anæmia and its results, which are with great difficulty differentiated from those of so-called hysteria.

Occurring, as it does, chiefly in the young, from fifteen to thirty-five, and in a large proportion of cases in females, who had, perhaps, been always somewhat weak and excitable, this malady is apt to be mistaken for or confused with hysteria in one or other of its Protean aspects.

The chief symptoms of this malady, as given in the manuals and homologated by Mr. Erichsen, are—

Pain, induced or increased by pressure on or near the spine, and aggravated by the contact of a hot sponge. This is not permanent, is most marked when patient is expecting to be touched, is perhaps absent entirely when patient's attention is diverted.

Paralysis, in bad cases, of the whole lower limbs, which are flaccid, cold, and insensible. Erichsen says—"The condition is, in fact, one of complete exhaustion of the spinal system below a certain level, that level usually corresponding with a line drawn round the body from the tenth dorsal vertebra."

Special senses, such as *sight* and *hearing,* are often seriously impaired and temporarily weakened, or even destroyed. From the fact that this condition is never in itself a fatal malady, no post-mortem examinations are recorded.

Allied to this condition so closely as with great difficulty to be distinguished from it are found cases of mental rather than physical perturbation, which, for want of a better name, Mr. Erichsen calls hysteria, which he has never seen as results of any of the ordinary accidents of civil life, even in females, but of which he has seen many examples, in both sexes, after the more severe types of railway collisions. Whether such cases are caused by the terror and suddenness of the accident affecting the mind only, or by any special vibratory thrill acting physically on the nervous system itself, it is impossible to determine. Probably the former is the more rational explanation, as the symptoms are mental rather than physical, and they are not apt to be followed by inflammatory or degenerative changes in the substance of the cord.

We will not quote Mr. Erichsen's eloquent account of these

symptoms; they are, in a word, an exaggeration of all the subjective symptoms of spinal injury, with a "nervous mimicry" of numerous diseases, aggravated by examinations, especially by the surgeons to the railway company, not in the least improved by treatment, but rapidly tending to get better after a certain number of months, and especially after the worry and anxiety of litigation is fairly over. Can we wonder that, such being the case, invidious and uncharitable remarks are made by lawyers and the suffering railway companies? and yet all through the case the *bona fides*, both of the patient and her medical advisers, may have been unimpeachable.

Mr. Erichsen would base his attempts at the diagnosis of these perplexing cases from those depending on real structural brain-lesions on three chief points:—1. That the mental condition had come on almost immediately after the injury, and has been continuous, not progressive. 2. The pain has a peculiar character of cutaneous hyperæsthesia, which is unattended by alteration in texture, loss of movement, or muscular rigidity. 3. The wonderful good health, notwithstanding the long idleness and mental vacuity.

The prognosis in these cases is generally favorable.

Another most obvious and necessary source of fallacy in diagnosis is, that the same sort of injury which will shake or wrench the spine is likely at the same time to inflict other injuries of which the patient may be unconscious.

Thus, lately, in examining a recent case of bruise of the muscles of the back, inflicted in a railway collision, we discovered a broken rib, evidently the result of a blow from an angle of the carriage or of a neighbour's elbow, of the reception of which blow the patient had been quite ignorant.

Mr. Erichsen's great experience and careful notes have enabled him to enrich his lectures with details of many of these curious, almost exceptional, phenomena. For example, under the head of "Sacrodynia," he describes a diffused pain over sacrum, loins, and hips, with tenderness on pressure and limitation of movement.

In the great majority of cases there is dragging of the *left* limb. This Mr. Erichsen explains by an ingenious theory—that in a railway collision the right hand and arm are most usually thrust forward to save shock, and that, consequently, the right side is advanced, and thus the left side gets the chief shock.

Mr. Erichsen explains this pain, which lasts for months and years, not by mere nerve-injury, but rather from a bruising of the fascial and aponeurotic structures of the neighbourhood, as well as strain of the great ligaments. The diagnosis of this

condition from ordinary spinal shock is rendered difficult from the manner in which the symptoms are masked and blended by the coexistence of both injuries at the same time.

Were we in a fault-finding mood we might take exception to the manner in which Mr. Erichsen has arranged his lecture on the complications of concussion, for we find *syphilis* occupying a place between *sacrodynia* and *nerve-injuries*. Now, surely the sacrodynia and nerve-injuries are rather symptoms which may or may not be explained, either by the spinal concussion or some other feature in the mechanical shock caused by the accident, while syphilis can only be a cachectic condition absolutely unconnected with the accident, and complicating, in the gravest manner, the explanation, diagnosis, and prognosis, it may be, of the very symptoms referred to.

There are also recorded, in this chapter, cases which seem to the non-forensic eye rather hard on the railway companies, and even to the medical eye a little far-fetched, in the explanation of the sequence of symptoms.

Thus, a concussion of the spine and a *longitudinal fissure of the mucous membrane of the posterior part of the anus and rectum* were put down as the results of a fall. These sad sequelæ were credited to an unfortunate railway company because the fall was in consequence of the sufferer's "*putting her foot into a hole in a door-mat in the waiting-room of a railway station*" (p. 220). This is all very well, and those of us who are not railway shareholders may take such cases with the philosophy with which we are enabled to bear the misfortunes of our neighbours; but in a footnote appended to this case we find the startling and alarming dictum, on the high authority of Lord Coleridge, that "if a patient, in going to the house of a medical man to consult him professionally, tripped in a hole in the carpet, or fell over a loose stair-rod, and injured himself, his professional adviser, whose counsel he was about to seek and pay for, would be liable for the injury sustained"—a truly awful and solemnizing doctrine, which has made us inspect our door-mats with much anxiety.

Another case, which also seems a little hard on the company, is an excellent illustration of a peripheral nerve-injury causing serious and rapid effects on the cerebral organs. A gentleman had his finger pinched in a door of a carriage, which injury, during an illness of eighteen months, resulted in twitchings, cramps, fits, and eventually death from cerebral softening.

We are at present watching a case in which several severe blows with a ruler inflicted on the *palm* of a child, have been followed during a period of five months by wrist-drop, wasting of the muscles of extensor aspect both of arm and forearm,

and by paralysis of the ball of the thumb. No fits or cerebral symptoms have as yet appeared.

All surgeons who have had much to do with the medico-legal aspects of railway cases will turn with interest to the chapter on this most difficult and delicate subject. Mr. Erichsen gives much excellent advice.

First and most important, we are never to forget that our province is to treat the patient; describe, if need be, his injuries; give, if we are asked, an opinion as to their severity and probable continuance; but we are *not* to be valuators or appraisers of their money-worth. Many circumstances with which we have nothing to do must aid the jury or a valuator to do this. Expenses and losses in business or in money will constitute almost the only factors for such a decision; we cannot decide on such topics. North of the Tweed, with a wisdom and shrewdness peculiarly Scotch, a *tertium quid* intervenes—the *solatium*, or recompense for the sufferings in body and mind, past, present, or to come; perhaps a pawky Scotch doctor might be asked to give an opinion on *its* amount; but, on the whole, the less we interfere with the money-question the better.

Any account of injuries to the nervous system as a result of spinal concussion would be incomplete without a reference to the organs of special sense as affected in the general mischief. Of these the eye is the most important, and any appreciable lesion of it is followed by loss of vision to a greater or less amount.

Mr. Erichsen considers the various forms in which injury of the eye or impairment of vision may appear under four well-marked and distinct heads.

1. Concussion of the eyeball and direct shock to the nerve; such injuries may be inflicted during a railway accident and the patient may hardly be able to tell how. Mr. Erichsen believes, and supports his belief by one well-marked case, that a smart, direct blow on the eyeball may at once paralyse the retina without giving rise to any organic mischief, laceration of tissue, or effusion of blood in the eyeball itself. Such cases of simple shock are most probably rare, there being more usually some effusion or laceration of the iris, choroid, or ciliary processes; in many cases some effusion of blood into the vitreous humour. Again, under this head are included cases of cataract developing after a blow on the forehead or eyebrow, as a result of nerve-injury.

2. *Nerve-injury* may also explain cases of extreme mydriasis, and consequent impairment of power of accommodation, seen after blows or falls implicating the forehead and eyebrows. Some of these cases are remarkable and inexplicable, especially as to why some cases suffer from eye-symptoms and others do not. A slight blow, which has hardly made a mark on the

forehead, in one case setting up a slow and gradual process ending in total blindness; another, much more severe one, having no effects at all. A case was recently observed by the writer in which a fall from a height of between sixty and seventy feet inflicted cuts of great severity over branches of fifth, both above and below orbit, and caused so great a shock as to fill the subconjunctival tissue with ecchymosed blood, and yet not a single symptom of impairment of vision followed; while many cases are on record in which amaurosis and other eye symptoms have followed comparatively slight injuries. Among other papers on this subject we may refer to a full series of such cases reported by Mr. Benjamin Bell, Surgeon to the Edinburgh Eye Infirmary.[1]

The ophthalmoscope has cleared up the diagnosis and greatly facilitated the prognosis of many of these cases, explaining the symptoms by subretinal effusions, lacerations of edge of the iris, &c.

3. By far the most important, insidious, frequent, and misleading symptoms are those directly caused by the spinal injury itself quite apart from any local injury to the eye or nerves—gradual weakness of vision, with a want of definition, *not improved* by glasses, passing into double vision, with photophobia, and even to the presence of permanent muscæ, flashes of light, sparks, and other subjective phenomena. Mr. John Tweedy has given an explanation of the symptoms under four heads. (*a*) Asthenopia, with want of accommodation-power for near objects for more than a few minutes at a time, the result either of impaired nerve-supply or of weakness of the ciliary muscle or internal recti. (*b*) Amblyopia, a paresis of the retina or optic nerve, rendering it incapable of receiving or transmitting impressions to the brain. (*c*) Anomalies of accommodation or refraction, hypermetropia, myopia, or astigmatism, if of sudden or rapid occurrence, are all grave symptoms. (*d*) Irritability from hyperæmia of retina, and inflammation of it or optic nerve.

In consequence of these varying conditions, the ophthalmoscopic appearances in railway cases will vary very much. The careful researches of Wharton Jones and Clifford Allbutt leave little to be learned in this interesting field. The observations of the latter seem to prove that in very acute and rapidly fatal cases of spinal injury the eye-symptoms are rarely observable; while in chronic ones, especially those involving the upper portions of the cord, they are rarely absent. Dr. Allbutt explains the impairment of vision in these cases by a hyperæmia at the back of the eye, probably dependent upon a greater or less extension of the meningeal irritation up to the base of the brain.

[1] 'Edinburgh Medical Journal,' July, 1856.

No account of the relations of eye-symptoms to spinal disease would be complete without reference to the interesting paper[1] of Dr. Argyll Robertson on spinal myosis, in which he gives an account of four cases of extreme contraction of the pupil related to and explained by diseased conditions of the spinal cord.

4. Impairment of vision from affection of the sympathetic is another explanation, closely related to No. 3, of symptoms so closely resembling those we have just detailed as to be practically indistinguishable from them.

In Lecture XI Mr. Erichsen has collected from his own experience a most interesting series of cases of certain forms of paralysis from cord-injuries which may complicate concussion of the spine. Compression of nerve-trunks near their origins, or as they escape from the foramina, by effusions of blood or inflammatory products, is one of the most potent factors of paraplegia after spinal wrench. In the upper extremity strains and bruises of supra-scapular, circumflex, and musculo-spiral nerves, may produce most well-marked and easily distinguishable symptoms. Of these the musculo-spiral is not only the most common, but presents the most-marked characters. Mr. Erichsen's account of some of these is very distinct and interesting. Coldness, wrist-drop, pronation, want of extension-power, are the almost invariable results. A case was lately under the care of the writer in which, after a very severe railway accident, involving injury to head and chest, the chief sequela was this condition of the right arm. The arm was cold, thinner and weaker than the other; the wrist-drop, pronation, and want of extension-power were well marked. Traces of severe bruising and tearing of muscles in the vicinity of musculo-spiral groove were easily seen. The surgeon for the railway suggested feigning and laziness, and said that the symptoms were due to long retention of the limb in a sling. The writer was able to point out that the patient, an uneducated man, made an anatomical differentiation of the injured nerve so precise, and yet so impossible to feign, as to render his *bona fides* in the matter quite unassailable. This movement was one of extension of the two distal phalanges of the fingers, quite apart from any movement of the ordinary extensor tendons, and was due to the combined action of the interossei and lumbricales muscles that do not receive their innervation from the musculo-spiral, but chiefly from the deep branch of the ulnar; all the interossei and the two innermost lumbricales receiving their nerves from this source, whilst the two outermost of the lumbricales receive theirs from the median nerve.

Paralysis of the posterior interosseous nerve must be a much

[1] 'Edinburgh Medical Journal,' Dec., 1869.

rarer injury, as it is so much better protected than the musculo-spiral; but of this injury Mr. Erichsen gives two well-marked examples.

The position of the different medical men in relation to the patient and to each other is sometimes very difficult and embarrassing. The railway surgeon has a most delicate part, one which requires tact, gentleness, kindness, and, above all, an unsullied honour so utterly above suspicion as to impress the patient with the certainty of his candour and fairness. Mr. Erichsen shows how difficult his position is, regarded, if not as an enemy, at least as a critic with a leaning to hostility—seen only along with another medical man—his questions evaded or fenced with—he will with difficulty elicit facts, and may carry away with him altogether deluding symptoms.

Still worse is it when the unfortunate patient is subjected to the torture of a sort of general consultation by a number of medical men. We once formed one of a conclave consisting of nine—the patient's own doctor, the railway company's local medical officer, two distinguished consulting surgeons, one distinguished consulting physician and one ophthalmic surgeon for the company, two consulting surgeons and an ophthalmic surgeon on behalf of the patient. A tenth would have been present had he had not missed a train, and two solicitors and their clerks waited outside the door. The house was a small one of three rooms, and the patient's bedroom barely held the faculty, even had they not had galvanic batteries, ophthalmoscopes, and note-books. That one afternoon's work must have cost about £200 for fees and travelling expenses alone, as eight of the ten had to travel more than twenty miles.

The whole question of the relation of medical evidence in actions for damages seems to us to need revision and careful consideration by the heads of our profession. At present the practical working is exceedingly bad. Doctors are pitted against each other like cocks in a cockpit. If ——— and ——— are on the one side, then ——— and ——— must be hired to appear on the opposite side to impress the jury. If the railway company, rich and a corporation, add a third or a fourth, the poor patient must also cap them by adding on his side. All these doctors will probably give evidence as to matters of fact almost identical, yet which can be placed before a jury by distinguished lawyers as having most varied effect. The appearance of wisdom and age which a doctor possesses, his being a "good" or a "bad" witness, as lawyers call it, become a part of his stock-in-trade of a market value to him.

Surely this is a degradation, and could it not be avoided? Could not the judge in such cases, by an order in court, depute

it to two men of known honour and skill to visit the patient, along with his own doctor, and draw up an independent report for the information of the court; the reporters to put in simple language their view of the history, nature, and prognosis of the injuries, and their results, the report to be the common property of both sides? It should be printed and put into the hands of the jury, and it ought not to include a single word as to the money value of the injuries.

That this feeling is gaining ground in the profession may be seen from the following extract from a letter written to us by a very distinguished surgeon in answer to a request that he would make a second witness for the plaintiff in a very simple railway case, in which the railway company had commissioned three medical men in addition to the local one:

"Besides the intolerable loss of time at the court entailed by these railway cases, I always feel a sense of degradation at being employed as a medical advocate according to the system which seems to prevail nowadays. According to my notion a man like —— (the senior railway surgeon) ought to give his evidence identically in the same way which ever side he is employed by; and the idea of getting additional witnesses on the one side because a certain number have been got on the other seems to me radically vicious, and I made up my mind some time ago to have nothing to do with these cases for the future where I could possibly help it. So far as the ends of justice are concerned I am perfectly sure that the patient's interest will be safe in the hands of —— and yourself.—Yours, &c."

Now, though all this is profoundly true, yet until some mutual understanding is arrived at, the bad custom of piling on witnesses will continue, for juries will be impressed favorably for the side which puts man after man into the box.

Perfect openness and candour regarding the opinions formed and the reports to be given would be a great help in preventing unseemly differences; a consultation of the medical witnesses of both sides would neither be impossible nor unsuitable. In one case, in which the bulk of the evidence was medical, an intimate friend and the writer were on different sides as to some surgical points. We discussed the case, found we agreed as to matters of fact and did not greatly differ as to matters of opinion. In that case justice was attained, and the honour of surgery did not suffer.

We are glad to see that Mr. Erichsen gives no uncertain sound upon one point of great importance. He lays it down that the report of the railway surgeon to the company is not confidential, but that the plaintiff may have access to it. The sooner he has access to it the better for the ends of justice and the honour of the profession. We have on more than one occasion been amused by the small airs of consequence and

authority put on by the railway official gentlemen after the consultation is over. On the question "Well, how do you mean to report?" to receive the answer "You have nothing to do with that, you will hear at the trial," is both irritating and impolitic —irritating, as implying a want of confidence in either your sense, or your sense of honour; impolitic, as the immediate effect of it is to rouse your ire against the poor railway company and also at once to impel you to get one or two more "eminent surgeons" to confirm the opinion you have formed. Indeed, in some cases, such as in fracture of ribs, where the sensible signs will in a few days be obliterated by the healing process, the necessity of a second opinion is rendered almost inevitable, while a word from the railway surgeon implying that he quite agreed with your diagnosis would at once save time, trouble, and expense.

With all good intentions on both sides, however, and even on the supposition that the medical witnesses, perfectly unbiassed, appointed by the court, and of the highest professional standing, do their best, still there will be great difficulties in framing the answers to the questions "Has the patient been injured, and how much has he been injured?" On this subject Mr. Erichsen gives clear and distinct advice. The *signs* or objective phenomena are often few and ill marked; the *symptoms* or subjective phenomena may be misleading. Exaggeration, conscious or unconscious, simulation, hysterical or knavish, are most perplexing; hence every precaution of cross-examination and watching must be used, and far greater stress must be laid on information obtained from objective phenomena, as indicated by the ophthalmoscope, the galvanic battery, the æsthesiometer, the thermometer, and the measuring-tape, than on what is gained by oral evidence of the patient and his friends.

As to the prognosis and treatment of spinal concussion, much will depend as to the amount and duration of inflammation of the cord or its membranes. Such inflammations are insidious in their progress, chronic in their duration, little amenable to treatment, and ought always to be regarded as grave lesions.

Mr. Erichsen's experience gives weight to a very positive statement which he makes.

"I have never known a patient to recover completely and entirely, so as to be in the same state of health that he enjoyed before the accident, in whom the symptoms dependent on chronic inflammation of the cord and its membranes and on their consecutive structural lesions had existed for twelve months. Such a patient may undoubtedly considerably improve, but he will never completely lose the traces of the injury. These will in some respects be permanent and show themselves in general or local weakness, loss of muscular

power, change in character, various head-symptoms, each trivial in itself, but collectively important, a cachectic and prematurely aged look, and digestive derangement. And though such a patient may live for fifteen or twenty years in a broken state of health, the probability is that he will die in three or four." (p. 311).

The treatment is to be conducted on the ordinary principles of surgery—rest, and support when movement is restored, hypnotics when needed; iodide of potassium and bichloride of mercury, each in its own set of cases; counter-irritation in certain cases, carefully regulated diet in all. It is to be measured by months, even years, not by days; it will need patience both on the part of the sufferer and the surgeon, a patience which, alas! is not always to end in recovery.

II.—Diseases of Women.

1. *Uterine Injections; 2. Chronic Metritis; 3. Treatment of Fibroids.*

Many practitioners, who, with all due deference to them, know little about the subject, assert that the importance and frequency of uterine disorders has been exaggerated; that in years gone by we heard little about flexion and ulceration of the womb. The obvious reply to these assertions is, that the more a complaint is studied the more frequent it appears. Besides this, women, finding that their diseases are more studied and better understood, doubtless have less diffidence in declaring their sufferings and seeking relief where formerly it was useless to complain; their statements being regarded as hysterical, their sufferings as imaginary or greatly exaggerated, and insult added to injury by a happy combination of the most nauseous repulsive mixtures, with blistering and other like agents.

Considering the rapid strides that have been made in this special study during the last thirty years, it could scarcely be expected that the opinion of the various authors who have written upon the subject would be in every way unanimous;

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[1] 1. *A Practical Treatise on the Diseases of Women.* By T. Gaillard Thomas, M.D. Fourth edition. Philadelphia. 1874.

2. *A Clinical History of the Medical and Surgical Diseases of Women.* By Robert Barnes, M.D. London. 1873.

3. *A Practical Treatise on Inflammation of the Uterus.* By James Henry Bennet, M.D. Fourth edition. London. 1861.

4. *Diseases of the Female Sexual Organs.* Vol. x of the *Cyclopædia of the Practice of Medicine.* By Professor Carl Schroeder. Edited by Dr. H. Von Ziemssen. London. 1875.

5. *Clinical Lectures on Diseases peculiar to Women.* By Lombe Atthill, M.D. Third edition. Dublin. 1875.

still it is a question whether more unanimity will not prevail in the next generation. The opportunities of becoming conversant with the views and practical experience of the workers of the present generation will doubtless materially facilitate a more comprehensive study of the subject in the future, and enable the student to arrive at more impartial and less special or limited conclusions than is frequently the case at present.

Dr. Thomas, in a recent lecture delivered before the College of Physicians and Surgeons, New York, on "General Considerations upon Uterine Pathology," remarked that—

"Nothing brings more discredit upon the gynæcological department of the profession than the uncertainty and the confusion of the pathology professed, many of its votaries, instead of taking broad and strong views, becoming the partisans of some special dogma or theory which is attacked by others, who hold some view equally narrow, incomprehensible, and exclusive. While some regard inflammation of the parenchyma as the great moving cause of uterine disorders, others attribute these to displacements of the organ, to irritation or hyperæsthesia of its nerves, to catarrhal inflammation of the mucous membrane, or to the inefficient restoration of the uterus after the structural changes consequent on utero-gestation."

We quite agree with him that the time has arrived when exclusive views of this kind should cease to prevail, it being admitted that each of these conditions may exert its influence in certain cases.

It may be of interest to contrast the statements of the authors whose works we are considering, upon a few points of practical interest, such, for instance, as the employment of intra-uterine injections, which has given rise to much controversy, and is still very variously estimated among professional men; the views regarding chronic metritis or areolar hyperplasia, and the treatment of fibroids.

Uterine Injections.—Schroeder tells us that the surest means that we have of producing a uniform effect upon the uterine mucous membrane is the intra-uterine injection of fluids. Such injections were used long ago by Lisfranc and Vidal de Cassis, and their employment is now becoming more and more general. The method, though efficient, is not without danger, and must therefore be employed with great caution. He mentions that "a series of published, and probably a much larger one of unpublished cases, show that the operation may produce dangerous symptoms, and even cause death.

Dr. Bennet states that—

"When disease really exists in the uterine cavity, the injections would, no doubt, do much good; and, were they safe, would be pre-

ferable to the solid nitrate of silver, applied with the porte-caustic; but there is reason to believe that uterine injections are not safe, and I now do not resort to them. Several deaths occurred in Paris, during my residence there, from metro-peritonitis, brought on by their use.

"This accident would probably have occurred much oftener than it has done in the hands of French practitioners, were it not that the natural coarctation of the os internum must have generally prevented the fluid injected from penetrating into the uterine cavity, where the disease is erroneously thought to exist."

He states also that he has so constantly found serious uterine tormina, partial syncope, and general hypogastric pain developed from the employment of intra-uterine injections, that he has now quite discarded the practice as unsafe and unnecessary.

Thomas, after a careful consideration of the whole subject, says:

"The deduction which the evidence elicited forces upon us is self-evident, namely, that at the same time that this method of treatment, systematically and carefully resorted to, is a valuable resource in endometritis, it is attended by many and great dangers; and though these latter may be lessened by precautionary measures, it will always remain a hazardous method for the general practitioner, though it may be comparatively safe in the hands of specialists skilled in uterine manipulations."

Dr. Barnes says—"The general conclusion at which I have arrived is, to restrict the use of intra-uterine injections within the narrowest limits. I rarely employ them now except in cases of urgent danger from metrorrhagia. We may obtain almost all the advantages that injections are capable of giving by other means—by swabbing, or solid, or in the form of ointment." This statement coming from one who does not hesitate to inject a strong solution of perchloride of iron in suitable cases of post-partum hæmorrhage may be regarded as conclusive. We would endorse the statement—most thoroughly, and commend it to the notice of those who speak of intra-uterine injections as if they were almost synonymous with vaginal ones and attended by as little risk.

The late Sir James Y. Simpson, in speaking of injections into the non-puerperal uterus in cases of dysmenorrhœa, says, "Never think or dream of throwing liquids into the interior of the uterus by means of any injecting apparatus, for severe and fatal inflammations are very likely to ensue. The consequences of injecting fluid into the cavity of the womb are so often dangerous and deadly that the practice has now been given up, I elieve, by all accoucheurs."

Dr. Atthill, in referring to the question, says—"I have never

tried it, as it is a practice not free from danger; and not alone that, but also much less certain and satisfactory in its results than other modes of treatment."

Chronic Metritis or Infarction of the Uterus.—Schroeder tells us that the opinions of surgeons are as yet exceedingly divided on the subject. In Germany there is so little uniformity of opinion that some gynæcologists pronounce chronic metritis to be the most common of all the diseases peculiar to women, and others almost deny the existence of such an affection. While Scanzoni includes under this term all the disturbances of nutrition which follow protracted nervous hyperæmia, Seyfert believes infarction of the uterus to consist exclusively in defective puerperal involution; and Klob, in his 'Pathological Anatomy of the Female Sexual Organs,' does not class the changes met with in infarction with the inflammatory processes, but with the neoplastic growths, and describes them under the name of "diffuse connective-tissue proliferation."

Schroeder himself states it to be his conviction, that we cannot dispense with the clinical picture of chronic metritis, for we should otherwise be obliged to separate closely connected pathological conditions having the same symptoms and requiring the same treatment. Neither does he consider the term "chronic metritis" to be so very improper, because it is probably nothing more than a war of words, whether that condition be called a hyperplasia of the connective tissue of a hyperæmic uterus, or the product of an exceedingly chronic inflammation. Indeed, he would be loth to dispense with the name "inflammation" for the condition in question, partly because the treatment needs to be decidedly antiphlogistic, and partly because in the early stages we always have the clinical symptoms of inflammation—hyperæmia, tumefaction, and pain. It should be noted, in addition, that all the cases in which the rather rare termination in induration—a change which occurs only at a late stage—has not taken place undergo from time to time exacerbations, which present the features of a subacute, occasionally of even a quite acute, inflammation. We therefore include under the term "chronic metritis" those cases also—placing them at the head of the list, because they are the most numerous—which originally arise independently of inflammation, such as defective puerperal involution, because inflammatory symptoms—hyperæmia, swelling, and pain—occur during their course, and also because the treatment of these etiologically separate cases is decidedly antiphlogistic. Simpson, indeed, expressly states that in a case of defective puerperal involution the treatment should be antiphlogistic, even though all positive signs of inflammation be wanting.

The collection of symptoms known as chronic metritis is thus made to comprise a large number of cases of etiologically different nature, but presenting clinically the same appearances, and requiring the same treatment.

Thomas in an able and interesting chapter on this subject, with the heading "Areolar Hyperplasia of the Uterus—the so-called Chronic Parenchymatous Metritis," remarks, that "so much evil has arisen for pathology and treatment from the use of the term "chronic metritis," and so clear a demonstration has been made that the condition so called is not one of true inflammation, that some other appellation is not only desirable but has become absolutely essential.

The appellations infarctus, engorgement, and hyperæmia, only convey a partial idea of the truth; they only announce one element of the condition—congestion; while that of irritable uterus ignores all structural change in announcing another element—nervous hyperæsthesia.

At the same time that the phrase "diffuse proliferation of connective tissue due to hyperæmia," which is employed by Klob, clearly defines the pathological condition, it is too long and burdensome to answer the purpose of a name to be conventionally employed.

Enlargement of an organ due to formation of new cells similar to those of the tissue in which they are developed has been styled by Virchow hyperplasia, in contradistinction to hypertrophy (hypertrophy signifying excessive growth of the elements of a tissue already existing; hyperplasia, the development of new tissue), which consists in increase of size from distension of cells already existing. As the condition of the uterus now under consideration is one arising from over-excitation of the vaso-motor and excitor nutritive nerves, a "formative irritation," as Klob styles it, and resulting in a numerical hypertrophy, it appears that the term areolar hyperplasia would more correctly designate it than any other which has hitherto been proposed.

Dr. Thomas sums up his conclusions in the following terms:—

1st. The condition ordinarily styled chronic metritis consists in an enlargement of the uterus due to hypergenesis of its tissues, especially of its connective tissue, which induces nervous irritability, and is accompanied by congestion.

2nd. Decidedly the most frequent source of this state is interference with involution of the puerperal uterus. A very large proportion of the cases of so-called chronic parenchymatous metritis are really later stages of subinvolution.

3rd. Areolar hyperplasia is often induced in a uterus which has once undergone the development of pregnancy, by displace-

ment, endometritis, and other conditions inducing persistent hyperæmia.

4th. The same influences may possibly produce it in the nulliparous uterus (most frequently they do so in the neck), but such a result is exceedingly infrequent.

5th. However produced, the condition is one of vice of nutrition, engendering hyperplasia of connective tissue as its most striking feature, and, although attended by many of the signs and symptoms of inflammation, it in no way partakes of the character of that process.

Bennet limits the term chronic metritis to inflammation affecting the body of the uterus. He considers it is more frequently partial than general; that is, that it generally occupies a limited extent only of the uterine tissue. In its partial form it is observed, in nine cases out of ten, in the posterior wall of the uterus, in its inferior region, immediately adjoining the base of the cervix.

He regards chronic metritis as generally the result of extension to the uterus of chronic inflammation of the cervix, the product of years of uterine disease, perpetuated by general constitutional conditions, though it sometimes occurs as the termination of acute metritis, whether puerperal or non-puerperal.

He states that

"Some of the leading *symptoms* of chronic metritis are erroneously attributed by many uterine pathologists to the displacement of the uterus which it *occasions*.

"Chronic inflammation of the uterus has a decided tendency to perpetuate indefinitely its existence owing to the periodical exacerbations to which the peculiar functions of the uterine system give rise, and which prevent chronic metritis from terminating spontaneously by resolution.

"Sometimes the enlargement of the uterine tissue gradually melts and disappears; in other instances the disease terminates by induration.

"When examined after death the inflamed region of the uterus is found enlarged, and more or less red and filled with blood. If the chronic inflammation is terminated by induration, the texture of the diseased part is more than normally dense, and of a greyish or greyish-red hue."

Atthill agrees with Thomas that "diffuse interstitial hypertrophy" conveys a more correct idea of the pathology of the affection, consisting as it does in an increased flow of blood to the part and subsequent static congestion, with increased growth both of the connective tissue and of the muscular fibres of the uterus; that of the former being greatly in excess. The blood-vessels become engorged, while the muscular structure is

softened, swollen, and, in his opinion, also frequently infiltrated with serum to such an extent as to produce well-marked œdema of the organ, especially of the cervix.

Treatment of Fibroids.—Dr. Thomas, speaking of absorption of fibroids by means of internal remedies, doubts "whether their absorption can be excited by any of those medicines styled absorbents." No such effect can be looked for with any confidence; indeed with our present experience such a result must be regarded as decidedly exceptional.

The hypodermic injection of ergotine as suggested by Hildebrandt he has adopted in a number of cases, and while he cannot claim such results as its proposer obtained, he is prepared to endorse it as one very promising of excellent results.

In reference to surgical procedures, he considers the two elements which govern success in the removal of these growths are, 1st, the degree of projection of the tumour into the uterine cavity, and, 2nd, the degree of dilatation of the cervical canal.

"Before all the operations practised for the removal of fibroids from the cavity of the uterus, the cervix must be fully dilated. 1st. The cervix may be gradually dilated, the attachments of the tumour broken little by little, and extension slowly effected by ergot. 2nd. The cervix may be rapidly dilated in part before the operation, and in part at the moment of practising it. 3rd. The cervix may be gradually and fully dilated before surgical interference is established."

He then describes the various methods adopted for removal—excision, ecrasement, and galvanic cautery, avulsion, enucleation and gastrotomy, giving clear indications as to the advantages and method of procedure in each operation.

Gastrotomy, he thinks, should be performed only when life is in jeopardy.

It would take up too much of our space at present to give even his conclusions. The whole subject is most exhaustively treated.

Dr. Barnes before entering upon the question of operative treatment alludes to the natural terminations, viz. 1. Absorption or atrophy. 2. Calcareous degeneration. 3. Gangrene or other form of decomposition. 4. Spontaneous expulsion or enucleation.

These he justly considers furnish most useful indications as regards treatment. After asserting that "the reality of fibroid tumours having been absorbed is too well established to admit of doubt," he further adds:

"My own experience lends little or no support to the proposition that internal remedies exert any influence in promoting absorption of the hard fibroid tumour. I suspect that the favorable opinion

as to their efficacy, which some authors have expressed, springs from the observation of the larger, looser textured tumours, and that the diminution was due to the absorption of fluid infiltration, the solid constituent remaining untouched."

With regard to enucleation, he considers "the larger tumours whose texture is continuous with the uterine wall are not proper subjects for these proceedings." He further remarks, "The difficulty of diagnosis between these and the encapsuled tumours is great," but does not, as we should expect, give us the ground upon which a diagnosis should rest. This is the more to be regretted, as there is no doubt it is from not bearing in mind the fact above mentioned "that failure and disaster have so often followed surgical proceedings." He mentions, "It is commonly necessary to dilate the cervix freely by incision and tents," preparatory to attempting enucleation, but fails to give us any opinion upon the relative merits of dilatation or incision of the cervix—the advantages and dangers of the two. He considers justification for attempting enucleation, avulsion, or other mode of removing large fibroid tumours will rest upon—

1. Uncontrollable hæmorrhages endangering life;
2. Signs of sloughing or decomposition of the tumour, with present or threatening peritonitis or pyæmia;
3. Dangerous pressure upon the bladder and rectum.

As regards gastrotomy, he thinks "the time has not yet come for forming a confident opinion." At present there is little ground for enthusiastic advocacy of the practice. The case may best be summed up by stating that the question is *adhuc sub judice.*

Schroeder asserts that "we have no internal treatment from which, in any individual case, we are justified in expecting good results with the least degree of certainty." At the same time the experience of Hildebrandt should, he holds, incite us to further attempts in this direction.

The account of surgical treatment, occupying only four pages of Schroeder's treatise, is vague and far from what we should have fairly expected in a work of such pretensions.

He tells us enucleation "in general is only applicable to submucous fibroids; at least it is only very exceptionally to those that are interstitial."

"If the cervix be obliterated, the enlargement may be most readily accomplished by an incision; if it is preserved, dilatation by the sponge tent may be employed."

The operation of gastrotomy "would not seem to be any more dangerous than that of ovariotomy."

Atthill, speaking of the administration of medicines, with the view of causing the absorption of fibrous tumours of the womb,

says he has tried fully and freely most, if not all, of them, and believes them to be of no use. He has no experience to offer as to the surgical treatment of these fibroids, but he is of opinion that it has sometimes been carried too far.

By way of general notice of the works before us, we may remark that Dr. Thomas's work is a model of perfection, classical, and yet clinical in all its details—concise, and yet ample in its description of disease; systematical, complete, and circumstantial. It is but five years since its first appearance, and already a fourth edition, a goodly octavo of 800 pages, with nearly 200 illustrations, has been called for. No wonder that the Germans, French, and Italians have deemed it worthy of translating into their own languages.

The author regards the subject from no limited point of view, neither inclining too much to the theory that inflammation is the prime factor in the production of all uterine disease, nor to the mechanical theory so strongly advocated by others.

The great charm of the work is that, under the one heading we are seeking, whether it be dysmenorrhœa, sterility, or metritis, we find everything we need, a veritable *multum in parvo;* not merely his own opinion, but those of any and every recognised author who has written on the subject, which is treated systematically under the various divisions of—Definition, Varieties, Synonyms, Frequency, Anatomy, Pathology, Prognosis, Predisposing and Exciting Causes, Symptoms, Physical Signs, Course, Duration and Termination, Complications, History, Diagnosis, Differentiation, and Treatment; these headings being, of course, varied occasionally to suit the requirements of the subject.

Dr. Barnes's somewhat bulky volume of over 900 pages professes "to give such a description of the medical and surgical diseases of women as will assist the medical practitioner in their diagnosis and treatment."

"To some physicians the bulk of the volume may seem excessive. I would suggest the reflection that this apparent excess may represent the extent of their neglect. . . . In a subject of comparatively recent inquiry, necessarily to some extent unsettled and open to controversy, a fuller statement of fundamental facts and more argumentative discussion are called for than are necessary in the exposition of the more generally cultivated departments of medicine."

We extract these remarks from the preface, that the reader may form an idea of the scope of the work.

To those who intend making the diseases of women a study we would by all means counsel them to procure the volume. There is a fund of information in it, much original research,

and many valuable contributions from the author's own practice and experience.

Dr. Bennet's work professes to be 'A Practical Treatise on Inflammation of the Uterus, its Cervix, and Appendages, and on its connection with other Uterine Diseases.' Thomas, in his 'Historical Sketch of Gynæcology,' has truly said of it that "no work of modern times, written upon any subject connected with our profession, has exerted a more decided and profound influence."

However others may differ from him, no candid mind can deny the obligation under which Dr. Bennet has placed his brethren by arousing their attention and directing their investigations into proper channels. It is now nearly thirty years since Dr. Bennet startled the professional world by his then novel assertions: "endeavouring to demonstrate the important fact that inflammation was the keystone to by far the greater part of the morbid condition which constituted uterine pathology, and unless the phenomena which it occasioned were recognised and taken into consideration, all was doubt, obscurity, and deception."

"These views, appearing as they did at a time when the field of uterine pathology was almost entirely uncultivated, and characterised as they were by a great deal of persuasive force, produced in this country a marked impression," many earnest investigators accepting them in their entirety, others again repudiating them with all the bitterness and opposition that only ignorance and prejudice could justify. It may be well here to state broadly what those views are.

"1st. Inflammation is the chief factor in uterine affections, and from it follows, as results, displacements, ulcerations, and affections of the appendages. 2nd. That menstrual troubles and leucorrhœa are merely symptoms of this morbid state. 3rd. That in the great majority of cases, inflammatory action will be found to confine itself to the cervical canal and not to affect the cavity of the body. 4th. The propriety of attacking the disease in its habitat by strong caustics."

It would be well for those who advocate so strongly the mechanical system of uterine pathology, who assert that patients suffering from uterine symptoms are almost universally found to be affected with flexion or alterations in the shape of the uterus of easily recognised character, if they would again carefully peruse the work of Dr. Bennet and test the doctrines therein stated by the experience of every-day practice. We feel confident their success would be greater than the mere mechanical system affords. The tendency at present is towards

the opposite extreme of the views so ably advocated by Dr. Bennet.

Schroeder's work is of more interest pathologically than clinically. We are much indebted to the Germans for their painstaking pathological researches, which have thrown much fresh light upon hitherto vexed questions; and were the clinical remarks as clear and ample as the pathological, we could speak far more highly of many of their modern treatises regarded from a practical point of view. Schroeder's volume forms no exception to these. There is very much in it of valuable and useful information, but we cannot endorse the announcement that it fully comes up to the present standpoint of clinical medicine; we notice many obvious defects in this particular matter, and the treatment generally is sketchy and vague.

Dr. Lombe Atthill's little work does not profess to be an exhaustive treatise, but merely clinical lectures on the diseases of women. In his preface he states that—

"In my capacity of examiner, first in the Queen's University, and subsequently in the College of Physicians, I was much struck by the utter ignorance evinced by the great majority of candidates on the subject of 'Diseases of Women;' nor was this ignorance confined to the evidently idle men. Thus even those whose answering on all other subjects proved that they had made good use of their time, were frequently unable to state correctly a single cause on which such a common and important symptom as menorrhagia might depend; and consequently showed themselves incapable of treating cases in which it might occur. These gentlemen, when remonstrated with, invariably alleged as an excuse that the numerous subjects they were required to study precluded their reading the admirable, but somewhat voluminous, works existing on uterine and ovarian affections, and which were the only ones attainable."

With the avowed object of supplying this want, he considers the subject solely in its clinical aspect, dealing with it for the most part from the stand-point of symptomatology, which, after all, is the one from which the student mainly regards it. It is all very well for the experienced gynæcologist, who has made diseases of women his special study, to impress the importance of regarding and treating conditions such as leucorrhœa, menorrhagia, sterility, &c., as symptoms of uterine or vaginal disease and not as primary affections, but it is only by experience we can determine what are symptoms of, and what are diseases *per se*, and this the student has yet to learn. For this reason we are not surprised to find that Dr. Atthill's work has attained to a third edition since its first appearance in 1871 Although it professes to be by no means complete or exhaustive, and in many instances is merely colloquial, it is, we are com-

pelled to say, without any wish or intention of saying anything severe, that it is too superficial; nevertheless we may regard it as a concise summary of the practice carried out by the author in the Adelaide Hospital.

III.—Kuss's Physiology.[1]

We had long been familiar with M. Küss's excellent little manual of physiology in the original, and are now very glad to welcome Dr. Amory's translation. The book is of handy size, well got up, and pleasantly written. It appears to be a transcript by M. Duval, who was formerly the Demonstrator of Anatomy in the Medical School of the University of Strasbourg, of a series of lectures delivered by M. Küss at that university. But M. Duval has done more than simply edit these lectures; he has enriched them, and greatly added to the value of the volume by supplying numerous notes which give the best and most reliable results of French investigation. The reader, therefore, not only obtains a good German physiological work, but one well annotated by a Frenchman, and, we may add also, to some, though only to a small, extent by an American. The additions made to the volume by M. Duval, if, as we presume, the notes are written by him, are considerable. Thus, in the account of the histology of the venous system, M. Ranvier's researches are given in full detail; in the section on special sensation, the views of Bernhardt on the muscular sense, of Rouget on the fibres of the iris, and of Vulpian, Prévost, Lussana, and Inzani on the nerves of taste, are adequately detailed.

The order in which the several subjects usually embraced in a work on physiology are taken up is somewhat unusual, and does not, we think, present any special advantages. M. Küss commences with an account of the different kinds of cells, passes at once to the consideration of the nervous and muscular systems, and of the blood and the circulation. He then returns to the cellular elements of the body, as represented by the epithelia, and then devotes separate chapters to the digestive system, respiration, animal heat, and phonation; once more returns to the cells in the description of the skin and its functions, and, after devoting a short chapter to the special senses, ends with an account of embryology.

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[1] *A Course of Lectures on Physiology, as delivered by Prof. Küss at the Medical School of Strasbourg.* Edited by Mathias Duval, M.D., and translated by Robert Amory, M.D. Illustrated by 150 woodcuts. Boston, 1875.

The great merit of the book is that it is original. The facts are well marshalled, and so given as almost always to throw a new light upon the subject. The accounts of the nervous and of the muscular systems are very good in this respect. Both are short, the two together only occupying about eighty pages, and yet most of the more important practical facts are mentioned. Those which are of little value to the student, as, for example, the whole subject of electrotonus, are passed over in a line, with just enough notice to call attention to them, and sometimes with a reference to the places where a detailed exposition of them may be found.

In the case of the nervous system the subject is dealt with as follows:—1. Histology. 2. Life of the nervous system. 3. Action of the nervous system. 4. Excitants of the nervous system. 5. Excitability of the nerve-elements. 6. General physiology of the nerve-centres. 7. Special physiology of the nervous system, including an account of the functions of the several nerves. 8. Anatomy and physiology of the spinal axis, including the medulla oblongata, with a description of the position of the various centres and the laws of reflex action, which are very fully and excellently given. 9. Functions of the encephalon. And lastly, an account of the sympathetic nerves.

Again, in the account of the muscular system in man we have —1. A description of the histology of both forms. 2. An account of striated muscle in its relaxed and contracted states, or, as he terms it, in No. I state and in No. II state, including the various chemical and physical changes observed during the passage of muscles from the first to the second state. 3. The physiology of smooth muscular tissue. Lastly, the adjuncts of the muscular system, cartilage, bones, joints, and their participation in locomotion, are considered.

Though, as a rule, well worked up to the present time, we notice here and there in the original some statements that recent observations have shown to be incorrect. Foremost amongst these is one respecting alcohol. The author still adheres to the notion of Lallemand and Perrin, that if alcohol be received into the system it merely passes through it, and is always found again in its proper form "in the blood and tissues, and especially in the nervous tissue, in which it appears to take up its abode for some time. In short it is not consumed, and its presence as an alimentary substance only serves by economising combustion to increase its utility." All this, of course, as is well known to our readers, has been exploded by Anstie and Dupré, who have shown without a shadow of a doubt that an ounce, or even two ounces, of alcohol can be taken internally in twenty-four hours by a healthy person without reappearing as such in any

of the secretions. A paragraph, however, succeeds, which undoubtedly contains some truth. M. Küss proceeds to say, "We can understand this—that alcoholic drinks may be indispensable in some degree to a man who is obliged to perform severe labour with insufficient nourishment; as to the fatal excess which so often succeeds a moderate use of these drinks, physiology shows us that *our efforts should be directed less against this than against the condition which makes the use of alcohol an imperious and fatal necessity for the working man.*"

In looking over physiological works we are always much disposed to take the account of the action of the heart as a test of their merit. The great differences of opinion that exist upon many points are sufficient evidence of the difficulties that stand in the way of direct observation of its complicated movements, and afford ample scope for the expression of individual opinion.

On turning to this section in M. Küss's work we read with some abbreviation, and in Dr. Amory's language, as follows:

"In order to comprehend the functions of the heart we must not think of it as we find it in the dead body, for there is no trace there of the *muscular elasticity* which is one of the most important properties of the latter, just as important as the contractility, and having a special purpose in that cavity of the heart called the auricle."

(We just stop to point out that from Dr. Amory's translation of the above sentence it might be inferred M. Küss holds that a dead body possesses both muscular elasticity and contractility, which is not quite an accurate version of what M. Küss really does say.)

"*Auricle.*—The chief function of the auricle, on account of its power of dilatation, facilitates the flow of the venous blood. It may be said *to have the same effect as bloodletting at the extremity of the venous system*, by which the pressure of the fluid is consequently diminished. During four-fifths of the time occupied by a cardiac revolution the auricle is in a state of repose, and fills with blood, or rather allows itself to be filled, for it exercises little or no active influence [*M. Küss says*, '*little or no active aspiration*'] upon the venous blood. It resembles at this moment a soap-bubble distended by air blown into it; thus it becomes the receptacle of the blood, the antichamber of the ventricle, a receptacle wherein a large quantity of blood accumulates, and, the auricular capacity being greater than that of the ventricle, the former may be refilled before its contents have been entirely emptied."

(This again is curiously misunderstood by Dr. Amory. M. Küss says, "For the auricle is of greater capacity than the ventricle, which it can immediately fill *without being itself completely emptied.*")

When the auricle is full of blood it contracts suddenly, and drives the blood towards the ventricle, as it were, in the twinkling of an eye. Its contraction lasts from one fourth to one fifth of the total cycle. By computing its times of relaxation (? acting) and repose we may say that the auricle is relaxed during eighteen hours out of the twenty-four.

The contraction of this cavity tends to throw its contents towards the ventricle, or to return them to the veins. There are no valves in the direction of the veins (Eustachian valve being excepted), or they are placed at a distance, and are consequently incapable of preventing this reflux; but the veins are full of blood—under feeble pressure, it is true, but nevertheless some resistance is thus offered to the return of the auricular contents. The condition of the ventricle is at this time entirely different; it is empty, completely relaxed, and consequently offers no resistance whatever; the part which it now plays in regard to the auricle is the same as that previously sustained by the auricle in regard to the veins; and the *elasticity of the muscle when in a state of repose* allows the ventricle to be distended with as little resistance as would be offered by a soap-bubble. Thus the blood of the contracted auricle, meeting with a slight resistance from the veins, and none at all from the ventricle, is precipitated into the latter and fills it. If the muscular tissue of the ventricle is diseased and its elasticity diminished, a certain reflux will sometimes take place into the veins, which is one of the causes of the pathological venous pulse; this venous pulse always exists to a slight degree, but is usually scarcely perceptible.

The auricle is not, however, completely emptied, and its opposite sides do not come into contact with each other. Its rapid contraction being terminated, it resumes the position of a passive organ, and allows the blood which fills the venous system to flow freely into its cavity.

Ventricle.—The ventricle is hardly full before the blood, by its contact with the walls of the cavity, occasions their contraction. The ventricular systole thus immediately succeeds the auricular systole; but the former lasts a long time, because the ventricle is obliged to empty its contents into a cavity which is already full of blood, and which offers some resistance to the entrance (*of more?*). By means of this contraction and prolonged effort the contents of the ventricle pass into the corresponding artery *without any reflux towards the auricle.*

How is this reflux towards the auricle prevented? By means of a special apparatus called the *auriculo-ventricular valves,* which really form a sort of sleeve or bag, hanging from the edges of the auricle into the ventricle, and alternately approach-

ing and withdrawing from the walls of the latter. The name "valve" shows that the *rôle* of this organ was not at first understood. It is now shown that the *tricuspid or mitral valve* does not serve as a plug, but is only a movable continuation of the auricle, acted upon by certain muscular powers. In fact, a large number of *papillary muscles,* having as many as 100 tendons in the right heart and 120 in the left, are inserted in the edges and external surface of this auriculo-ventricular apparatus. When the ventricle contracts these papillary muscles also come into play. It was formerly supposed that these muscles and the tendons belonging to them must serve to prevent the false valves from growing too rigid (M. Küss really says, "to prevent the supposed valve from being too much stretched") under the influence of the retrograde effort of the blood, and from being turned inside out in the auricular cavity. But their function is entirely different, for if the finger be introduced into the auriculo-ventricular region at the moment of the systole of the ventricle, we find that the kind of funnel which hangs from the auricle to the ventricle is continued; it even appears to lengthen itself out, and the finger, as it were, is drawn into the interior of the ventricle. In fact, the first result of the contraction of the papillary muscles is the lengthening of the auricular cone, the edges of which are afterwards brought near each other. While this hollow cone descends into the ventricle the sides of the latter contract, and approach the cone in such a manner that the auriculo-ventricular apparatus acts as a sort of hollow piston, which penetrates the ventricle and comes into close contact with its walls, and thus the ventricle empties itself completely, the contact becoming perfect between its sides and the auricular prolongation.

Nearly all the standard works admit, without discussion, the theory of the occlusion of the auriculo-ventricular orifices by the simple mechanism of a plug or valve, just as in the case of the arterial orifices, but without remarking the entire difference of structure which distinguishes the auriculo-ventricular valves from the semilunar valves of the aorta and of the pulmonary artery. This theory has become, up to a certain point, the property of Chauveau and Faivre on account of the interesting experiments which they have so often made upon horses killed instantaneously by section of the bulb, and in which artificial respiration was kept up. If under these circumstances the finger is introduced into one of the auricles and the auriculo-ventricular orifice explored, the tricuspid valves will, at the moment that the ventricles begin to contract, be felt to straighten, push their borders (scarcely a good translation of *s'affronter par leur bords,* literally, appose their edges), and stretch in such a manner as

to become convex and form a *concave dome* (in the original, *multi*concave dome) above the ventricular cavity. This method of proof does not always furnish such decided results, and many observers—among others Spring and Onimus—have met with one entirely different (not a literal translation, the original being, "the finger thus introduced has given quite different sensations to a large number of observers"). The latter found the auriculo-ventricular orifices effaced by the contraction of the muscular fibres, which at this level really form a sphincter (this is the case in the heart of birds, but not of the mammalia; this sentence appears to be an interpolation of Dr. Amory's, though it is not so indicated). The papillary muscles being now contracted, lower the valves, and these, supporting themselves against the sides of the ventricles, have the effect of driving the blood engulfed between them and the corresponding sides into the arterial orifices. Such is, in short, the working of the auriculo-ventricular membranes. This is the only theory which accounts for the existence and arrangement of the papillary muscles. It appears incontestable that the contraction of the papillary muscles transforms the auriculo-ventricular cone, that is, the infundibulum left between the opposite sides of the valves, into a veritable tendinous cord, more or less hollow, between the interstices of which the blood is unable to make a passage by which it may flow back into the auricle. What becomes of the blood thus passed between the sides of the ventricle and the hollow piston which penetrates its cavity? Under the influence of the contraction of the ventricle and of the working of the auriculo-ventricular system, which acts as an *expulsive apparatus*, the cavity of the ventricle has a tendency to completely disappear, even to its base, by means of the fleshy columns (columnæ carneæ), whose contractions bring the edges of this base in contact with that of the auricular *plunging cone*. The blood, being unable to return into the auricle, must escape by the arterial orifice of the ventricle (pulmonary artery or aorta). We must, however, observe that these arteries are already, by means of the foregoing contraction, filled with blood subjected to considerable pressure, which may be estimated at one fourth of an atmosphere.

We can easily conceive that, in order to overcome this pressure, great force is required on the part of the ventricle; it, therefore, contracts slowly, and with much force; contrary to what we have seen in the case of the auricle, the *ventricular systole occupies quite an appreciable space of time*. It is for this reason also that the walls of the ventricles are much thicker than those of the auricles, and in proportion to the resistance

that has to be overcome those of the left ventricle being thicker than those of the right.

Thus the pulmonary artery (or aorta, left ventricle) is forced to receive the blood which the ventricle pours into it. *The ventricle is completely emptied*, its contraction is no longer necessary, and it is relaxed; it is now that the heart is still. We represented the total duration of a cardiac revolution by *five*, the first fifth being occupied by the contraction of the auricle (one fifth), the three following fifths by the contraction of the ventricle (three fifths), in the last fifth the heart being in entire repose. We know that during these four latter fifths (three fifths of ventricular systole and one fifth of total repose) the auricle is quite still. Speaking generally, the revolution of the heart is divided into three periods: the first of auricular systole, the second of ventricular systole, the last of entire repose. The typical length which we have assigned to these three periods may vary greatly, according to circumstances and individuals, and even in animals; the second period, that of repose, presents the greatest number of varieties. Among the cold-blooded animals, the Batrachians particularly, there is a long interval of repose after each contraction of the heart. But why, when the heart is in repose, does not the blood which has been driven into the artery return to the ventricular cavity? Because the arterial orifice (pulmonary or aortic) is furnished with three semilunar or sigmoidal valves, which are thrown out by the retrograde pressure of the blood, and completely close the corresponding orifice. There is no need of a lengthened explanation of this mechanism, which is plain to any one who examines the skeleton (? anatomical disposition of the parts). At the moment when the blood has a tendency to flow back again, the *gusset*-like form of these valves, the orifice of which is turned towards the arterial cavity, presents a sort of trap to the blood, by which the valves are forced out, and thus occlude the passage. The nodule of Arantius, which is placed in the middle of the free edge of each of these valves, has no doubt the effect of making the occlusion more complete.

To sum up:

"1. The auricle contracts instantaneously and without much force, that it may throw the blood into the ventricle, which is only too ready to receive it. At all other times the auricle is in a state of relaxation, of slow and progressive distension, which produces the effect of bloodletting at the terminal extremity of the nervous system.

"2. The ventricle contracts slowly and strongly on account of the resistance which it has to overcome, and which is occasioned

by the tension of the blood by previous contractions accumulated in the arteries.

"3. The auriculo-ventricular valves are not valves, but are entirely distinct apparatus. The semilunar valves are true valves."

Such is the description of the action of the heart given by MM. Küss and Duval; and it will be seen that, whilst it differs in several important respects from generally accepted views, it is given with much clearness, and yet with as much fulness as the subject demands.

We are not at all prepared to admit the accuracy of the view which denies the function of valves to the auriculo-ventricular valves, though it is obvious that it is a highly ingenious one, and, as is stated in a note, has been the subject of a lively discussion, last year, in the Académie de Médecine.

In speaking of the *impulse* of the heart, M. Küss denies that either the displacement, the recoil, or even the torsion of the heart, has much to do with its production, but considers it to be principally owing to the change in the condition of ventricle, which, at first flabby and soft, stiffens throughout to expel its contents.

In regard to the sounds, he attributes the first sound partly to muscular contraction, and partly to the sudden tension of the auriculo-ventricular valves. The second sound is, of course, attributed to the closure of the semilunar valves.

In view of the recent discussion between Dr. Broadbent and Mr. Galabin, on the subject of the cause of the dicrotism of the pulse, it may be interesting to state that M. Küss is in accord with Dr. Broadbent, attributing it exclusively to the elasticity of the artery, which, having been distended by the ventricular systole, returns to its former size. "The slight ascent interrupting the line of descent marks the exact moment when the arterial elasticity restores to the blood-wave the force which it had stored up, and which would be lost in a rigid tube, being expended in friction."

The question of diapedesis or protrusion of the globules M. Küss relegates to a note, and gives only faint adhesion to it; but it must be remembered that the work was written in 1872 or 1873.

The section on the influence of the nervous system on the heart and blood-vessels is given very clearly and well.

M. Küss is a vigorous opponent of the view so strongly maintained by Dr. Bennett, Robin, and others, of the spontaneous formation of cells in fluids, and says, bluntly, "No one has really observed the formation of cells in the midst of an amorphous liquid (blastema)." He is a staunch upholder of the doctrine of "omnis cellula e cellulâ."

We must say a few words in regard to the translation. Upon the whole it is fair, and sufficiently readable, but Dr. Amory has evidently not read his clean copy carefully; or, what seems to us more probable, has sent his rough copy to press and has paid but little attention to the correction of his proof-sheets. Moreover, some very remarkable liberties have been taken with the original in suppressing considerable portions of many of the paragraphs, in some instances very much to the detriment of the argument. To show that we are not making these strictures without some reason, we shall make a few references to the very first chapter, on the physiology of the nervous system. First, as to careless translation, M. Küss, after explaining that the encephalon is composed of a series of layers or stages of globules connected by fibres, proceeds to say—

"Ces étages ont reçu divers noms, le plus superficiel d'entre eux se trouve en contact avec la voûte crânienne, et se presente sous la forme d'un surface ondulée qui enveloppe le tout, c'est la substance corticale de l'encéphale (*substance grise de circonvolutions cérébrales*)."

This passage Dr. Amory translates thus:

"These stages have received different names; the most superficial of them is in contact with the fornix, and appears in the form of an undulating surface enveloping the whole, and is called the cortical substance of the encephalon (gray substance of the cerebral convolution)."

This is clearly a piece of sheer carelessness, for a moment's consideration would have shown him that the voûte crânienne does not mean the fornix, but the bones of the skull. So, in the the very next paragraph, M. Küss writes—

"Nous savons de plus que des globules nerveux partent des prolongations que les font communiquer les uns avec les autres; ainsi un groupe de ces prolongements fait communiquer dans le cerveau la couche superficielle des globules avec la moyenne; c'est la *couronne radiante* ou *rayonnée;* un autre plus profonde lie la couche moyenne à la couche inférieure."

Dr. Amory's translation runs (in the very first line putting the cart before the horse)—

"We know, moreover, that the nerve-globules start from prolongations which thus unite them to each other. In this way the superficial layer in the cerebrum communicates with the middle or inner layer; and thus constituting the *corona radiata* (fibrous cone); lying deeper the *thalamus opticus* or the *corpus striatum*, unites the middle with the lowest layer"—

thus bidding defiance alike to grammar and sense.

In the section on the pressure of the blood (p. 143) Dr.

Amory's translation runs—"When an artery is opened, especially near its beginning, a jet of blood is seen, which rises to a great height (as much as two *inches*)." M. Küss says "two meters," which is equal to $6\frac{1}{2}$ feet.

In the section on the sense of taste (p. 406), M. Küss says, "Schiff succeeded in dividing the lingual nerve *above* the point where it is joined by the chorda tympani." Dr. Amory translates it "*below*."

Secondly, in regard to the omissions, portions, chiefly in the first part of the work, appear to have been omitted for no obvious reason; for instance, in the original, at the end of M. Küss's account of locomotion, M. Carlet's views on walking and running are rather fully given, but we can find no reference to them in Dr. Amory's translation; nor is there, to make up for this omission, any account of Marey's observations made with his cardiographic apparatus. Again, certain paragraphs in the account of the histology of the nervous system are left out which are correct enough in the original.

Thirdly, in regard to clerical errors in one page (p. 27), we notice two errors in spelling and three or four in grammar.

As a parting word we may, however, observe that, though we have felt it to be our duty to point out the want of careful revision of the translation in many places, we must still do Dr. Amory the justice to say that for the most part the sentences run smoothly, and bear but few marks of being a translation.

We must not pass over without a word of notice the illustrations. These, though often quite diagrammatic, are highly suggestive, and are a noticeable feature of the book. On the whole, M. Küss and Duval's work merits warm commendation, and is a valuable addition to our physiological manuals. It may be safely put into the hands of the student, who, if he masters its contents, will be possessed of all the knowledge he is likely to require in practice, and, at all events, will be in a fitting condition to commence the study of more complete treatises.

IV.—Baths, Waters, and Climate Cures.[1]

In editing this work Dr. Weber has endeavoured to supply what was certainly wanted in England, a general view of the principles of balneology and of the use of mineral waters.

[1] *On the Curative Effects of Baths and Waters; being a Handbook to the Spas of Europe*, by Dr. Julius Braun, *including a chapter on the Treatment of Phthisis by Baths and Climate*, by Dr. Rohden. An abridged edition, edited with notes by Hermann Weber, M.D. 8vo, pp. 635. London, 1875.

We have had treatises of great merit, by Gairdner in 1832 and by Daubeny in 1837, on the chemistry and geology of mineral waters. We have had more or less popular accounts of foreign and domestic spas by the late Drs. Granville, Glover, and Edwin Lee, by Dr. Madden and by Dr. Spencer Thomson. Of more recent and somewhat more scientific works we have in England those of Dr. Althaus, Dr. Sutro, and of the writer; and although some of these, and especially the more recent of them, give a short outline of the principles of balneology, yet none of them profess to give a complete view of the subject. It is rather surprising that none of our spa physicians have come forward to supply the want, and perhaps still more so that, popular as the water-cure and Turkish baths and sea-bathing are in this country, yet no English writer has of late years investigated these subjects. The work of the late Dr. E. Johnson on hydropathy is the best English exponent of it; but we have to look to France for the only two complete works, those of Fleury and of Beni Barde, the last English treatise of importance on the medical effects of water being that of Dr. Currie, now seventy-five years old. We have had no contribution of any great value, founded on original observation—written for many years on, at all events—on the internal use of the mineral waters of England.

Granting, then, that we were in need of a fuller exposition of the principles of balneology, and that we have had to look to foreign sources, there was considerable choice as to the foreign works that might be translated. Confining ourselves to the German literature of the last ten years (for the French have no very good manual on the subject), there was Seegen's excellent work, now ten years old, of which we hope that we may soon see a new edition; there was Helfft's much-used manual, greatly improved in its last edition by Thilenius; there was Kisch's handbook, useful also, but, like Seegen's, perhaps saying more of Austrian baths than was wanted in England; and, latest, there was Valentiners' handbook, a collection of monographs on the various classes of mineral waters, written by men familiar with the use of the particular spas which they describe. Different from these and of more-marked individuality was Braun's book, written in a livelier style, and one that keeps up the attention better, partly owing to the bold onslaught on waters which the author does not himself use. There was still another, in spite of its title not a very practical work, which contains a vast deal of information, but it would require to be condensed, and, indeed, to be remodelled for English readers; we allude to the learned Dr. Lersch's 'Praktische Balneologie.'

Of all these writings probably Valentiners would have given

the fairest and fullest representation of the present state of balneology, and we are glad to hear that a new edition of his treatise will shortly appear in Germany. In the meanwhile we are thankful to have got the translation of Braun now before us, and shall proceed to say a few words about it.

The main work of translation was intrusted to Miss Bunnett, a lady who has produced many excellent translations from the German, and whose recent decease we have been sorry to observe. Laborious as she must have found the work of rendering a technical medical treatise, she has on the whole done it well, although a good many German phrases occur which might very easily have been avoided. For instance, we stumble on "excellence and commendability of arrangements"—on "critical patients turning in waltzes." "Already by Sydenham" means as long ago as the date of that physician. We hear of a "large building for graduation." We are told that "medical establishments are desiderata which would result in a diminution of mortality from phthisis not yet obtained." Usually the meaning of these passages is quite plain, although the Germanisms are quaint, but sometimes it is hard to understand what meaning it is intended to convey. What is "the alternative of either diversion or collectedness?" These and other such blemishes should have been removed from a work of such size and importance. We think that the value of this book is to be found rather in its exposition of principles, than in its account of spas; and the arrangement of the matter is one main cause of this.

Theoretically it is well to discuss bathing and drinking apart, but practically it is inconvenient, when we have to consult a work, to have to refer to an account of a mineral water under the two heads of bathing and of drinking. As mere guide-books many of the works enumerated above, and even smaller ones, are more useful than Braun's. Dr. Weber has done something to supply its deficiencies, and we only wish that he had given us the results of his large experience with a less-sparing hand.

The great characteristics of Braun's work, are its critical spirit and the vigour with which it attacks popular beliefs. He is unwilling to admit anything of which he cannot explain the reason, especially the chemical reason. He undervalues the warm, indifferent waters because their mineral contents are so scanty. He does not see why they should do good, and therefore doubts their activity. In like manner he disparages sulphur waters, because he cannot explain the action of minute quantities of sulphur. He regards lime as always heavy and indigestible, which in one sense it undoubtedly is, although in his later editions he has slightly modified his opinion. Iron he is especially averse to, and more particularly to Pyrmont, the

great iron spring of his neighbourhood. Dr. Braun himself practises at Rehme, a salt spring, and naturally,—for we all praise our own favourite remedies,—lauds that class of waters; and as they are chiefly employed for bathing, the greatest share of his commendation is bestowed on that variety of baths, and especially on those of Rehme and Nauheim, which are warm and contain more carbonic acid than others of that class do. Dr. Braun has, of course, a right to speak with confidence of springs with which he is so well acquainted; but our quarrel with him is, that he speaks with equal confidence and often in a depreciatory tone concerning springs of which he has no experience. His specialty is the treatment of incipient tabes dorsalis, for which he states that Rehme possesses great advantages. There is no question that Braun's free criticism has given an impulse to the study of the effects of mineral waters, and that a perusal of his work should be very useful to us in England.

Instead of now entering on the dry task of an analysis of a hand-book, perhaps it will be more interesting to our readers to present them with a slight sketch of the present state of baths, and of the more recent doings in balneology, referring by the way to some points that struck us on a recent visit to the Continent.

We found that the rules laid down respecting the diet of patients are by no means so arbitrary now as they formerly were. Many articles are now allowed which used to be forbidden. There are still a few bath doctors who keep up the crusade against butter. Easy, however, though it is to laugh at some of the injunctions that are still given, there is no question that English patients very often make a mistake in eating and drinking as usual, while taking waters, and in not availing themselves of competent local advice; and this reminds us of the bad effects which we have witnessed in English patients who have thought it wise to act on the instructions that had been given them, we think injudiciously, by their own physicians at home; and who had in consequence not availed themselves of the services of the physicians on the spot, and their long experience of the operation of their waters.

We observed, also, that as a rule waters are now ordered in smaller quantities, and that patients are not drenched now as they used to be at some baths. It is not considered necessary to produce the disagreeable effects of what is called a bath crisis. One use of small doses we particularly remarked, and this was in chronic diarrhœa. Physicians, with their alkaline waters at Vichy, their alkaline sulphated at Carlsbad, their salt-iron waters at Kissingen and Homburg, their pure indifferent waters at Plombières, all professed to be able to cure this obstinate form of disease. By the same rule the hot earthy waters of

Bath ought to be as efficacious. We should like much to have satisfactory evidence on this head.

Then again, waters are now used in diseases in which they were not formerly much employed. Thus the Homburg and Kissingen springs have both been found very useful as alteratives in gouty cases, both before and after bathing in warm waters; and there is difference of opinion as to whether a course of such waters should precede or follow the bathing. Much must depend on the particular case, but under all circumstances the course of the salt waters should be a mild one. The *nach cur*, as it is called, after bathing, should be of a more or less tonic character. We are of course aware that the weak salt waters of Wiesbaden, and the still weaker ones of Baden-Baden, have always been more or less drunk in gout, while they have been employed in baths.

The accounts of the efficacy, at all events temporary, of alkaline waters in diabetes continue to be confirmed. Vichy and Carlsbad are still the places where the best results appear to be gained; but of late years Neuenahr has been put forward, especially by Dr. Schmitz, as producing at least as striking effects; and if it really does produce them, the Apollinaris water, in close proximity to Neuenahr and containing as much carbonate of soda, ought to be equally useful. But there is always, in considering the effects of treatment on diabetes, a difficulty in knowing how much is attributable to dietetic and how much to medicinal treatment; and we find that the practitioners at Ems, where the waters, along with a little common salt, have somewhat more carbonate of soda than at Neuenahr, declare positively that their waters do no good in diabetes. Dr. Seegen, of Carlsbad, continues to work with unabated zeal in the elucidation of this disease, and by his investigations respecting the conversion of albuminates in the system has satisfied himself, that the nitrogen of such substances does not quit the body solely through the urine and the fæces, but partly also in a gaseous form, and that occasionally to a very considerable amount.

Another constitutional affection, which also requires special dietetic treatment—the increased formation of fat—is very satisfactorily treated at many baths, and perhaps most so at Carlsbad and Marienbad. Kissingen or Homburg waters are also used for this purpose, but are not nearly so effective in the dissipation of superfluous fat. The best treatment is found not to be by violent purging, but by the continued use of moderate doses. Such treatment is much safer than the violent sudation of Turkish baths, which indeed are dangerous for the weak fatty hearts that are apt to be present in fat subjects, for whom also violent lowering treatment by the sulphated waters

is by no means suited.—It has long been supposed that, valuable though sulphate of iron is in its medicinal form, yet its solutions in mineral waters are heavy and indigestible. This is probably in reality the case; but Dr. Knauthe, of Meran, has undertaken the defence of the sulphated springs of the Tyrol, and brings forward evidence of their use in small doses having proved very satisfactory, especially in children. The Tyrolese themselves have used these waters chiefly as baths, as they found that they were too strong for drinking. Waters which can only be taken by the spoonful, diluted with common water, are not very convenient ones for use. Still, if these facts are confirmed, the Tyrol waters and those of Levico (which contain arsenic also) may come to be more employed. The spring at Trefriw, in Wales, is said to be efficacious, and the old sulphated springs at Sandrock, in the Isle of Wight, and in one or two Scotch localities, as Hartfell, may deserve to be examined again. The question is also suggested, whether something might not be systematically done to make our simple English chalybeate waters lighter on the stomach by the addition of carbonic acid. We do not learn with what degree of success an experiment of this kind made a few years ago at Harrogate has met.—Another novelty is the recommendation of the use for baths of acidulous waters, which have been hitherto used chiefly for drinking. As it is now admitted, that what are called steel baths, owe their efficacy to the large quantity of carbonic acid they contain, and not to their small amount of iron, the proposal seems to be quite a rational one; and where acidulous springs are copious, and they are largely charged with carbonic acid, we see no reason why they should not be used in this way. A proposition of this nature has been made respecting the waters of Bilin, which contain much soda and a large supply of carbonic acid.

Non-absorption by the skin is still the prevailing doctrine; and the theory of the operation of indifferent waters remains nearly *in statu quo*. That springs, not containing more than the usual quantity of salts present in many drinking waters, but of an elevated temperature, should produce the really wonderful effects which undoubtedly are to be witnessed at Gastein, Wildbad, Ragatz, Plombières, and sometimes in a smaller degree at Schlangenbad, and even at Buxton, is undoubtedly remarkable; and no influence of mere heat and changes of temperature affords a satisfactory explanation. The electrical properties of such waters have been studied a good deal of late, and it seems to be made out that the presence of very minute quantities of salts causes very distinct electrical changes, but in what way this fact bears on such curative action of these waters, we are as far from knowing as ever.

Very great attention has been paid to the local treatment of pharyngeal and laryngeal affections; and the springs of brine, as at Reichenhall in Bavaria, and at Bex in Switzerland, of salt waters, as at Soden, of alkaline, as at Ems, and of sulphur waters in many places, are now systematically applied. Of late years the inhalation of gases and of pulverised waters has become a common practice in the treatment of pulmonary complaints. Patients spend some hours in rooms having their atmosphere impregnated with the waters of the source, thus inhaling a certain amount of its vapour. At Lippspringe and Inselbad, and at Panticosa, patients are supposed to be the better for inhaling the excess of nitrogen with which their waters are charged. At Reichenhall salt waters are pulverised; and at Weilbach and other places sulphuretted vapour is inhaled. This last procedure has been so widely followed of late in France, that a few words may be said about it.

Gargling the throat with sulphur waters, applying the spray to the back of the throat, and inhaling the vapour of sulphuretted hydrogen, have all found especial favour in France, and may be seen as practised at Marlioz, Allevard, Enghien, Amélie-les-Bains, Eaux Bonnes, and at most of the Pyrenean springs. The effects of these inhalations are thus described by the French. First, there is a sedative action along with a general feeling of comfort; then comes a period of discomfort, circulation and respiration both being irregular and accelerated; next comes the stage of tolerance, in which the pulse is slightly slower than in its normal state; and lastly, if the sitting be continued too long, there is the stage of intoxication, with headache and even with vertigo. The process suits lymphatic and nervous better than sanguine temperaments; delicate people bear it quite as well as strong ones; women and children are more readily affected than men. The therapeutic effects attributed to inhalation are, the relieving of congestions of the bronchial mucous surfaces, especially of capillary bronchitis, promoting the absorption of plastic deposits, above all the relieving dyspnœa and bringing into work portions of the lungs that have been inactive. Inhalation sometimes induces neuralgia of the sixth pair.

Such effects of sulphur waters, although they are ascribed to cold as well as to warm ones, have never been known in England; indeed, English sulphur waters were pronounced injurious in phthisis. Yet we have a large body of French physicians who bear testimony to their undoubted utility. But we may remember that the more imaginative of them have detected distinct proof of arsenical action on the system in the waters of Bourboule; and that we in England have gone on attributing the cures often effected at Kreuznach, to the action of iodine.

So much is there in a notion which we have once got hold of.

Dr. Schliep endeavours to carry out with the Baden waters the treatment of gastric catarrh by the stomach pump.

Baths of carbonic acid are everywhere falling into desuetude, we believe deservedly, for we never had any satisfactory account of their use proving permanently beneficial, and it certainly was not unattended with danger. It is remarkable that, while carbonic acid is less employed in its simple state, greater importance is daily attached to the presence of that gas, whether in drinking waters or in those employed for baths. Almost all popular waters used in affections of the digestive organs and of the uterus, are largely charged with it. So also are all the steel baths which are so largely employed, and the presence of this gas in the waters of Rehme and of Nauheim gives these a character different from that of the ordinary *Solen*. We have to lament in England the absence of carbonic acid from our waters.

As to the season for visiting baths, there is some little change. Bath cures are somewhat more frequent during winter than formerly, at such places as Aix-la-Chapelle, Wiesbaden, Baden-Baden, and Amélie-les-Bains. They can only be carried on in comparatively large towns, where the establishments are not closed for the winter.

Bath cures are also attempted somewhat earlier in the season, and no doubt the class of cases that resort, for instance, to Carlsbad, for congested and enlarged livers, may derive more benefit from drinking the waters while the temperature of the air is moderate, than during the intense heat of July and August. The bath physicians have also more spare time in the spring to give to their cases.—If there be still much to find fault with in many of the hygienic arrangements of spas, yet there is no doubt that this is an era of great improvements in them. Some of those that have come under our notice during the last two or three years have been these. New and well-equipped baths have been provided in many of the Wiesbaden hotels. In Schwalbach a new Curhaus is ready to be opened. In Homburg baths are now provided, and the abundant presence of carbonic acid in its waters is thus utilised. A large bathing establishment has been formed at Kissingen, the Curhaus is being extended, and, if all the arrangements are not quite what might be desired, there is a prospect of defects being remedied under the new arrangements that will come into force next year. At Weilbach a new bath house is in course of construction, and on the picturesque height of Falkenstein, to the south of the Taunus, a sanatorium for phthisical cases is rising at an elevation of 1600

feet. At Wildbad the baths have, within the last few years, been completed, and the extent of the gardens has been increased. The railway is at length rendering the kindred waters of Gastein easy of access. At Baden-Baden the great building, which is to supply vapour baths from the natural heat of the waters, is on a magnificent scale, and is approaching completion. The new baths at Cauterets, in the Pyrenees, are quite models of their kind. At Eaux Bonnes, a new spring has been utilised. Ischia has got new and admirable baths. Dax has a large establishment. We have all heard of the new baths at Harrogate and at Bath; it would be tedious to carry this enumeration further.

To turn to a subject of some economic importance, among the improvements of mineral waters may, we think, be fairly reckoned the better methods now adopted for filling bottles with mineral waters and exporting them. The practice of exportation is a very old one (for we have had supplies in London of Seltzer, Spa, Pyrmont, Harrogate, and many other waters for more than a hundred years), but it was never very satisfactory, and especially in the case of iron waters there was almost always a deposit of some of the salts. This has been in a great degree got over by Fresenius's method, and iron waters now keep very well, as do alkaline and saline ones. The use of carbonic acid and the exclusion of atmospheric air are the great secrets of success.

This improvement is particularly valuable in the case of waters called table waters, and which are used more for dietetic than for strictly medicinal purposes, although some are employed abroad for incipient phthisis (indeed Seltzer water was once used in such cases in England), others for the lithic-acid tendency, and all of them for dyspepsia.

These waters may be classed (1) as simple acidulous alkaline waters. They are abundant in the Rhine district, where we have the Heppingen, the Kron-Thal, and the Apollinaris springs close together, with hundreds such in the Eifel. Gieshübel is the chief of the many springs of this kind near Carlsbad; it is largely exported, and, if it were more conveniently situated, would be a formidable rival to the Apollinaris water which has recently obtained, and justly, such extended popularity; the Geilnau water in Nassau is another of this class. The French waters of Chateldon and of Châteauneuf are akin in qualities. (2) Another description of similar waters, but containing a little more chloride of sodium, is represented by the long-popular Seltzer water, to which Roisdorf may be added, and the very admirable Tönistein Heilbronn near the Rhine, which, in addition, has a good deal of carbonate of magnesia and a very fair supply of iron. (3) To these must be added what may be called the French class of acidulous waters, in which a certain proportion of

lime is present, such as St. Galmier, Pougues, Medaques, Soulzmatt, and Soulzbad.

Waters like these, weak though they be, are of very considerable value in the regimen of patients, and it is very desirable that they should be sold at a price which shall render their use by all who may require them, tolerably easy. The facility with which the more important waters, such as those of Vichy, Vals, Carlsbad, Bilin, Marienbad, and many others, can be procured away from their sources, has rendered the imperfect imitations of them which were at one time welcome in the absence of anything better, now quite unnecessary. Table waters present a drink that is extremely palatable in warm countries. Seltzer water has long been sent to India. And it is difficult to suppose that such waters do not meet a real want, when we hear of Germans in New Zealand ordering a supply of their favourite Gieshübeler.

There is another class of waters which are still less rarely drunk on the spot, but which are largely exported. They are called by the Germans bitter waters, owing to the taste of sulphate of magnesia. They are aperient, and their value has long been appreciated. Thus the true Seidlitz water (what we used to employ under that name bearing no analogy to the natural water) was long known in England as a convenient purgative. It contained chiefly 13·54 parts of sulphate of magnesia in the 1000 parts. Two other waters of the same class were imported from Germany some years ago; they were superior to the Seidlitz and have of late years been very popular, and recently a Hungarian water has been introduced, which bids fair to be the most popular of the three. The following table of the chief constituents will show the relative composition in 1000 parts of these three waters:

	Friedrichshall.	Püllna.	Hunyady.
Sulphate of magnesia	5·1	12·12	16·0
„ soda	6·	16·11	15·9
Chloride of sodium	7·9	—	1·3
„ magnesia	3·9	—	—
Total with other salts	25·19	32·7	35·
Free and half-combined carbonic acid	166·3	69·	278·5

It is obvious, therefore, that the Püllna and Hunyady contain more of the purging salts, Epsom and Glauber salts, and Friedrichshall more chloride of sodium. It has been presumed that the presence of chloride of sodium would render the Friedrichshall water more tonic and less lowering, but the practical result is that this saline water is a less satisfactory aperient than the other two; its action is less certain, and it must be taken in greater quantity. Still, it is used to a large extent at German

spas to supplement weaker waters. It is much used in Germany, and often in small doses as an alterative.

The Püllna is a very good purgative, and we have known it taken habitually for many years in small doses, without losing much of its laxative effect. Its composition, however, is not always fixed; but the Hunyady is a still better water, acting in smaller quantity as an aperient, and generally very mildly and without any griping. The Hunyady also contains the largest quantity of carbonic acid, which helps to make it more palatable, and has a minute portion, nearly a grain, of carbonate of soda. A half or a wineglassful of the Hunyady taken at bedtime produces a couple of soft motions next morning; one or two wineglassfuls taken in the morning produce four or five motions, at first soft and afterwards watery. The action is pleasant, and there is a wonderful absence of griping. It is obvious that a convenient and sure purgative, the taste of which is mild and scarcely unpleasant, which will act when taken in small doses, is a valuable addition to our remedial measures, available in the treatment of almost any affection where an aperient is called for. A great many of the first German physicians have after ample trial expressed their opinion that it is the most certain and most comfortable in its operation of all the bitter waters, while on an average one half less is required to be taken. The special indications are in costiveness, especially in that of pregnancy, in portal congestion with tendency to hæmorrhoids, and with sluggish action of the liver. There are six wells close to each other near Ofen, of almost identical composition, and their waters are mixed together to supply the Hunyady; it is not surprising to learn that there is already a large exportation of the water.

We shall now say a few words about new baths which are rising in importance. If they are called new, however, it is only in a certain sense, for most of them have been long known, though only of late brought more prominently before the public.

In France perhaps the newest is Bourboule, with warm alkaline waters containing an unusually large amount of arsenic. It is in Auvergne, within a few miles of Mont Dore, and has suddenly been converted from scarcely the dignity of a hamlet, into a place containing perhaps a dozen large hotels. No doubt its waters are remarkable, and deserve to have a fair trial. Since its rupture with Germany, France has been very anxious to supply waters like those of that country, and it has endeavoured to give increased development to the strong salt waters to be found at Salins (Jura), Salins (Savoy), and to Salies (Bearne). There is no reason why she should not succeed in

her efforts. It is also attempted to develop the resources of Challes, close to Chambéry, which possesses a very remarkable spring, strong at once in sulphur and in iodine.

One of the places that have been rising into extended use in Germany is Elster, which, no longer a resort merely for Saxon officials, is becoming a formidable rival to Franzensbad and Marienbad with similar waters. The little salt station of Kösen, prettily situated on the Saal, bids fair to become a popular bath. Hall, in Austria, is rising into notice since attention has been called to the large quantity of iodine contained in its waters. The chalybeate springs of the Black Forest, Petersthal, Griessbach, Rippoldsau, are becoming better known. Badenweiler is now a great resort of northern Germans in affections of the chest; it also is in the Black Forest. In Switzerland, Ragatz (Pfeffers) has regained the repute of ancient days, and St. Moritz and Tarasp are visited, as they never were in early times. A regular *sool* bath too has arisen at Bex.

We need scarcely allude to distant Bohemian or Hungarian waters, which the English are never likely to resort to much; otherwise we might refer to Johannisbad, called the Gastein of the North, and to Luhatschowitz, which has some of the strongest alkaline waters in Europe. More within reach are the strong alkaline waters of Bilin, near that crowded place Teplitz, which are more visited since the erection of the new bath establishments; and the pleasant acidulous springs of Giesshübel near Carlsbad, which have lately been vastly improved by their spirited proprietor. Of the future of these two last, or of an admirable chalybeate watering place, Königswarth, which is close to Marienbad, it is difficult to speak with certainty. If they were at a distance from other spas, no doubt they would become popular, but they are all of them too close to large springs of higher importance. Bocklet has admirable iron waters, but has never had any success as a watering place, owing to its too-close vicinity to Kissingen, while the feebler waters of Brückenau which is further removed, have enjoyed great popularity. They have, however, something more than merely weak chalybeate springs to recommend them.

We believe that nothing very new is to be found in Italy. The waters of Castrocaro, with their unusually large amount of iodine, are perhaps becoming a little better known; and the iron springs of the Tyrol, such as the carbonated ones of Rabbi and Pejo, and the sulphated ones of Levico, Ratzes and Mitterbad, are now known a little beyond the limits of their own neighbourhood.

The French have various mineral waters of importance in Algiers, and a good deal has been done to attract visitors; but we cannot at present extend our view in this direction. We hear

nothing of any changes in Portugal, which contains so many and so valuable sources; and we have long given up sending consumptive patients to Lisbon or to Cintra; the political disquiet of Spain must have interfered with some signs of activity which appeared to be discernible a few years ago at some of the baths in Guipiscoa, within the very province which has been the chief scene of the protracted struggle.

Turning to England, if we cannot point to the discovery of any new spring of importance, there have at least been signs of activity at some of the watering places. A good deal has been done to keep the Woodhall spa before the public, and advertising appears to be a necessity of new spas; and if the latest analysis of its water shows that it contains a moderate amount of iodine, it is undoubtedly unusually rich in bromine, although it is a question whether the quantity of bromine present in any mineral water is therapeutically of very much importance. At Droitwich, with its abundant supply of the strongest salt waters, a company started a bathing establishment, which, though now in other hands, has been carried on successfully; and we hope that the system of the German *soolen* will be worked out satisfactorily there. Up to the present moment the baths have met with a great amount of patronage, and we learn that arrangements are being made to meet the growing resort to them.

Two of the places which have made most rapid progress of late years, are situated in comparatively out-of-the-way districts, and both possess sulphur and chalybeate waters, from the chemical constituents of which one could not predicate a great deal; but they are both steadily rising in popularity, and the growth of Lisdoonvarna in the remote west of Ireland equals that of Bourboule, at least in places of entertainment. We wish we could add that the comfort of Irish hotels equalled that of French ones. If the constituents of their waters be not very powerful, both Lisdoonvarna and Llandridnod, in Central Wales, enjoy the advantages of excellent air, and it is well that we should have baths of our own, and baths in remote districts, which may prove useful when patients do not find it convenient to go to a distance. Let the Irish keep up Lisdoonvarna, the Welsh Llandridnod, and let the Scotch cherish Moffat, Bridge of Allan, and Strathpeffer.

And this brings us to another view of the subject, the nationality of baths. A French bath has a character of its own; even an Austrian one differs considerably from a German one. There is also a nationality of doctors; a French one will usually recommend a bath in France, a German will point out the advantages of some German spring, while a Vienna physician is pretty sure to send his patients to one of the Bohemian watering

places. In many cases these physicians may give sound-enough advice, for each country contains a very great variety of waters. An English physician, however patriotic he may be, can scarcely confine himself to England, for there we have not the variety of waters that is required to meet the indications of many cases of disease. Baths may also acquire a sort of character from those who frequent them; thus at this moment Homburg, St. Moritz, and Cannes are essentially English watering places, while others may almost be called Russian or American. Schlangenbad, for instance, has proved particularly attractive to Russians. Now that they have deserted their favourite Baden-Baden, the French can scarcely be said to have a bath out of their own country, which they specially patronise.

Such matters are practically of importance to attend to, although they may at first sight appear beneath the dignity of grave medicine.

Health resorts may be divided into the gay and the dull: places such as Homburg, Harrogate, Scarborough on the one hand; such as Neris, Weilbach, or Lowestoft on the other. It is always important to know how far what are called life and gaiety, are essential to the enjoyment of the individual; a man with a large family of young children will prefer a dull, small village in Wales as a sea-side place to Margate or Eastbourne, although a London club man would find life intolerable in the former. Then again, baths may almost affect a religious character; Free Church clergymen affect Strathpeffer, the Dissenters are in their element at Llandridnod, while the Irish priest is in his full glory at Lisdoonvarna. We even find a hill station in the Tyrol, Obladis, recommended as particularly suited for Ultramontanes, where Northern Germans need not show themselves, although the society is represented as one happy family quite tolerant of differences. Somewhat different are baths where faith is the professed element of cure, as at the praying baths in the south of Germany, or at the miracle-working well of Lourdes. It is also noteworthy that there is a strong religious element in many English hydropathic establishments.

It is satisfactory to find that the German watering places have borne with little loss the abolition of their gambling tables; and what they may have lost is fully made up for by their increased respectability. Ems, Wiesbaden, Homburg, Baden-Baden, have scarcely fallen off in the number of visitors; and the father of a family need now have no doubts as to the propriety of taking his daughters to Baden, or to Spa. The last place, we believe, has suffered more than others; but baths or spas, of permanent value, will not suffer in the end; and Saxon is, we believe, except a few Spanish ones, the only bath that maintains

the evil pre-eminence of having tables. It is not creditable to Switzerland that it should support the lowest class of gamblers.

We shall next allude to two questions in medical ethics, which remain undecided. In some baths the doctors, or some of them, appear at the well every morning to compare notes with their patients. They say that this is very satisfactory to both themselves and their clients, as it saves them the trouble of unnecessary visits during the day. Others, again, think the practice beneath the dignity of the profession, and regard it as a mode of touting for practice.

Then again, it has been usual until lately to give in medical handbooks the names of the bath physicians. Braun, after giving them in the two first editions of his book, excluded them from the third, but they have been re-introduced into the English translation. It has been argued in favour of this, that English practitioners will be able to write to bath doctors about their patients. But we do not see, if they are not acquainted with them, that merely putting a medical address on a letter increases its value; and many object, to the practice of giving the names of physicians, that an author naturally recommends his own friends, and that to give a list of doctors is to put them on an equality with a list of hotels, or of shops.

The great development in almost every country of Europe of establishments professing to be hydropathic, is remarkable. An airy healthy locality is usually selected, and the regular hours and the mixed society of such establishments, which in one sense may be considered to be hotels or *pensions*, contribute much to their good effects. Connected with them now there usually are vapour and hot-air baths; electricity, too, is often applied in various forms; artificial baths of many kinds are prepared, and among them those of pine extract have become very popular; compressed-air apparatus are also not unusual adjuncts. So that a patient who goes to a hydropathic establishment does not confine himself to a cold-water cure, but has the choice of a number of curative agents at his command, which require only to be judiciously applied to make them of very considerable value.

We do not think that the water cure, in its literal sense at least, has made much progress. We should rather say that cold-water treatment is less thoroughly carried out now than it once was, and that the treatment at hydropathic establishments is far more varied than it at one time was, being by no means exclusively hydropathic. But the great development of the cold-water cure has been in the treatment of the more-acute forms of disease, partly in hospitals and partly in private houses. Every one has read of the share which cold

water has in the treatment of phthisis at Görbersdorf and at Davos, of the successful treatment of typhoid with cold baths. Moreover, acute rheumatism and pneumonia have been subjected to the same agent, certainly with quite as much success as by the ordinary modes of treatment. The ideas thrown out long ago by Currie are at last bearing fruit.

As to the whey cure, we decidedly hear less of it; on the contrary, the milk cure has been making progress. The results of koumiss treatment, which was first made known in England about ninety years ago, may be noticed here; apparently the remedy has not gained in general popularity, though there is not wanting evidence of its possessing remarkable restorative properties, when used in sufficiently large quantities. We learnt at Wiesbaden that the koumiss establishment, the advent of which at that place is hailed by Dr. Weber, did not answer, and has been removed; nor do establishments of the kind appear to be really succeeding elsewhere. Milk is found, as a general rule, far more useful in threatened phthisis than whey is. The French have used it extensively in the cure of chronic diarrhœa and dysentery the consequences of tropical disease, and declare a complete milk diet to be a very valuable remedy in these intractable diseases. But the great movement has been in the employment of milk diet in albuminuria and nephritis. In some hospitals all cases of these affections are treated solely by it, and it has been lately said to be useful in dropsy. Skimmed milk has been recommended in diabetes, and buttermilk retains its reputation. Both milk and buttermilk continue to be used with advantage in chronic gastritis and ulceration of the stomach, and milk contributes a favourite article of diet in typhoid fever. It is better to commence the use of the milk gradually, and after a time to make it the sole diet. There are some people who cannot digest milk, for whom the cure is of course inapplicable. But in the case of many who are at first disordered by it, who, for instance, get diarrhœa, the milk diet may after a time answer.

It is hard to say whether the grape cure has made any progress of late. We are inclined to think that it has not; however, the list of diseases said to be amenable to it includes nearly every known malady, although it is doubtful whether we know much more of the therapeutic action of grapes than the contemporaries of Pliny did. The old grape cure, when the diet was exclusively of grapes, has now quite gone out, and it is not surprising that it should have done so, as it induced disagreeable purging and great debility, with more or less stomatitis and irritation of the gums and teeth. On the other hand, the proper use of the grape is better understood, and it is found that a small grape cure, of about a pound and a half of

grapes daily, with nourishing diet, is tonic and invigorating, and even fattening; whereas the use of the full quantity, that is, of seven or eight pounds daily, even along with full diet, is distinctly lowering, and causes a good deal of irritation in the intestinal canal. When fully employed the grape cure is useful in some abdominal congestions, and is said to be so even in chronic diarrhœa. It is only in its milder application that this special treatment can be considered to be useful in threatened phthisis, and it must be remembered that it is usually assisted by pure country air, and it can scarcely be doubted that the climate of Meran contributes as much as the excellency of the Tyrol grapes to making the cure popular at that place. On the lake of Geneva, on the Rhine, and at Dürkheim, we fancy that less is heard of the grape cure now than formerly. Sweet fleshy grapes are the ones that should be selected, and are most efficacious. It is found that the grapes, if eaten off the vine, are too cold; it is therefore better to have them gathered before. It need scarcely be said that the skins and seeds are not eaten. But as it is no very easy matter to most people to extract the juice of eight pounds of grapes, to make the process easier squeezing machines are used by some, so that the patient need only swallow the expressed juice; nay, it has been proposed to bottle it for use, notwithstanding the risk of fermentation. It may be remembered that the great characteristic of grape diet is the immense quantity of grape sugar present, and its extreme poverty in protein compounds. It has been calculated that it requires at least a pound and a half of grapes to supply the amount of protein comprised in an egg. It has been said in a general way that a grape cure without a nutritious diet may be considered to resemble a course of Carlsbad or Marienbad, and one with full diet a course of a carbonated chalybeate to which sugar has been added. But such analogies are somewhat vague. The milk and whey and the grape cures are so intimately connected with the influence of the climate of the place where they are practised, probably an elevated one, that we come naturally to the subject of climate.

A large division of Braun's work is devoted to an examination of climatic health resorts. It is prefaced by an account of phthisis and of its modern treatment, by Dr. Rohden of Lippspringe, a northern bath much resorted to for chest complaints. He has completed his task satisfactorily; and although the one subject of phthisis occupies a disproportionate space in the volume, much of what is said of the bath treatment of phthisis will, owing to its novelty to them, be welcome to English readers. A consideration of the effects of climate reminds one that the English, who have done so much to create

the foreign winter resorts, such as Pau and Cannes, Mentone, and St. Remo, and have set a fashion in this respect to the Continent, figure better in climatology than in other subjects. Of general works we have Sir James Clarke's volume, which was so long the only standard authority; and of a later date Scoresby Jackson's book, which was not appreciated as it deserved, besides many such works as those of Madden, Edwin Lee, Bennett, Williams, and Thorowgood. The Germans have lately come forward; Sigmund's and Reimer's books are very judicious. The French have not written very much on their own health resorts, except in detached pamphlets, although Armand has produced a book of some value on general climatology. Still, we want a work which will give a connected view of the whole subject. The nearest approach to this is Biermann's volume which was published three years ago.

The war with France made a residence in that country unpleasant to Germans, and led to a good many stations within the Italian frontier coming into notice. For instance, Bordighera, near San Remo, which boasts of little dust and of the facilities it offers for getting at once into the woods. Then various places along the coast towards Genoa and beyond it, as far as Sestri and Spezia, have been recommended, each having some little specialty of climate; and in the south, if Amalfi has been little more than suggested (and it has a very sheltered, if rather confined, situation to recommend it), yet Catania has risen into considerable notice. A great many spots about the Italian lakes, as Lugano, Pallanza, Streza, Cadinabbia, Arco, all lying southward of Meran, are new winter stations, and their merits are discussed with much fairness by Sigmund, although he has perhaps something of an Austrian's natural partiality for Meran. The Germans, too, inland people though they are, now that the idea of a navy of their own is a favourite one, are constantly discussing the advantages of sea voyages and of arranging ships for invalids, which are to move about in the winter season from one favoured climatic spot to another.

But the great novelty of modern times has been the attention paid to mountain climates, not only as *nach curs*, but also in the treatment of phthisis. The movement, commencing at Görbersdorf in the north of Germany, has spread to the Alpine climates of Davos and St. Moritz, and it has been a fashion to send chest cases to these heights. It therefore may be worth while to consider at some little length the mode of life and the medical treatment of patients at these elevations, but we shall first say a few words on the grounds on which these Alpine residences have been recommended.

It is scarcely necessary to enter into the theoretical reason

on which great elevations have been recommended in phthisis, as after all it is a question of experience how far phthisical patients really benefit by the treatment they receive at those places. Broadly stated, the two chief reasons for sending patients to them have been, that they may inhale rarified air (which is supposed to contribute greatly to the immunity of the natives from phthisis), and enjoy a climate in which phthisis is said to be unknown. Dr. Rohden has some very sensible remarks on this subject, although defects in the translation render his meaning somewhat obscure. He says that though natives of some Alpine districts, and natives also of certain districts scarcely above the level of the sea, appear both to enjoy an immunity from phthisis while at home (which circumstance obviously cannot depend on amount of atmospheric pressure), this immunity does not accompany individuals when they change their localities; it is therefore at best but a very partial immunity. Then, too, the immunity, such as it is, probably mainly depends on the populations being sparse and their occupation healthy; for, under certain circumstances, natives of mountains, for instance the watchmakers of the Jura, suffer much from consumption. Even if the immunity were made out to be a real one, it would only convey the presumption that the climate of such a district might help to ward off phthisis, scarcely to influence the disease materially when once declared. It by no means follows that, because there is little phthisis among the natives of a place, its climate must be favorable to persons born elsewhere; or, because there may be a good deal of phthisis in the natives of a place, that such a climate may not under certain circumstances be advantageous as a temporary resort to the phthisical. Besides, it is doubtful *à priori* whether Alpine climates, in which acute pulmonary attacks, emphysematous condition of the lungs, and diseases of the heart are not infrequent, should necessarily be favorable to phthisical patients.

In our sketch of Davos, as we have not absolutely visited it, we shall mainly follow the account of Dr. Vacher, a French physician, who saw it in mid-winter, and formed a favorable opinion of it.

The village of Davos lies at a height of about 6500 feet, in a valley running north-east to south-west, open at each end, enclosed on the north and south by high mountains. In winter a cold north-east wind frequently begins to blow along the valley at 1 p.m. The sun does not shine in the valley till 9 a.m., and disappears by 3 p.m. The soil is covered with a layer of snow from six to nine feet deep, which lasts from November to April. December and January are the coldest months. The mean temperature of the four months is 23·15°. The nights are often very cold—their

temperature sometimes as low as —13°. How do patients live under such circumstances?

As before sunrise the temperature is very low—occasionally 5° to 4°—there would be danger for the patients if they showed themselves before that hour in the open air. They therefore keep themselves shut up in their rooms, where, by the aid of double windows and doors and of stoves, a constant temperature is kept up of from 59° to 68°. But when the sun begins to illuminate the valley, the patients leave their homes, and walk in the sunshine. They do so sometimes even in light clothing. The solar radiation is so powerful, that patients have to protect their faces from it; and one of the most curious sights of the valley is, with the snow six feet deep and the temperature in the shade 5° to 4°, to see ladies walking about with umbrellas, and men with their hats covered with ample folds of cloth, to guard against the *coups de soleil* which are so dangerous at this elevation. When the sun begins to descend under the horizon—*i. e.* about three o'clock—the patients hurry to re-enter their hotels, for the change of temperature is as rapid at sunset as at sunrise, and the thermometer often falls 30° to 50° in a few minutes.

As to the regimen of the patients, their diet is substantial, and what may be called fattening; they have plenty of meat and butter and milk, and are allowed wine. They generally gain in weight, and much importance is attached to this increase of weight, as it is regarded as a sure sign that tubercularization is not making further progress. All the patients who are strong enough are douched with cold water for a few minutes, and the more delicate ones are only sponged with cold water. They have daily what is called respiratory exercise or gymnastics, which is intended to bring every portion of the lungs into use. It consists in taking very full inspirations.

We shall not enter here into an examination of what is common to all mountain resorts of sufficient altitude—the purity and comparative rarity of the air, which, therefore, requires increased action in the lungs to procure for their function the requisite amount of oxygen; nor need we remark that, at all events in the first instance, high elevations cause increased rapidity of pulse, and a notable acceleration of respiration. But it must be observed, of Davos specially, that patients who visit it are exposed to great changes of temperature. The thermometer may stand at over 104° in the sun, while in the shade it is only 0°. The characteristics of the climate are, great heat of the sun during the day and intense cold at night. The great heat of the sun is probably owing to the intensity of solar radiation caused by the small amount of moisture in the air; for though

there is difference of opinion on the subject, the dryness of the air is practically proved by the length of time that meat will keep fresh. During the dry season patients do best at Davos; they suffer most when moisture comes with the melting snow in March.

Those who think most favorably of Davos maintain that phthisis with tendency to hæmoptysis is treated there successfully, that patients with laryngeal affections do not get on particularly well, and that the class which does best, is that of patients who suffer from excessive bronchial secretion; they mend rapidly.

For ourselves, although the experiment is said to have been more successful than could have been expected, yet we cannot but think that it is one not lightly to be made. Life may be endurable there in a fine winter, and when there are ample supplies of food; but it often happens that, instead of there being a constant clear sky, there is a succession of snow-storms; the weather is admitted to be most disagreeable when the fall of snow takes place in November, and very much worse in March, when the melting commences. Last winter we believe the patients were, owing to communication being stopped, nearly starved at Davos, and they had extreme difficulty in getting away, for it is a seven hours' drive, even in fine weather, from thence to the nearest railway station; and even if the weather is fine, this is a most serious inconvenience to patients when Davos does not suit, and who may be obliged to change their quarters in the midst of the winter season.

On the whole, pleasant and useful though such heights are in summer, we believe that their use in winter is only a passing fashion, chiefly with Germans, which will soon disappear. Patients will in the main do better by resorting to the southern stations at moderate elevations, or at the level of the sea. In these they, and especially Germans, will find a milder winter temperature than they have at home, and an earlier spring. They will find the houses more sunny, the days longer and much oftener fine, with little snow or ice, and a great many inducements to be in the open air, which they have not at home. Germans will seldom have to complain, like the English, of the inferiority of the butcher meat.

We think that the Germans are making more of a move in the right direction, when they endeavour to attract patients to a town with moderate winter temperature and many local advantages, especially in the superior comfort of its houses, such as Wiesbaden, than in sending patients to fresh settlements like Davos. Canstadt and Baden-Baden also put in their claims as comfortable residences, with comparatively mild winter tempera-

ture; we say comparatively, because all these places have a continental winter, and a degree of cold which we in England should reckon as severe.

At the present moment, what we believe to be the greatest practical difficulty with reference to the search for health resorts for patients is, not to select a place for them to winter in, but to tide over the spring. It is not only that you must hurry away before April from Davos, but a general exodus sets in from all the places on the shores of the Mediterranean before the end of April, when it is too early for delicate patients to return to the north of Europe. There are some spots in the mountains behind Cannes and Nice where summer quarters may be had, but they are not, on the whole, comfortable places, or well suited for invalids. At this season the various places on the southern slope of the Alps or about the Italian lakes, many of which we have enumerated, may be available as transition stations; but they may be at an inconvenient distance for those who have spent the winter on the Riviera and wish to return to England in the summer. For them perhaps some of the Swiss stations are more convenient, being less out of the way, and some of the lower hill stations, or *sommer frische*, as the Germans call them, may be visited as early as May, which is a good season for a whey cure, or what suits phthisical patients usually much better, for a milk one. For patients for whom it is expedient that they should not visit their homes even in summer, a number of comfortable establishments have sprung up of late years, at moderate heights, both in Switzerland and in Germany, and many of the Pyrenean baths are good summer residences for such patients, as are likely to benefit by the use of sulphur waters.

We shall now express our general views respecting the present and future of health resorts of all kinds, whether valued for climatic merits or for the character of their mineral waters.

Braun's work, and others of a similar character, have undoubtedly led to a more critical study of the effects of health resorts of all kinds. The influence of the heat and cold of baths, the comparative value of the different constituents of mineral waters, the purity of the air, the amount of ozone present, the altitude of the place and its hygienic condition, have all been more carefully studied; and the English have given a considerable impetus to the hygienic improvement of such places.

The arrangements of English watering places are, as a rule, superior in a hygienic point of view to those usually to be found abroad. Much has to be done in almost every foreign watering place to improve the drainage, and to remove smells—amidst

which, by the way, the population seems to thrive, and, indeed, these strong palpable odours seem to be less injurious than more disguised gases. We have often been struck with the fact of medical men occupying quarters, in which it was at once palpable that there was something defective in the drainage. Nothing appears to have been done to improve the unsightly and odorous retreats in the neighbourhood of some baths, although their offensiveness has been pointed out by the medical officers on the spot, notably in the woods at Carlsbad and Marienbad. Besides local improvements, and also meteorological observations, we are in want of fresh clinical reports of the effects both of water and of air cures. We hear much of wonderful cures, little of failures, and in either case we are seldom favoured with the particulars of cases. Of course there are a few exceptions to this which we might quote. The French give tolerably full reports of the cases treated in the Pyrenean baths, but there is a great want of clinical histories of pulmonic cases sent to the Riviera.

If we look forward to the future of balneology, we think that the prospect is encouraging. As places are brought into closer communication with each other by improved facilities of travelling, physicians begin to have a better comparative knowledge of the value and of the properties of other waters besides their own. District meetings of those who practise at spas, have become pretty frequent, and have led to a discussion of the practice of different establishments, which must help to give larger views than those likely to be held by the specialist of any one. There are several weekly and at least one half-yearly journal of balneology in Germany, and the German medical journals readily admit contributions having reference to mineral waters. The case is very much the same in France, and there they have a general centre in the Société d'Hydrologie and in its journal. The evil in the way of propagating zymotic disease at our watering places by means of convalescents who ought to be kept in a state of isolation, was discussed fully at the late Social Science Meeting at Brighton, and the best remedial measures were considered.

If we turn to England specially, the future is not quite so promising. Nevertheless, a commencement has been made in the right direction. Great improvements have been made at Bath, at Harrogate, at Buxton and elsewhere, but the profession generally does not take much interest in climatology or balneology, and bath physicians are very sparing in communicating accounts of their practice. The chemistry of our waters has been neglected, and we have no good analyses except of a few of the chief ones. A general fresh analysis of the mineral waters of

England is much wanted, and it might be very fitly undertaken by Government by the agency of the School of Mines or otherwise.

Our physicians are rarely well informed concerning climates or waters. We have recently observed a writer who has paid attention for a long time to such subjects, group together "saline chalybeate waters, such as Ems, Schwalbach, and Spa," which shows how loosely we all talk of different waters.

Regular courses of lectures are delivered in German and French medical schools on balneology and climatology, and a knowledge of these matters is considered to be a necessary part of the equipment of an accomplished physician. We have no instruction about them given in England; we believe that Dr. Sutro delivered a course of lectures on mineral waters at one of our schools, but this was some years ago. We observe that Dr. Th. Williams is about to deliver the three Lettsomian Lectures before the Medical Society of London, on the influence of climate on consumption. This is a move in the right direction; but we shall not be satisfied until courses of lectures on such subjects are delivered in at least several of our medical schools. At present the virtues of mineral waters are but partially understood, and are undervalued. JOHN MACPHERSON.

V.—Imbecility and Idiocy.[1]

THE census of the population for 1871 represents that at that period England and Wales contained 39,567 lunatics and 29,452 idiots and imbeciles, Scotland 6792 lunatics and 4621 idiots and imbeciles, and Ireland 16,505 of the former and 6704 of the latter class. The latter quotation has been taken from vol. i, 'Vital Statistics.' It is obvious that these figures cannot be relied upon as accurate, because, with the exception of returns from public establishments, they have been supplied by unscientific and, in many instances, uneducated enumerators.

Considering the important and specific objects in view, it was

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[1] 1. *Séguin. Opera Omnia.* 1846, 1866, 1873.
2. *On the Imbecile and Idiot.* By Dr. DUNCAN and Mr. MILLARD. 1866.
3. *Annual Reports, Idiot Schools, Massachusetts, down to* 1874.
4. *Annual Reports, School for Idiots, Pennsylvania.*
5. *Annual Reports, School for Idiots, Kentucky.*
6. *Annual Reports, School for Idiots, New York, down to* 1875.
7. *Annual Reports, Earlswood Asylum for Idiots, down to* 1875.
8. *Reports, Schools—Lancaster, Larbert, Gayfield Square, various years.*
9. *"Education of the Imbecile."* By DORA GREENWELL. 1869.
10. *Some Cases of Microcephalic Idiocy and Cretinism.* By Dr. IRELAND. 1875. Reprint.

expected that the more trustworthy machinery of the respective Boards of Lunacy available might have been appealed to, and have afforded more precise and trustworthy data on the subject; but the English Commissioners briefly stated that the total number of lunatics, idiots, and persons of unsound mind is 62,027; and the Scotch Commissioners that the number of lunatics reported to them is 8069; while the Irish Inspectors give 18,307 as the gross numbers in 1873; so that we positively receive less-discriminating information from these official sources than from the loose and approximative decennial numbering of the people. But, even accepting these totals as supplying the gross, unanalysed mass of diseased and defective minds existing in our communities, it is demonstrable that they do not include large classes of individuals who must be regarded philosophically, if not legally, as coming under the category of imbecility, and as, under certain circumstances and at certain periods of life, entitled to claim civil, medical, or moral aid. Such are the backward children (les enfants arriérés), the dolts, dullards, the partially incapable and ineducable who are to be found in every school; yet even some of these, by a change in the kind of tuition, or in social or domestic surroundings, may outlive their infirmity and hebetude, and may be developed into useful though dependent members of society.

Such are, likewise, the individuals for whom Miss F. Hill pleads so powerfully and pathetically as "Children of the State," who, born and bred within a workhouse, are starved, stunted, perverted, poisoned, by the constant contact, example, and indoctrination of the inmates, often their own parents and relatives. They grow up without thought, feeling, and conscience in keeping with their nurture, grow up to love their home, restricting their hopes, ambition, and usefulness to the standard there required. They prove utterly profitless and cumbersome in the external world, and invariably return to their starting-point; perpetuating, in their career, a race of paupers and drones, unless withdrawn at an early age from the reach of infection and placed under more natural auspices and more faithful guides.

These outcast foundlings often gravitate towards a lower depth and merge into that criminal class which, although confessedly characterised by such weakness and waywardness as to render them in the opinion of many observers moral idiots or only partially responsible, and to identify them with the insane or the imbecile, must be equally regarded in the statistics with which we are dealing. In this country sporadic cases occur, and generalisations obtain circulation, which countenance the notion of the identity here suggested, under the influence of

which the life of a culprit may be spared on the ground that he is a simpleton, feeble in intelligence and ignorant, and of animal and brutish nature; but bolder views prevail in France, where we find Voisin (père) commissioned to examine a prison population amounting to 500, reporting that 315, or two thirds of the number, presented the lowest type of cranial development. So impressed was the physician with the connection between juvenile depravity and disease that he founded a school for the reformation or restoration of vicious, passionate, obdurate, degraded children—an adumbration of our own reformatories—which was, we believe, attended with very moderate success.

In the official documents which we have previously consulted, the Commissioners record that in England 15,018, in Scotland 565, and in Ireland 3130, persons of unsound mind are detained in workhouses. In these instances the individuals are recognised as lunatics and are deprived of their liberty and social rights. In Scotland they are confined in special lunatic wards suitably appointed and officered, differing little from an ordinary asylum except in being succursal to the workhouse, and as generally possessing fewer advantages and luxuries. These lunatic paupers form part of the grand total of patients falling under the supervision and direction of the Commissioners, but not of the vast concourse of infirm, non-productive, diseased and degenerated beings who constitute so large a proportion of the inhabitants of workhouses, and who, though neither certificated nor inspected, are acknowledged to be and are treated as of feeble mind.

It was found a few years ago, when Mr. Gathorne Hardy was at the head of the Poor Law Board, that the workhouses in the metropolis were crowded, and, so far, diverted from their original purpose, by the presence of paupers, who, though not recognised as insane, were incapable, in consequence of limited intellect or of mental peculiarities, of acquiring the most elementary knowledge, of understanding the simplest duties of ordinary life, or of maintaining themselves. In consequence of this it was enacted that four large hospitals should be prepared, under the direction of the Poor Law and Parochial Boards, for the reception of these unfortunates. Two of these houses first erected, after being in operation for only eighteen months, contained, respectively, upwards of 1600 imbeciles and idiots. In 1873 a third asylum was constituted by the appropriation of the smallpox hospital, Hampstead, where in six months 571 of the same class had already accumulated, of whom 87 were idiots and 86 epileptics. But this large number does not represent all the idiocy of the metropolis. At the time

of the Act it was calculated that there were 40,000 imbeciles and weak-minded in workhouses in and around London.

To complete this gloomy picture it is only necessary to transfer to our pages the sombre colouring presented in the report of the establishment at Caterham, 1873-4. Of 504 persons admitted 22 were idiots, 94 were imbeciles, 54 imbeciles with epilepsy; 1 imbecile with chorea, 278 dements from various causes, 39 maniacs, and 11 melancholics.

In the official reports quoted it appears that in England 836 and in Scotland 138, recognised idiots are at present undergoing training and tuition in institutions or schools licensed by the Commissioners; but how insignificant this number must be to that actually placed in seclusion without training or tuition, may be gathered from the fact that in one of our smaller asylums containing 476 inmates 39 are idiots, and that in one of our largest asylums with 1400 inmates, 105 belong to the same class.

As all idiots consigned to captivity are either selected in accordance with some theory of curability and educability, or have been deprived of liberty as obnoxious or dangerous to society, it might be well to determine, even although arbitrarily, whether they may be gathered into genera or groups upon recognised principles of classification. The only arrangements which it is necessary to introduce are those of recent date, not because they are necessarily the best, but because they have been constructed since the true characters of idiocy and imbecility have been determined, and have been taken into account in their treatment; and since the first step in separating their victims from the general mass of those labouring under mental impoverishment or impairment was made by Esquirol when he wrote his now-famous definition, "L'homme en démence est privé des biens dont il jouissait autrefois; c'est un riche devenue pauvre; l'idiot a toujours été dans l'infortune et la misère." This distinguished philanthropist aimed at more elaborate distinctions, and founded a psychological system upon the power of language, or the use of words as signs of ideas. In his first degree or species of imbecility, speech is free and facile; in his second, speech is difficult, the vocabulary limited. In his first grade of idiocy single words or short phrases are used; in the second monosyllables only are articulated and cries; in the third mutism exists. This classification is based upon legitimate grounds so far as it embodies gradations in the power to articulate; but it cannot be admitted as an index of general or special intellectual or emotive powers, or as establishing distinctions between different forms of idiocy, as the dumb are not necessarily at the bottom of the scale, nor the loquacious at the top, as the former may display many manifestations of ability, and the latter may

have a large store of vocables and verses without understanding a word of what he has committed to memory.

Dr. Duncan and Mr. Millard have divided the pupils coming under their charge into eight classes, partly as the members of each are capable of exercising different degrees of locomotion, and partly in relation to their mental or moral incapacities, but the scheme is neither practical nor philosophical. A psychological basis has been suggested by Bucknill and Tuke, classifying the weak-minded as, 1st, those who exhibit nothing beyond reflex movement known as excito-motor; 2ndly, those whose reflex acts are consensual, or sensori-motor, including those of an ideo-motor and emotional character; 3rdly, those who manifest volition, whose ideas produce some intellectual operations and consequent will. These views harmonise with popular opinion, but it is abundantly clear the specific distinctions, resting almost entirely on the functions of the nervo-muscular system, must exclude a vast majority of the individuals whom it is desirable to classify. An anonymous author has placed all idiots under five categories, as they are one, two, three, four, and five sensed; but this proposal, which might be serviceable so long as the senses are held to be the sole inlets to impressions and the means of building up the intelligence, but it leaves untouched the original faculties by which such sensations are received and comprehended, and does not embrace the original feelings, passions, and propensities of our nature. It will be observed that all these speculations recognise, and legitimately recognise, the phenomena of psychical action as the foundation upon which classification should be attempted. Dissatisfied with this course, a daring, and, it is conceived, a rash innovator, has propounded a proteiform arrangement in which idiocy may be divided into the ten following groups:

"1. Hydrocephalic idiocy. 2. Eclampsic idiocy. 3. Epileptic idiocy. 4. Paralytic idiocy. 5. Inflammatory idiocy. 6. Traumatic idiocy. 7. Microcephalic idiocy. 3. Congenital idiocy. 9. Cretinism. 10. Idiocy by deprivation, that is, by loss of two or more of the senses."[1]

This experiment not merely sets all the admitted principles of nosology at defiance in electing a different ground of distinction for every species—in one, pathological, in another etiological, in a third symptomatological—in one physical, in another psychical—one involving chronic, another acute degeneration—but in application it would be open to still graver objections, as almost all the species in the catalogue are convertible—in other

[1] Dr. Ireland, 'Journal of Mental Science,' October, 1872, p. 333.

words, resolvable—into each other; traumatic idiocy including a great number of cases of congenital idiocy, paralytic idiocy being the consequence of several species, and congenital idiocy comprehending at least five; so that one patient might readily be placed in several groups. Dr. Wilbur, one of the most eminent practitioners connected with this department, says, in criticising this scheme, "The essential thing in idiocy is the mental deficiency, no matter on what physical cause depending." "The eighth group, or congenital, may also include any one, or, in fact, almost all of the preceding. Of several of them it may be said that they are not only not incompatible with certain others, but very commonly associated with them; in other words, many idiots could be properly ranked in two or more of the categories named."

In the present state of crass ignorance of the external causes and of the cerebral alterations producing weakmindedness, our diagnosis in many cases being necroscopical, all such attempts as that under consideration, however ingenious, must prove abortive and deceptive, and it would be infinitely safer to trust to any assortment of the sufferers into those labouring under intellectual privation or perversion, in whom scholastic or subjective treatment might be tried, and into those where privation or perversion of the sentiments, passions, and propensities indicates the necessity for moral or objective treatment; or to deal with and act upon the characteristics and peculiarities in each individual, or in those very circumscribed groups which are allied by close affinities, as we now propose to do. In execution of the purpose in view it has been deemed expedient to describe them in one class, differing widely among themselves as to the degree of privation, but associated together by the characteristic of the non-development of the mental powers. It is likewise expedient to present a picture of the most striking features of this class, which, though living amongst us, a race within a race, are separated from us by such an unmeasurable distance as scarcely to be recognisable as members of the same family. Such a description is necessary in order to comprehend the difficulties to be overcome in raising them from their isolation, and in bringing them into relation with ourselves. Idiots and imbeciles may be met with in all respects naturally formed, or with heads of extremely small size, or enormously large, or irregular, square, rounded, compressed, elongated, goose-shaped, or perfectly natural in appearance and proportions. It is of small import to our present discussion whether microcephalism be due to the premature closing of the sutures of the cranium or to pithecoid derivation; but the heads of living,

walking, speaking beings have presented so small a circumference as—14, 14⅛, 14½, 15 and one line, 14 and one line, 18 and four lines,[1] and 13 inches.[2] The latter measurement refers to the head of Antonia Grandoni, mentioned in Dr. Ireland's 'Microcephalic Idiocy and Cretinism,' p. 23, who, notwithstanding her rudimentary cranium, spoke intelligibly, even sensibly, danced, sang, played the cymbals, was vain, imitative of secular as well as sacred proceedings, had a good memory stored with ordinary facts, but "every attempt to instruct her was without success."

They are generally of small stature, often mere dwarfs. The average height of 172 males was 64 in., of 116 females 60 in., being about three inches in the one sex, and seven in the other, less than the average height of the inhabitants of this country. Imbeciles were undoubtedly included in this calculation, as the genuine idiot is of very diminutive size, even when well formed. They are the Aztecs of this country. A speaking, singing, tyrannical manikin of 30 years old and 36 inches in height was recently sought for, and pulled up from behind a roll of leather, in order to be introduced to his visitor. They are feeble, puny, anæmic, destined to an early death. According to Howe, the average duration of life in the congenital idiot is about 12 years. An idiot is certainly old at 25, and it is rare to find one above 30. The spine is often deformed; great deviations from symmetry exist in other parts of the skeleton; the arms and legs are disproportionally long or short; the toes and fingers may be of the same length, or webbed. They are generally left-handed. The features may be of exquisite delicacy, or coarse, grotesque, hideous, vacant. The eyes are often widely separated, squinting, rolling, with dilated pupils, insensible to light. The nose is flattened, or extremely gibbous, and with the rest of the features repulsively prognathous. The lips are protuberant, gape; the teeth are irregular, decayed, never shed, or shed several times; the palate may be highly vaulted. The chin projects or retreats in a marked manner. The thyroid gland is often increased in size, even in this country. The ears are large, projecting, misplaced, and often without sculpture. Many cannot sit, stand, nor walk, nor leap. Of 574 examined, 53 were as helpless as infants; 74 as children of two, and 94 as children of seven years old. Those who acquire the use of their limbs roll, swagger, vacillate, leap, or are in incessant movement. Many are quadrupedal, and in various features and habits so closely resemble the lower animals as to justify Linnæus in his curious identification of such individuals as addi-

[1] Shuttleworth, 'Brit. Med. Assoc. Journ.,' Oct. 9th, 1875.
[2] Ireland, 'Edinburgh Med. Journ.,' August, 1875.

tional species of the genus homo, and in his formation of the following table of varieties:

1st. Juvenis Lupinus Hessensis, 1544. (A young man found in Hesse among wolves.)

2nd. Juvenis Ursinus Lithuanus, 1661. (A young man found among bears in Lithuania.)

3rd. Juvenis Ovinus Hibernus, Tulp, Obs. iv. (A young man found among wild sheep in Ireland.)

4th. Juvenis Bovinus Bambergensis, Camerar. (A young man found among herds of oxen near Bamberg.)

5th. Juvenis Hannoverianus, 1724. (A young man found in Hanover.)

6th. Pueri Pyrenaïci, 1719. (Two boys found in the Pyrenees.)

7th. Puella Transisalana, 1717. (A girl found in the Dutch province of Overyssel.)

8th. Puella Campanica, 1731. (A girl found in Champagne, and since named Mlle. Leblanc.)

9th. Johannes Leodisensis, Boerhaave. (John of Liège.)

10th. Puella Karpfensis, 1767. (The girl of Karpfen.)

11th. Juvenis Averionensis, Anno Republicæ Gallicæ octavo. (The Savage of the Aveyron in the year eighth of the French Republic.)

Had this zoological synopsis been framed during the present century, it would have contained allusions to the Batrachian or the Simian type—the goose-idiot of the Wakefield Asylum; the bird-idiot of Cesare Lombroso, "who chirped, leaped on one leg, and before putting himself in motion stretched out his two arms like wings, used to hide his head under his armpit, and chirped strongly when frightened or at the sight of a stranger;" and the rabbit-idiot of Conti Angelo de Spairago, "so called from his habits of moving the nose and lips and from being timid and fond of green vegetables, such as salads and cabbage, and also from his stamping with his feet when frightened, as rabbits do."

A large number cannot use their hands for apprehension, in moving objects, in dressing or washing, or for any deliberate purpose. Fitful rays of genius, if the possession of a particular power or powers in a high degree can be so named, and great delicacy of muscular power, frequently dissipate this gloom. A certain idiot is recorded to have displayed such exquisite skill in painting as to justify the cognomen of "Raphael." But, as if in obedience to the law which permits the cultivation but not the positive *enlargement* of the faculties of the weak-minded, his efforts were confined to the representation of cats. He drew and painted cats, kittens, grimalkins, of all sorts, shapes, and

shades, and in every possible combination of circumstances, but ever and always and nothing but cats, and fairly earned the additional appellation of the "Cat Raphael." A few years ago there was seen in Earlswood the beautiful model of a ship. Those who are competent to declare the perfect exactitude of every block and rope pronounced the vessel ready for sea; and to a landsman the structure from stem to stern, from keel to masthead, from the admiral's cabin to the cockpit, appeared a marvellous and perfect specimen of naval architecture. Four years had been spent in completing this, the second attempt of the builder. His original failure was attributable to his ignorance that wood could be bent by being placed in hot water. This effect was his own discovery; in fact, the whole model is a creation of his own constructive powers. He is the son of a gardener in an inland county, and, although he passed several years in the carpenters' shop of the institution of which he is an inmate, previously to the commencement of the model he had never seen the sea, nor a river, nor a ship, save the representation of one in the centre of an old cotton pocket-handkerchief. When his work was nearly finished he visited a dockyard and made a few changes. This inspection suggested coppering; and the "Victoria" was safely raised by machinery of his own invention, executed by three idiots, from her scaffolding, and he then engaged in making a stage upon which this gigantic plaything, which received from him a sort of hero-worship, might be launched "to walk the waters as a thing of life." He was taught to copy drawings, and some of his productions were so excellent and curious as to have reached the royal palace. Yet the lad, at 24, remained a well-formed, pleasant-looking, small-headed, large-pupilled idiot. He could articulate a few words or imperfect words, but in general babbled.

A certain proportion of idiots cannot perform the act of sucking, and individuals arrive at the age of puberty who never seize or chew their food, but require to have it placed in the pharynx. Vision is imperfect, but chiefly from the inability to fix the eye. Not only do they distinguish colours with difficulty, like young children, but hearing seems limited to particular sounds or classes of sounds. The Sauvage d'Aveyron was unmoved by the discharge of a pistol, but was roused by the cracking of a nut. Yet certain individuals are keenly alive to musical talents, acquire a facility in singing and playing, and examples exist where great proficiency has been acquired and where further enlightenment has been achieved through such means. The sense of smell may not exist at all, or appear exalted, as in the blind, conferring a power of distinguishing particular woods, per-

sons, stones, metals; or it may be perverted, seeking gratification in offensive and digusting effluvia. Taste is generally dormant, but may be displayed in a craving for stimulants and articles yielding strong impressions. While the sense of touch is sometimes concentrated in some unusual spot, such as the soles of the feet, the thighs, &c., general sensibility is defective. Idiots bear all maladies—burns, scalds, and other injuries—with comparative indifference. A hole has been drilled through the cheek and forcibly enlarged, so as to join the canthers of the lips, by the patient, without any betrayal of suffering; and two idiots amputated with a piece of their night-vase, &c., a large part of the organs of generation, and then treated the matter as a capital joke. They are dull, inactive, hebete, of dirty habits, either from unconsciousness of the calls of nature or want of power over the sphincters or degraded tendencies; they are gluttonous or of capricious, morbid appetite, often preferring offal, garbage, or even ordure, to ordinary food. Of 500 examined, 116 ate double the quantity by measurement which satisfied healthy children of the same age. Of 389, 204 were masturbators; and although this vice may be practised to a frightful extent, instinctively or from local irritation and suggestion, it is often associated with salacity. Some of these habits seem to show the reappearance of instincts which can only have belonged to man in a low animal condition, even before he emerged from savagedom, and which have died out in the race long ago; such, for instance, as the gnawing of the umbilical cord of the infant by an idiotic mother.

They are often mutes, or deaf-mutes, articulate with difficulty a few monosyllables, use a very limited vocabulary, or merely emit cries. Of 300 examined, the deficiency in language was as five to ten of healthy persons. Of this class was Massieu, the celebrated pupil of Sicard, who was raised from being a dumb, dull, half-wild shepherd-boy, familiar with his flocks and a few inanimate objects only, to form the most profound definitions of Deity, eternity, time, gratitude, thus refuting an opinion attributed to Griesinger, that a mute idiot could not form an abstract idea. In an analysis of 20 speaking idiots, 7 are found to have acquired many words; 1 many known words, without attaching meaning to them; 2 to employ an unknown tongue; 1 to use two words—a noun and an adverb; 1 to use three—one noun and two pronouns; another three, consisting of a noun, pronoun, and adverb; a third three—a pronoun and two adverbs; and others of the series four, five, six, and eight words respectively. Many have a notion of plurality; a small number can count so far as three, others so far as ten, and can do spontaneously a simple mental addition, but almost never subtract.

They have originally, and sometimes through long years acquire, no notions of the qualities of external bodies, extension, form, colour, nor of distance, time, &c., and what knowledge in these respects is subsequently exhibited is the result of that accidental contact with matter which is unavoidable, or that rude teaching to which they may have been subjected. It might appear a presumptuous attempt to settle disputed points by affirming, as appears to be the case, that this class, or many members of it, are destitute of any notion of personal identity; and, on the other hand, of the existence of other things than themselves, the *not me;* but the great aim of all primary treatment is to bring them to a consciousness of *self*—to an association of impressions with *themselves,* and then to bring them out of that individuality in which so many appear to be bound up, to reach or rouse consciousness by external impressions, and to impart a personality to the mind and a knowledge that it is not alone in the world. A large majority have no instinct of self-preservation, no fear, no sense of order, arrangement, no propensity to acquire as property, although some of them are pilferers or hoarders; they have no spontaneity, few affections, and none of those sentiments or that moral sense which give to man his proudest distinction and connect him with a higher state of existence. While, however, incapable of exercising the intellectual powers, of perceiving, grouping, comparing, separating, recalling their experiences, or of arriving at truths from such premises, and while even under instruction all impressions are received as isolated, as separate, solitary facts, and are neither connected nor classed, positive capacities are manifested amid the general negation. They may be imitative to an extraordinary degree; they may possess powers of calculation; they may be cunning, cruel, lascivious. Examples of an extraordinary development of the memory are frequent, even where correct notions of the past, the present, and the future are wanting. Facts, incidents, sounds, words, are recalled with an opulence and accuracy that would baffle the trained orator or actor. An idiot is described as, though unable to read, able to repeat hundreds of lines of verses by the sound. There are chronological imbeciles, who, ignorant of history and the connecting links, can give the date of every great battle, calamity, or event, repeating them as isolated facts devoid of interest and meaning. There is the calculating idiot, who, though when seen pale and apparently in the clutch of death, told in twenty-five seconds how many moments a person had lived, the date of his birth being given. His calculation proved to be correct, making due allowance for the leap-years; and when asked, supposing a railway waggon to be 12 feet long, 7 feet 4 inches broad, and 3 feet 2 inches

high, how many barley-corns would it hold, reckoning twenty-seven to the cubic inch? the correct answer was given in thirty-one seconds, and without any other explanation than that it came into his head.

There is the historical idiot, who, on the slightest hint or cue being supplied, recounts in measured tone and terms, but incapable of going beyond his part and disturbed by any attempt to induce divergence, anecdotes or passages connected with *every* part of English history, with the Reformation, the Norman Conquest, the reign of Alfred—who follows the inquirer in order to exhaust his store, and is much annoyed if interrupted before he reaches such climaxes, as "the punishment then for killing a fat buck was as great as for killing a king now," and "if chroniclers are to be trusted the herdsman's wife boxed the king's ears soundly for neglecting the loaves." Without reference to clock or watch, in the dark, without any clear conception of the object or meaning of the divisions of time, certain idiots can state almost the precise hour or minute—manifestations which justify the belief in original innate capacities, or in mental processes depending upon what to us appear inadequate causes.

Idiocy, then, may be described as the non-development of all or a portion of the psychical powers, from vicious or abnormal conditions of the organization, or from other causes unknown. It does not, perhaps, in any case, amount to total privation. All or many of the faculties are found rudimentary, existing, but feeble and acting independently.

In order to impart a more specific, though still merely approximate character to such a delineation, a portion of this island, containing 3,360,018 inhabitants and an idiotic population of 2600, was carefully examined by a psychologist, aided by medical experts, when, of 2236 of the latter visited in their own homes, 1341 were found to be idiots and 895 imbeciles. Of this number 1220 were males, 1016 were females, 96 only were in affluent, 469 in comfortable, 420 in indigent, circumstances, while the remaining 1260 were depending upon public charity. It seems fair to conclude that of these 1700 are so situate as not merely to be a burden to their natural guardians, and a pernicious and depressing influence to their family, but to be debarred from all means by which their infirmity may be mitigated. It is true that in an active temperament training is intuitive, that spontaneous imitation and the stern realities of poverty supply a rude education, and accordingly it was found that, of the cases examined, 81 could discharge some household work, that 134 could go one or more messages with various degrees of exactitude, that 18 were entrusted with the care of children, but

only 8 with the more important office of taking charge of horses; that 43 could carry water, 11 could kindle the fire, 33 could knit, and 220 could engage in some description of field or farm labour; but of all these 23 only were able to occupy themselves so efficiently and so regularly as to secure a livelihood. No one can forget one of those scenes in 'Waverley' which, as a series, form an exquisite psychological exposition and a commentary upon this precarious mode of subsistence:—"'I dare say ye never ken'd that a' the eggs that were sae weel roasted at supper in the Ha house were turned by our Davie? (Gellatly). There is no the like o' him ony gate for powtering wit his fingers among the het peat ashes and roasting eggs.' While she spoke he lay with his nose almost in the fire, muzzling among the ashes, kicking his heels, mumbling to himself, turning the eggs as they lay on the hot embers, as if to confute the proverb—there goes reason to the roasting of eggs."

But while these willing white slaves are found useful hewers of wood and drawers of water in connection with the farm, the Ha house, and the people to whom they may be said to belong, they could not generally be industrious and useful were these connections destroyed—were they placed upon another farm, in another Ha house, and in new circumstances.

Besides the mere mental aptitude to acquire and exercise trades and handicrafts, it must be remembered that in many members of this class there are physical impediments, organic diseases, which directly interfere with usefulness and exertion. Thus, of the 2336 there were found 93 with heads preternaturally small, 35 with heads preternaturally large, 48 labouring under deformity of frame, 32 were positive dwarfs, 17 were choreaics, 207 were epileptic, and 99 were paralytic; making a total of 631 individuals disqualified by early or existing diseases, or conditions, for domestic training or for applying it when received.

But, directing the attention to another aspect of the state of these starvelings, and where no such grave lesion can be traced, there are discovered 13 altogether insensible to pain, 31 who are blind, 44 who are deaf, 249 who are mute, 29 who cannot stand, 22 who cannot sit, 101 who cannot walk upright, although some of them contrive to move on all-fours, 103 who cannot feed themselves, 315 who cannot master a button or a tie or dress themselves, and 7 who resist all attempts to clothe them.

There is still a more painful phase to be contemplated; 162 had no knowledge of religious truth, while to the vast majority no test as to this could be applied. 64 were erotic, 60 were drunken, 37 destructive, 271 were of dirty and degraded habits, from simple inattention to the calls of nature down to devouring

foul and feculent matter, 86 had no knowledge of time, 143 had no knowledge of the value of money; and while some 581 had been subjected to some kind of training, in almost every case that pursued towards healthy children, and had been cast aside and abandoned as *ineducable,* 6 only had been so influenced by this process as to deserve in any sense the designation "educated." In one of these the result demonstrates how much and yet how little had been done by education. He was 18, the idiocy congenital; he had a brother and sister in the same condition. The head was of very small size, features goose-shaped; he was blind, vacillated in walking, spoke indistinctly, and invariably repeated the last word of any question addressed to him, and was a pupil in a school for idiots and a blind asylum; there, under tutors, was conceived to have made great progress in religious knowledge and geography. This was accomplished by increased training of perception and memory. He could repeat twelve psalms, twenty paraphrases, and an immense number of isolated facts in geography. But if interrogatories necessary to elicit this knowledge were varied in order or manner, he became confused and unable to answer. If asked the capital of France he would answer confidently "Paris on the Seine;" but if further questioned where is and what is the Seine, he hesitated and was at fault. In intelligence and all other respects he remained an idiot.

In exploring the course taken in emancipating these exiles from their bonds, solitude, and degeneration, it will be discovered as one of the curiosities of literature that the first step and impulse in the education of the imbecile originated with the philosophers, or with medical men acting as philosophers, and not with the physicians. They desired to solve a problem rather than relieve humanity, to decide a controversy rather than to eradicate an evil. The dispute as to innate ideas is perhaps coeval with thought. The opposing parties represent different forms or tendencies of the human mind. The discussion has been, and is now, conducted with various degrees of energy and interest; but periods have occurred when grand crises or triumphs were anticipated. The beginning of the last century was one of these; at that time there was captured in the woods of Hanover a naked hairy boy about twelve years old, supposed to have been a deaf-mute abandoned by his parents, who uttered no sounds, walked on all-fours, or climbed trees like a squirrel, and fed upon bark, grass, and vegetables; smelt such objects when offered to him, striking his beard when they were grateful to his palate. He was at first harmless and obedient, but resisted the shelter of roofs, clothes, and shoes. Before this genuine unsophisticated man

could be corrupted, or his primitive mental condition disturbed by civilisation, he was brought to this country by order of George I, and entrusted to Dr. Arbuthnot, that the mind might be explored, analysed, and reduced to its elements by such means as then existed, and thus the great problem worked out. The experiment failed lamentably in every way. The philosopher was frustrated in his search for the confirmation or refutation of an *à priori* philosophy. Peter the wild boy proved to be an ineducable idiot, who could not be taught to perform the simplest manual act without superintendence, nor to articulate above a few syllables such as "rio sho" for King George, and "qui ca" for Queen Charlotte, and whose education was concluded by his contracting a very decided taste for brandy. His advent, however, and the investigation to which it gave rise, may have directed the attention of the humane to the class of which he was an unfavorable specimen; and was certainly regarded by the wise and learned, such as Lord Monboddo, as of greater importance than if the astronomers had revealed "30,000 new stars to our knowledge."

At a more recent era, almost within our recollection, when the daystar of science was most vainly believed to have dispelled the darkness brooding over the world, and when the reveries of Rousseau had assumed the dignity of a creed and his followers constituted a school of philosophy, the Sauvage d'Aveyron was hailed as the exponent of the perfect and unsophisticated man; and the observation of his mental condition and his education were accepted as the mission of one celebrated man who participated in these views. Itard is said to have devoted himself to this task for seven long years, and to have exhausted all the originality and ingenuity and zeal which he undoubtedly possessed in building up the civilised or artificial upon the base of the savage, or pure man, and in the application of his axiom that "the senses are the soul." For, according to Esquirol, "none that have come after have ever surpassed him in miracles of patience and ingenuity, in artifices brought to bear upon continually recurring obstacles, in the separate training of every separate sense." No such magnificent results as were hoped for followed, but the attempt was not altogether fruitless. A few impressions were communicated to Victor, and certain expressions of tenderness and gratitude elicited; and, although the opinion of Pinel, biassed probably by his incredulity as to the educability of the class, that he was an idiot, was fully confirmed, he was immured for life in an asylum. In short, the propriety of assimilating much of the training of the weak-minded to that employed in the case of deaf-mutes was tardily recognised.

In 1815 the spontaneous improvement of certain idiots in Salpêtrière attracted notice, but only in the same way that rare or anomalous cases do in an hospital; or, if it did suggest the hope that amelioration was practicable, it did not lead to any measures for the relief of the sufferers, nor even to the supposition that medicine and art might place numbers of outcasts upon the outskirts of the pale of humanity.

Dr. Poole, of Edinburgh, in 1819, formulating what had been but a dream and a desire in the heart of the benevolent, appealed at once to the judgment and the conscience of his countrymen, in advocating a scheme for the enlightenment and enfranchisement of the imbecile. Although the seed thus sown seemed to have fallen upon the rock, it grew and bore abundant fruit in England and the United States; Guggenbuhl, in Switzerland; Saegert, in Germany; Seguin, in France; Wilbur, in America, have all recorded success, though in very different degrees, in elevating the condition of the weak-minded; but our object is less to narrate the history of the movement than to describe the principles upon which it proceeded and the results which it accomplished.

All education must proceed upon some assumption that faculties *pre-exist*, even when it is held that all ideas are received from without. There is presupposed a receptivity within, an activity which is to be roused, a volition to be brought into operation. The only method by which this can be accomplished consists mainly in meeting and mitigating bodily disease, and it may undoubtedly be detected in a large proportion of cases; by change of climate, as in the case of the removal of cretins to the Abendberg under Guggenbuhl; by the use of cod-liver oil, tonics, nutritious diet in the strumous; by means, if we possess such, tending to produce or repair nervous matter, and by the continued employment of all physical appliances calculated to give strength and precision to the muscular system; and, lastly, by stimulating the external senses, and through them the nerve masses by powerful and frequently repeated excitation. Powerful impressions must rouse attention. Pain excited a child supposed to be dumb to say, "Why do I suffer thus?" Anger has effected a similar miracle. A few words written on a slate by a mute were effaced. His indignation found vent in the exclamation "Who rubbed it out?" It has been observed that the stimulus, whether physical or psychical, whether galvanism or gratitude, appears to act upon passive or dormant portions of the brain, and thus indirectly, perhaps, upon the organs of nutrition, so that the general health improves under tuition, even though nothing be taught. But it is likewise noteworthy that, according to the experience of many teachers, especially

of Duncan and Millard, true insanity has burst forth during the same process, either as a direct consequence of the excitement, or because symptoms having the same origin as the mental deficiency become developed under this agent, and still more frequently that great acuteness of the intellect may be produced where no knowledge has been communicated. But the more obvious mode of proceeding is by long-continued and frequently repeated impressions, by observing and taking advantage of aptitudes and peculiarities, even foibles, by repressing tendencies which overshadow and obstruct the ordinary operations of mind, by enlisting one sense to help or correct another, by multiplying impressions of a pleasing and suggestive kind, by constant contact with healthy, and strong, and governing, and loving minds, and by engaging that disposition to imitate with which they are so generally endowed. Even microcephalism, hemiplegia, chorea, and epilepsy are said to yield to this course. In considering works upon this subject every one must be convinced that the training of every *individual* idiot would require the time and exhaust the resources and zeal of a special preceptor. That, as in the case of the deaf-mute, it is the true mode of proceeding is certain. It is believed that the success of Saegert, in Prussia, chiefly depended upon the prosecution of a system approximating to this model, in so far as the employment of a large number of teachers is concerned. To such a method of individualisation, to the marvellous instrumentality of the sense of touch, and his own enthusiasm may be attributed the triumphs of Pereira, who is said to have imparted not only speech, but the accentuation of his native province to the dumb. Every group of muscles may be said to be specially educated. The hands are guided to an object, closed upon it, fixed to it, made to grasp it, raise it, throw it. Whenever the hand can lay hold of a body it is guided along the bars of the horizontal ladder, in order to bring the muscles of the arm or chest into operation. Self-balance is induced by inclosure in a box reaching above the waist, where the rough contact with the sides suggests at once the presence of an external object, the idea of support, and the instinct of standing erect. The use of the legs may commence in a go-cart, but walking in a straight line must be afterwards rendered possible and practicable by placing the pupil between boards, as in a narrow passage, or by the aid of rails or ropes, or of a belt held in the hand of the guide. With the exception of such expedients, the instruments employed, and the gymnastic exercises of the drill ground of a school for imbeciles, as described by Millard, &c., do but slightly differ from those used elsewhere, except in so far that every movement, whether it be the tossing of a bâton, the ascent of a ladder, or

rotation upon a horizontal pole, is taught gradually, tentatively, cautiously, and with a lively conception that the pupils are in reality infants and not well-grown children. It should be understood that instruction in trades and muscular acts, as in digging, pumping, carpenters' work, basket-making, tailoring, &c., depends exclusively in the application and extension of these primary lessons; and that, although the constructive dexterity of the operator may excite admiration, he is not necessarily higher in mental acquisitions than his associate who passes his hand along the edges of a table, or who enjoys the improving fun and frolic of the shop-play or kindergarten. Muscular exercises should precede all efforts to impart ideas, both because they rouse attention and consolidate the frame. Practical men have found that in proportion to the development of the muscular powers, to the co-ordination and precision of movement, there occur mental growth, a display of volition, a disappearance of brutal passions, unregulated appetites, and vicious habits. Contemporaneously with such training the functions of the nervous system must be addressed, acted upon, and placed in relation to the external world. This may be effected through any of the special senses, but great importance has been attached to obtaining access to consciousness through the eye. One operator, with this object in view, is said to have sat opposite to his pupil for five weeks, and after various manifestations of restlessness, annoyance, curiosity, wonder, he was at last rewarded by fixing the attention, by establishing a connection with the consciousness, and obtaining a control over the will of the now emancipated thrall. Similar success has attended the labours of others, and has suggested to the sanguine recollections of Cheselden's patient, the cries of the lunatics liberated by Pinel from the oubliettes of Bicêtre, on beholding the sky after years of darkness, or the hymn of the Cretins on the Abendberg to the rising sun; but, in general, the process is far more tardy and less trustworthy. With a similar object vision is stimulated and taught by placing the child in a darkened room traversed by a ray of light, or in which there is one luminous point, or by guiding the eye to phosphorescent objects on the wall, or by causing brilliant objects to pass rapidly in the line of vision and by marked contrasts of colour and forms. The mind seems brought *out* by what is sent *in*. Touch is excited by alternations of heat and cold, by smooth and rough surfaces; taste and smell by penetrating odours and vapours, such as ammonia, cayenne pepper, &c. The ear is exercised by sounds of various kinds and intensity, of bodies falling and breaking, the contact of sonorous bodies, the difference of musical notes, the voice as expressive of joy, fear, pain, and by directing the atten-

tion to the causes to which their difference is referable. Taking advantage of the natural or acquired power of imitation in order to induce the employment of the organs of speech, it is necessary first to direct attention to the mouth by touching the lips, teeth, tongue; by placing a hard object between the lips or jaws; by suspending weights to the jaws, &c., to stimulate the tongue to motion; and, secondly, when the pupil knows that he is possessed of such organs, to direct his attention to the different acts of articulation as presented on the face of the teacher. Where speech is attempted, words are taught as they can be associated with objects already familiar; where this is not the case, the principle followed in the development of the voice is to observe the natural course in infancy and to pursue it. Thus children begin with sounds like pa, na, bo—not with a vowel, but with a labial consonant followed by a vowel, and they love to repeat the syllable. Such sounds, as they are much easier than vowels alone, or than consonants preceded by a vowel, should form the first lessons. Even this plan must be modified according to circumstances, as some pupils cannot make a long and pure vocal sound, and must be taught by imitation to do this before they can articulate, and in some whose lips do not contract it may be necessary to begin with linguals before labials, as the difficulty of the latter may discourage them. A case may be given as showing the method better than mere rules. A hydrocephalic child with a high deeply grooved palate talked constantly but could not articulate. The letters which he attempted to pronounce were "N" as in "not," which he repeated many thousand times in a day, M as in ma, and P simply. He could remember and sing tunes. As he could pronounce labials he was first made to practise *them* to distinguish Pa from Po, and Ma from Me. From P and M he was led to B through Bo and Bu. V and F were next attained by placing his under lip beneath his upper incisor teeth, and this led to the sibilants S, Ch, N, J. By elevating the tongue anteriorly La was pronounced, and next the labials T and D, and thence to the gutturals. He completely acquired speech.

Modifications of this plan have been proposed and pursued, but they are closely allied both in principle and results with the course now described.

The attention is attracted and the perceptive faculties are cultivated by lessons in objects; form and size are taught by presenting blocks of different sizes and shapes which the pupil is required to insert into corresponding cavities in a board. Thus we are told that in teaching such pupils to draw a figure so simple as a square, they could not be brought, except at the

expense of countless artifices, to see that, in order to succeed, the four lines must be made long enough to meet each other at each corner. Colour is taught by wooden figures of the same size and form, but of different hues. Number can only be imparted by placing repeatedly before the eye visible and tangible processes of addition and diminution by means of blocks bearing numbers corresponding to those written on the board before the pupil.

It would be foreign to our present undertaking to dilate upon the obstacles to the fruition of all such efforts by the complication of actual insanity, by that effacement of previous impressions which occur if instruction be suspended or even if it be changed in nature; nor would it avail to describe the extreme slowness and imperfect success which characterises the scholastic teaching which must follow the measures enumerated, or the difficulties which attend the germination and growth of the moral nature, the correction of vicious propensities and the engendering and elevation of the higher sentiments. It is probable, however, that the most striking effects in this struggle with ignorance and stolidity and corruption have been achieved in the training of the emotions, a training which consists, as in the sane and sound, in influencing the mind through the perception by examples of love, affection, benevolence, probity, and under this genial and humanising process dispositions are changed, new impulses imparted, and even to such an extent that "learning to love his brother whom he hath seen" he attains to some knowledge of a love for "God whom he hath not seen."

This brief sketch of the vast and protracted and systematic arrangements which are required in the professed education of the undeveloped intelligence may naturally suggest an inquiry as to the benefits conferred. Notwithstanding the benevolent interest extended towards those of weak mind, the more correct views entertained of the diseases under which they labour, and the acknowledged results which have followed exertions on their behalf, considerable doubt and uncertainty as to the state and stage of the enterprise have arisen. The wish was father of the thought. It was hoped and therefore believed that perfect and mature beings could be created out of the rude or ruined materials, the positively undeveloped or diseased organizations. It has been suspected that these hopes were founded upon the effects recorded of the labours expended by the philanthropists originally engaged in the cause upon the higher order of imbeciles, or the lower order of rational men. Before measuring the actual benefits realised it may be well to contrast the opinions and anticipations of the men of experience or wisdom who despaired with those who hoped, in one sense, "not wisely but

too well." On the one side we find Luther declaring that "an idiot boy might be drowned without sin, as he had no soul," and Esquirol announcing the frightful prognosis "Enfin on ne guérit point d'idiotie," an aphorism confirmed in degree at least by Guggenbuhl, who confessed, after trial and failure, the genuine non-cretinoid idiot to be hopeless. On the other side, in 1860, Dr. Parrish, after a sad series of assurances that should it be expected to endow imbeciles with the capacities of poets, philosophers, statesmen, merchants, or inventive artisans "it will not pay," bursts forth into the following emphatic and almost epigrammatic vaticinations:—"But if it be that you are to make them happy where they were sad, then you increase the sum of human happiness, and it *will* pay.

"If it be that, where they were solitary and idle, you bring them forward to be social and industrious, then you add to the sum of domestic comfort and productive toil, and it *will* pay.

"If it be that, where they were profane and vulgar, you lead them into the sunlight of moral purity, and teach them to fear God and keep His commandments, then you increase their sense of moral obligation, and remove a plague-spot from the family altar, and it *will* pay.

"If it be that, where they could not love even those who loved them most, you can hold them against your warm heart till their coldness melts away, and they are all aglow with affection for parents, home, and friends, you widen the sphere of human sympathy and the scope of human kindness, and it *will* pay.

"If it be that, where they could not speak, you may be instrumental in arousing the slumbering power to utterance, it *will surely* pay. And if it be that your own ears may hear the once silent tongue eloquent with the out-gushings of a liberated spirit, as it pours forth its praises to its deliverer, then it *will more* than pay. All this has been accomplished."[1]

Again, Dr. de Vitré expressed his conviction at the foundation of the Albert Idiot Asylum "that at least ten per cent. can be restored to society as useful members of it; there are only six per cent. of the whole family of idiots in this country who are incapable of improvement, one half of the remainder can be so far benefited by training as to be made able to attend to their own wants with some degree of propriety."

The fulfilment or reconciliation of these adverse expectations will best be gathered from the experience of the practical workers and observers in the field to which they refer, although it may be difficult or impossible to arrive at precise conclusions from the data which they have collected when the diversity of

[1] 'Seventh Annual Report of the Pennsylvania Training School for Feeble-minded Children,' 1860, p. 9.

the mental capacities, physical condition, and social circumstances of the individuals whose education has been attempted are considered. Much importance was deservedly attached to the wonders supposed to be wrought in the school at Bicêtre, and a decided impulse was imparted to the scheme in operation or contemplation for the amelioration of the misfortunes of the imbecile, by a popular interpretation of these wonders in this country. Yet it is remarkable that, on an analysis of the cases, twelve in number, introduced by Séguin in his work and intended alike to illustrate his method and decorate his triumph, *three* are not idiotic; that in *three* no curative measures were attempted, in *one* they were interrupted, in *two* success was partial, and in *three* only could it be or is it by the reporter designated as complete. The first is that of an enfant arrière, who, besides medical treatment, receives and responds to the training of ordinary children; the second is a case of dementia induced by masturbation, which is treated and cured by medical means; the third is a backward child who is left in the same condition; in the fourth, in which idiocy is combined with chorea and rachitis, a capacity to climb, speak, and count are imparted; the fifth and sixth are epileptic idiots, cases which, contrary to all preconceived opinion, are found to be as susceptible of improvement as those of healthy structure. The success in one of these cases was such that a puny, staggering, stuttering, rickety child acquired entire control over the muscular system, performed the most complicated movements, spoke and deported herself so as to *seem* what perhaps she was not—the admired of all admirers in a ball-room, where her teacher met her in after-life. Some years subsequent to this report, M. Parchappe, Inspector of Asylums in France, stated that Bicêtre contained 130 children, of whom a few only had passed the age of eighteen. All were attacked with idiocy in various degrees. In 83 cases the idiocy was simple; in 45 it was complicated with epilepsy. Of the total number, 40 were taught shoemaking and joinery; the remainder were engaged in household or agricultural work. The shoemaker's shop was attended by twenty inmates, who produced 1034 pairs of shoes; that of the joiners was attended by an equal number, and yielded, from works of various kinds, 4000 francs. The physical, moral and intellectual education was not neglected. All shared in different degrees in a simultaneous instruction in reading, arithmetic, writing, grammar. Dancing, fencing, drawing, were objects of special cultivation. Thirty discharges took place during the year. Family motives led to a large proportion of these, and fifteen had but commenced their education, having spent less than a year in the establishment. Of fifteen who had been

subjected for several years to systematic training, ten had experienced a radical amelioration in their condition, and had so profited as to be able to engage in a trade, and "to maintain themselves by their own labour." Subsequent investigations by a medical expert failed to trace out these instances of development or restoration, or to determine whether and how far the means of self-support had been acquired.

Manual operation and mechanical dexterity are very naturally, but most fallaciously, accepted as tests or indices of general progress; and proficiency in an art, school exercise, or in memory, is held to be equivalent to expansion of mind and to maturity of volition and spontaneity. For example, four trained imbeciles, all of considerable muscular strength, and regarded as fair examples of what long-continued culture may effect, were recently watched when engaged in chopping straw in a farmyard. Two stood smiling, leaning on a wall, apparently looking on, but required, upon all occasions, to be reminded when it became their duty to relieve their companions; and although the attendant ceased to feed the machine, by supplying straw, they continued to turn the wheels as lustily and rapidly as if results were following their exertions, and apparently unconscious or thoughtless of the object for which their strength was put forth. Such observations strengthen the suspicion that has arisen that even educated idiots must remain parasites. They require some will in addition to their own to guide and govern them; some impetus, some impulse, some one to depend upon, to hold by, to hang upon. Although Dr. Parrish's sanguine foreshadowings have scarcely been realised, the accounts from America, where there are now nine idiot schools for upwards of 1000 scholars, are most cheering, although it is very observable that the brilliancy of the results fades as we approach the present day. Of about twenty reports, including those from Massachusetts, Pennsylvania, New York, we may summarise the totals as—1. Though improved, they are still idiots, but human idiots. 2. Their distinguishing acquisitions are industry, usefulness. 3. Mechanics and agriculture are the best means of education, thus trained to take care of themselves and be useful to others, and girls to be serviceable in household duties. 4. Improvement in all in regulation of passions, in capacity for useful employments. 5. Improvement in backward children, improvement of evil habits in ill-disposed children, and to such a degree as to enable them to be placed under discipline in public schools. 6. "True it is that the school acquirements of our pupils are very moderate at the best. True it is that the industry of our boys upon the farm is but moderately productive. True it is that the manufactured articles of our workshop indicate but

a moderate degree of skill in handicraft; and yet, comparing even these results with the stupidity and ignorance, the utter incapacity and unwillingness for work, from which they have been developed, may they not be regarded as a clear success?" Even this very modest estimate must be qualified by the consideration that deformed, diseased, epileptic, insane, and depraved children are excluded from participation in the advantages of such establishments, upon similar grounds to those which regulate the selection of entrants in this country. 7. In consequence of the accumulation of incurable or intractable cases an asylum for chronic idiots is proposed. Pupils improvable, and displaying growth in the right direction, as is evidenced in digging, sawing, and splitting wood, domestic drudgery, conveyance of messages, &c. 8. Work prescribed, as training, attended with profit. Doubts cast on the educability of pure idiots. 9. Certain of discharged pupils "almost self-supporting," Pupils become expert tailors under supervision. 10. Improvement in bodily condition. To conclude this retrospect, the hopes of these reporters invariably transcend the facts; the future is brighter than the present.

In Great Britain the results in the institutions to the annals of which we can obtain access are more precise and somewhat more encouraging. Duncan and Millard, in 1866, gave brief narratives of forty-one cases which had been under their care for years, of the most profound as well as the least degraded species of idiotism, which we shall still further epitomise. These children, besides mental defect, laboured under, in some cases, epilepsy, in others mutism, &c. In one instance retrogradation took place; in 7 no amelioration was observed—several of these suffered from phthisis; and in 1 the deprivation was so intense that the food had to be placed in the mouth; in 5 symptoms of alienation interfered with treatment; in 1 imitation, memory, and reasoning were so highly developed as to render the pupil of the highest rank of imbeciles; in 4 the effects of training consisted of improved docility, quietude, self-control, courage; in 1 a few additional words were added to language; in 25 distinct improvement was traced, although in very different degrees and of very different kinds. For example, 1 was enabled to sit, 1 to stand alone, 1 to restrain cries; others manifested elevation and aptitude by laundry work, making beds, sewing, pumping, gardening, field labour, mat-making, strengthening of powers, helping companions, improvement of moral or religious tone and of temper, increased cleanliness and powers of observation, and recognition of property in a toy, the curing of hysteria and malignity, by becoming less wild or troublesome, quicker, and, on the whole, better behaved. Many of these acquisitions could only be brought into play and sustained under supervision.

A gardener whose work was worth eight shillings a week could not spend his gains; and it should be noted that, contemporaneously with acquired intelligence and acuteness, there appeared sometimes, though rarely, deteriorations, such as antipathies, pilfering propensities, &c., indications that the vicious as well as the virtuous and higher attributes of human nature may occasionally be educed under the system of management pursued.

In the magnificent establishment at Earlswood, containing, 1875, 594 pupils, where every known means of development—medical, moral, physical, educational—are profusely and persistently applied, the advancement may be regarded as commensurate with the exertion and self-devotion of the staff and with the ample machinery at their disposal. On examining the annual retrospects by Dr. Grabham, which may be designated the bulletins of the mental health of the community, for the last five years, and which are minute and interesting, though less comprehensive and utilisable for statistical purposes than might be desired, it would appear from a table furnished by the schoolmaster that the results in 1875, in his department, were that 38 had much improved, 75 had improved a little, 72 had not much changed, 3 appeared to have retrograded, and 14 had been at school but a short time; and there is added the pertinent remark that there is much, such as the instruction of a blind idiot by raised letters, the writing of 474 letters, 195 without dictation or direction, and the engraftment of habits of good temper, cleanliness, love of order, obedience, &c. &c., which cannot be reduced to a tabular form. In these documents, five of which we have examined, besides tables of the gross number of individuals who speak, read, write, sew, distinguish colours, &c., there are presented specimen illustrations of the amount of improvement obtained in 36 pupils, which we shall endeavour to analyse. It should be kept in mind that this amount is sometimes wholly the product of training in the school; sometimes it has been superadded to original faculties and previous though imperfect instruction; and, lastly, that many of the acquirements can be exercised only under superintendence and assistance, and that in every case there has been pronounced amelioration in the moral condition. The actual additions to mental power are represented by articulation from a few letters up to words of all sorts; the use of language in a simple form; reading, from single letters and words by spelling, up to reading New Testament and simple stories with pleasure; writing, from strokes and single letters and words on slates up to the composition of infantile correspondence; counting, from 1 or 2 figures up to 150, and learning multiplication table; calcula-

tion, from addition up to multiplication and division; needlework, from hemming by hand or machine up to fancy work; knowledge of Scripture history, drawing, vocal music; of coins from 3 upwards, of colours from 7 upwards, of weights from 2 upwards, of time by clock; affording assistance in dressing, teaching, and guiding companions, household work and trades; greater breadth of intelligence and information, and greater powers to converse. Hopes were entertained by their guardians that four of the pupils whose biographies have been here condensed and classified might eventually become self-supporting; and a fifth, who was about to return home, cherished the belief that she would be able to support her grandmother. Two of the 36 were paralytics, but their progress does not seem to have been retarded by their infirmity. Similar pictures have been drawn of what has been accomplished since the opening of the Albert School, to which are appended the following humble but hopeful comments: "Of these, 11 have improved to a very notable extent, most of these being well advanced in practical industrial employments: 7 others have made considerable progress at school; and the remaining 22 (some of them very young cases) have, without exception, improved more or less in habits and in general conduct."

From Ireland we have failed to procure reliable information as to the number of weak-minded persons under tuition, probably because the schools appropriated to this purpose have not been placed under the jurisdiction of the Inspectors in lunacy; but we learn from an able paper by Dr. Hack Tuke, in the October number of the 'Journal of Mental Science,' that very ample provisions have for years been in operation for employing education in the cure and alleviation of mental disease and decrepitude, under the direction of the physician, Dr. Lalor, in the Richmond Lunatic Asylum, Dublin. This vast institution sheltered (Decr., 1874) 1039 patients, of whom 496 were prescribed regular and systematic courses of instruction and discipline in reading, writing, dictation, grammar, geography, arithmetic, mental arithmetic, drawing, object lessons, marching, music, &c. A certain proportion of the large number thus subjected were, of course, persons of perverted but not necessarily enfeebled minds, and it might be difficult to separate the effects of knowledge as a remedy from the effects of knowledge as a stimulus. The author concludes his highly laudatory narrative as follows: "It seems to me, indeed, impossible that the occupation and diversion of the mind which a school (including music, singing, &c.) provides, can be other than beneficial. The immediate effect in causing actual recovery may not be apparent, and Dr. Lalor did not pretend that such was the case, but I was

informed that an excited patient not unfrequently becomes tranquil after being brought into the class."

The most recent and important supplement from the United States is that incorporated with the last report of the New York Asylum for Idiots, 1874-75, by Dr. Wilbur, the most eminent, moderate, and modest exponent of his own practice among his American colleagues and collaborateurs; in which, after wise and cautious revelations of benefits conferred, he says: "We do not propose to create or supply faculties absolutely wanting, nor to bring all grades of idiocy to the same standard of development or discipline, nor to make them all capable of sustaining creditably all the relations of a social and moral life, but rather to give to dormant faculties the greatest practicable development, and to apply those awakened faculties to some useful purpose under the control of an aroused and disciplined will" (p. 20). In conclusion, it is obvious that, while ample evidence is accessible of the educability, within certain limits, of various classes of the weak-minded, the indiscriminate application of the recognised means of producing improvement to all the different groups and gradations of the class, and without distinction as to the abilities of its members, though widely separated by physical organisation, constitution, and primary capability; between those who were undeveloped because they had received no culture, and those who were incapable of receiving the most judicious and long maintained culture; that much misapprehension has arisen as to the whole matter; that much disappointment and discouragement have been encountered at various stages of the struggle against the difficulties known to exist. The purpose of these remarks cannot be misconstrued. It cannot be supposed that they point to the neglect or imperfect performance of a national duty. If any inference can be drawn from them it is that, while they record partial success in the cultivation of a formerly barren field, they suggest a new means of national usefulness and distinction, wide and deep channels for that overflowing wealth and overflowing charity which appear ever ready to meet legitimate calls. We know the country to be prosperous, contented, healthily disposed beyond precedent; we know that what were the privileges and luxuries of the rich a hundred years ago are now the necessaries of the poor; that knowledge is accessible to all, almost forced upon the indifferent and reluctant, that the mind of the community is quickened and stimulated and moved by enlightened and generous impulses, and yet there is amongst us a race in bondage and darkness and degradation, who scarcely realise their own existence, who scarcely hear the footfall of that civilisation which rushes past them and leaves them the waifs and strays of man-

kind; yet who are capable of emancipation and partial enlightenment, who have a right to our sympathy equal to that of the suffering, the blind, the dumb, the heathen.

VI.—Present Aspects of Lunacy.

Are lunatics on the increase? How can we best provide for them? Do asylums serve the purpose contemplated by their establishment? Must we continue to build and to enlarge asylums in order to meet the demands made for the accommodation, supervision, and safe custody of the insane? Such are some of the leading questions relative to the public provision for the insane which yearly, and indeed almost daily, press for a satisfactory reply. No such reply has yet been made to any one of them. Doctors differ and wrangle over them; sociologists touch them daintily and with hesitancy, and politicians grope in the dark after a practical solution because they have no established principles of action to guide them.

Lunacy is at the same time a complex and a variable condition. It has both its legal and its medical aspects, and with regard to these no harmony obtains between the lawyers and the doctors. The determination of its existence cannot always be settled satisfactorily; and uncertainty prevails with regard to the degree of responsibility to be associated with its presence. The forfeiture or the suspension of civil rights is another proof for debate in insane cases; as is likewise the extent and degree to which seclusion and coercion shall be carried. Viewed, again, as a departure from health, insanity is seen to offer the greatest variety of forms; and opinions differ widely as to the best means of dealing with many of them. Nor is there concurrence of views as to the time at which a change of management or treatment is admissible, nor as to the character of the change desirable. And, lastly, with regard to the social position of the patients and the kind of provision to be made for them in accordance with it, no definite course has yet been determined upon, either by the medical profession or by the public and their representatives.

In each division of the United Kingdom is to be found a Board of commissioners or inspectors entrusted with the general oversight of the insane and of asylums, in the presumed interests of the former. For many years these commissioners have exercised their functions, and in the lapse of time have considerably enlarged and strengthened them. They have issued yearly reports replete with information; have proposed certain im-

provements in asylum administration, and have both directly and indirectly led to an amelioration in the condition of lunatics. They have also instructed the public mind with regard to the general requirements of the insane. But, in our opinion, after due recognition of their good deeds, they have not grappled with the great questions touching the provision to be made for them, in view of their increasing numbers and their varied wants, and in harmony with the progress of opinion and experience.

It is nevertheless to these important officials that Parliament and the public naturally look for guidance. Amid the dissonant opinions prevailing some certain note, some distinct course of action, some definite expression of opinion with regard to the great questions referred to in the first paragraph of this article is looked for. Unfortunately the oracles return no certain sound to our questionings. To some of them they have hitherto turned a deaf ear. With regard to others, they have, with all the resources they possess for inquiry, contributed little information to guide us in our search for a solution. Yet withal they have been assiduous in certain ways; they have annually compiled multitudes of tables of figures in which statisticians may revel; have written scores of pages upon asylum minutiæ of structure and management, and have devoted as many to praise or to chide asylum officers. All such literary contributions may be well enough in their way; but they have unduly preoccupied time, during which great social questions regarding the insane have either sprung up anew around them, or have taken on new growth and new forms, unheeded or little considered.

These remarks are not equally applicable to each of the three lunacy boards. On the part of the Scottish Board a wider and more adequate conception of its position and functions is always to be noted in its reports; and on that of the Irish inspectors there would seem a peculiar shyness—not national, certainly—in calling public attention to their doings; for they take no pains to secure the notice of their returns; and it is quite clear that the State provision for the insane in Ireland and their supervision is no Irish grievance. Taking the material placed at our disposal, we shall endeavour to extract the most important facts furnished by it.

The question of the actual and relative number of the insane is one that always addresses itself to the public curiosity and concerns the public interest. It is a melancholy circumstance, and one very discouraging to our philanthropic views respecting the treatment of the insane, to see and to hear of the yearly enlargement of old asylums and the erection of new ones; to find that all this activity in building does not overtake the demands for

accommodation, and that each year adds considerably to the relative proportion of our insane population.

These circumstances and facts have been brought to our attention year after year without interruption. The last published lunacy blue-books, now before us, furnish simply a repetition. In so doing they also supply, in our estimation, proof that the existing system of dealing with lunatics is a failure, and that it is a vain attempt to uphold and extend it. However, to turn to figures. In England, on the 1st of January, 1875, the Commissioners testify to the existence of 63,793 lunatics under their cognizance, and to that of 219 others found lunatic "by inquisition, who reside in charge of their committees."

For some not obvious reason the Scottish Commissioners are a year in arrear of their English colleagues in their official returns, which are brought down only to January 1st, 1874. At that date there were 8069 recognised mentally unsound persons under the jurisdiction of the board; and taking the annual average increase in their number, deducible from the tables, to be 100, we may calculate their number to have amounted on the 1st of January, 1875, to 8169. Consequently the known lunatics of Great Britain constituted one year since an army of 72,000—a terrible legion of dwarfed and thwarted minds; a miserable residuum of modern civilised society, precipitated as a result of its vanities, its luxuries, its vices, and its ignorance!

But, alas! these figures do not convey the whole truth. Rightly enough the Scottish officials refer to a class of "unreported insane," which they estimated in 1858 as amounting, in Scotland alone, to 2000, "of whom the larger proportion belonged to the classes little removed from pauperism."

The English Board attempts no estimate of these "unreported insane." It has not the full and accurate information respecting the state of insanity in England, gathered after a minute research throughout the country, such as is possessed by the Scottish Board. Yet if we accept the estimate of the Scottish Commissioners, and apply it to England, we shall in proportion to the population of the two countries—7 to 1—have to add to the tale of English lunatics as many as 14,000 more than presented by the official returns, making in round numbers 78,000 insane persons in England and 10,000 in Scotland.

This estimate, so far as England is concerned, must be reckoned as below the mark; for it is to be remembered that the ratio of the recognised insane is as 26 in England to 22 in Scotland in every 10,000 of the population, whence it would be fair to calculate on a higher proportion also of unreported insane

in the former kingdom, and to put the lunacy of Great Britain at 90,000 individuals.

So far as concerns the officially known insane the increase is pre-eminently remarked in pauper lunacy. There is an ever increasing number of pauper insane in asylums, pressing on the accommodation yearly added, and, at the same time, there is an increase of lunatic paupers resident in workhouses. Thus of the 63,793 lunatics enumerated by the English Commissioners, 56,403 are paupers.

"In the course of the last seventeen years . . . the insane paupers have increased from 16·14 in 1859, to 23·25 in 1875 per 10,000 of the whole population; whereas in the same period the private patients have merely increased from 2·53 to 3·09 per 10,000 of the population.

"The relative increase, however, of the population, and of the two classes of the insane under care in the seventeen years past is to be expressed as follows:

"The total population has increased from 1859 to 1875, 21·63 per cent. The private patients *under care* have increased in number, as compared with 1859, 48·39 per cent; and among the pauper patients in the same interval the increase of those *under care* has been 77·47 per cent."

During the same period, and especially during the last five years, the ratio of paupers to the population has considerably declined. Whence it is that the proportion of pauper lunatics to the pauper population of the kingdom has attained the high figures mentioned.

The Scotch report bears like evidence. From Table VI, exhibiting the proportion of paupers to population, of pauper lunatics to population, and of pauper lunatics to paupers, from 1861 to 1st January, 1874, it appears that, although there is a fall of 22·6 per cent. in the pauperism of the country, "there is a rise of 10·5 per cent. in the proportion of lunatic paupers to the population. . . A still greater rise of 34·2 per cent. has taken place in the proportion of lunatic paupers to ordinary paupers, and this has been yielded by a steadily progressive annual increase."—(p. vi, Report.)

Between the dates in question the proportion of paupers receiving parochial relief on account of lunacy has risen from 6800 per 100,000 in 1861, to 9127 at the beginning of 1874.

The statistics of Ireland appear to tell the same tale. From a memorandum taken from the Irish census of 1871 we find that the proportion of lunatics and idiots to the sane population has doubled in ten years; and that with regard to these afflicted beings lodged in workhouses, the numbers were in 1851, 1623,

in 1861, 1885, and in 1871, 2955, and this great augmentation has occurred contemporaneously with declining pauperism.

The Scottish Commissioners have been at the pains to throw into a tabular form the statistics at their disposal, so as to present the relation of the sexes to insanity. The result that comes out is that, whilst the number of female paupers is 166 per cent. more than that of male paupers, that of female pauper lunatics only exceeds that of male pauper lunatics by 17·9 per cent. But, inasmuch as the number of patients under 15 is so small that it cannot affect the figures, the number of lunatics of the two sexes above 15 is next examined in relation to the general population above that age. From this calculation it appears that males were to females in the population at large as 100 to 118·6, and that the ratio of male to female lunatics was 100 to 116·2. Hence, whilst we get an explanation of the preponderance of female above male patients in asylums, we at the same time perceive that women are not more liable to insanity than men. This further fact also comes out, that in the case of congenital insanity males preponderate, "the excess being greater than would be due to the excess of males over females among very young persons in the general community" (p. xi).

We need not on this occasion repeat the observations made in some previous reviews of the reports of the Lunacy Boards, in explanation of the annual growth of lunacy—a growth out of proportion to the increase of population. Many causes may be appealed to in explanation, and for the purpose of demonstrating that the constantly enlarging numbers do not afford evidence of positive increase. For our part we must confess that we remain unconvinced of the comforting conclusion sought after, and feel assured that there is an increasing development of insanity among our people. The actual ratio of lunatics to the population advances year after year uninterruptedly; and this happens although many of the causes that have been assigned to account for it are progressively decreasing in scope and intensity.

Moreover, the marvellous accession to the ranks of pauper lunatics, as above exhibited, bespeaks, in our estimation, a positive increase of insanity. It may fairly be admitted, that some of this increase is due to the readiness of people to rid themselves of the cost and trouble of their mad relatives by transferring them to asylums, where they are gratuitously treated; that asylum accommodation is everywhere enlarged, is more accessible and more appreciated and sought after, and that the sop recently given to ratepayers, by which the half of their burden is transferred to the Consolidated Fund, has encouraged the transmission of lunatics to public institutions.

But, on the other hand, it must be remembered that the lunatic inmates of workhouses have not undergone any decrease, but to a slight extent the reverse, notwithstanding the multiplication and perpetual enlargement of asylums; that the percentage of those resident with their relatives or others, though it may have undergone a slight decrease as compared with the numbers ten years ago, yet it has remained for the last few years, as well in Scotland as in England, practically stationary. Moreover, the ratio of accumulation of insane in asylums should by this time be somewhat influenced by an increasing percentage of deaths, seeing that the proportion of old lives must be ever advancing. So, again, the attractive influence of asylums, so much insisted upon, must be on the wane; for those institutions are no longer novelties, and are everywhere, at least in England, very accessible. Lastly, we cannot acquiesce in the notion of a large reserve of lunatics, ready at hand to furnish every new asylum and new ward—a notion freely used by those who discredit an increase of insanity; for it is one that would imply a very imperfect inspection of the lunacy of the country on the part of the Commissioners.

These considerations weigh with us in concluding that insanity is actually on the increase, and that it is so to a greater degree among those who are paupers, or who become pauperised by the occurrence of sickness. And if such be the case it becomes a question of no mean importance to solve, viz. what causes are at work among our poorer classes in inducing mental disorder? In replying to this problem a further one will present itself, viz. as to the relative prevalence of the disorder in urban and in rural communities, and the connection discoverable between the development of the malady and the employment pursued. We have not the data before us to attempt the solution of these queries. The Scottish Commissioners, indeed, contribute some of considerable value with regard to the comparative presence of insanity in rural and in urban districts, and in the several counties of Scotland; and something may be gathered from the tables in the English report showing the number of known lunatics in each English county. These returns may assist in arriving at general inferences, but all the necessary ones are wanting to enable the inquirer to seize upon the productive causes of mental disease in the classes in question. Nevertheless, the conclusions of the Scottish Commissioners are well worth referring to.

Thus it appears that whilst Highland counties have, in proportion to population, a much heavier persistent burden of pauper lunacy than manufacturing counties, yet that the latter have a higher proportion of lunatics coming under treatment;

in other words, a higher ratio of fresh cases, representing active and transitory forms of insanity.

To Deputy-Commissioner Dr. Sibbald especial credit is due for the careful investigation of the influence of urban and rural conditions on the production of pauper lunacy. The results are given at length in a special report printed in the Appendix to the Report of the Scottish Commissioners, and we commend their careful study to our readers. The general conclusions at which he arrives are set forth also by the Commissioners themselves, but they are too long to introduce into this article. But we may quote one or two, because they are opposed to preconceived notions largely entertained. For instance, Dr. Sibbald concludes that "the proportion of pauper lunatics to ordinary paupers is in the gross nearly the same in urban and rural districts;" and that "there is no evidence in the statistics of death to justify the conclusion that a much larger proportion of persons suffering from fatal disease become pauper inmates in urban than in rural asylums" (p. xlvii).

It would be both interesting and instructive to seek an explanation of these facts. Dr. Sibbald's observations contribute many particulars in elucidation, but much remains to be done

The practical equality in the numbers of lunatics among paupers in urban and in rural districts is shown, as might be supposed, not to be tantamount to an equal production of insanity in town and country; for the fact is that the number of fresh cases in town, far exceeds that noticed in country districts. Thus, in the principal towns of Scotland, the proportion per 100,000 of the population of registered lunatics, "classified according to parish of residence," is 62; in large towns, 41; and in small towns and rural districts, 35. Thus the ratio of insanity would, so far as this statement goes, appear to be directly proportionate to the size of towns, or the density of population. Such an inference would be a crude one; for density of population is simply an accidental element in the production of insanity. The operative causes are those conditions of life which are especially found in large cities, which are productive of, and associated with degenerescence of the race, coupled with the circumstance of the drifting into the lower masses of town-dwellers of a number of lives vitiated both mentally and bodily. The same group of causes which contribute to the production of pauperism contribute also to that of pauper lunacy.

The causes which serve, notwithstanding the much larger production of insanity in towns, to bring about the equality of chargeable lunatics in urban and rural districts are well examined by Dr. Sibbald. The chief one is the removal of

patients from town to country districts on account of their chargeability to the latter. Another cause is the greater number of recent and curable cases sent from towns into asylums. Again, a reverse set of circumstances tend to increase the ratio of chargeable lunatics in rural districts. Many of those sent from urban areas represent incurable and chronic lunatics; the forms of insanity originating in rural populations belong rather to the chronic and incurable category, and further by emigration an undue proportion of beings decrepit in mind and body is left as a residuum in the rural community. One of the conclusions reached by Dr. Sibbald exhibits the potency of these conditions. It is, that "the percentage of the population who remain chargeable as pauper lunatics at the end of the year is much smaller for urban parishes than the percentage remaining chargeable to rural parishes."

Before quitting the subject of the increase in the number of the insane, we would advert to the table given in the Scottish report (p. xxx), to show the progressive history of the inmates of asylums. Such tables as this are of high value, for they furnish the only real and reliable basis for discovering the rate of accumulation of the insane. Indirectly, moreover, they contribute data towards estimating the value of lunatic lives, and the probabilities of recovery by the lapse of time, as well as towards discovering what is the death-rate in relation to the length of residence. "One of the most interesting features of this table is the large number of patients discharged recovered during the two first years, and the great and sudden diminution of such discharges which occurs in the third year." For whereas in the former period an average of 40 per cent. are so discharged, there is a sudden fall to 16 per cent. among those who have been asylum inmates two and three years. The table also shows that the death-rate diminishes according to length of residence. "It follows from this, that in comparing the death-rates of different asylums, allowance must be made for the varying relation which the numbers admitted bear to the numbers resident,—in other words, we must take into account the proportion of recent to old-standing cases. In parochial asylums, for instance, the movement of the population is much greater than in royal and district asylums, and to this we mainly attribute the higher mortality which occurs in the former."

Thus far we have examined the present aspects of insanity in relation to the absolute number of the insane in Great Britain, to the question of the increase of their number, and to the directions in which that increase has taken place, noting the ratio of the occurrence of insanity in the two sexes and in

urban and rural populations. To the consideration of one other aspect we must now restrict ourselves: viz. to the distribution of the insane, or, in other words, to their location in public and private asylums, in the lunatic wards of workhouses, or in private dwellings as boarders with relatives or others. In connection with this inquiry we shall have to introduce considerations respecting the rate of discharges and deaths occurring under the different forms of detention.

Taking the same decennial period from 1864 to 1873 inclusive, for England and Scotland, we find (from Table IX, p. 13) that, in the former division of Great Britain, the proportion per cent. of pauper lunatics in asylums, hospitals, and licensed houses may be practically considered to have been stationary. The highest ratio was reached in 1870, when it stood at 61·92, and the lowest existed in 1864, viz. 58·56, and the average amounts to 60·50. The proportion for 1874 and 1875, not included in the decennium taken, was for each year respectively 60·07 and 60·58. In Scotland, on the contrary, between 1864 and 1873 inclusive, there has been an increase in the number of pauper lunatics in royal and district asylums of 15·2 per cent. (Table XXVII, Report, p. xxxviii), although the proportion so maintained has not yet reached that found in England, even if the 1·2 per cent. detained in private asylums be added. For from the table just quoted the highest proportion of the pauper insane of the country provided for in asylums was attained in 1870, when it stood at 56·7 per cent. Since that year it has been 56·5 and 56·3, the remainder being disposed of thus, viz. 20 per cent. in parochial asylums and poorhouse wards, and 23 per cent. in private dwellings. With these figures before us we do not quite understand the congratulatory remarks made by the Scottish Board at p. 50 of their report,—"that accommodation for all the pauper lunatics of Scotland has been provided in public establishments. One of the leading objects of the Lunacy Acts has thus been accomplished, and in this matter Scotland stands in favorable contrast with England and with most countries of Europe."

These comments are made *à propos* of the continued decrease of inmates, and particularly of pauper inmates, in private asylums; and it is added that, "excluding those in poorhouses, of all the pauper lunatics in establishments on 1st of January, 1875, only 0·12 per cent. were in asylums." In 1873, according to Table XXVII, the ratio was 1·2 per cent.

Nevertheless, granting the advantage of this reduction in the number of pauper insane detained in private asylums, we do not perceive the superiority claimed by Scotland over England in the extent of the public provision made for those afflicted

beings. For instance, of the 60·50 per cent. pauper lunatics found in English asylums, public and private, only 5 per cent. on an average are inmates of licensed houses; that is to say, nearly 56 per cent. of the entire number are under treatment in public establishments, a proportion practically identical with that found in Scotland.

We will not discuss the question how far the 5 per cent. living in private institutions may be worse off than their fellows in the public asylums, but it seems clear that the residue of the insane population excluded from the last-named establishments is not only not greater in England than in Scotland, but on the whole is presumably as well placed in one country as in the other. If a difference in favour of Scotland is, in respect to their position, to be found, it is in the case of those lunatics disposed of in private dwellings; for, whilst in the portion of the kingdom just named, these are placed under the well-organised and careful official supervision of the members of the Lunacy Board, they are in England left to the ill-regulated and uncertain action of parochial officers, who are not directly under the control of the Commissioners.

Notwithstanding the vigorous efforts made to overtake the demands for the accommodation of pauper lunatics in public asylums (and particularly when, some years since, this course was held to be an essential requisite for proper treatment, and workhouses were strongly deprecated as most unfit receptacles for the insane), the percentage of such patients has in England undergone no diminution, but, on the contrary, has shown an increase. Ten years previously the proportion per cent. was 24·29, but at the beginning of 1875 it had risen to 27·6.

The same circumstance is noticeable in the Scotch returns. These show a rise in ten years from 18 to 20 per cent. Conscious of the failure to secure public asylum space for all lunatics, in accordance with the humanitarian doctrines once held, the English Commissioners have for some years past perceived the necessity of sanctioning the construction of lunatic wards in connection with workhouses; and such wards, which by their magnitude might rightly be called asylums, have consequently been provided by many populous parishes. Very correctly recognising that, for a considerable proportion of the chargeable insane of the community, the expensive mode of construction adopted for asylums, their costly appliances and staffs, were not necessary, the Commissioners urge the placing of chronic incurables in such separate and preferably detached wards of workhouses. The highest development yet taken by chronic wards is seen in Middlesex, where the overwhelming accumulation of chronic insanity has driven the parishes of the

metropolis to unite in the building of the three huge "District asylums" at Leavesden, Hampstead, and Caterham, containing severally 1804, 537, and 1868 inmates. These establishments, however, are defined as workhouses by the Lunacy Acts; and in the English returns their occupants are comprised within the sum total of pauper lunatics found in workhouses, and which reached on the 1st of January, 1875, the number of 15,376.

In Scotland, likewise, many "poorhouses" have lunatic wards associated with them, but for the most part detached structures. The Commissioners enumerate twenty-three parishes as having established such wards. But besides "poorhouse" wards there exist a class of small pauper institutions known as "parochial asylums," and numbering six in all.

These "parochial asylums" have not a precise counterpart in England. In the latter country parochial lunatic wards are especially intended for chronic cases considered harmless; but the Scottish institutions in question seem rather to represent small asylums for the treatment of recent insanity.

In the six parochial asylums now existing there were 760 inmates on the 1st of January, 1875, of whom 443 were females and 317 males. The largest of these institutions is that at Glasgow, containing 207 patients; the smallest that at Paisley, with 48.

The movements in the populations of these asylums are greater, both by recoveries and by deaths, than among the insane otherwise provided for. The higher rate of recoveries "is," as the Commissioners observe, "largely due to the fact that the patients received into these asylums comprise a greater proportion of persons labouring under the ephemeral forms of insanity than those received into public and private asylums." Likewise, with respect to the high death-rate observable, this is attributable to the higher proportion of admissions in comparison with the numbers resident, and, as we would add, also to the circumstance of the higher proportion of recent cases in the population, inasmuch as the death-rate ranges higher in recent than in chronic insanity. On the other hand, the absence of these conditions of high rates explains what may be called the immobility among the inmates of "poorhouse" lunatic wards. Discharges among them on the ground of recovery could hardly be looked for, considering that those selected for detention in them are chronic and presumably incurable patients, who have previously been, as a rule, under asylum treatment, and whose admission has received the sanction of the Lunacy Commissioners. Nevertheless, the statistical tables show that 8 per cent. of their number ultimately recover and are discharged.

In the English returns as many as 6856 appear as out-door pauper lunatics, but of the condition and history of these the official report gives no information. It is only in Scotland that this class of the insane is under effective supervision. There the number amounts to 1442, or nearly to one fourth of the whole population of pauper lunatics. They are severally placed in ordinary dwellings approved of by the Commissioners and under their licence. Of 290 so placed since 1862 no fewer than 230 were direct transfers from asylums. Apart from the saving in the cost of maintenance by this plan of boarding-out selected cases of imbeciles, is happily obviated "the unnecessary removal of harmless and incurable patients from all participation in the scenes of ordinary life." At the same time the conviction of the Commissioners is, that the condition of the vast majority of them is satisfactory; nay more, even among these presumed incurables it is the happy lot of some to recover. Thus, in the course of 1873, 20 of the 1442 under supervision are noted to have been discharged recovered.

Further, if prolonged life be deemed an advantage to them, this also is allotted them, for their rate of mortality is only 5·8 per cent., the lowest ratio occurring among the insane however located. The history and working of the Scotch plan of boarding-out pauper lunatics is set forth fully in an appendix to the present report from Dr. Paterson, one of the Deputy Commissioners, who has given special study to the matter. This special examination of the whole subject will arrest the attention of all interested in the most important question of the day —how to dispose of our chronic lunatics?

Here we must stop, although we had marked many other topics as deserving discussion. Especially should we have desired to have commented upon the changes observable in England and Scotland as being in progress with regard to the management of asylums; we must, however, refrain, from want of space, and refer our readers more particularly to the general observations on the subject in the Scottish report.

By way of general conclusion we will observe that there is printed in these parliamentary blue books much unnecessary detail, and a useless profusion of statistical tables touching particulars of no importance to people outside the offices of the Lunacy Boards. In place of such uninteresting matter we should be pleased to receive from the Commissioners, particularly those for England, something of the nature of a résumé of their observations and experience, which might year by year fairly represent the state of lunacy in the kingdom, and the opinions prevailing relative to the best means of providing for and of treating the insane.

VII.—Croup and Diphtheria.[1]

THE references given as the foundation for the present article are only a few of those which may be enumerated among the communications and the published works lately issued on the subject of the relations existing between diphtheria and croup. Stated broadly, there is and has been for some years a kind of discussion, or rather difference of opinion, among the profession on the following question: "Are croup and diphtheria distinct diseases, or are they identical in their nature?" But this broad statement by no means conveys an adequate idea of the real points at issue, and it will probably be found, on examination, that many of the disputants have really mistaken the grounds of the contention, and that both parties are, in fact, often agreed upon principles while they are debating about the meaning of words.

In order to clear the way, then, it is necessary, in the first place, to define what is meant by the two words employed in the discussion. In reference to diphtheria there is no difficulty

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[1] 1. *Clinical Lecture on Croup and the Diseases that resemble it.* By Sir WILLIAM JENNER, Bart., M.D., K.C.B., F.R.S. 'Lancet,' Jan. 2nd and 16th, 1875.

2. *On Certain Points relating to the Etiology, Pathology and Treatment of Diphtheria.* By GEORGE JOHNSON, M.D., F.R.S. 'Lancet,' Jan. 2nd and 16th, 1875.

3. *A Lecture on the Relation between Croup and Diphtheria.* By G. JOHNSON, M.D. 'British Medical Journal,' Sept. 18th, 1875.

4. *On Tracheotomy in Croup and Diphtheria.* By GEORGE BUCHANAN, M.D., Professor of Surgery in the University of Glasgow. 'British Medical Journal,' Sept. 4th, 1875.

5. *Report of Paper and Discussion on Diphtheria and its Relations to so-called Croup at the Royal Medical and Chirurgical Society of London.* 'Lancet, Nov. 13th, 1875.

6. *On Diphtheria and the Diseases allied to it, or which may be mistaken for it.* By R. H. SEMPLE, M.D. London, 1871.

7. *On Croup and Diphtheria.* By R. H. SEMPLE, M.D. London, 1872.

8. *History and Meaning of the Terms Diphtheria and Croup.* By Sir J. R. CORMACK, M.D. 'British Medical Journal,' April 24th, 1875.

9. *Lectures on the Principles and Practice of Physic.* By Sir THOMAS WATSON, Bart. Fifth edition, 1871. Article DIPHTHERIA.

10. *Flint's Practice of Medicine.* 1868. Articles DIPHTHERIA and CROUP.

11. *Aitken's Science and Practice of Medicine.* 1872. Articles CROUP and DIPHTHERIA.

12. *Russell Reynolds's System of Medicine.* Articles CROUP and DIPHTHERIA. Vol. I, 1866.

13. *Ziemssen's Cyclopædia of the Practice of Medicine.* OERTEL on EPIDEMIC DIPHTHERIA.

14. *Virchow's Pathologie und Therapie.* BAMBERGER on CROUP and DIPHTHERIA.

15. *Hueter—Tracheotomie und Laryngotomie. Handbuch der allgemeinen und speciellen Chirurgie.* Erlangen, 1872.

whatever, because, as is well known, the word is derived from the Greek noun signifying a skin or pellicle, and no disease can be pronounced as entitled to this appellation unless this skin, or pellicle, or false membrane (to use the strict language of pathology) be found in some part. But it by no means follows that the false membrane is found only in the larynx or trachea; for it is often seen, and indeed it almost always begins, on the fauces, the tonsils, or the uvula, and it may advance to the nose, to the œsophagus, to the Eustachian tube; or it may even be found on the conjunctiva, on the skin, on the glans penis, or on the vagina. Now, to call cutaneous or conjunctival or vaginal diphtheria by the name of croup is a *reductio ad absurdum*, and therefore, in this sense, diphtheria and croup are totally different diseases.

The definition of the word croup, however, is by no means so easy, because the word conveys no etymological meaning, and is popularly used to imply very different and even opposite conditions. The words croup or roup were formerly used vulgarly in Sweden and Scotland, owing probably to the similarity of the word to the sound signified, to designate any kind of noisy or stridulous breathing; and even at the present day, in Scotland, among the colloquialisms of the country, it is usual to speak of a person who has a cold as being "roupy." Croup was first used as a medical term by a Scotch physician writing to Dr. Mead, of London, in the year 1713; and the first monograph in which it was distinctly indicated as the title of a specific disease was that of Francis Home, published in 1765. In this little treatise a disease is described of a very fatal character, and presenting the peculiarity of the presence of a false membrane in the windpipe, which false membrane was regarded, and not unreasonably, by Dr. Home as the cause of death. But since the publication of Dr. Home's treatise the word croup has been employed not only in Great Britain, but in France and other countries, to denote almost any disease of the larynx or trachea, whether a false membrane be present or not; and in the subsequent treatises on croup, as, for instance, the article "Croup" in the late Dr. Copland's Dictionary, or in the treatises bearing the same title by Jurine, Albers, Double, Valentin, Vieusseux, and many others, the writers clearly denote at least three different varieties of the disease, as it is or is not attended with the presence of a false membrane, or is characterised only by spasmodic or what were called "nervous" symptoms. Hence they make the division of the disease into inflammatory, membranous, and spasmodic croup.

The appearance of the well-known epidemic at Tours, in France, in the year 1818, and the careful study of the disease

by Bretonneau and his then pupils Trousseau and Velpeau, led to the adoption of the word *Diphthérite*, to indicate the chief feature of the prevailing malady. Bretonneau, while adopting the word croup as used by Francis Home, entirely excluded all cases in which no false membrane was discernible, and thus eliminated from the category the ordinary inflammatory affections, and also those which were spasmodic. Bretonneau distinctly states that *his* diphthérite is identical with the croup of Francis Home, but he prefers the former appellation as more significant of the pathological nature of the disease. There cannot, then, be the smallest doubt that the (tracheal) diphtheria of Bretonneau and (pseudo-membranous) croup are the same disease in the eyes of that author; and Trousseau, Guersant, and Bouchut entertain exactly the same opinion. When Bretonneau first used the word diphthérite as synonymous with croup, he had not yet been made acquainted with cutaneous diphtheria, nor with the development of the diphtheritic false membrane in several other regions of the body, and hence it is necessary to prefix the locality of the affection (laryngeal or tracheal) when placing it in juxtaposition with croup; and with regard to the latter, it is necessary to prefix the word "membranous" or "pseudo-membranous" to distinguish it from the "nervous" or spasmodic, or from the ordinary inflammatory forms of so-called croup. Hence those who argue for the identity of the two affections express their views in the following formula: viz. that tracheal or laryngo-tracheal diphtheria is the same disease as pseudo-membranous croup.

On the other hand, there are many writers, of no mean authority, who contend that the false membrane of diphtheria and that of croup are essentially distinct, are caused by different pathological conditions, and are characterised by very different symptoms. The idea of croup entertained in Great Britain before the publication of Bretonneau's researches was that of a disease commencing with symptoms of acute inflammatory fever, stridulous breathing and loud cough, rapidly running on to the production of a false membrane in the larynx and trachea, and terminating the life of the patient by suffocation. It was held as an axiom in therapeutics that as the false membrane was the necessary result of the inflammatory process, its development might be prevented by what were called antiphlogistic remedies, or that, even when formed, it might be absorbed or dissipated by medical treatment. According to this view, which was held by many, and indeed most medical writers, both British and foreign, before the time of Bretonneau, the fatal issue of the disease might always be averted if the practitioner were only prompt and energetic enough in the application of his remedies.

Dr. Cheyne, in his well-known 'Essay on Cynanche Trachealis, or Croup,' published in 1801, leaves no doubt on the mind of the reader that such was his opinion, and he distinctly and emphatically recommends the copious abstraction of blood, the use of tartar emetic every three or four hours, and the administration of brisk purges.[1]

Sir Thomas Watson, who formerly adopted the views of Dr. Cheyne and his followers, has, in the last edition of his well-known 'Principles and Practice of Physic,' published in 1871, admitted that his opinions have undergone a complete change in recent years. As showing how little diphtheria was known in this country until a very late period, this distinguished physician states that he had been more than a quarter of a century in practice before he ever met with a case of diphtheria, and that he had scarcely so much as heard of such a disorder,[2] to which his attention was particularly drawn only when the malady invaded our shores from the north of France about 1857. But he warns his class not to suppose that diphtheria is therefore a new disease, because, although he had not known it under that name, he had seen it under the form of what was called croup. "Is the disease," says Sir T. Watson, "which has for so many years been called, in this country, *the croup*, and which is attended with a membranous exudation in the larynx, anything else than diphtheria? I believe it is not." And he goes on to remark that most writers on this disease have given the history of acute catarrhal laryngitis with the morbid anatomy of diphtheria, thus confounding together two diseases very different in themselves, and requiring very different treatment. The author concludes the section on diphtheria by the statement, "You will understand that I give in my adhesion to the opinion that croup, accompanied by false membranes in the larynx and trachea, is always diphtheria, whether in the child or in the adult, and that simple laryngitis is never associated with the exudation of false membrane."[3]

These views as to the relations existing between the disease previously called croup and that now called diphtheria were by no means original, nor are they so represented by the distinguished author, who, indeed, candidly admits that he has become a convert to opinions which he formerly opposed, or, at any rate, did not support. In point of fact, those who had carefully studied the writings of Bretonneau, Trousseau, Guersant, and other French writers on diphtheria, and had also had the opportunity of witnessing cases of the disease when it broke out as an epi-

[1] Op. citato, pp. 25 and 26.
[2] Op, citato, p. 888, vol. i.
[3] Op. cit., vol. i, p. 903.

demic in this country, in 1858 and subsequent years, had arrived at the conclusions which Sir Thomas Watson now adopts. The late Dr. Hillier, for instance, in some communications published in the 'Medical Times and Gazette' in 1859 and 1862, adopts the views of the French writers; Dr. Semple, who had translated and edited, for the Sydenham Society, a collection of French memoirs by Bretonneau and many others, announced his opinion in 1864, in presenting a recent specimen of diphtheria to the Pathological Society, that the disease was no other than what was formerly called croup, and expressed his views subsequently at greater length in papers read at various medical societies, and noticed at the time in our columns; and Dr. George Johnson had worked out the same views by independent study and practical examination of the facts.

It must be admitted that, in the mean time, the literature of the subject—that is to say, of the relations existing between croup and diphtheria—was generally in a very unsatisfactory state, and the inquiring student who endeavoured to obtain clear ideas on the matter must have found himself bewildered rather than instructed by consulting the books intended for his enlightenment. The old view of croup, which included all maladies characterised by stridulous breathing, was intelligible enough, however it might be opposed to correct notions of pathology, just as asthma, fever; and dropsy were respectively used by our ancestors to denote, each of them, diseases really differing most widely from one another, but agreeing in some one obvious symptom. Thus bronchitis, emphysema, &c., were included under the name of asthma; typhus, typhoid, febricula, and relapsing fever were all included under fever, and renal, hepatic, and cardiac affections were all grouped under the general head of dropsy. Again, in reference to the point immediately under notice, the idea of croup, as entertained by Cheyne, was quite intelligible; for that author draws the picture of a disease characterised by inflammatory symptoms, and proceeding, by the natural course of those symptoms, to the formation of a false membrane in the windpipe, but capable of arrest by the adoption of strenuous medication. The only question is, whether the picture so drawn is a true one; whether the false membranes, so well and accurately delineated in the plates accompanying Dr. Cheyne's monograph, are really the results of ordinary inflammation, and capable of being absorbed or arrested in their development by bleeding, purging, and tartar emetic; and whether his successful cases were really attended or threatened by the development of any false membrane at all. Sir Thomas Watson's suggestion is that Cheyne, and others who followed him, really confounded together two distinct diseases, namely, tracheal diphtheria,

which was generally fatal, and afforded the originals of the coloured plates; and ordinary inflammatory laryngitis, which represented the successful cases, and which recovered, perhaps in consequence, perhaps in spite of the treatment adopted, but in which there never was any false membrane.

These views of Cheyne and others, we say, however we may now question their pathological correctness, were at least intelligible; but when diphtheria became established among us, and it was necessary to describe its features in contradistinction to those of croup, it became evident that the diagnosis was a very difficult one to establish. Not that it was difficult to distinguish the epidemics of diphtheria from the attacks of ordinary infantile laryngitis which prevails in cold countries every winter, but that it was difficult to show that the false membrane which undoubtedly characterises tracheal diphtheria was different from the false membrane which also characterises the so-called membranous croup.

Thus, on turning to the respective articles on croup and diphtheria in some of our most esteemed medical treatises and cyclopædias, we find that either the features of the one are delineated under the title of the other and *vice versâ*, or that under one head both are described. Thus, in Dr. Aitken's 'Science and Practice of Medicine,' under the head of CROUP, the author tells us that there are two distinct forms of the disease: one manageable, the other fatal; one attended with the secretion of mucus or pus, the other with the formation of false membranes. In the section on the treatment of croup, the operation of tracheotomy is described as being performed successfully by Bretonneau, Guersant, and Trousseau, although, as is well known, every one of those authors describes the disease for which they operated as being tracheal diphtheria. Again, under the head of DIPHTHERIA Dr. Aitken describes many of the symptoms of croup, and, while recommending the operation of tracheotomy, makes no mention of the French writers just mentioned who have advocated and performed it. In Dr. Austin Flint's 'Practice of Medicine' (1868) the information conveyed on this subject is equally unsatisfactory, and indeed contradictory, for the author tells us under the article "Laryngitis with Exudation of Lymph," that the affection occurs generally in children, and is commonly known as "true croup;" and he goes on to say that "laryngitis is essentially the same in diphtheria and in the affection called croup" (p. 252). But nevertheless he goes on to state that diphtheria and croup are quite different diseases, and that "the former is more appropriately placed elsewhere than among diseases affecting the respiratory apparatus."

The writer of the articles "Croup" and "Diphtheria" in Reynolds's 'System of Medicine' evidently finds himself equally embarrassed in describing the diagnostic distinctions between membranous croup and tracheal diphtheria; for in the article "Croup" he admits that "diphtheria implicating the air-passages produces the effects of croup with very similar symptoms;" and he also states in the same article that "epidemic croup is strictly diphtheria" (vol. i, p. 270). Nevertheless, the writer strongly maintains that croup and diphtheria are essentially distinct diseases, but, after a very able examination of the different points of distinction, he adopts as the only diagnostic anatomical character between them the more or less ulceration of the mucous tissues involved, a distinction first drawn by Virchow.

The mention of this circumstance leads us to inquire into the opinion held by the German pathologists on the point now in question. It is quite true that Virchow describes in general a number of morbid conditions of the mucous membrane in various parts of the body, and which, according to him, are distinguished by their comparative superficiality or deep-seatedness. To the former he gives the general name of *croupous*, and to the latter that of *diphtheritic*. It must be observed that he uses these words in a totally different sense, or, at any rate, in a far more extended one, than Bretonneau or any of the French writers, for he applies them to all the inflammatory or febrile affections of the mucous membranes, and thus includes under the name of diphtheria such diseases as metritis, nephritis, typhoid fever, dysentery, and even cholera. According to this view, which is wholly hypothetical, the croupous inflammation tends to a deposition of morbid material *on the surface* of the membranes (*auflagerung*) and the diphtheritic to a deposition *within* them (*einlagerung*).

But on turning to the German authors who have written specially on croup and diphtheria, in the sense in which these words are usually employed by other European writers, we find that their opinions really agree with those advanced by Bretonneau. Bamberger, for instance, who is the writer of the articles, in Virchow's 'Pathologie und Therapie,' on the diseases of the mouth and throat, divides the subject of croupous inflammation (*croupöse Entzündung*) into croup and diphtheritis, and he distinctly states that these two diseases are not distinguishable from one another by any anatomical or clinical features, but are rather to be regarded only as different degrees of the same morbid process induced by varying conditions hitherto undetermined (*durch verschiedenartige, und unbekannte Verhältnisse bedingte Gradunterschiede desselben Processes anzusehen*).—Virchow's 'Pathologie und Therapie,' vol. vi, p. 17.

The hypothetical views of Virchow are not shared even by other German pathologists, for Wagner maintains that diphtheria and croup are one and the same disease, differing only as to their locality in the throat or in the windpipe. But a new phase in the pathology of diphtheria has lately developed itself in Germany and elsewhere, namely, in regarding the false membranes as caused by, or essentially consisting of, the presence of minute organisms, or monads, called by various names, and described by their respective discoverers. This view, which is entertained by Oertel, Hueter, Trendelenburg, Nassiloff, Tommasi, and others, by no means establishes any point of distinction between croup and diphtheria, but is rather intended to show that the parasitic origin of the disease separates the croupo-diphtheritic group, as it is termed (*croupös-diphtheritisch*), from other affections Thus, in Oertel's article, "Diphtheria," in Ziemssen's 'Handbuch,' the author divides the disease into the catarrhal and the croupous form; and he states, moreover, that the croupous inflammation especially occurs in children, and the diphtheritic inflammation especially in adults (op. citato, vol. ii, 'Acute Infectionskrankheiten,' p. 587). Oertel, therefore, evidently regards the age of the patient as the only distinctive mark between croup and diphtheria.

Hueter, like Oertel, upholds the parasitic theory of the origin of diphtheria, and maintains, in his treatise on 'Tracheotomy,' that there is no practical or clinical difference between croup and diphtheria. "I cannot recognise," he says, in the treatise referred to, "any division of the croupous and diphtheritic maladies into two sharply defined diseases, and it appears to me not unimportant, in reference to the indications afforded for tracheotomy by these affections, to regard them as identical in their origin, as was maintained by Bretonneau and other French writers."[1] He thinks that all the distinctions drawn by some German authorities between croup and diphtheria may be explained by the simple assumption that the former arises from a local infection of less intensity than that which gives rise to the latter. Hueter reconciles all the phenomena of croup and diphtheria with the parasitic theory he has adopted, and he ingeniously argues that if any difference exists between the two affections, it depends upon the greater or less activity of the monads, which are the agents in both, in penetrating the mucous membrane. If these monads sink deeply into the tissues and enter the circulating fluids, the disease attacks the whole system, and is diphtheria, but if they have less penetrating power and rather tend to pass to the surface, the disease is croup. But after a very minute and almost wholly hypothetical explanation of the parasitic theory, the author con-

[1] 'On Tracheotomy and Laryngotomy,' p. 20.

cludes by observing that all the differences described by others and by himself, may be explained by supposing the reception in the tissues of an infectious matter which is different in degree, but identical in its nature. He states that he has succeeded in proving, experimentally, the identity of the croupous and diphtheric membrane, for when both were inserted into the dorsal muscles of a rabbit they produced the same form of diphtheric myitis which killed the animal. He also refers to the numerous cases occurring to every medical practitioner, in which, notwithstanding the classical distinctions drawn in books between croup and diphtheria, it is quite impossible to determine to which division the cases respectively belong. He himself remembers instances of undoubted diphtheria which descended through the larynx to the trachea, and there produced only the superficial membrane supposed to characterise croup; and, on the other hand, he recollects cases of undoubted bronchial croup, which ascended to the fauces and terminated in convalescence, with complete paralysis of the pharyngeal muscles, a circumstance which, according to all the statements in books, can only supervene in diphtheria. From these and other considerations, Hueter declares that he recognises no fundamental difference between croup and diphtheria; "in an etiological point of view, croup and diphtheria are the same infectious disease" (*in ätiologischer Beziehung ist Croup und Diphtheritis dieselbe Infectionskrankheit*—'Tracheotomie,' p. 24). After these expressions of opinion by Hueter, it necessarily follows that in his practical directions as to the indications for tracheotomy, he entirely discards the rule sometimes laid down that the operation ought to be performed in croup and not in diphtheria; and inasmuch as the danger is the same in both cases, the surgeon must be guided, he says, by the chances of saving his patient's life, and not by theoretical considerations as to the origin and progress of his malady.

It is almost needless to observe that French medical writers, since the time of Bretonneau, almost unanimously regard croup as identical with tracheal diphtheria, but it is not so generally known that the Italian medical authors have arrived at the same conclusion, after a careful examination of the facts collected during a recent and severe epidemic of diphtheria in Florence and its vicinity. In a report drawn up after collecting a great mass of information, the Florentine physicians unanimously expressed their opinion that the history of great diphtheric epidemics, and of that which recently occurred, demonstrated the fact that the croupous and diphtheritic form were only phases and modifications of one and the same morbid process, differing only in respect to time and locality (*fasi e modalità in ragione di tempo e di sede d'uno stesso processo morboso*).

Turning again to British medical writers, we find that at the beginning of last year Sir Wm. Jenner, in a clinical lecture published in the 'Lancet,' on "Croup, and the Diseases that resemble it," distinctly announced his opinion that diphtheria affecting the larynx and trachea is one of the varieties of the disease generally called croup. He commences the consideration of the subject by showing the various causes which impede the passage of air through the larynx, and constitute the diseases which are all included under the popular name of croup, viz., œdema of the aryteno-epiglottidean folds; inflammation and suppuration of a cyst in those parts; suppurative inflammation of the tissues around the larynx compressing that organ; catarrhal inflammation of the larynx; membranous inflammation of the larynx; paralysis of the larynx, and spasm of the larynx. He then describes the cases of spasm and paralysis of the larynx, usually passing under the name of laryngismus stridulus; and next, those cases of catarrhal laryngitis, which sometimes are, and sometimes are not, attended with spasm. But membranous inflammation of the larynx, says Sir William Jenner, is croup. "Is there then," he proceeds, "a membranous inflammation of the larynx distinct from the acute specific disease diphtheria? Are there a true croup and a diphtheritic croup?" And he answers the questions in the negative; for, as he says, if in the hands of the best pathologist of the day, the larynx of a child were placed who had died from membranous inflammation of the larynx—the so-called idiopathic croup—and another larynx from a child who had died from diphtheritic inflammation, the authority appealed to would be unable to distinguish the one from the other, inasmuch as there is no anatomical character by which they can be distinguished. Sir William goes on to show that neither are there any clinical characters by which the two diseases can be separated; for though he himself had formerly thought the presence of albumen in the urine was a mark of distinction, he has found that the same condition was present in both affections. As for the argument that croup is not liable to spread and diphtheria is, he points out that even scarlatina often develops itself in one individual and does not extend to others. "I am inclined to the belief," he adds, "that there is no such disease as idiopathic, simple, membranous inflammation of the larynx. I say I am inclined to this belief. I am not sure that it is true; but as I formerly thought that the weight of evidence was in favour of their non-identity, I am now inclined, from my further experience, to think that the two diseases are really identical, that the so-called croup is really diphtheria."

It happened that, simultaneously with the appearance of Sir

William Jenner's Clinical Lecture on Croup, two papers likewise appeared in the same journal and in the same numbers, by Dr. George Johnson, "On Certain Points in the Etiology, Pathology, and Treatment of Diphtheria." In these communications Dr. Johnson repeats the opinions he had formerly expressed and published as to the identity of the so-called croup with diphtheria. "I wish to express emphatically," he says, "my entire concurrence in the conclusion long since arrived at by Bretonneau, Trousseau, and all the leading French pathologists, that all cases of so-called croup which are associated with formation of false membranes in the air passages are essentially diphtheritic; and, on the other hand, that what we in this country call inflammatory croup, or catarrhal laryngitis, never results in the formation of false membrane." . . . . "It is certain that membranous croup and laryngeal diphtheria, as we now see them, are one and the same malady."

The coincident appearance of Sir William Jenner's lecture and of Dr. Johnson's papers attracted the attention of the staff of the 'Lancet,' and some articles appeared in the editorial pages advocating the views adopted by those two physicians; but the tone of these articles seems to show that the writer was unaware that similar opinions had been expressed long before. The ventilation of the new, or apparently new, doctrines, called forth a number of communications in the pages of several medical journals; and while Dr. George Johnson, Sir John Cormack, and Dr. Semple strenuously maintained the views now set forth by Sir William Jenner, and previously by Sir Thomas Watson, other physicians still maintained the old view, which, for the sake of distinction, may be called Cheyne's view, as to the nature of croup and its essential difference from the disease called diphthérite by Bretonneau.

It is quite evident, however, in reading many of the communications on the subject, that the writers have mistaken the real points at issue, and have laboured to show that ordinary laryngitis is altogether a different disease from diphtheria, a point which the advocates of the identity of membranous croup and laryngo-tracheal diphtheria do not appear to contest. The upholders of what we may call the unitarian theory, in fact, strongly maintain that acute laryngitis is not diphtheria at all, and cannot be confounded with it, the obvious ground of distinction being the presence, in the latter, of a false membrane. Dr. Wilks and Dr. Moxon, who uphold what may be called the dualistic doctrine, are evidently fully aware of the real points of the controversy, but they argue that ordinary irritation or inflammation may give rise to the production of a membrane in the fauces or the windpipe; and Dr. Moxon states that he has seen

false membranes, identical with those of croup and diphtheria, caused by a child drinking scalding water, and the same result he attributes to many other ordinary causes of disease, such as the fumes of an acid, the presence of a bean in the trachea, or exposure to the east wind. He records a case from his own experience where, in an urgent case of dyspnœa in a child, he assisted in performing tracheotomy with temporary benefit. It was immediately pointed out, however, in letters by Dr. George Johnson and Dr. Semple, that Dr. Moxon's case was not one of diphtheria at all (as there was no false membrane), but one of ordinary laryngitis; and it was urged by them that general experience shows the results of such causes as those alluded to by Dr. Moxon to be the exudation of mucus or pus, but not of false membrane. It was not denied that scalding water would raise a blister, just as cantharides might do on a previously healthy surface, but this fact by no means explained the occurrence of the specific disease constituting membranous croup or diphtheria.

In a recent discussion (the most recent at the present time of writing) on the subject now in question, at the Royal Medical and Chirurgical Society, the proceedings commenced by the reading of a paper by Dr. Semple, in which the author reviewed the historical evidence of the existence of diphtheria in different countries and at former periods, and endeavoured to show that the epidemics which were described by some Spanish, Italian, French, German, Swedish, and other medical writers, were no other than the diphtherite of Bretonneau, although described under different names. The paper, however, was not wholly historical, for it was illustrated by several clinical cases, and indeed, it is stated in the report that a recent specimen of laryngo-tracheal diphtheria accompanied the communication, and was shown to the society. Dr. Semple maintained that the specimen he exhibited presented all the appearances of the so-called croup as described by Home and Cheyne; and he stated that he had compared it with pathological specimens marked "Croup" in the Museum of the London College of Surgeons, and that the appearances were identical. His conclusions were, not that diphtheria and croup were synonymous words, but that croup, as usually understood, comprised at least three different diseases, namely, laryngismus stridulus, infantile laryngitis, and tracheal or laryngo-tracheal diphtheria. To quote the words of the published report, "he thought that diphtheria (although under many different names) had probably existed, like other diseases, from all antiquity; and although its features had not been described with sufficient distinctness by the ancient Greek and Roman medical writers, its epidemics had been clearly indicated by Spanish, Italian, Sicilian, French, German,

Swedish, American, and British authors, from about the end of the sixteenth century down to the present time. He considered that diphtheria as an epidemic did not visit London until about the year 1858 or 1859; and he regarded as the last European epidemic of diphtheria the visitation of the disease in Florence, and the neighbourhood of that city, from 1860 to 1870. But although diphtheria, as an epidemic, had prevailed only at fitful and irregular intervals in the history of the world, sporadic cases had always existed; the case accompanying the paper was of that description, and would probably have been called 'croup' by a former generation."

Dr. West, who may be considered perhaps as the chief advocate of the non-identity of (tracheal) diphtheria and membranous croup, although he did not altogether adopt the views in the paper, did not materially oppose them, and indeed, in the report of his speech on the occasion, he admits that many of the cases he attended in former years under the name of croup must have been diphtheria, and though he still held the opinion that the two diseases are distinct, "his belief was becoming less positive in consequence of the increasing evidence on the other side." Remarking that the last word had not yet been spoken as to the relations of croup and diphtheria, he suggested the appointment of a committee (which suggestion was afterwards unanimously adopted) to investigate the whole subject. In the course of his observations he relied upon the opinions held by Dr. Cheyne, and seemed to approve the practice of bleeding, which, according to that physician, had been successfully employed in cases of croup.

Sir William Jenner, who was in the chair on the occasion, repeated substantially the views he had expressed at the commencement of the year (1875), and to which we have already referred. With great candour he repeated that his views had lately undergone a change in consequence of larger experience, and although he was not yet quite sure upon the subject, he regarded the evidence as all but conclusive as to the identity of tracheal diphtheria and membranous croup. He thought that Dr. Cheyne had confounded catarrhal laryngitis with croup in his well-known essay; and he observed that a similar mistake was made by Dr. Cheyne and his contemporaries in reference to another disease in 1817 and 1818, when relapsing fever was described by them as typhus; and because many patients recovered who had been bled, it was assumed that bleeding was the remedy for the latter disease.

In conclusion, we may observe that although, as Dr. West remarks, the last word has probably not yet been spoken as to the relations existing between croup and diphtheria, yet the

points at issue have now been narrowed into a very small compass. The advocates of what we may term the unitarian view do not maintain, as far as we can learn, the identity of tracheal diphtheria with infantile laryngitis, but precisely the reverse; and, *à fortiori*, they entirely exclude from consideration all cases of what are called "spasmodic croup." The only questions to be now solved are, in the words of Sir William Jenner, "Is there a membranous inflammation of the larynx distinct from the acute specific disease diphtheria? Are there a true croup and a diphtheritic croup?"

VIII.—Recent Literature on Pathological Anatomy.[1]

Twenty years is a long period in the history of any science, and in a young science like that to which these two books are devoted, it is not unfrequently sufficient to revolutionise the data upon which that science is founded. When we consider the enormous amount of work which has been done towards the solution of some of the riddles of pathological anatomy in the last twenty years; when we reflect upon the army of workers, young and old, good, bad, and indifferent, who have flooded medical literature with work as original as, in many cases, it was ephemeral; when we consider the improved methods of investigation, the hardening processes, the staining processes, the high object-glasses, and the hundred-and-one other processes, optical, chemical, and mechanical, which have been placed at the disposal of histologists in the time mentioned, we might well expect on comparing the science of to-day with its ancestor of twenty years ago, hardly to be able to recognise them as belonging to the same family. We confess to feeling some relief, however, in finding that we were mistaken; that pathological anatomy now is substantially the same as it was in our student days; and that those to whom we naturally turn for instruction in these matters have been able to find solid ground in what we feared was in a great measure an ever-shifting, if not a baseless, quagmire.

Sixteen years have elapsed since the last edition of Dr. Wilks's book was published. It has been out of print for many years, and second-hand copies have been sold at a premium at the medical

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[1] 1. *Lectures on Pathological Anatomy.* By Samuel Wilks, M.D., F.R.S., and Walter Moxon, M.D., F.R.C.P. Second edition. London, 1875. 8vo, pp. 672.

2. *A Manual of Pathological Anatomy.* By C. Handfield Jones, M.B. F.R.S., and Edward H. Sieveking, M.D., F.R.C.P. Edited by J. F. Payne M.D., F.R.C.P. Second edition. London, 1875. Crown 8vo, pp. 864.

book-stalls. It was one of those books for which there was a constant demand, but, for some reason, the author or the publisher seemed reluctant to reprint the first edition or to issue a second. We confess we can hardly understand the course taken by author and publisher. Dr. Wilks's original book, as far as it went, was absolutely truthful. The author had abstained almost entirely from theory, and had limited himself to a simple and truthful recountal of naked-eye appearances. His word-pictures were as true to nature as are the paintings of Carswell; imagination had no share in his work, and therefore we think that had Dr. Wilks contented himself with merely reprinting his old edition, the demand for it would scarcely have proved less than that for the first edition. We are not quite sure of the advisability of yoking together youth and maturity, and we are by no means certain that both of these books would not have been improved by divorcing their double and triple authors. The efforts which we occasionally detect to bring the old theories into harmony with modern facts, and *vice versâ* (especially in 'Payne and Sieveking's Manual'), remind us of the praiseworthy attempts made by the advanced school of divines to explain the Book of Genesis by the help of modern science. Dr. Moxon and Dr. Payne are both in the very first rank of English modern pathologists, and we should welcome from either of them a book for which they were entirely responsible. We can quite sympathise with the elder pathologists if they were unwilling to stand sponsor to all the facts and theories which their younger coadjutors might advance; but we nevertheless feel that it is for the benefit of science that its young pioneers should be allowed free exercise of thought untramelled, and without let or hindrance of any kind.

In Dr. Wilks's book the graphic style is retained, and many of the descriptions are identical with those in the first edition. For this we are grateful; but why, we ask, have we been deprived of the references to the actual specimens from which the descriptions have been taken? With the help of the old edition it was possible to correct or strengthen the effect of the word-picture by inspection of the actual object to which it referred. Such a course is no longer possible with the new edition. Again, the very convenient tables of morbid conditions affecting various organs or tissues have been omitted, and we confess that this omission seems to us to be a distinct loss to the value of the book. Again, in the former edition, theory, imagination, and generalisation held no place, but the present editor is nothing if not imaginative; and we find interwoven with Dr. Wilks' descriptions, passages which, although out of harmony with the design and intentions of the work, are nevertheless often so fresh and bristling with imaginative talent, that we feel we have lost much in that the new editor has not worked single handed.

We have now exhausted our adverse criticism; and although we

distinctly state that, in our opinion, the profession would have gained much if Dr. Wilks had re-edited his own book, and Dr. Moxon had given us one of his own, still we gladly welcome the book in its present form; and may say at once and without reserve that the work is a most excellent one, and one which, while it cannot but be welcome to both student and practitioner, is highly creditable to the British School of Pathology. Opening the book at the first page, we come upon the section which treats on diseases of the bone, and this we think will be found fully equal to, if it does not surpass, any other section in the book. The edition of 1859 begins, "MALFORMATION, *Excess of Development.* Examples of this are seen in *these supernumerary vertebræ, &c.*" In 1875, however, we have "Parts that are repeated in series in the body, such as vertebræ and the fingers, are occasionally produced in more than their proper number. Examples of this are seen in the supernumerary vertebræ, &c." Now here we have a generalisation which we think, on the whole, the editor had better not have made, because many parts which are not repeated in series (unless two form a series) are produced in more than their proper number. Supernumerary mammæ, supernumerary spleens, a double uterus, a double vagina, clearly cannot be included in this generalisation, which seems to us to be useless and deceitful. Again, on page 3, we find appended to an excellent description of the varieties of *Spina bifida* the following remark:—"These conditions are analogous to common hydrocele, in being irritative dropsies; they probably depend on irritation by the nerve-centres in fœtal life." Many objections to this assertion must occur to every thoughtful reader, but surely Dr. Moxon should have told us why he speaks of hydrocele as an "irritative dropsy," and upon what ground his theory of nervous irritation is founded. On turning to his account of hydrocele, we find it ascribed to "chronic inflammation" of the tunica vaginalis, and nervous irritation is not mentioned. On the whole, therefore, we think that this second pseudo-generalisation had been better left out. Generalisations like this had no place in Dr. Wilks's original edition and, as we have before said, they are not in harmony with the work.

Under the heading of "Inflammation of the Bone," there is much new matter which seems to us so thoroughly good that we make no apology for the following lengthy extract:

"In speaking of hypertrophy of bone we have followed the common idea of the dry hard bone of the skeleton. This simple notion of bone suited very well for the description of the various kinds of augmentation and alteration of its form which constituted hypertrophy; but it is most important now that we come to consider the more violently active changes in bone to remember that the hard bone of the skeleton is only part of the entire bone; indeed, *the bone of the skeleton is but the skeleton of the bone.* In a

living bone, the hard osseous tissue, or *tela ossea* proper, is covered outside with periosteum; the cavities or cancelli within it are filled with medulla; vessels either arteries or veins permeate the substance running in the Haversian canals. Even the lacunæ are occupied by cells, whose processes stretch in the canaliculi to communicate with the contents of the lacunæ around. A living bone contains all these soft parts and its life is maintained by them. Further, it commonly also is furnished with cartilage at its articular extremities, whilst ligaments, tendons, muscles, or fat are more or less intimately united with its surface, altering not only the periosteum, but the texture of the true bone at the points of union; so that if you will carefully consider the surface of any bone, say of this femur, you will soon learn to distinguish easily those parts which have been in continuity with the several kinds of tissue which enclose the femur. Here, where the fleshy muscle took its rise; here, where the gastrocnemius tendon was attached; and here, where the adipose tissue lay in contact with the bone about the joint, you can discern altogether different kinds of surfaces which you will find then quite characteristic, so that in this way you can tell by examining the bone whether flesh, tendon or fat was connected with any given part of its surface. But the most important connection of the hard *tela ossea* is not this anatomical connection with the parts around for the purpose of supply; it is rather the historic or physiological connection which it has with the soft parts, in that the bone arises from the cartilage periosteum or medullary tissue (ultimately nearly all from the medullary tissue), and it is always ready to return into the soft state again. The dry bones we are familiar with all our life impress us with a natural but false idea that the bone is a permanent and passive sort of thing, having no proper vitality or versatility."

We know of no other writer who has put these well-recognised facts so forcibly before his readers.

In the section on diseases of the muscles, the following paragraph, which is at variance with generally received notions, arrested our attention:

"In progressive muscular atrophy or Cruveilhier's paralysis the muscles waste away because of a degeneration of the anterior grey horns of the spinal cord. The tissue of the muscle in the several cases we have examined has shown little more than a simple wasting. * * Dr. Lockhart Clarke describes a fatty fibrous, and waxy degeneration of the muscle in this disease; others also describe a fatty change. We would not deny that some fibres may be found so affected, but it is remarkable how simple the atrophic process commonly is, the fibres growing smaller and smaller, still preserving their striæ until the then remaining parts break into yellowish grains."

Again, when speaking of the pericardium, Dr. Moxon gives us the benefit of his experience on a condition which we do not remember to have seen described elsewhere:

"In the same way as cancer may occur on the serous sac, so may tubercles in cases of general tuberculosis. They are seldom, however, present without an inflammatory process being present at the same time, and thus the affection is analogous to a tuberculous pleurisy or peritonitis with both of which the tuberculous pericarditis is sometimes associated when no other tubercle is found in the body, the whole forming a peculiar diathetic disease of the serous membranes to which sufficient attention has not been given."

These two last quotations will show that the present editor is "Nullius addictus jurare in verba magistri"; that he has eyes of his own, and that although he is well acquainted with the views of Continental and other authorities, he is not inclined to follow them in any slavish manner. We have always felt some sympathy for Naaman when he asked the foreign prophet "Are not Abana and Pharpar, rivers of Damascus, better than all the waters of Israel?" and we have a similar feeling of respect for the author of this work, who gives us in his valuable volume abundant evidence that in his eyes the knowledge to be gained in his own school is fully equal to that which can be found in the most vaunted of the Continental pathological laboratories.

In the preface we are told that the section on the lung is still "wholly due to Dr. Wilks himself," and it cannot but be interesting to see what effect all the recent talk about "phthisis" has had on the judgment of this most learned pathologist. In the sixteen years which have elapsed, the guinea-pig experiments have been made; Cohnheim has given to leucocytes their proper pathological rank; Klein and others have established the importance of the *lymphatics;* Niemeyer has propounded his embolic theory, and Virchow's views on cell multiplication, which then were paramount, have descended somewhat from their high position. There can be no doubt that Dr. Wilks has been at the pains to master all the speculations of recent histologists, and to wade through the speeches of debaters: indeed, there is evidence that nothing which has been advanced has escaped his notice, and yet we observe that the views put forward in the present edition are substantially the same as those advanced sixteen years ago; and perhaps we cannot pay Dr. Wilks a higher compliment than by saying that, in our belief, the views which he advanced in his first edition are substantially those at which the majority of the profession, after endless discussion, have arrived.

In 1859 we find it distinctly asserted that the millet-seed tubercle was formed in the wall, and the ripe yellow "tubercle" chiefly in the cavity of the alveoli; and it was also asserted that while the former was a definite, recognisable, and apparently "specific" growth, the latter might arise, and generally did, from other sources than the miliary tubercle. These views have hardened somewhat

since their first appearance, and now Dr. Wilks apparently absolutely restricts the word tubercle to the grey granulation which he recognises as a "lymphoid" growth, composed of a number of cells growing in a fibroid matrix, and springing from the walls of the alveoli themselves. Addison was the first to put forward the view that "phthisis" was distinctly a pneumonic process, having often a connection accidental or consequential with tubercle, but not necessarily dependent upon it, and that the behaviour of the pneumonic processes in phthisical lungs, whether towards softening or towards a hardening fibroid change, depended more upon constitutional than upon local conditions. This view of Addison was that adopted by Niemeyer, and it is that to which Dr. Wilks was advancing in 1859, and to which, as it seems to us, he adheres in 1875.

On page 360 we find a description of a morbid condition which seems to us a little too original, and which the editor will do well to alter in future editions, although we see the description has descended to him as a legacy.

"*Stomatitis Ulcerativa.* . . It is connected with a constitutional disturbance; and in order that you may recognise the disease of which I speak, I may state that I make allusion to that which is so readily cured by the chlorate of potash."

It would have been better, we think, to have described the condition in order that its proper remedy might be selected, than to ask a student to form his diagnosis by reading the prescription.

Under the heading "*epulis,*" the editor favours us with the following amusing and highly characteristic description:

"This is an ancient name for tumours on the gums; but tumours on the gums are of many kinds, and have a general likeness to tumours in other parts; hence a difficulty in settling which tumours of the gums are to be called epulis. There is no constant practice on the point, as you may judge when we tell you that Virchow, we think rightly, gives the term a topographical significance, and calls all tumours of the gums "epulis," so that there is a myxomatous epulis and sarcomatous and fibromatous epulis, while the College of Physicians, in its nomenclature, goes curiously to the other extreme, and after placing all possible tumours of the gums under No. 385, makes epulis No. 386, but gives it no import, so that it stands like an ancient title without estates."

Going through the volume we could give numerous instances which would show that the work, though altered, is none the worse for having passed through a second edition; and although we cannot say of the present as we could say of the last, that the assertions contained in it are substantially "for all time," we cannot altogether grumble because a necessarily dry subject has been enlivened here and there by scientific flights of fancy.

Passing on to consider the second work with which this review purposes to deal, we may say that we have, notwithstanding that it is the older book, dealt with it after the other because our conviction is that its importance is not equal to that of the work of Dr. Wilks. The first impression made upon us is certainly one of pity for Dr. Payne. The work was never one of a very high order of merit, and was not worthy of being retained as the ground-work of a manual professing to place the reader *en rapport* with modern pathological notions. One of its worst features is the employment of a number of old woodcuts, which involves a miserable attempt to work them into the text. In 1854, when the first edition was published, the group of *sarcoma* was unknown, and the correct histology of malignant tumours was hardly guessed at. Woodcut 31 is more than a quarter of a century old, and professes to give a picture of the various cells to be found in encephaloid cancer. Fig. 37 is an antiquated and miserable sketch of a "scirrhous tumour of cerebrum." We cannot believe that Dr. Payne would have admitted these misleading pictures or others like them into the text of a book of his own. They are in very strong contrast to the excellent histological drawings in Dr. Moxon's book, and are often a direct contradiction of the few modern drawings with which Dr. Payne has favoured us. While these glaring anomalies remain, it is impossible to recommend the work for the perusal of students.

Much of the old text is as bad as are the old woodcuts. The following is a reflection made by Mr. Handfield Jones on "compound cysts," and it is sad to think that an author should so cherish the effusions of his youth as to be unable to summon courage to expunge them when his matured judgment ought to tell him that the time has arrived to do so:

"The impression left on the mind of the observer after a minute examination of the compound cysts, is, that they are of the lowest type of organisation, resulting apparently from a depraved degenerated formative action, which, withdrawing blastema from its proper uses in the system, hurries it with a wasteful expenditure into useless and injurious elementary shapes. How precious is the stringency of the law of our organic constitution which is comparatively seldom infringed by such terrible aberrations."

Such jargon as this is suited for a seventeenth-century doctor on the stage, but we blush for the honour of British pathology that it should still find a place in an important text-book.

The good and the bad are so interwoven in the work that the labour of winnowing becomes an impossible one. The lesson to be learnt from it is this: that only those who are actually working at the subject of pathology should presume to write upon it, and that those who are actually working at it should not jeopardise their reputations by becoming responsible for matters in which they have

had no part. We hope that Dr. Payne will, at some future time, give us an opportunity of reviewing his own views on pathological subjects. By appearing in company with two retired pathologists (for whose labours in the past the profession will long owe a deep debt of gratitude), he has clearly done himself an injustice, and we regret that it is beyond our power to rectify it.

IX.—Valette's Surgical Clinique.[1]

These lectures are the *résumé* of twenty-seven years of practice in the hospital at Lyons, and if they presented a fair picture of the surgery of that city, we fear there will still be much room for improvement. We believe, however, that they do not do so, as much excellent and original work has come during the last ten years from Lyons, which is not so much as mentioned in this work. We may give, as an example, the observations on subperiosteal surgery, by M. Ollier. The style of the anthor is diffuse in the extreme, yet it is not unpleasing; his sarcastic allusions to those who differ from him often being extremely amusing. In no part of the work is there any attempt at what may be called the scientific treatment of his subject, but there is much sound and useful empirical surgery, which cannot have failed to benefit his hearers. The first lecture is devoted to a brief history of the surgery of Lyons during the last hundred years, and of the great men who have preceded M. Valette in the office he now holds. Amongst these, Gensoul, Bonnet, and Prevaz are the best known For Bonnet the author has a sincere and undisguised admiration. He is quoted repeatedly in almost every lecture, and the author does not attempt to conceal that he has taken him for his model, and has scarcely ventured to stray beyond his teaching. One lecture is devoted to a comparison of the relative safety of ether and chloroform, and M. Valette pronounces unhesitatingly in favour of the former. He does not deny that deaths occasionally occur under the influence of ether; but in the cases he has been able to collect, there is a remarkable difference between these and the simillar accidents from chloroform. A large number of the fatal cases under chloroform have occurred when the anæsthetic was being administered to a patient apparently in good health, and suffering from some trivial local malady. On the other hand, those occurring under ether have always been in cases of the gravest character, and in which the patient was either exhausted by previous

[1] *Clinique Chirurgicale de l'Hotel-Dieu de Lyon.* Par A. D. Vallette. Paris, 1875.

disease, or suffering from the shock of some severe injury. Again, he points out that death, when resulting from the effects of ether, has always been preceded by warning symptoms; whereas with chloroform it has often been sudden, and utterly without warning of any kind. He tells a somewhat amusing tale of Bonnet, relating to this subject. Bonnet, although he had lost several patients from chloroform, persisted in continuing its use on account of its greater convenience. At last, he himself wished to have some anæsthetic during the extraction of a tooth. When seated in the chair, ready for the operation, the dentist asked him, "What do you say, M. Bonnet; shall we use ether or chloroform?" He reflected a moment, and then replied, "Use ether." "From that day chloroform was banished from his wards in the hospital, and from his practice in the city." The lectures on wounds and their treatment show a considerable advance on the methods of treatment usually employed in France a few years ago. The author recognises in the admission of air, and consequent decomposition of the discharges, the chief cause of the dangers and complications of wounds. He admits that the fever which follows the first reactionary fever after the shock of the injury is due to absorption of putrid matter from the wound, and that inflammation and suppuration are the certain consequences of the presence of decomposing discharges or clots between the two surfaces of the wound; but the only means he is inclined to adopt to prevent these evil consequences are frequent injections of some antiseptic solution, or, whenever possible, the use of caustics instead of the knife, so as to produce a surface incapable of absorbing any septic products that may form on it. Twenty years ago he attempted the treatment of stumps, by inserting them through a hole in the side of square bath into a liquid, so as completely to shut them out from any contact with the air. The limb was surrounded by an india-rubber collar, to prevent the water escaping into the bed. He, however, abandoned this treatment after a few trials, not because the cases did not do well, but he could never get the india-rubber collar to fit; in some cases it would strangulate the stumps, so as to cause troublesome œdema, and in others, the unfortunate patients would be constantly wetted by the leakage from the bath. He mentions Guyon's "pausement ouaté" briefly, but does not seem to have much practical experience of it. He makes use of drainage tubes in the treatment of extensive wounds, but seems to value them as much for the facilities they offer for injecting the wound, as for any use they may be in carrying off the discharge. He seems entirely to have failed to grasp the idea that where no decomposable matter is, no decomposition need be feared. If the serous discharge is freely drained from the wound, and if no clots of blood are left between its living surfaces, the repeated injection of irritating antiseptic fluids is quite

unnecessary, and can only be productive of increased suppuration and delayed union. Throughout the whole work he shows a great predilection for the use of caustics. His rule, apparently, is, cauterize wherever you can, crush or tear if you cannot cauterize, and cut only when you cannot help it. Wounds may be divided, he says, into three classes: "Subcutaneous wounds, always harmless; cauterized wounds, not always, but very frequently, harmless; bleeding wounds, or those produced by a cutting instrument, these are other things, being equal, infinitely more dangerous than the preceding." The caustic he prefers is the chloride of zinc paste. The actual cautery he has little respect for, and seldom uses. As a means of applying a combination of strangulation and cauterization, he has invented instruments which he calls the caustic ligature and the caustic clamp. The caustic ligature is, in fact, more of a clamp than a ligature, in the strict sense of the word. It consists of two metal rods, deeply grooved on one side. The grooves can be filled with the chloride of zinc paste, and the rods can be brought forcibly together by means of a couple of screws. The caustic clamp is a pair of forceps grooved in the same way; the blades are brought together and secured by a screw, as in an ordinary ovariotomy clamp. He recommends the use of these instruments in a great variety of cases. In castration, after having exposed the testicle by the knife, the cord is seized in the clamp, and the gland cut away from below. After the operation for strangulated hernia, in which he always opens the peritoneum, he seizes both sides of the incision into the sac with his clamp, and so, he asserts, completely and safely closes the opening into the abdomen. He uses the same instruments in treating hæmorrhoids, prolapsus, and erectile tumours, and, in a somewhat modified form, for the cure of varicocele. The clamp is also recommended for excision of fibroid tumours of the uterus. That these instruments may be useful in some cases we do not doubt, and that the antiseptic properties of the chloride of zinc diminish the dangers from pyæmia and septicæmia is also probable; but their use destroys any hope of union by first intention, and must therefore be looked upon by English surgeons as a retrograde step in the treatment of wounds. It would also be supposed that the pain caused by their application must be very great, but this the author denies; and he asserts the combination of pressure and cauterization is less painful than either of these means alone. The clamp, when applied, is left on for a time, varying from twenty-four to forty-eight hours, or even more, and at the end of that time the parts beyond it are said to be reduced to a firm, dry, inodorous slough, which separates slowly, leaving a healthy granulating sore, which rapidly heals. Another modification of the above instruments he calls the "caustic seton." It consists of the grooved rods, as before; but one is passed through the cyst to be

operated on, and the other is laid on the skin outside. They are then brought together forcibly by screws. Before applying the superficial rod, he rubs the skin freely with potassa fusa along the line in which it is to lie. This apparatus he employs in preference to injections of iodine in the treatment of cystic goitre, and he also uses it in opening chronic abscesses, and in the cure of vaginal hæmatocele. In short, the safety of cauterization, and its great superiority over the knife when there is any possibility of employing it, and the excellence of the instruments he has invented to simplify its use, form at least one half of the matter in these lectures. In spite of all precautions to the contrary, however, erysipelas does occasionally occur in his wards; but in a lecture devoted to the subject, he states that he can always cure it in from two to five days. His treatment consists in the internal and external use of perchloride of iron. He gives it in doses of forty drops of a 30 per cent. solution every few hours, while at the same time the whole of the affected part is rubbed with a similar solution. This latter part he considers the more important, and, when possible, does it himself. It is necessary, he says, that every particle of the affected skin must be thoroughly soaked in the solution, not merely painted over with it, and this should be repeated twice a day.

In the treatment of varicose veins, the author is a strong advocate of the plan of injection. For this purpose he first tried perchloride of iron, but after some time abandoned it, as troublesome inflammation not infrequently resulted from its use. He has since then employed a solution composed of 1 gramme of iodine, 16 grammes of tannic acid, dissolved in 500 grammes of distilled water. He asserts that the clot formed by the solution is capable of being readily absorbed. During the operation a bandage is applied tightly round the limb above the vein to be operated on, and this is not to be removed for three hours after, for fear of embolism. The "iodotannic" solution is injected to an amount varying from ten to twenty-five drops. The effect is to cause immediate coagulation of the blood at the part operated on. At first there is no pain, but after a few hours a severe burning sensation sets in, and the vein begins to inflame slightly in each direction. This never reaches any serious degree, but it is sufficient to cause obliteration for some distance above and below the spot injected. The author states that there is no fear of embolism. He has operated in more than 200 cases without any accident, and he has found the results much more permanent and complete than after any other operation.

One lecture is devoted to a case of what is called intermittent hæmorrhage. This is a condition we do not remember having seen described in any surgical text-book, although a pamphlet has been written on it by Professor Bouisson, of Montpellier. The patient underwent an amputation of the thigh. All went well till the fifth day;

on that evening he felt a peculiar sensation in the stump. Next day he noticed the same feeling, at the same hour; and the following day it returned again, but this time accompanied by a copious hæmorrhage and the like symptoms appeared again the day after. It then occurred to M. Valette, from the periodicity of the symptoms, that they might be dependent on some malarial taint, and on questioning the patient, it was found that he had suffered from ague. Large doses of quinine were at once administered. At the regular hour next day the symptoms returned, but less severely; the following day sanguineous oozing appeared on the surface of the flaps, but no actual bleeding, and from that time he made a good recovery. M. Valette also relates a case in which epistaxis showed the same periodicity.

It is comforting to find, from the frequent allusions to quacks of all sorts, "rebouteurs," "rhabilleurs," "recogneurs," "bandagistes," &c., that we are not at any rate worse than our neighbours in this respect, although it is customary to suppose that in France these irregular practitioners are dealt with much more severely by the law than they are in England. M. Valette records several cases of malpraxis under their hands, but no mention is made of any punishment following the offence.

Want of space prevents our noticing the author's views on some other subjects, but we have given enough to show the general character and style of his surgery.

XI.—Chapman on Medical Charity.[1]

The contents of this volume originally appeared in the pages of the 'Westminster Review,' and although they gained by the medium of that very able journal considerable publicity, it was a wise act on the part of Dr. Chapman to reprint his papers in the form of an independent treatise, accessible always to every one desirous of studying its subject.

There is this much unfortunate about the subject-matter of this book, that it is looked upon as a doctor's question and a doctor's grievance, and the general public will scarcely condescend to give it a passing consideration. This circumstance itself, however, should impel the profession to keep the subject before the public; to agitate

[1] *Medical Charity; its abuses and how to remedy them.* By John Chapman, M.D.

and agitate, until the evils of the existing system of medical relief obtain recongition and removal.

It is equally needful, and it easy also to show, that the abuses of medical charity injuriously affect not only medical men, but likewise the whole community; and that, in the interests of the social economy and the moral wellbeing of the country, they call for remedy.

As a contribution in aid of these objects, Dr. Chapman's book is of the greatest value. Numerous letters have appeared in the public papers, and many pamphlets have been printed, setting forth particular abuses, and illustrative of special defects; but nowhere else than in this volume have we met with a comprehensive examination of the whole matter. Moreover, the examination has not been made without great care and minute inquiry; nor can it be charged with unfairness, although some of its conclusions cannot be grateful to various parties associated by interest or wedded by prejudice to the prevailing policy in regard to medical charity.

Dr. Chapman divides his book into two parts: the first treating of the extent and abuses of medical charity; the second of the method of administering it. Collecting from the best sources of information at his command, the author arrives at the conclusion that the annual sum expended in London in gratuitous medical relief amounts to £600,000. This enormous sum represents only the amount dispensed by so-called medical institutions, hospitals, and dispensaries, and does not include the still larger sums raised by rates for the medical relief of sick paupers and for the care and maintenance of lunatics, imbeciles, and idiots, in county and other asylums in Middlesex. Nor, again, does it represent the annual monetary value of the property in lands and buildings occupied by the metropolitan medical institutions, and which, calculated at five per cent., would add above £300,000 more to the total amount devoted yearly to medical relief. As Dr. Chapman pertinently puts it, the whole sum expended would, if divided equally among the medical practitioners of London, give to each one, irrespective of any private practice, an annual income of £264.

The creation of this enormous income by the munificence of a past and of the present generation may be rightly pointed to as a worthy monument of Christian charity. To do so is both justifiable and right; but at the same time it is equally just and right to inquire how this wealth is disposed of; whether it is doing all the good it might; whether, by wise and economical arrangements, it cannot be made to produce greater benefits; and whether it be not possible that, by faulty administration, it may even not do harm.

These questions are discussed and replied to in Dr. Chapman's treatise; and it is to be regretted that the replies are unfavorable. He shows that the advertising hospitals only tell half their tale to

the public; that the plea of indebtedness and approaching insolvency is most recklessly employed, and that there is much needless expenditure. Amongst the endowed hospitals, examples of extravagant expenditure are still more marked; and in all the medical charities the relief offered is largely abused by unfit applicants.

The unrestricted granting of medical relief is a source of medical pauperism, morally detrimental to its recipients. The development of medical charity has been of late years excessive and disproportioned to the actual wants of the community. Various are the motives for instituting new hospitals and dispensaries. The usual ones are stated by Dr. Chapman at p. 33, and, as he remarks, the multiplication of such institutions is a phenomenon of spontaneous generation. Each new one in it is turn begets another; "the establishment of an hospital in any given district multiplies indefinitely in that district the demand for that gratuitous aid which hospitals afford;" and the more rapid the increase of its medical pauperism, so more urgent become the demands of the district for augmenting the charity and fostering it.

At p. 53, the author presents a summary of the evils or abuses of medical charity which he has pourtrayed. This summary deserves to be printed separately, as it presents a series of theses for public consideration, which need to be impressed upon the public in every practicable manner.

In the second part of his treatise Dr. Chapman reviews various incidents in the history of hospital administration which indicate the great faults pervading it, and, to some extent, the direction in which remedial measures need be applied. He likewise describes various efforts at reform, and ultimately propounds his own ideas respecting the proper administration of medical charity. He accepts provident, self-supporting dispensaries as one means towards counteracting the present abuse of such charity; but, at the same time, considers that such institutions need be associated with a reformed hospital organization, and with the introduction of the Irish District Dispensary system, with additional safeguards against its recognised abuses. He would farther put this district dispensary system in relation both with the general hospitals and with poorhouse infirmaries; would establish provident hospitals in connection with provident dispensaries, and would remodel existing hospitals by making them "public hospitals," placed under public supervision and control.

The scheme is a large one; but in its main features commends itself to approbation. It would be curative of many of the evils of medical charities now acknowledged; it would reduce to a definite organization what now is an incoherent congeries of institutions, having like general objects, but often competing, and in conflict with each other; it would place reliable medical treatment within the

grasp of every individual requiring hospital care, whether he paid for it, or received it gratuitously as a fit object of charity.

Impressed with these opinions of Dr. Chapman's proposals for the administration of medical charity, we recommend our readers to a careful examination of his views, to aid them, if nothing more, towards arriving at satisfactory conclusions respecting a question of the last importance to the profession, and in respect to which some decided and definite course need soon to be taken.

A propos of the subject of medical education which incidentally comes under Dr. Chapman's notice in the scheme of hospital administration he propounds, he subjoins the following sensible remarks:—"We think the great multiplication of medical schools which has taken place in the metropolis is a great evil, that in this respect we should do well to approximate to the system exemplified in Paris, and that in any case three distinct schools . . . . would more than suffice for all the medical students likely to assemble in the metropolis. We believe that were there only one large school, the students of which would be admissible to each of the metropolitan public hospitals, it would be possible to insure that its professional chairs should be filled by men of the highest eminence," to the inestimable advantage of the students.

Bibliographical Record.

Turner and Mivart on Anatomy.[1] — In ancient days, when observers of nature were few and books comparatively rare, men learned as they could, not as they would. Here and there a few honoured ones, favoured by wealth, had the opportunity of attending lectures on certain given subjects, these lectures being delivered by men who had, as far as lay in their power, studied the subjects which they taught. In those days it was not permitted that every observer should be also a lecturer and teacher. Many, indeed, of the latter class of men, men highly gifted with faculties and powers of observation, no less gifted than some of those whose names are traditional in the history of anatomical science now, have died, and their knowledge with them, unknown and unhonoured, simply from want of opportunities of making their knowledge public. The names of great authors are few indeed, and with few exceptions these very authors would probably have remained unknown had it not been for circumstances in a great measure accidental. Claudius Galenus, as great a theorist as ever lived, would never have attained his wonderful reputation had he been as poor as many of the medical students are of the present day, despite the increased facilities and advantages of studying medicine and its collateral sciences at home and abroad. His great name, though dimmed by the irresistible light of truth, had never risen but little beyond zero if he had studied only under the anatomist Satyrus, the Hippocratic disciple Stratonicus, and the empiric Æschrion. The then famous school of medicine at Alexandria was the birthplace of Galen's reputation. The same may be said of the less successful though far better anatomist Bartholomeo Eustachio, an accomplished classic and a student of medicine at Rome. The

[1] 1. *An Introduction to Human Anatomy, including the Anatomy of the Tissues.* By William Turner, M.B., Professor of Anatomy in the University of Edinburgh. Part I.
2. *Lessons in Elementary Anatomy.* By St. George Mivart, F.R.S., &c.

influence of Cardinal Borromeo did much to secure for him the chair of medicine in the Collegia della Sapienza. Fallopius, though an able and talented man, enjoyed the patronage of Cosmo I, the first Grand Duke of Tuscany, and the unlucky Vesalius was not less favoured. Thus, many deservedly great teachers, whose works have been handed down to posterity, might, and probably would, have remained obscure and unknown if they had been less blessed by circumstances beyond their control. But modern anatomists are no less indebted to these teachers for utilising and promulgating their knowledge, little comparatively as indeed each contributed.

The long array of familiar ancient as well as modern names, *e. g.*, Albinus, Boerhaave, Casser, Fabricius, Haller, Haase, Lancisi, Morgagni, Meckel, Neubauer, Pacchioni, Santorini, Soemmering, Scarpa, Wrisberg, Weitbrecht, and a host of other able anatomists, illustrate the fact that anatomical knowledge has been perfected to its present status very slowly. Progression does not seem to have been coincident with the opportunities of study. The history of anatomy is sadly defaced by the evidence of imperfect and inaccurate observation. Authors and teachers have been too eager to see and describe tissues or organs as previous observers had done, or to drag themselves into notoriety by repetition, in many notable instances being as wide of the truth as the assertions which they assumed to deny.

The dissipation of the old and well-nigh worn-out ideas that all animals are especial and independent creations has given rise to a new era in the study of anatomy. Modern teachers know well that it is impossible to understand human anatomy from a study of the human subject alone. If, then, it is impossible, as assuredly it is, is it not a mistake to continue to teach it in the old-fashioned method in the medical schools?

Students generally have a great objection to the introduction of comparative in the course on human anatomy. This objection is based upon ignorance and prejudice partly, but is mainly due to a bad system of teaching.

While ignorant of everything pertaining to anatomy in general, the student commences his knowledge of one of the most comprehensive sciences by learning the bones of the human body. Probably a few casual remarks are made at the introductory lecture to the course on the similarity of the human to the vertebrate skeleton, and then for once and for aye the comparative elements are avoided during the course. The thinking public outside the medical schools exhibit far greater interest in natural history, comparative anatomy, and, in short, science generally, than those who have advantages of study unequalled

in the history of the world—the student-factors of the medical profession.

It is now several years since Professor William Flower, the talented and enthusiastic Conservator of the Hunterian Museum at the Royal College of Surgeons, brought out his inimitable book on the osteology of the mammalia. It was thought at the time, and suggested to teachers, that this work would inaugurate a new era in the teaching of skeletal tissues. But instead of this valuable text-book usurping the place of the volumes on "Human Osteology," its use is confined to a few science-teachers and to some hard labourers in the paths and fields of science, such as the pupils taught by Huxley at South Kensington Government Science Schools. The proper study of mankind, it is often asserted, is "man." An egregious fallacy so far as concerns the study of man's physical organization. A mere student of man, and man's anatomy, is a narrow-minded and ignorant individual at the best. The present elevated status of man, at the summit of the great cone of organic and structural existences, has not been arrived at suddenly. Whatever, then, can throw light upon the means by which, and through which, this status has been arrived at must be a source of interest and a necessary knowledge. One might with equal justness state that a thorough and comprehensive knowledge of the hydroid polypi could be derived from a study of a single specimen of the great family, as infer that human anatomy can be learned from the human subject alone. It is impossible. Under such conditions the whole fabric is a vast and uninteresting desert of unreadable and non-understandable factors. Progressive knowledge is impossible from this degraded standpoint, though the commonly accepted one. The sooner those in authority in our great medical schools recognise the fact the better—that the method now adopted of teaching human anatomy is a bad one, and calculated to thwart true progress. The system of teaching by lectures has been inoculated into every medical school in the universe, has been persisted in, and is still maintained. In some measure it is of service, but it is as at present carried out a sad waste of valuable time and knowledge. What can a lecturer tell to his class on human anatomy in an hour that they cannot obtain from one of the scores of text-books in half an hour? Nothing! Then it is a sheer waste of time and money. Human anatomy never can be or will be taught as it ought to be until it becomes the last, instead of the first, of the subjects which a student has to learn. The method of teaching first, we believe, adopted by Professor Huxley at South Kensington, is the method which must ere long be adopted in every medical school. If it is not,

then we shall have the anomaly of schoolmasters being far more conversant with general and special anatomy, and science generally, than medical men are. Huxley begins with the Torula and Penicilium, progresses through the various grades of invertebrate and vertebrate animals, and ends with man. The medical schools begin with man, and end with him. The result is that their students know little, or nothing worth knowing, of the very subject which, in the future, they profess to understand—health and disease.

It would be a better and far more instructive method to commence the study of anatomy from the dog; but it would be still better to begin with structures as low in the scale of nature as possible, and to substitute demonstrations for lectures. Make the students trust more to themselves by insisting upon sketches or diagrams and self-written descriptions, to be afterwards corrected by the teachers, and then the three great elements of progress will have been mastered, viz., how to observe, how to describe, and how to learn.

Mivart's little book of 'Lessons in Elementary Anatomy' is a step in the right direction. It is as beautifully as it is plainly written. The author tells us the book was "*intended for teachers and earnest students of both sexes* not already acquainted with anatomy." There can be no doubt that much will be learned by even a casual perusal of the book—information of the most valuable kind. It is highly deserving the commendation which has been so generally bestowed upon it, and will stimulate enthusiastic naturalists to push their studies still further into the delightful regions of structural anatomy.

Professor Turner's book "has no pretence to be an exhaustive treatise on the subject." It is "an exposition of the principles on which the human body is constructed." We like books that have no pretences about them. Such is the work before us; and though confined more exclusively to human tissues, is by far the best, the clearest, and most readable book we have ever perused. It is a book that every medical student should read; and it is a book that every non-medical student might read with considerable advantage. The public owe much to the publishers for suggesting, and to Professor Turner for acceding to the suggestion, of reproducing the article, first written for the new edition of the 'Encyclopædia Britannica,' in a separate state. Professor Turner has as inimitable a style of writing as he has of demonstrating the tissues with which he is so familiar. Few men can methodise in so clear and masterly a style, describe so clearly, and, on the whole, produce so cheerful and readable a book.

Schiff on Vivisection.[1]—The accusations of unnecessary cruelty to the lower animals made against Professor Schiff, of Florence, are matters of common notoriety, and the atrocities said to be perpetrated by him in his physiological laboratory may almost be said to have excited a thrill of horror throughout the civilised world. Among other charges it has been stated, and no doubt believed, that a number of dogs were kept under experiment all day and all night, but especially during the night, and that the howls of agony from these poor brutes were so loud and so continuous as to disturb the repose of the inhabitants around. It appears, in fact, that as long ago as 1863 not only was a report to this effect current in Florence, but a lawsuit was actually commenced against Dr. Schiff for the public nuisance said to be thus occasioned. At that time, however, the Professor successfully proved that the accusations were unfounded; that no experiments were performed in the night at all; that the animals experimented upon, being rendered insensible, did not utter any cries during the operation; that the noise of the dogs was really the barking of healthy animals, jealous of the attentions bestowed upon their companions; and that the noises heard at night did not proceed from the laboratory, where everything was quiet, but from the numerous dogs kept in the vicinity. Dr. Schiff, in 1864, wrote a memoir explaining these facts and also containing details as to the mode of pursuing his investigations; but as he finds that his proceedings are still misunderstood and misrepresented, more especially in English and Florentine journals, he has issued a new edition of his memoir, which is that now before us, and which contains some additional matters, chiefly in relation to the effects of curara, and to the preference which ought to be shown to ether over chloroform in producing anæsthesia.

Dr. Schiff's memoir, besides denying the specific charges made against himself, contains a really eloquent defence or apology of the practice of vivisection in general, though perhaps the term vivisection can scarcely be applicable to many of his proceedings, considering that some of them are conducted (such as many of those on digestion) on perfectly healthy and uninjured animals, and that many others are carried on after death. To explain this latter statement it is only necessary to observe that several of the functions necessary to be studied by the physiologist may be and are performed after the consciousness of pain has been abolished by poisoning, by wounding the spinal cord, and, in fact, after the actual death of the animal experimented upon. Some physiological phenomena con-

[1] *Sopra il Metodo seguito negli Esperimenti sugli Animali viventi nel Laboratorio di Fisiologia di Firenze.* Cenni del Prof. MAURIZIO SCHIFF. Pp. 78. Firenze, 1874.

On the Plan pursued in the Experiments on Living Animals in the Physiological Laboratory of Florence. By Professor MAURICE SCHIFF.

tinue to present themselves for a certain time, either spontaneously from the remaining irritability of the tissues, or from the stimulus of galvanism, and it need scarcely be stated that the lower is the animal in the scale of organic nature the longer do its automatic movements continue; and the apparent writhings and contortions of the muscular fibres are, in such cases, wholly independent of pain. But when operations are necessarily attended with pain to the lower animals, Dr. Schiff indignantly denies that he inflicts it in his experiments, and he asks whether it is likely that, after having refused, for twenty-five years, even *to be present* at a painful experiment unless it was performed under the influence of anæsthesia, he would amuse himself in his own laboratory by witnessing the sufferings of unfortunate brutes, and listening to their cries of torture. He distinctly states that it is a general rule adopted in his laboratory not to perform any operation which may be painful to any animal without first rendering it perfectly insensible by ether, and this rule applies, not only to the higher classes of animals, but even to frogs.

Dr. Schiff gives numerous reasons for preferring ether to chloroform in his experiments, and he states that etherization, carried to the last grade of insensibility, never becomes dangerous to life as long as the movements of respiration are maintained. Even when the inhalation of ether is carried further, and death appears to have supervened, life may be restored if, at the moment of the paralysis of the thoracic walls, a species of artificial respiration is immediately commenced. His experience has proved that in more than 3000 cases, in which ether was employed with the intention of preserving the life of the animals, and with the adoption of the necessary precautions, there was not a single case of death. Chloroform, on the other hand, destroyed a considerable number of animals when he wished to push the anæsthesia to the penultimate grade, that is to say, until the contraction of the diaphragm became so weak that an aperture in the abdominal cavity no longer caused a prolapse of the intestine. He has assured himself that in etherization the pressure of the vessels is maintained at a height which is almost normal, and always compatible with the continuance of life, even after the cessation of the automatic respiration, and he has ascertained by the manometer that the sanguineous pressure is raised before the moment of death. On the contrary, in the case of chloroform, it has been found by the manometer that under similar circumstances the pressure of the blood is lowered before the automatic respiration has ceased, and the pulse has been often seen to disappear almost entirely in the manometer, the pressure of which fell to twenty-five or thirty millimètres while the animal was still breathing by its own efforts. Dr. Schiff thinks that chloroform ought to be banished from practice as an anæsthetic, except in cases where there is an extraordinary resistance to the effects of ether, and even then it might be allowable to

mix a little chloroform with the ether, and to commence the anæsthesia, which may afterwards be continued with pure ether.

Dr. Schiff enters into several other matters relating to general physiological subjects, and to his own defence in particular. Among the topics relating to himself he states that, although he employs cats, rats, frogs, and some other of the inferior animals, with the aid of ether, for the purpose of his experiments, and is not very particular as to the sources from which he obtains them, or as to their future lot after he has employed them, yet, in the case of dogs, he is very particular not to make use of any except those which are condemned as useless or troublesome, and he even claims for himself the credit of rescuing many of these animals from death and neglect, and opening to them new hopes of a social and affectionate life (*per cui si aprono nuove speranze d'una vita sociale ed affettuosa*). It is almost amusing to contrast the picture presented by Dr. Schiff of his canine companions with that given by his accusers. He tells us, in fact, that the animals are all perfectly happy, and are even delighted with many of the experiments of which they are the subjects (such as those on digestion), and even the dogs which, under anæsthesia, have, perhaps, lost some internal organ, are as happy as the rest, and are unconscious of their loss. We give this cheerful view of the Florentine laboratory in the Professor's own words, and it may afford some consolation to those who represent the scene as one of continual suffering and pain:

"The reader," he says, "not being a physician or a physiologist, who accompanies me in my laboratory and sees a number of healthy-looking, well-fed dogs, would take them to be uninjured and newly arrived, if their sprightliness and the caresses with which they welcome my arrival did not make him suspect that my acquaintance with them was not so recent as the state of the animals might lead him to suppose. Would he believe me if I assured him that three of these dogs had lost their spleen, and the others different ganglia of the sympathetic nerve in the abdominal or thoracic cavity?"—P. 62.

Baths of Homburg.[1]—The title of this book sufficiently indicates its nature and its object. We have looked through it, and can say that it gives a full and good account of Homburg and of its therapeutic resources. Of course it is only natural that the most that is possible should be made of them.

We observe that Dr. Hocher is strongly of opinion that gouty patients would benefit more by first drinking the waters of Homburg, and then going to a warm bath; whereas it is usual to send such

[1] *Bad Homburg and its Resources for the Use of English Visitors, especially.* By Dr. Hocher, Resident Physician at Homburg. Bad Homburg, 1875. 8vo, pp. 67.

patients to Homburg after they have gone through their course of baths.

The climate is described with tolerable fulness, and its dry ozoniferous air is descanted on at some length. Dr. Hocher is right in this, for it is its airier and fresher climate very much that makes patients prefer Homburg to places with just as good waters, but with more confined air. He especially recommends Homburg as a transition resting-place for English on their way to the heights of St. Moritz.

Griffiths on Prescribing.[1]—This little work has been written with the praiseworthy object of instructing beginners in the study of medicine, how to prescribe accurately, both in respect to the intelligibility of the language employed, and to the due combination and preparation of the drugs indicated. Dr. Griffiths advocates the continuance of the use of the Latin language in prescriptions, but he warns his pupils that the classical Roman tongue is not that in which an orthodox prescription is written.

We regret to observe that in many parts of this work the Latin is neither elegant nor accurate, and the errors made are certainly not all printers' errata. In the list of words and phrases most frequently employed in prescriptions many mistakes occur, as, for instance, "continuantur remedia," let the medicines be continued, for continuentur; "alutem," for alutam, leather; "more dictu" for mere dicto; "vas vitrium," for vas vitreum, a glass vessel; "vitrium," for vitrum, glass; "vomitio urgente," for vomitu or vomitione urgente, &c. It is quite possible that these and other mistakes may be slips of the pen, but they should have been carefully avoided in a work having for its express object the inculcation of accuracy in writing prescriptions.

The precautions laid down as to the presence of incompatibles in a prescription appear to us to be occasionally superfluous, and sometimes even erroneous. For instance, the prescriber is warned not to order calomel in combination with any preparation containing a trace of prussic acid—an error not very likely to be made; and, immediately after stating his intention of adducing important instances of incompatibility, Dr. Griffiths gives the example of dilute hydrocyanic acid being not unfrequently prescribed with alkalies, and explains that a cyanide of the metal is thus found which is not less active than the acid itself. The tyro, therefore, would be puzzled to know whether prussic acid and the alkalies are incompatible or not. Again, we are told that tannic acid may be prescribed with the protosalts of iron, though not with the persalts; but the student would find it difficult to discover *what* protosalts of iron would remain unchanged into persalts during the mixture of the ingredients, and Dr. Griffiths gives no information on the point. We are also told

[1] *Lessons on Prescriptions and the Art of Prescribing.* By W. Handsel Griffiths, Ph.D. L.R.C.P.E. Pp. 150. London, 1875.

that if bicarbonate of potash be added to a solution of tartaric acid, bitartrate of potash is thus formed and is at once precipitated, but if tartaric acid be added to the salt of potash no such result will ensue. With some explanation this statement might be useful, but Dr. Griffiths gives none, and the inexperienced reader is led to suppose that the precipitation of potash by tartaric acid depends *only* on the circumstance whether the acid be added to the alkaline carbonate or the alkaline carbonate be added to the acid, which cannot surely be Dr. Griffiths's meaning.

Mistakes and shortcomings such as those just mentioned detract most materially from the value of Dr. Griffiths's little book, which, to a Latin scholar, is very meagre, and to one unacquainted with that language is almost useless. To a good chemist, also, the rules laid down and the precepts enforced will betray numerous inaccuracies, while to a beginner in chemistry the information offered is too scanty to be of any service. The formulæ, however, of which there is a great number, are generally very good, and the division of prescriptions according to their physiological action will be found useful.

Greenhow on Addison's Disease.[1]—This volume is based on the Croonian Lectures, and embodies everything that has been written upon this disease up to the present time. It forms one of those useful monographs that the practitioner is so glad to refer to when any obscure or doubtful case of this nature occurs in his practice, or when, the nature of the case being clear, he is anxious to acquaint himself with the latest hints as to treatment. Apart from this, however, the work is of great interest, whether viewed from a practical or pathological standpoint; and we are sure the profession will feel indebted to Dr. Greenhow for his researches, and the exhaustive manner in which he has treated the subject.

Lecture I contains a short biographical notice of Dr. Addison, a history of his discovery of the remarkable disease which bears his name, and a full description of all the clinical symptoms and pathological appearances characteristic of the disease. These descriptions are drawn from the author's own clinical observation and post-mortem examination of numerous cases, and are illustrated by coloured and microscopical plates, showing the appearances of the diseased organs and discoloured skin and membranes.

These illustrations form a valuable addition to the letter-press, and facilitate materially the study of the disease. It is a feature in medical works that we should be glad to see more generally adopted.

In the first part of Lecture II the author reviews the evidence

[1] *On Addison's Disease; being the Croonian Lectures for* 1875, *delivered before the Royal College of Physicians, revised and illustrated by reports of cases, with Illustrations.* By Edward Headlam Greenhow, M.D., F.R.S. London, 1875.

proving the reality of the connection between one particular lesion of the supra-renal capsules and the clinical symptoms which are found associated with it. The second part of the lecture deals with the various misconceptions which have prevented the general recognition of the truth of Addison's discovery, and shows that they have no foundation in fact.

Thus discoloration of the skin in cases of mania at the climacteric period, cases of internal cancer, poverty and dirt, pityriasis versicolor, syphilis, long-continued hepatic disease, chronic phthisis, &c., have in many instances been mistaken for the discoloration due to Addison's disease; but, as the author remarks, "in none of them did it present the really characteristic features of the bronzed skin of Addison's disease, nor were any constitutional symptoms recorded but such as were easily referable to the morbid condition present in each case."

Disease of the supra-renal capsules not unfrequently exists without producing any discoloration of skin, but then they present totally different characters from the particular lesion of those organs existing in "Addison's disease."

Lecture III explains the author's views with respect to the nature and causes of Addison's disease. In the first part he shows the probable mode of production of the clinical symptoms, by means of the morbid processes going on within and around the supra-renal capsules, and supports these views by quoting numerous clinical and pathological facts from his own cases and from those of other British and foreign physicians. In the second part of the lecture the constitutional and local causes of Addison's disease are fully discussed. The disease is shown by pathological evidence to have a very strong affinity with the tubercular diathesis, and to have originated apparently, in a few cases, as part of a general chronic tubercular process. In a more considerable number of cases it is shown, by the same evidence, to have been caused by the spread of inflammation from diseased or injured adjacent parts; and again, in some cases, to have evidently resulted from physical shocks or temporary injuries, although there was no pathological evidence of any local lesion. Lastly, an overwhelming proportion of the cases on record is shown, by statistical evidence, to have occurred in persons of those classes which are engaged in active manual labour, or are most exposed, by the nature of their occupations, to local injuries from accident or over-exertion. The lecture concludes with remarks on the diagnosis, prognosis, and treatment of Addison's disease, which are most clear and explicit, treated in an eminently practical manner, and cannot fail to prove of interest to every intelligent practitioner who desires to keep pace with the progress of modern research.

The author with much diffidence remarks—"No one can feel more strongly than myself that the opinions I have been led to form

with respect to the obscure pathological and etiological processes in Addison's disease rest, as yet, upon an inadequate basis of facts." Still, it is to honest conscientious workers like Dr. Greenhow that we can fairly trust for further enlightenment upon this and kindred subjects. The profession owe him no small debt of gratitude for his painstaking and laborious investigations, which have certainly done much to advance our knowledge of this obscure yet fatal malady.

The value of the work is further enhanced by reports and references to the cases cited in the lectures.

Appendix A contains detailed reports of thirty-seven illustrative cases, selected out of the whole number upon which the author's conclusions are founded. It is not often we have such exhaustive illustrations of case-taking; they are models of perfection.

Appendix B contains a complete list of the 333 cases referred to in the lectures, classed in the groups into which they are divided in Lecture II for the purpose of analysis.

A full bibliographical reference is given to the publication from which each case has been extracted; and the age and sex of the patient, together with the principal facts adverted to in the lectures as bearing upon the etiology of Addison's disease, are subjoined to the reference in every case in which they have been recorded. This, we need scarcely say, will prove invaluable to all future investigators. We congratulate Dr. Greenhow upon being the author of such a complete and exhaustive monograph.

Porter's Surgeon's Pocket-book.[1]—The circumstances under which this little book has been brought before the profession are alone sufficient to secure for it a favorable reception. When a prize was offered a few years ago by Her Majesty the Empress of Germany for the best essay on the 'Practical Treatment of the Wounded in War' the author was induced to compete. His experience during the Crimean campaign, the Indian mutiny, and the Franco-German war, was such as to give him an intimate acquaintance with the subject. The adjudicators were Professor Billroth, of Vienna; Baron von Langenbeck, of Berlin; and Socin, of Basle; and these distintinguished men awarded a prize to Surgeon-Major Porter's essay. This fact in itself is enough to warrant its publication and to prove its value.

The altered conditions of modern warfare make it necessary to reconsider many important questions. Not only has surgery advanced by the introduction of anæsthetics, the use of antiseptics, the improvement of mechanical appliances, &c.; but the general progress

[1] *The Surgeon's Pocket-book; being an Essay on the best Treatment of the Wounded in War.* By Surgeon-Major PORTER, late of the 97th Regiment, Assistant-Professor of Military Surgery, Army Medical School, Netley. London, 1875.

of the arts and sciences has also brought about notable changes in the whole military system. Projectiles are very different from what they were at the time of the Peninsular war, are thrown with a far greater force, and produce very different effects; while in those countries which are furnished with railroads it is obvious that the facilities for the transport of the wounded have altered the bearings of some surgical questions. This was very apparent in the records of the recent American war. To all these questions Mr. Porter has paid particular attention. His aim has been, within the compass of a small pocket-book, to condense all the most necessary information for the use of an army surgeon. This includes rules both for the hygienic management of troops in the field, and also for the succour and treatment of those wounded in battle. Thus, for example, he has sections upon the dress of the soldier, upon foot-soreness, upon dietary scales, upon field ovens, extemporary water-filters, datrines, &c. &c.; also upon the appliances needed for emergencies in the field, the carriage of the wounded, ambulances, &c.; as well as upon all the regular methods of surgical treatment which can only be carried out in hospitals.

Surgeon-Major Porter quotes largely from the best authorities upon civil and military surgery. But in addition to this he illustrates many important points by graphic touches from his own experience, which both enliven his pages and afford evidence of the practical acquaintance of the author with the details of his subject. Thus, for example, in speaking of depressed fractures of the skull in which the trephine has been used, and of the importance of perfect quiet in the after-treatment, he mentions the following case:

"During the Indian mutiny (1857) a striking instance of this came under my observation. At the battle of Sultanpore a soldier of the 10th Foot was wounded in the head, causing a depressed fracture, which rendered him insensible. The surgeon of the regiment elevated the depressed bone, and sensibility immediately returned. The sufferer was progressing satisfactorily, when the necessity for shifting camp produced great disturbance, and brought on a train of unfortunate cerebral symptoms, from which he died."

Again, in speaking of wounds of the œsophagus, Mr. Porter relates the following incident:

"During the Crimean war a complicated case of bullet-wound of the larynx and œsophagus came under my immediate care, the particulars of which may be recorded here. At the assault on the Great Redan, the sufferer, having mounted the parapet, and while in the act of reloading on his knees, was shot from below by one of the enemy, who was immediately at the bottom of the parapet. He fell on his side, did not feel much pain, but could not speak. On arrival at the camp hospital there were two wounds in the neck; the ball entered at the pomum Adami, and made its exit at the anterior edge

of the sterno-mastoid muscle of the right side. There was very little hæmorrhage from either wound, and no displacement of the thyroid cartilage. The patient was suffering from frequent cough, with bloody expectoration, loss of voice, and nausea; bubbles of air appeared at every expiration at the wound in the larynx—that of entrance; and when he attempted to drink, some of the fluid came through the wound at the anterior edge of the sterno-mastoid—the wound of exit. This latter symptom continued for five days, and then ceased altogether; for twelve days the air continued to pass through the upper wound, and then ceased. The cough during that time was troublesome, the expectoration being very copious, which caused much loss of rest. During the first six days there was slight emphysema about the upper wound, but it gradually disappeared. . . . The wounds healed completely; but the voice was much impaired, being little more than a whisper."

Speaking of the profound collapse which sometimes results from injuries of the abdominal viscera, the author mentions the two following cases:

"The shock in these cases is of the severest description, amounting, in some instances, to prolonged collapse. The writer, on a recent occasion, saw an instance of an artilleryman being thrown from the limber while in rapid motion, a wheel passing obliquely over the abdomen and pelvis. He was immediately attacked with prolonged collapse, vomiting, pains, bloody urine, and great restlessness, from which he never rallied, and he expired in a few hours. The post-mortem examination gave evidence of extensive hæmorrhage into the peritoneal cavity, it being full of large clots. There was a fracture of the left ramus of the ischium and pubes. There was no external wound of any consequence, the skin beiug only a little congested where the wheel had passed over it."

The other instance is this:

"During the Indian mutiny a case of wound of the bladder came under my notice. The infliction of the wound by a bullet was immediately followed by a collapse, cold clammy skin, pallid countenance, vomiting, and frequent desire to pass water, which was bloody. The unfortunate sufferer never rallied, and died in a few hours."

In addition to descriptions drawn from his own experience, the writer adds to the interest of his book by referring to some of those hair-breadth escapes which are so frequently mentioned in the annals of military surgery.

Thus the little volume is made as readable as a condensed text-book can well be, while at the same time it is a useful and reliable guide for the surgeon in the field.

Transactions of Paris Surgical Society.[1]—The Surgical Society of Paris was founded in 1843. The Society, like most of its kind, is bound by its laws to publish annually a report of its proceedings. This has been done continuously since 1849, under the title of 'Bullétins de la Société de Chirurgie.' From the necessarily limited space in these publications, papers of any great length could not be fully reported, and these were brought out separately in the so-called 'Memoirs,' which appeared irregularly as supplementary to the bulletins. The Society having fortunately come to a legacy, has determined to expend some of its increased resources in improving the published records of its meetings. Since the beginning of the present year the bulletins and memoirs have been fused into one, and are published monthly at the moderate price of eighteen francs per annum.

The advantages of this mode of publication, by which a complete printed report of the meetings of the Society is laid before its members, while the discussions are still fresh in their minds and their interest unabated, are too obvious to require comment.

In addition to the original paper, abstracts are furnished of the discussion which it gave rise to—a plan which might be followed with advantage by some of our societies in this country, as the discussion is frequently of more value than the original paper.

Anger on Hypospadias.[2]—This interesting pamphlet contains an account of a complicated case of hypospadias completely cured by a plastic operation. The difficulty of such operations, and their invariable want of success, has led most surgeons to abandon them altogether. In the case here recorded both Nélaton and Rochet advised the patient to remain content with his unfortunate condition, which advice, however, he declined to take, and at last, by his importunity, persuaded M. Anger to attempt an operation for a complete eure. The patient was a young man, aged 16. The urethra opened at the angle between the penis and scrotum, and the glans was firmly bound down by a fibrous band to a spot just above this point, in such a way as completely to double the organ on itself. This latter part of the deformity was relieved without difficulty. Two transverse incisions were made about a quarter of an inch apart, to such a depth as completely to set the penis free. The edges were then brought together in a transverse direction by sutures, and the penis gently extended on the abdomen by means of an india-rubber band attached to a belt round the waist. This continuous and gentle extension proved of the greatest service, and M. Anger

[1] *Bullétins et Mémoires de la Société de Chirurgie de Paris.* New Series. Vol. I.

[2] *Hypospadias—Peno-scrotal, Complique de Coudure de la Verge Rechessement du Penis et Urethro-plastie par Inclusion Cutanée.* Par M. THÉOPHILE ANGER. Paris, 1875.

attributes to it in a great measure the perfect success of this part of the operation. Three months after this, at the earnest request of the patient, and, as before said, against the highest advice, M. Anger attempted the formation of a new urethra. For this purpose he turned up a long rectangular flap on the left side of the penis, reaching from the glans to below the abnormal orifice of the urethra. This flap was left attached on the side nearest the middle line. He then raised a similar flap from the opposite side of the penis, leaving it attached on the side furthest from the middle line. The first flap was then folded over, so that its cutaneous surface was turned inwards to form a lining for the new urethra. Six points of suture were passed through the free edge and through a small part of the raw surface, on the right-hand side, and then carried through the base of the second flap, and secured by lead buttons. The free edge of the second flap was then attached to the skin of the left side, from which the first flap was cut. The two raw surfaces were thus brought accurately into contact, and a good epithelial lining was formed for the new urethra. The skin used was, moreover, that natural to the part, and from its elasticity would allow erection to take place without producing any abnormal curvature of the organ. The result of the operation was at first somewhat disappointing. Cystitis set in as the result of the retention of a catheter. It was followed by orchitis. There was great œdema of the scrotum, and, to crown all, an attack of erysipelas. In spite of these difficulties a good urethra resulted for about one inch and a half from the glans, but the urine passed, as before, from the root of the penis.

Seventeen months after a second operation was performed to close the lower part of the urethra, and this time with complete success. The patient now passes water easily from the tip of the penis, and there is no fistula at any part. Erection is perfect, but accompanied by a very slight downward curvature.

The author draws two practical conclusions from his first failure—that the operation should not be undertaken until at least six months or a year after any previous incisions for relieving downward curvature, and that the catheter should be only retained for twenty-four hours at most, and after that the water should be drawn off as required. He thinks the method of operating might be extended to penile, tracheal, and perhaps vesico-vaginal fistulæ. At any rate, his success in this case is such as to encourage surgeons to repeat the operation when occasion arises.

Davy on Surgical Appliances.[1]—This small pamphlet csntains a detailed account of the various ingenious inventions which Mr. Davy has made in surgical mechanisms. His tubular needle, with a con-

[1] *New Inventions in Surgical Mechanisms.* By Richard Davy, F.R.C.S. 1875.

cealed reel of wire in the handle, is well known as saving much trouble in operations for vesico-vaginal fistula, ruptured perineum, &c. His india-rubber padding for splints, consisting of parallel lines of tubing fixed to a perforated sheet, is an extremely cleanly and comfortable appliance, especially in compound fractures, excision of the knee, &c. Being non-absorbent, it can be used again and again, and requires less frequent changing than the ordinary wool or tow pads. Some ingenious modifications of the ordinary Scarpa's shoe are described at some length, and are well adapted to accomplish the purpose for which they are intended.

Among the other inventions here described is a modification of Holt's dilator, in which the wedge can be introduced in two parts, so that the dilatation is greater at the stricture than at the orifice of the urethra. This seems rather a needless complication of the instrument, as no harm can possibly arise from "dilating the meatus to the same calibre as the structure." In the operation of lithotomy Mr. Davy suggests the use of what he calls a "landing-net for calculi." It consists of a net of silk or canvas, made to slide on a curved wire bent like one blade of a pair of midwifery forceps. He has frequently successfully tried it on the dead body, and if its use is found to be as easy on the living it would doubtless prove extremely valuable in the removal of soft phosphatic calculi. A sharp- or blunt-pointed tenotomy knife, some retentive catheters, some instruments for puncture of the bladder from the rectum, and a new bone-forceps, are also included among Mr. Davy's inventions.

Labarraque on Hypertrophy of Breast.—The author, while an "interne" in the wards of M. Hardy, at the Hôpital Saint Louis, had the opportunity of seeing two cases of true hypertrophy of the mamma, and being desirous of knowing the opinions of authors upon this interesting subject, he carefully collected a large number of cases, and analysed the views of the authors who recorded them. The results of this inquiry are embodied in the work. He claims no originality, but merely desires to report as completely as possible on our present state of knowledge. For this purpose he has consulted sixty-eight authors, and he publishes, more or less in detail, thirty-three cases of this somewhat rare affection. The author very properly limits the term hypertrophy of the mamma to those cases in which there is a uniform, continuous, and painless increase in size of the whole gland, unaccompanied by the formation of any adventitious growth. The conclusions at which he has arrived may be briefly summed up. As to the pathological anatomy, in the great majority of cases there is a true hypertrophy of all the elements of

[1] '*Etude sur l'Hypertrophie Général de la Glande Mammaire chez la Femme.*' Par le Docteur Edouard Labarraque. Paris, 1875.

the gland—"that is to say, an exaggerated production of fibrous tissue and enlargement of the galactophorous ducts, which are here and there dilated and narrowed again, and are distended, sometimes with transparent, ropy mucus, sometimes with milk, and sometimes by masses of casein or fat." The acini are also occasionally found to be larger than normal. From the hypertrophy of the fibrous tissue being in excess of that of the other elements of the gland, Virchow is inclined to call the disease a diffuse fibroma, whilst Velpeau separated one variety under the name of "fibrous hypertrophy."

Of 26 cases in which the age is stated, 6 occurred below sixteen, and may therefore be supposed to be connected in all probability with the normal development of the gland at that age; 15 occurred from sixteen to twenty-six; 4 from twenty-six to thirty-six, and 1 at forty-eight. It is thus evident that the disease is almost confined to the period of functional activity of the mamma. Any condition having an influence on the activity of the mamma or genital organs may act as a cause, especially disturbances of menstruation, pregnancy, or repeated sexual excitement. As possible causes, the scrofulous diathesis, injuries, and individual predisposition are mentioned. With regard to symptoms, the cases the author has collected show few but such as arise from the enormous size the gland attains to. Respiration and even locomotion have sometimes been interfered with. Complications are few. The author has recorded one case complicated with abscesses, one in which numerous cysts formed in the enlarged breast, and one in which gangrene rapidly followed a contusion. The cases here collected show the prognosis to be most unfavorable. In no case has complete recovery occurred, although occasionally the condition has become stationary, or even a slight diminution in size has been observed. Drugs seem to exercise but little influence on the growth, and uniform pressure has had but little effect. When the size of the mamma becomes so great as really to render the patient's life a burden to her, amputation is the only resource. This operation has been frequently performed, usually with a favorable result, in spite of its formidable nature. In one case M. Boyer removed both breasts successfully, with an interval of twenty-six days between the two operations; the mass removed amounted to more than one third the total weight of the body. The breasts weighed, speaking roughly, 30 lbs. each, and the whole of the rest of the body 101 lbs.

If the disease be left unrelieved, death may occur directly as a consequence of it, either by a condition of gradual emaciation and exhaustion, or by the formation of abscesses accompanied by profuse discharge. As a rule, however, the malady seems to have no direct tendency to shorten life.

The author has done his work carefully and fully, and the thanks

of the profession are due to him for thus collecting in a small compass the numerous scattered observations concerning this rare and interesting disease.

On Tubercle of the Cerebellum.[1]—This monograph is described by its author as a contribution to the study of tumours of the cerebellum. The presence of tubercle in this part is almost peculiar to infancy and children, and is more frequently met with in male than in female children. The predisposing cause is generally the same as that which favours the development of the disease in other organs, more particularly the brain. The anatomical characters are also the same as presented by tubercular deposit in other organs. Dr. Cubasch states that tubercle is more frequently met with in the grey than in the white substance of the organ; being developed at first in the vascular tissue of the pia mater, whence it extends into the grey matter and then to the white substance, of which the tumour causes the absorption or wasting. The tubercles vary in number, and in size from that of a millet seed to that of a goose's egg; occasionally the mass occupies an entire lobe of this organ, when it may be assumed that the extensive deposit has resulted from a coalescence of many smaller deposits. In proportion to the rate of growth of the tubercles is the amount of sympathy of the surrounding parts. In the majority of instances Dr. Cubasch has found that death has occurred from intercurrent disease. In many other cases the fatal result has arisen from inflammatory changes, induced in the adjacent membranes, extending to the brain itself. The several changes that arise in the brain and its meninges are discussed at some length by the author. From the table of authorities and cases cited by Dr. Cubasch it is seen, as might be expected, that the existence of tubercle in the cerebellum is in a large number of cases associated with hydrocephalus, pulmonary and mesenteric disease.

The diagnosis of this particular form of tuberculosis is, as Dr. Cubasch admits, difficult. The subjects thereof being children, the initiatory symptoms are prone to be overlooked; and it is obviously difficult to ascertain the amount of lesion of sensibility or motility. Still more difficult is it to ascertain the integrity of the powers of co-ordination in children who have not learned to walk, or who have unlearned the art, as is not unfrequently the case with them during illness. The symptoms of a tubercular tumour will obviously be generally the same as of other tumours; the diagnosis will be guided by the age and constitutional condition of the patient, the family history, and the absence or coexistence of other tubercular affections. The whole catalogue of symptoms of cerebral disease is laid

[1] 1. *Die Tuberkulose der Kleinhirns.* Von Dr. W. Cubasch. 8vo, pp. 175. Zurich, 1875.

under contribution by the author for the purpose of establishing his diagnosis. Dr. Cubasch does not lose sight of the fact that the physiology of the cerebellum is still obscure, therefore capable of affording but uncertain aid to diagnosis. Disturbance of muscular co-ordination, he points out, is a symptom of primary importance in this respect.

The histories of several cases are appended, and are illustrated by graphic illustrations of the tumours.

We would add that as the able contribution of a young but hard-working and zealous pathologist, this essay should not be overlooked by the students of nerve pathology.

Gray's Descriptive and Surgical Anatomy.[1]—The appearance of a seventh edition of 'Gray's Anatomy' is proof sufficient of its having won its way to public favour by its generally excellent character. We believe it to be the greatest favourite as a treatise on descriptive anatomy among students. They can especially appreciate the value of its very numerous illustrations, which go, as far as such aids can go, towards making anatomical descriptions easy. They show well what the student has to look for, and where to look for it, when he uses illustrations aright in conjunction with diligent examination and study of the actual "subject" in the dissecting room. No testimonial from the critic is now wanting to recommend a work which has run a most successful career during seventeen years, and six previous editions, in the preparation of which every care has been taken to increase its accuracy and render it a trustworthy expositor of the important science it deals with.

American Journal of Obstetrics.[2]—The August number of this excellent journal, which is ably edited by Dr. Munde, contains some valuable communications, and an admirable "Report on Diseases of Women for the year 1874" by the Editor. Dr. Goodell contributes a very sensible "Clinical Memoir on Turning in Pelves narrowed in the Conjugate Diameter," in which he fairly sets forth the advantages of version. A subject of even greater interest in America than here, owing to the excessive infantile mortality during the summer months in the States, is considered by Dr. Dawson in an article on "Alimentation and Gastro-intestinal Disorders in Infants and Young Children," wherein he shows the great importance of suitable food. There are other good original communications which deserve to be read, and instructive reports of the meetings of the New York and Philadelphia Obstetrical Societies, which show how commendably some of our American brethren endeavour to advance this department of medicine.

[1] *Anatomy, Descriptive and Surgical.* By Henry Gray, F.R.S., &c. Seventh Edition, with an Introduction on General Anatomy. By T. Holmes, M.A., &c. London, 1875.

[2] *American Journal of Obstetrics.* August, 1875.

The "Quarterly Report on Obstetrics and Diseases of Women and Children" is the ablest *résumé* of the subjects included in it that we have met with. It is, as such a report should be, thoroughly cosmopolitan, Dr. Munde's polyglottic acquirements specially fitting him for the execution of such work.

Transactions of the Obstetrical Society of London.[1]—The present volume very fairly illustrates the value and importance of the work done by this flourishing society. In it we naturally look for an advanced exposition of the state of obstetrical science and practice, and we are not disappointed. This society has very wisely adopted the practice of recording the discussions which take place at each meeting, and we are thereby presented with practical suggestions, reflections, and comments on the vast variety of topics embraced in a year of the society's work; for, besides elaborate papers, we observe that there are usually presented at each meeting pathological and other specimens, new instruments, and short communications, all of which afford material for discussion. Thus, the latest views of some of the leaders of the profession are, from time to time, elicited.

Among the more elaborate papers that by Dr. Tilt, the President, on "Lymphangitis in Pelvic Pathology" deserves especial commendation. It is a much-needed and very welcome addition to our knowledge of the pathology of women's diseases. "Puerperal Thrombosis" is the subject of a paper by Dr. Playfair, who has paid much attention to this grave lesion.

Dr. Gervis started an instructive discussion by a practical paper on "Retroversion of the Gravid Uterus," which we hope has done something to improve the diagnosis and treatment of this accident. "The Indications afforded by the Sphygmograph in the Puerperal State" is the title of a suggestive paper by Mr. Fancourt Barnes.

This volume also contains a valuable general index to all the previous volumes of the Society.

Loomis's Lectures on Medicine.[2]—It was formerly the habit almost entirely, and even yet it prevails largely, in the United States, to import their medical literature from England and the continent of Europe; but there is now distinct evidence that our American brethren will soon amass a series of works in medicine, the product of their own physicians and surgeons, and be less dependent on foreign sources than heretofore. In fact, they have succeeded in advancing a step farther than mere producers on their own account; for we have daily brought under our notice, especially

[1] *Transactions of the Obstetrical Society of London for the Year* 1874, being the Sixteenth Volume.

[2] *Lectures on Diseases of the Respiratory Organs, Heart and Kidneys.* By ALFRED L. LOOMIS, M.D. New York, 1875.

by the enterprising firm of Sampson Low and Co., lists of American medical treatises imported into this country, and several of which have already won for themselves a reputation amongst us. We may, for example, refer to Grose's 'Practice of Surgery,' Flint's 'Practice of Medicine,' and Dalton's 'Physiology.'

We may now add to these highly valued volumes the one now before us, as a work on the portions of practical medicine of which it treats of considerable merit, especially as a concise outline of the generally recognised principles of the medical art, well adapted to the use of students.

As the title intimates, the volume consists of lectures delivered by its author, as Professor of Pathology and Practical Medicine in the University of the City of New York. From this point of view it must consequently be judged; and neither the pathologist nor the therapeutist must look to it as a complete and exhaustive treatise on his own special subject. Moreover, from its origin, its style partakes of a colloquial character, which really renders its perusal more easy, although the critic might find occasion not unfrequently to find fault with its composition. The author, moreover, claims it as an advantage that the knowledge he has to impart has been called forth in the way of lecturing; for, says he, "I am confident that the stimulus of the lecture-room made prominent many practical points which would have been passed over had I attempted a complete and systematic treatise upon the subjects under consideration."

Confident, as it would appear, that he has done a good work by publishing the present instalment of his course of lectures, Dr. Loomis further states that it is his "purpose, at some future time, to publish, in a similar form, lectures upon other important subjects connected with practical medicine." We shall be pleased to see his purpose carried out.

In the mode of dealing with the several topics discoursed upon, no special feature is observable. He presents a definition of each malady, enumerates varieties, describes the morbid anatomy, recounts the signs and symptoms, and lays down the principles of treatment, and discusses the prognosis. Perhaps, we should say, that the differential diagnosis has especial attention bestowed upon it. In the matter of treatment he is particularly opposed to the antiphlogistic system. Even in acute inflammations of the chest he deprecates the so-called antiphlogistic measures. The revulsion of opinion which, in this country, appears to be showing itself in favour of bloodletting in some, at least, inflammatory maladies, has certainly not dawned upon the mind of Dr. Loomis. Stimulants and quinine in both croupous and catarrhal pneumonia, and opium in acute pleurisy and acute pericarditis, are the therapeutical agents he extols. Not only bleeding, but antimony and calomel, veratrum viride and aconite, are excluded from his armamentarium. Neither

does he approve of the cold compresses to the chest in inflammatory affections, as advocated by Niemeyer and other German physicians. Nevertheless, like other teachers, he is, however unwillingly, bound to the axiom that there is no rule without an exception; for, with regard to the use of venesection, he is fain to admit that "there is one condition in which it seems to me that a patient with pneumonia may be bled with advantage, and that is when there is evidence that the heart is engorged with blood, accompanied by the evidence of sudden pulmonary congestion and œdema. Under such circumstances a free bleeding, if the patient is vigorous, will unquestionably give relief. . . . It is an overcrowding with blood of that portion of the lungs which is not the seat of the pneumonia that is to determine the question of blood-letting."

In concluding this notice we must, in justice, remark upon the admirable large and clear type, which renders the reading of the volume a matter of pleasure.

Hartley on Air.—The substance of this treatise is a course of lectures delivered in 1874 at the Royal Institution. Having to address a mixed audience, many of whom were presumably unacquainted with chemistry, Mr. Hartley had in consequence to deal with his subject in a popular manner, avoiding, as far as practicable, scientific terms. At the same time, whilst conveying the facts of science, he took the opportunity of unfolding to his hearers the mode of scientific inquiry, and narrated the principal processes employed for the discovery and elucidation of the various facts connected with the chemical history of the atmosphere in all its relations to animal and vegetable life. To make his meaning clearer he has added engravings of the most important apparatus used in conducting the researches he has described; and in connection also with his record of Pasteur's investigations respecting the organisms found in air, has given several illustrations.

There are altogether five lectures and sixty-six illustrations. In his first lecture he reviews the gradual progress of the knowledge of the atmosphere in its physical and chemical characters. In the second, he gives the history of carbonic acid in air, and recounts the modes of examination employed to ascertain its proportion, and, generally, the degree of impurity existing in the air. In the third, he discourses on the laws governing air considered as a gas, or mixture of gases, and takes the opportunity to refer to the principles of ventilation. The two remaining lectures are occupied with the history of the investigations of Pasteur and others on the dust and minute organisations diffused in the atmosphere, and on the part these play in various chemical and chemico-vital processes.

[1] *Air and its Relations to Life*. By W. NOEL HARTLEY, F.C.S., &c. London, 1875.

There is a vast amount of information given in this small work, and it is given in a manner best fitted to awaken the interest and to sustain the attention of the reader. It is a most desirable book to put into the hands of the young student.

New York Academy of Medicine Transactions. — This first volume of the new series of the 'Transactions of the New York Academy of Medicine' is, in the manner in which it is "got up," a very close copy of the 'Transactions of the Royal Medical and Chirurgical Society of London,' on the model of which Society, indeed, the the New York Academy itself has been instituted. And when we come to examine the record of transactions and the papers read at its meetings, the daughter institution appears to be a worthy descendant of its progenitor, and, indeed, promises to be a worthy rival in the race for distinction and usefulness.

At present, however, neither in the number, extent, nor variety of its papers has it reached the development of the English Society. The present volume consists of 393 pages, devoted to the history of the proceedings of the Academy, and of thirty-four others, occupied with lists of officers and members. But in the 393 pages are comprised not only the substance of the papers read at meetings, but also the most important portions of the discussions following, together with the narrative of the business part of the meetings, the election of Fellows, and other not scientific matters. Further, the volume contains the transactions of three years (1871, 1872, and 1873), and consequently the yearly extent of the work of the Academy appears to be much more limited than that of the Medical and Chirurgical Society, which presents its Fellows with a goodly-sized annual volume.

Nevertheless, if not so prolific in its work, the New York Academy has an equal claim with its English prototype on our esteem by the merit of the scientific communications made to it. Still it must be admitted to be a defect that two and three years elapse in the case of the American Society before its 'Transactions' are published, and before the professional world at large has the opportunity to make itself acquainted with the labours of its Fellows.

Another point that strikes us on looking over the list of papers read, is the comparatively less attention given to surgery, for this division of the medical art is poorly represented in the list.

The list of Fellows comprises the *élite* of the profession practising in the cities of New York and Brooklyn and their vicinity, with a sprinkling of eminent practitioners in more distant towns. Among the officers and the contributors of papers best known in English

[1] *Transactions of the New York Academy of Medicine.* Instituted 1847. Second Series, vol. 1. New York, 1874.

professional circles are Drs. Gaillard Thomas, Austin Flint, Dalton, Peaslee, Fordyce Barker, Detmold, and Joseph C. Hutchinson.

The evil of the delay in the publication by the Academy of its 'Transactions' may be exemplified by the appearance, at the end of 1874, of the original memoir, read in 1871, relating Dr. Dalton's "Experiments on the Glycogenic Functions of the Liver." Did such a paper not find publicity through any other channel than the volume of 'Transactions,' it would operate prejudicially for the fame of its writer, and be disadvantageous to the interests of scientific medicine.

To attempt an abstract of the twenty-six papers or essays contained in this volume is a task beyond our power; whilst to enumerate their titles would be an unprofitable proceeding. We will therefore content ourselves with cordially recommending our readers to study these 'Transactions' for themselves, promising them that they will gain a considerable accession both to their theoretical and practical knowledge. The volume itself should find a place in every medical library aiming at completeness as a repository of works representing the steps of progress in medical science, and designed to afford material for reference to those engaged in studying the literature of any special subject in medicine.

A concluding hint may be given to the editor of the 'Transactions,' namely, that when about to issue another volume, he should revise the list of names of corresponding Fellows, inasmuch as the spelling of the names of several of the non-American celebrities is at fault, and our respected late Hunterian Professor, Richard Owen, is not, so far as we are aware, an English knight, and entitled to the prefix "Sir." The editor has evidently been moved to attach the little word from a right sense of the proprieties of things.

Ashe on Medical Politics.[1]—We rarely perceive much good result from grants of money for prize essays, and, *à priori*, we should look for less than the usual amount from money appropriated to call forth essays on medical politics. The subject itself is productive of small fruits to the profession; it is also one that is ever presenting new phases, and which appertains, therefore, rather to the periodical literature of the day than to elaborated treatises. We do not blame the author of this present prize essay for writing it under the temptation of a premium, but we question the wisdom of the much-respected surgeon who devoted the capital necessary to institute a periodical prize on medical politics.

What Dr. Ashe has written he has written well. He has thought over his subject, has expressed many commendable sentiments, has claimed in behalf of the profession a better social status than it has

[1] *Medical Politics: being the Essay to which was awarded the first Carmichael Prize by the Council of the Royal College of Surgeons, Ireland,* 1873. By Isaac Ashe, M.D. Dublin, 1875.

hitherto enjoyed; has set before it high principles of honour, and has indicated some reform in the education of its members. At the same time we must confess to not discovering aught else than we have been accustomed to read in our weekly journals and in occasional pamphlets occupied with kindred topics. He starts with two principles as necessary to be kept in view in medical politics, namely, organization and co-operation. He considers the profession to be sadly overstocked, reckoning that in Ireland the excess equals 24 per cent. of the whole number of medical men in practice. This fact is probably beyond controversy, and receives support from the circumstance, now patent to everybody, of the large influx of young Irish medical men into England and into the public services. He does not calculate the excess of medical practitioners in England, but evidently the notion has got abroad in Ireland that there is plenty of room in that country for the superfluity of doctors found in the sister island.

In our apprehension this notion wants controverting, for it is bringing about in increasing proportion the very evils of excessive supply Dr. Ashe is eloquent upon. The remedy for the excess he has to offer is not likely to be fully acted upon, and is also open to objection. It is that the several licensing bodies should co-operate in keeping under the number of members entering the profession, and chiefly do so by increasing the rigour of examinations and the cost of admission. To further aid the object in view, he commends to their adoption a more liberal organization; advocating that all their members of whatever grade be admitted to a share in their government. This, again, is but a weak remedy. Men engaged in the business of life care little for college doings, and would be as little disposed to pay the additional charges he would impose upon them for the equivocal honour of recording their votes when called upon. The little college parliaments he fondly pictures would, in our opinion, be as little popular with medical men generally as municipal councils are at present. On the woman-doctor question he ranges himself among the advocates for the admission of women into the profession; but he contends that their education should be carried on apart from that of male students. He has no fear that the addition of women to the body of male practitioners of the kingdom will augment the excess of medical supply he has bewailed; and finds not only logical arguments to support his notion, but substantiates it, to his own satisfaction at least, by algebraical formulæ.

The Irish dispensary system he commends generally to adoption, taking care, however, to point out some abuses attending it. He would insist on the preliminary education of medical students being more complete than is even now required; proposes changes in the examinations for diplomas, and suggests innovations on the general plan of paying examiners.

Most of our readers will, we apprehend, be satisfied with this very general sketch of the character and contents of the first Carmichael Prize Essay. Those of them who are thoroughgoing politicians, and cannot have too much of medical politics, will possess themselves of the volume.

Schoepff on the Climate and Diseases of America.[1]—This *brochure* must be regarded as a curiosity in medical literature. It consists of a few letters written by Dr. Schoepff, and addressed to Professor Delius, of Erlangen, long lost sight of, but now brought again to light by Dr. Chadwick.

The writer was a surgeon of the Anspach-Bayreuth mercenary legion, subsidised by England to fight its battles against its American colonists, then in rebellion, but who a few years later on achieved their independence. As now presented to us, the *brochure* is divided into two parts, the first dealing principally with the diseases of America, the second with the climate and weather of that country. The letters are dated from New York, but the writer had visited or been stationed in other "provinces," viz. Pennsylvania, Connecticut, New Jersey, Delaware, and Rhode Island (spelt by him Rhod-Eyland). In the last-named territory he passed a considerable time.

His remarks are those of a shrewd and intelligent observer, and in general are little flattering to the citizens or to the climate of the United States. To duly estimate them, we ought to bear in mind that the Americans represented the "enemies" he and his compatriots were called upon to fight, and that consequently he could not regard them or their surroundings with perfect impartiality.

At an early page he speaks of causes of degenerescence at work, and hazards the remark, since often repeated, that "it is doubtful whether even the present number of inhabitants would be maintained without the steady immigration from Europe." Among the diseases mentioned, he especially comments upon "sunstroke," and notices, in connection with the liability to its occurrence, the superior wisdom of the English army authorities in providing a light clothing for summer wear, instead of the woollen and close-fitting garments his countrymen were invested with.

Dr. George Johnson will be particularly pleased to learn that the purgative treatment of cholera was pursued a century ago. Dr. Schoepff says, "attacks of cholera, with incessant bilious vomiting and purging have been common among our soldiers during the hottest seasons, but they are arrested in one or two days by the administration of emetics or cathartics—and quite often without them—as

[1] *The Climate and Diseases of America.* By Dr. JOHANN DAVID SCHOEPFF. Translated by JAMES READ CHADWICK, M.A., M.D. Boston (U.S.), 1875.

soon as the unusual, and perhaps injurious, quantity of bile . . . has been ejected."

Among the curiosities of his experience he notes finding "in at least two thirds of the autopsies" he made, "large or small clots in the cavities of the heart and the large vessels; they are white, and of a thick viscid consistency, like the fibrinous coat on the blood in inflammation . . . and there was good reason to think that their formation was of remote date." This frequent occurrence of clots he states to be beyond his experience "in the various anatomical schools" which he had attended, and is inclined to attribute it to the habit of drinking spirituous liquors indulged in by his men. The allowance was a quart of rum every seven days, but many of the soldiers drank twice that quantity.

Another curious piece of information obtained is, that the people of Rhode Island were opposed to inoculation then generally practised as some protection against the havoc of smallpox epidemics; and that they were wise enough to practise quarantine, sending away any affected or supposed to be affected by variola to a small adjacent island, separated by a strait only "fifty paces wide." If we may admire the wisdom of isolating the contagion, we cannot speak in praise of the manner of dealing with its victims, who, according to Dr. Schoepff, were allowed to shift for themselves on the little island as well as they could, with the assistance of a keeper, chiefly occupied in collecting and transporting the supplies of food and necessaries deposited on the shore for their use, by boats from the opposite side. A physician rarely visited the forlorn patients, being afraid of carrying back the contagion with him, and consequently was generally satisfied with inquiries after them made from the opposite shore.

Dr. Schoepff's experience of the climate led him to most unfavourable conclusions respecting it. After remarking on the extremes of temperature of different seasons, and on the variability occurring every few days, he observes, that "if America should ever have a Thompson . . . he cannot imagine which season of the year the poet would find it worth his while to celebrate. The only moderately agreeable months are September and October." And, moved with disgust at the country and climate at large, and sighing, no doubt, for dear Fatherland, he tersely expresses his opinion thus of the land of his expatriation: "To judge from the variations of the weather, one would think that this stretch of territory was transported every year from under the line up to the north pole."

We have quoted enough of this tractate to indicate to our readers the character of its contents and the abundance and interest of the information conveyed.

Sanitary Report of Phildelphia.[1]—The sanitary reports of American cities rather indicate what may and should be done for the public health, than the results of definite and coercive legislation. There is much good work done under a species of semi-authority and sufferance, and by volunteer exertion, but the plaint is the lack of a central authority and of administrative power to make sanitary supervision an effective reality. The water supply of a considerable portion of the city of Philadelphia is abominably polluted, but no sufficient authority is found to remedy it; there are numerous and deplorable nuisances, but no efficient inspectors; there are many factories and workshops, but no laws to secure their hygienic condition, or the physical well-being of those employed in them; there is evidence unmistakable of the sale of unwholesome and adulterated food, but it is no body's business to meddle with it and protect the public; there are on all sides complaints of building operations in defiance of sanitary law, and no one with authority to attend to them and take action against them; and lastly, intramural interments stand condemned in all civilized communities, but the public authorities of the American cities have no power to stop them.

Sanitarians in this country make loud complaints of the indifference of Parliament to their representations: it would mollify their injured feelings to consider what the sanitarians of the United States have to contend with under a republican government and almost universal franchise. If they have to contend against the inertness and indifference of honourable members in Parliament, they are happily rescued from the influence of the short-sightedness, the mistaken notions of economy, the obstinacy and the wilfulness of the masses who mistake licence for liberty, and who can in a local legislature meet with time-serving politicians to abet the cause of ignorance and sanitary blindness.

Considering the conditions under which the members of the Board of Health of Philadelphia have to conduct their operations, it is most creditable to them that they are able to present a report such as that before us, containing much information of great moment on vital statistics.

During the year 1874, the birth-rate increased and the death-rate decreased; but in the same period the number of marriages diminished. The population was 775,000, and one child was born alive to every 41·90 persons; one person out of every 58·36 was married, and one in every 50·85 of the population died. The number of living births to every thousand of the population was 23·86; the number of

[1] *Report of the Board of Health of the City and Port of Philadelphia, to the Mayor for the Year* 1874.

deaths 19·66 per thousand; and the excess of the birth-rate over the death-rate was 4·20 in a thousand. An examination of the census taken in 1860 and again in 1870 shows, that the coloured population of the city is almost stationary. At the same time the ratio of living births to 1000 living is, in the case of the white inhabitants, 24·07; and in that of the coloured only 16·70. A high birth-rate prevails during the early part of the year, and is succeeded by the lowest birth-rate of the year, which invariably occurs in the month of April. The birth-rate again increases in the third quarter, and continues above the average rate through the remaining months of the year. The autumn and winter months are invariably the most prolific; the spring months the least so.

With respect to infant mortality the tables show that, in the period of five years, from 1870 to 1874 inclusive, the mortality per cent. of infants under one year of age was 25·16; of children generally under five years of age, 40·49; and of those under ten, 44·15. In other words, one fourth of the deaths annually in the city were among children under one year of age; over two fifths among children under five; and about nine twentieths, or nearly one half of the total number, among those under ten.

Among the assigned causes of child mortality, "cholera infantum" heads the list, after it come marasmus, debility and inanition, then convulsions, and following it in order inflammations of the lungs and of the brain, and scarlet fever. The average maximum mortality of children under five is in July, although that in August in some years is as high.

With a few quotations of the tables showing the death-rate in some of the principal cities of the United States, we must close the notice of this valuable report. Charleston heads the list by its rate of mortality, viz. 38·96 per 1000 of the population; Savannah follows next with 35·38; Troy stands at 29·00, and New York at 27·61. The lowest rate in the list is found at Toledo, where it is only 10·90. The statistics for the district of Columbia are given, so as to show the relative mortality of the white and of the coloured population; that of the former being only 15·71, whilst that of the latter reaches 30·77.

If these last-quoted figures do even approximatively represent the relative proportion of deaths generally in the States among the white and the coloured inhabitants, we can at once understand why the coloured population of Philadelphia does not increase, and may further deduce some sinister conclusions as to the future of the black race in the United States. And ethnologists should be able to derive some lessons touching the admixture of heterogeneous races from the self-same and collateral facts.

The Retrospect of Medicine.[1]—We have much pleasure in keeping this old-standing and useful retrospect of medicine before our readers. The extent of medical journalism is now so considerable that it is impossible for any practitioner to keep himself acquainted with more than a tithe of the suggestions and observations on pathology and practice to be found in the many periodicals now placed before the profession. Consequently, medical men must feel deeply indebted to such gleanings from those storehouses of professional information as are supplied by the indefatigable editors of this 'Retrospect.' These gentlemen, indeed, pursue the even tenor of their way in restricting their abstracts to British medical literature, although, as we have before pointed out, this limited retrospect on their part does not faithfully fulfil the programme of their title-page, that their half-yearly volume contains "a retrospective view of *every* discovery and practical improvement in the medical sciences." The word we have put in italics is too pretentious, and it would be more correct to define their work as a survey of the discoveries and improvements in medicine made in Great Britain.

Dictionary of Medicine and Surgery.—With commendable speed this magnificent Dictionary of Practical Medicine and Surgery continues to be published by the enterprising firm of Baillière and Sons, of Paris, under the able editorship of Dr. Jaccoud.

The nineteenth and twenty-first volumes are now before us, containing articles by (among other well-known names) Abadie, Beni-Barde, A. Foville, Hardy, Després, Hirtz, Luton, Tardieu, Chauvel, Math. Duval, and Raynaud.

In the nineteenth volume Abadie describes fully the anatomy and pathology of the iris and its operative surgery. There is an exhaustive paper on iodine, occupying fifty pages, by Barrallier and Buignet; Luton and Després present a complete examination of the medical and surgical anatomy and pathology of the intestines, with an account of the surgery of those viscera; Heurtaux discourses fully on cysts; Hirtz on intermittence and intermittent fevers; whilst Poncet and Sarazin and Chauvel address surgeons on all points connected with the anatomy, healthy and morbid, of the leg and inguinal canal, and with the operations practised on those parts of the body.

The twenty-first volume contains very complete articles on the anatomy of the hand by Math. Duval, and on the operative surgery of that member by Chauvel, illustrated by woodcuts; a lengthy essay on disease, viewed generally, is contributed by Maurice Raynaud; whilst the lymphatic system, both in health and disease,

[1] *The Retrospect of Medicine: being a Half-yearly Journal of every Discovery and Practical Improvement in the Medical Sciences.* By W. Braithwaite, M.D., and James Braithwaite, M.D. Vol. lxxi, January to June, 1875. London, pp. 384.

receives ample consideration from MM. le Dentu and Longuet. Two other articles may be singled out for notice, by reason of their importance and completeness, namely, those on the anatomy, physiology, and pathology of the jaws and of the breast; the former of the two by Després, and the latter by Lannelongue.

It remains for us only to repeat our highly favorable opinion of this great work, and to wish it such an amount of success that its enterprising publishers and contributors may reap their due reward.

Medico-Chirurgical Transactions.[1]—This annual *compte-rendu* of the chief medical society of this country, whilst being creditable to the society, presents no communications which rank above the average—none which impart to it singular importance. The greater part of them are occupied with medical doctrine and practice, and the whole of the papers partake of that practical type which is characteristic of the British School of Medicine. They are calculated rather to perfect our acquaintance with disease, than to develop new doctrines in pathology.

The papers published number in all fifteen. Those not concerned with the record of clinical experience, and which may be set down as contributions to pathological science, are: the essay on "Urinary Crystals and Calculi; being observations on some of the circumstances determining the forms of crystaline deposits in urine, and on some of the conditions under which renal and vesical calculi are produced,' by Dr. W. M. Ord; the 'Observations upon the Elimination of Urea in certain Diseases,' by Mr. Samuel West; and the shorter paper 'On the Histology of the so-called Nutmeg Liver,' by Dr. J. Wickham Legg. Also Dr. Galabin contributes an account of the construction and use of a new form of cardiograph.

We will not single out any of the clinical dissertations for special remark; but will refer our readers to the volume, which will be found to well repay their study.

Pathological Society Transactions.[2]—All our readers are familiar with the 'Transactions of the Pathological Society,' and know that by referring to them they may obtain the record of cases and a description of morbid structure which may greatly aid them in the understanding of pathological processes, and may, to some of their number, furnish material leading on to the discovery of pathological laws or principles. The classification of the specimens exhibited under the particular heads of the anatomical and physiological system they appertain to, is a great help to those who wish to refer to the volume. The introduction of figures in illustration is another excellent feature.

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[1] *Medico-Chirurgical Transactions published by the Royal Medical and Chirurgical Society of London.* Second Series, vol. xl. London, 1875.

[2] *Transactions of the Pathological Society of London.* Vol. xxvi. Comprising the Report of the Proceedings for the Session 1874-75. London, 1875.

The *pièce de résistance* in the present volume is the "Discussion on the Germ-Theory of Disease," which is reported at length. We trust that the Society will prosper and grow in usefulness, ever adding to the store of well-observed facts from which we must deduce both the principles and practice of our profession.

Transactions of the Philadelphia College of Physicians.[1]—This volume consists of papers read before the Philadelphia College of Physicians, between April 1874 and June 1875. So long ago as 1793 the College commenced the publication of 'Transactions,' but for some cause, not stated, this commendable proceeding soon came to an end, and was not recommenced until 1842, between which year and 1849 three volumes appeared. A second series, constituting four volumes, was put forth between November 1850 and February 1874; from 1857 to 1874 the 'Transactions' having first appeared in the 'American Journal of the Medical Sciences.' We trust that the present venture by the College of publishing its 'Transactions' in the form of an annual volume will prove a success.

After the preliminary lists of officers, fellows, and associates of the College, follow twelve papers on medical and surgical subjects which have in the course of the year been read at the College meetings. Probably the two which will be perused with most curiosity and avidity are those on the autopsy on the bodies of the Siamese Twins, and on the propriety of an operation for the separation of the two brothers. The former is contributed by Dr. Harrison Allen, and the latter by Dr. W. H. Pancoast. These papers are well illustrated, to show the relations of the organs in the twins and the character of the band which united them together. There was marked lateral curvature in both bodies, and in each the ribs were twenty-two in number, consisting of seven true and four false. There were many irregularities of the internal viscera, both in the matter of position and of form and structure.

It would trench too much upon our space to abstract the account given by Dr. Allen of the construction of the band uniting the two men; and, in fact, a verbal description would be obscure without the accompanying illustrations.

As no examination of the brain was allowed, the conclusion arrived at at the time of the decease of Chang, that his death was due to a cerebral clot, and that to this cause was attributable its suddenness and the occurrence of hemiplegia, could not be confirmed. His twin brother is supposed to have died from syncope induced by fright—"a view which the over-distended bladder and the retraction of the right testicle would appear to corroborate."

"The circulation in each individual of the twins was practically

[1] *Transactions of the College of Physicians of Philadelphia.* Third Series, vol. i. Philadelphia, 1875, pp. 192.

complete, since the demonstration of continuity between the portal systems, although satisfactory, invites the conclusion that the amount of blood which passed from one to the other side of the band must have been, in the condition of the parts at the time of demonstration, very inconsiderable, and was not competent in all probability to modify the performance of any act of the economy. In the fœtal and early period of extra-uterine life the vessels must have been more capacious, and associated with a large tract of liver tissue. It follows, all things being equal, that an attempt at division of the band in early life would have been accompanied with more venous hæmorrhage than at any subsequent period."

Dr. Pancoast discusses at length the oft-put question as to the propriety of separating the brothers. He found that neither the vascularity of the band nor the narrow isthmus of hepatic tissue would have proved an obstacle to division. Likewise it would have been easy to cut through the ensiform cartilages at their line of junction; but besides the connecting structures just mentioned, there was also a union between the diaphragms of the two individuals, and Dr. Pancoast believes that to have cut through the band, so complicated in structure, would have been attended with great danger. In the later portion of their life—they reached the age of 63—any operation would have been more hazardous, as the autopsy showed a very general and extensive atheromatous state of the arteries.

"We are," writes Dr. Pancoast, "not sufficiently informed as to the condition of the twins in childhood to decide whether an operation would then have been justifiable. But if it ever was to be performed, then was the proper time, before they had acquired their full mental and physical development."

After referring to the parallel cases recorded by Dr. Böhm and Dr. Fatio, in which an operation was performed, Dr. Pancoast proceeds to say that should a case like that of the Siamese Twins occur again, and an opportunity be presented of operating in infancy, he would undertake to do so. Moreover, he holds that on the death of Chang it would have been the proper thing to have at once effected a separation.

It should be mentioned that the uniting band contained peritoneal pouches, and that in early fœtal life the peritoneal cavities of the two were, without doubt, continuous. Dr. Allen describes two hepatic pouches in nearly the same place, and also two pouches called umbilical from their association with the round ligament.

The other papers contained in this volume are: the "History of a Case of Adenoid (Hodgkin's) Disease, with Remarks, and an Analysis of Fifty-eight recorded Cases," by Dr. James H. Hutchinson; a case of "Fracture of the Neck of the Scapula," and one of "Excision of the Elbow," by Dr. Ashhurst; a notice of a "New Opera-

tion for certain cases of Cleft Palate and Bifid Uvula," by Dr. W. S. Forbes; a "Record of Experiments on the Laryngeal Nerves and Muscles of Respiration in a Criminal executed by Hanging," by Dr. W. W. Keen; a contribution on "The Use of Nitrite of Amyl in various forms of Spasm," by Dr. Weir Mitchell; a case of "Acute Tetanus successfully treated by the Inhalation of the Nitrite of Amyl;" "Remarks on Diabetes Insipidus, and its Treatment by Ergot," by Dr. Da Costa; case of "Encysted Dropsy of the Peritoneum, with Suppuration," by Dr. Ewing Mears; and lastly, "Remarks on Quinia as a Stimulant to the Pregnant Uterus," by Dr. Albert H. Smith.

Dr. Weir Mitchell adheres to his belief in the value of nitrite of amyl in arresting epileptic attacks. The difficulty in obtaining its good results arises from the rarity of cases in which there is time to secure its full inhalation. "I have never seen it fail," writes Dr. Mitchell, "where there was time to use it." He does not consider it to be established that this agent has, "in most cases, any capacity to lessen the probability of a return of the fits, but of its power to arrest the actual convulsion there can be no doubt."

Of its probable value in other convulsive diseases he speaks very hopefully; and states also that he has made frequent use of the nitrite "in a variety of forms of disease, chiefly spasmodic, some of them hysterical, and some of indeterminate birth and relationship. Its influence over cases of hysterical angina is as well marked as in those of men, or in non-hysterical attacks of this disorder. I have twice employed it in forms of disease which are akin to angina, are not infrequent, but lack a distinct name."

This instructive paper concludes with some sagacious suggestions of the application of the nitrite of amyl to the diagnosis of cerebral disorders.

The contribution by Dr. Forbes of a case of acute tetanus successfully treated by nitrite of amyl might be considered a rider to Dr. Mitchell's paper. It was a case of extensive burn, and the tetanus set in violently on the fourth day after the accident, with a temperature of 102°, a pulse of 133, and a respiration of 32 per minute, marked opisthotonos and paroxysms of spasms. The amyl was given, by inhalation, in doses of five drops twice a day, was continued 46 days, and happily restored the patient to health, no other agent being employed beyond good nourishment. Dr. Forbes recounts other cases in which this agent has been employed, and occupies the latter portion of his paper by a disquisition on the pathology of tetanus.

The use of ergot in diabetes insipidus is illustrated by Dr. Da Costa, by a case that was under his care. He first tried the drug hypodermically, but found it when so administered to cause much local disturbance; he consequently gave it by the mouth after-

wards for eleven days in one drachm (of the fluid extract) doses, and then increased to two-drachm doses, three times a day. From the time its use was commenced improvement followed. It will be an important gain in therapeutics to have the virtues of ergot established as a cure for the frequently intractable and fatal disease, diabetes insipidus.

The profession stands indebted to the College of Physicians of Philadelphia for this instructive volume of 'Transactions.'

Chadwick on Christianity and Paganism.[1]—This book does not fall within the compass of works we undertake to review. It is a religious composition, setting forth an odd creed of its author, which, in his estimation, is Christianity, whilst the general belief of professing Christians is Paganism. It is with him a pagan and reprehensible doctrine to hold the existence of an immaterial, immortal soul; yet, as a stout believer in an after state of existence, he upholds man's destiny to such an existence, but in what shape man is to enjoy it he leaves us in oblivion. With these few memoranda we must leave Dr. Chadwick to the class of readers who can enjoy the popular amusement of practising permutations and combinations of texts of Scripture so as to evoke the most extraordinary results in theology and religion.

Transactions of the Clinical Society of London.[2]—This last-published volume of the 'Transactions of the Clinical Society of London' is on the same model as previous ones, and contains an equally interesting collection of records of cases, contributed by the Fellows. The one which of all the rest will command most attention as a medical curiosity is the "case of excessive and long-maintained high temperature after spinal injuries, with recovery," by M. J. W. Teale. Its remarkable features have already been largely commented on in the various periodicals and need not be reproduced here. The postscript to the paper as read before the Society notes that slight exertion, nearly a year after the accident and spinal injury, caused a rapid and curious rise of temperature, so that from 99° to 101°, the normal range with her since her recovery, it will run up to 103°.

As usual, the account of the general work of the Society is preceded by the annual address of the President, and as the holder of the office happens to be Sir W. Jenner, the address will be referred to and read with pleasure and profit. He opens his address by allusion to the specified object of the Society, viz. "the collection of reports of cases, especially of such as bear upon undetermined

[1] *Christianity versus Paganism. Seven letters.* By JOHN CHADWICK, M.D. London.

[2] *Transactions of the Clinical Society of London.* Vol. viii, 1875, pp. 192.

questions in pathology or therapeutics;" and remarks thereon, that so far as his "knowledge extends, there is no disease, even the most common, respecting which there are not undetermined questions in regard of its pathology and its treatment." The same he avers respecting the etiology and the natural history of diseases at large. Taking this expression of opinion as his text, he enlarges upon it, showing how far we are off a perfect or clear knowledge of the etiology of infectious diseases, even of those that have been most fully studied. After commenting on this matter of etiology he turns to the state of our knowledge of the treatment of disease, and to the extraordinary divergence of opinion on this subject even with reference to common diseases, uniform, so far as we know, in their pathology. He points out the kinds of evidence to be adduced in favour of the influence of any drug on the course and termination of a disease; insisting upon a thorough investigation of each individual case and the securing a careful weighing of each special feature it presents, and likewise upon the necessity of remembering "that all acute diseases run a more or less definite course, and that the tendency of all internal acute diseases occurring in the previously sound, and not of traumatic origin, is, if left to itself, to terminate in health." To this individual observation of cases of disease, with the view of testing the value of particular modes of treatment, he would add the "analytical" method, whereby the reputed efficacy of particular drugs should be subjected to experiment and analysis by numerous observers in properly selected cases, and he throws out the excellent suggestion that "committees might be formed from members of this Society" to carry out the researches in hospitals, where alone the necessary numbers of the particular class of cases required can be looked for.

Sir W. Jenner concludes with some most sensible observations on the treatment of acute specific or epidemic diseases, animadverting on the irrational vaunting of specific remedies for them. In his apprehension, "there are no grounds for expecting that a cure will ever be found for diseases of this class, i. e. for expecting that a drug or medicinal agent will be discovered capable of arresting the progress of the organic changes which, set in motion by a special cause, and following each other in definite and ascertained order, constitute what we call an acute specific disease. For in place of being diseased actions, these several organic changes, the evidence of which we call symptoms, are, so far as our knowledge extends, processes, the first of which being called into action by some external cause—e. g. the poison of the disease—are essential for the restoration of the intimate organic changes to the order and intensity of which constitute health." He illustrates his meaning by reference to cholera, and further on rightly says. "The physican cannot transmute by a special drug the sequence of processes which

we call, say, an attack of cholera, into the sequence of processes which we call health, any more than the chemist can transmute iron into gold."

We need quote no more to awaken our readers to a due recognition of this most philosophic and valuable address, happily called for from its author as President of the Society, by precedent and custom.

Atlas of Skin Diseases.[1]—Three parts of this important illustrated treatise on skin diseases are now before us. It may be regarded as a bold venture on the part of the author and publishers, seeing that the great medical book-publishing society—the Sydenham Society—has, during several years, been bringing out a folio reproduction of the valuable plates of Hebra, in illustration of the text of the elaborate treatise on diseases of the skin (also published under its auspices) by the same distinguished German physician. But notwithstanding the advantages accruing to a publication brought out by a large and influential society, from the possession of ample funds and the absence of risk commercially, we are inclined to think that our countrymen generally will give preference to this private English venture; not simply, however, because it is a home production, but by reason of the manner of its execution, the excellent delineation of disease, and the natural colouring of the plates.

From the advertisement on the cover, we learn that the work is to be completed in eighteen fasciculi, containing seventy-two plates, and each plate one or more figures. One part is to appear every month, costing six shillings and sixpence; which besides the pictures will also contain letterpress description of the diseases represented. We learn also that the work is based upon the old and most esteemed 'Delineations of Cutaneous Diseases,' by Willan and Bateman; a classical work never out of date, and an honour to the medical literature of our country. "Of the seventy-two plates, more than half will be original, the remainder being those used in 'Willan and Bateman,' but in many cases improved so as to more faithfully represent the appearances of disease. The new plates have been made from original drawings . . . The letterpress is entirely new."

In the accuracy and value of the latter the subscribers may have the fullest confidence, since it is from the pen of Dr. Tilbury Fox, one of the foremost in the rank of physicians who have made cutaneous diseases a special study, and one who has the great advantage of experience and observation in the wards for skin diseases at University College Hospital.

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[1] *Atlas of Skin Diseases: consisting of a Series of Coloured Illustrations, together with Descriptive Text and Notes upon Treatment.* By Tilbury Fox, M.D., F.R.C.P. London, 1875.

We trust that the publishers will obtain the large circulation for this great work which, they tell us, they anticipate.

Lindsay on Asylum Pensions.[1]—The interest of this book will be with those engaged in the care and management of asylums. It is largely made up of tables, setting forth the incomes and wages paid in the several asylums of the kingdom to their officers and servants, and also the number of pensioners and the allowances made them.

The object of the author is to make this information the common property of the managers or committees of asylums throughout the United Kingdom, so that they may have proper data for fixing both salaries and pensions.

Among outsiders the notion has prevailed that our public lunatic asylums offered good berths to medical men. The returns made with reference to the income and allowances given to some asylum superintendents show that the notion is not ill-founded. Moreover, the value of such appointments is much enhanced by the power entrusted to committees of visitors to grant pensions. Dr. Lindsay would have fifteen years' service entitle a superintendent or other officer or servant in an asylum to a pension for life, equal in amount to two thirds the total money-value of office at the date of retirement.

Small blame to Dr. Lindsay for standing up for the emoluments of asylum officers. We hope he may be successful in his efforts to obtain a liberal scale of pensions, and the establishment of the right of old servants to superannuation. Hitherto too much latitude has been given to committees by a sort of permissive legislation; and the consequence has been unfairness and great inequality in different establishments. He considers asylum superintendents in position and emoluments should be reckoned to stand on the footing of a physician in moderate, general, or special practice in a large city, or in good practice in a provincial town; and he assesses the income of such practitioners at £2000 per annum; intimating that if it be not so much it is their own fault. We fear Dr. Lindsay's assessment is too high, and that many such practitioners would, so far as emolument is concerned, be happy to exchange it with many asylum superintendents who have not half that income.

Effectual Reform in Man and Society.[2]—It is from no want of specifics for the moral and social regeneration of mankind that we are still denizens of a wicked world, and men at large are at the

[1] *Tables illustrative of the Practice of British Hospitals for the Insane, as regards the Superannuation of all Classes of Officers and Servants.* By W. LAUDER LINDSAY, M.D. London, 1875.

[2] *Effectual Reform in Man and Society.* By HENRY TRAVIS, M.D. London, pp. 167.

present day not very ready to believe in and accept a new one. Most of our readers will have heard or read of the "New Moral World" devised by Mr. Owen at the beginning of the present century, and attempted to be realised at New Lanark, in Scotland, and at "New Harmony," in the United States. The attempts were more or less transparent failures; but, says Dr. Travis, not from defect in the general principles of the scheme, but from an error into which Owen was betrayed as an individual. However this may have been, the social system of Owen has wellnigh dropped out of view; nothing like justice, according to Dr. Travis, having been done to it. The purpose of the present volume is, therefore, to place the system again before the public, purged of the errors formerly associated with it, and the conviction is strongly expressed that it is a system necessarily on the eve of adoption in civilised nations, because matters have by the progressive deterioration of social life come to a dead-lock.

Surrounded as we are by so many and grievous conditions of moral and social life, it is quite refreshing to read of the entire metamorphosis of the race which is to follow the inauguration of the new system pourtrayed, and at the same most encouraging to know that the secret of this system is most simple and comprehensible. There is no need of a preliminary purgation of society by blood or fire, after the fashion of the Communists, but mankind are to be drawn to it by the silken cords of enlightened benevolence. The "root-cause of good" is to be substituted for the "root-cause of evil;" the former to be arrived at by an intelligent appreciation of the fundamental truth, "that the formation of man's determinations (and of his opinions and his character) is dependent upon conditions in the individual, or internal, and in the outward means by which he is influenced, or external," and of the accessory and included truth, "that man forms his determinations, or is a personal agent in the forming of them."

By the recognition of these truths we can arrive at the knowledge of the causes of evil and of good to man. Man produces the evil; but he is caused to produce it by his surroundings and faulty education, with its consequences, the formation of injurious *habits* of thought and feeling; and man must be reclaimed from his evil courses by instruction in the knowledge of the fundamental truths, and by arriving at the conviction that his own happiness and that of society hinges on the maxim, "to do nothing to others which he would not have others to do to him, and to do to others in all things as he would have others do to him."

Thus the new character to be acquired is twofold: intellectual, by the possession of true knowledge; and moral, by the union of this knowledge with the desire to promote the happiness of every individual, in enlightened benevolence.

Such is an outline of the principles of this new leaven which in Dr. Travis's opinion is to leaven society. They have not the charm of novelty; but have been preached to mankind by one philosopher and another for ages, and we regret to add, with small effect. The only aspect in which they have some novelty is the greater insistance upon education from infancy under the conditions of "enlightened benevolence," and the isolation of the disciples of the doctrine in communities regulated by its principles.

That much may be effected with submissive subjects by a complete system of autocratic government—at least autocratic in the first instance—and with children by education from infancy, cannot be denied. The results of Owen's experiments at New Lanark are decisive on this point. But the important problem is, how far the system proposed is applicable, and how far it is likely to be effectual for the mass of mankind? It is in regard to these questions that the system appears to be at fault.

It is an excellent system on paper; but it too much presumes on human creatures being automata; and assumes it as a fact, that, granting that the external circumstances of education and the external surroundings are the same and excellent in all ways, the individual character produced under them will in all cases be the same and satisfactory; and that in their determinations individuals will always be guided by the principles they have been trained in, and be never the sport of passion or selfishness. It also assumes that all minds are equally educable and open to beneficial influences, and that deficient and disordered minds do not enter in the community.

Further, the education of infants and children is represented almost as a mechanical act; they are rather the children of the community than of their parents; for they are withdrawn at the earliest period of independent existence from their parents, and subjected to the example and regulations of school life and of its surroundings. Dr. Travis assures us we may "educate the young with certainty to acquire a delightful character, under the guidance and example of enlightened benevolence and by other beneficial influences;" the only formula required being to place children in the common playground and to tell each of them, so that they may grasp the dictum, "never to injure his playfellows, but, on the contrary, that he is to contribute all in his power to make them happy."

This removal from their parents the author also looks upon as an advantage of his system; as setting the parents free from the care and trouble of their offspring, and allowing them to pursue their occupations or recreations unimpeded by domestic cares and interruptions. But he loses sight of the other side of the matter, and takes no heed of the disadvantages of removing children from those parental influences which constitute one of the chief bonds of society and the basis of the dearest relationships of life. Besides this, we

are unbelievers in the happy results to be obtained from this mechanical style of education. We have a notion that there is something else in children worthy of cultivation over and above the intellect, and that to turn out a crowd of children duly certified to have conformed to this or that code of regulations and influences, and to have been crammed with approved facts and principles, is not to make usèful and moral citizens, or independent and self-reliant ones.

Lastly, there is a weakness in the system discoverable in the admission that intractable members of the society may be encountered who will not duly imbibe the influences of intelligent benevolence. For these recalcitrants an outside world is necessary for banishment. But, unluckily, such an outside world must be an antagonistic society to the happy land of intelligent benevolence, will procreate an uneducated offspring placed under the most unfavorable influences, and will possibly endeavour to propagate its evil, rebellious principles even among the citizens of the social paradise. And we cannot shut our eyes to the vision of battles between the inhabitants of the two opposed regions, parallel to those which Milton has told us of with so much genius and imagery. On the other hand, we hold the opinion that, could the principles of the Christian religion be indoctrinated from childhood, and were external influences rendered more consonant with them, much more might be done towards the reconstruction and regeneration of society than by the system advocated by Dr. Travis, or than by any other cold, philosophic panacea. The former supplies an element of enthusiasm to the character, of which the latter is destitute. In fine, we must decline to regard "effectualism" (the name Dr. Travis would give his favourite and improved system in preference to Owen's title, and the one, Socialism, it was popularly known by) as the remedy for the evils of our present social condition. At the same time, we shall be delighted to see the education of children more fully and rationally carried on, and the external circumstances or influences surrounding them in their families and in the world greatly ameliorated. Nor should we be otherwise than pleased to know of the formation of oases of social and moral advancement, in the shape of villages modelled according to the plan of Dr. Travis, even though the very mundane plan of forming a limited liability company for their institution, as he suggests, were to be adopted.

Medicinal Plants.[1]—The enterprise which marks the production of this excellent illustrative work redounds much to the credit of Messrs. Churchill, the publishers. To carry on to completion a work of this sort involves a large capital, and is indicative of great faith in the profession and of a thoroughly well-understood need of it as well for practitioners as for students.

Undoubtedly, there is much need of an illustrative book of medical botany, for it is many years since one was published, and the copies of such in existence are rather cherished specimens in the libraries of book-collectors than articles available to the student.

As yet we have only two parts before us of this new publication, and the publishers have not committed themselves to any engagement as to the total number of which the work will consist when completed; but it is quite clear it will be a treatise of considerable magnitude.

To include all "medicinal plants" would be wellnigh equivalent to including most plants known to us; for very few is the number to which medicinal virtues have not been ascribed. It was therefore necessary for the editors or writers to make a selection, and this they have done, using as a foundation the British Pharmacopœia, all the plants in which will be figured. But besides these a number of others, included in the Pharmacopœia of the United States and of India, or which are in common use or of value in the arts, will be also illustrated and described. As far as possible, the advertisement further states, the figures will be drawn from living examples, and "each plate will be accompanied by letterpress, comprehending a full description of the plant in plain scientific language, its nomenclature, geographical distribution, &c., as well as an account of its properties and uses, with full references to previous descriptions and figures, and to more special treatises."

Looking to the execution of the work we may congratulate the subscribers on its general excellency, as well as regards the figures as the letterpress. Each part is supplied at the small price of five shillings, and contains eight coloured plates, representing the plant, or a portion of it sufficient to distinguish and identify it, and also illustrations of its flowers and fruit, in section and otherwise. Plants having minute flowers, or whose inflorescence is crowded, are at all times difficult of illustration; and it is only fair to remember this when we come to criticise such plants as the mints, where the whorls of minute flowers are so dense or closely packed that the distinctive features of individual flowers are not distinguishable. However, where this difficulty of illustration obtains we have still

[1] *Medicinal Plants: being Descriptions, with Original Figures, of the Principal Plants employed in Medicine, and an Account of their Properties and Uses.* By Robert Bentley, F.L.S., and Henry Trimen, M.B. London, 1875.

the general habit of the plant to serve for its identification, and this condition is in the plates before us well rendered by the artist.

The typographers deserve great praise for the excellence of their work. The editors have also well performed their task, contriving to present in a moderate compass a general history of each plant, its botanical characters and accredited properties and uses, with notes on its active principles. We must, however, take exception to the substitution of the word "official" for officinal; and, though in a less degree, to that of the term "medical" for medicinal. These words are not rightly interchangeable, for they do not express the same idea.

We commend this valuable work to our readers, and trust that they will give liberal support as subscribers to it, and thereby encourage both publishers and writers in the arduous and costly undertaking.

A Guide to the Microscopical Examination of Drinking Water.[1]— The object of the author of this book appears to be to display by diagrammatic drawings the forms and characters of suspended particles—mineral, vegetable, and animal—found in water. In his title-page he writes *drinking* water, but we apprehend he does not wish to convey to his readers the notion that all or many of the organisms he figures are to be found in water supplied to communities for drinking purposes, whether from wells or reservoirs. He must certainly mean that such beings are to be found in streams and ponds and ditches, from which a thirsty soul might from necessity drink. We hope our water analysts will ever look in vain for many of the specimens of living forms displayed in this book. It would wellnigh drive a teetotaller into perpetual alcoholic imbibition to suspect he was constantly drinking in such ugly and formidable-looking animals; whilst the votary of Bacchus would find ample grounds of apology for his devotion.

On the other hand, did Dr. Macdonald propose to represent the denizens generally of water, he has not half done his task, for not a tithe of their varieties has he figured. It would have been a more correct title to give his book, to have called it a sketch of some of the more common organisms, animal and vegetable, found in water.

To notice, however, what he has done, and how he has done it. He has given twenty-four plates representing each a greater or less number of figures, chiefly of organic objects, found in water. They are mostly the more common of their kind, and may be taken to be representative specimens of the classes and orders to which they belong. After the plates follow fifty-six pages of letterpress, in

[1] *A Guide to the Microscopical Examination of Drinking Water.* By J. D, Macdonald, M.D., F.R.S. With twenty-four lithographic plates. London, 1875.

which he presents an outline of the natural history of the organisms he has described, noting the classification of these infusorial beings, both vegetable and animal; and, on the whole, produces a fair outline of the matter, furnishing a useful companion to the microscopic student who is interested in making acquaintance with the forms and nature of the tiny creatures he finds floating in the water of ponds and ditches almost everywhere.

Nevertheless, if we are to look at the book as representing the present state of knowledge respecting the infusoria and other minute beings Dr. Macdonald figures and briefly describes, we must express our great disappointment with it. He apparently is not acquainted with the numerous and accurate researches of late years into the structure and affinities of the infusoria. At all events, he has chiefly followed Dujardin, whose treatise is out of date as a scientific book, and whose figures are shadowy and unreal. The illustrations given by Dr. Macdonald are, for the most part, perhaps sufficient for identification in generic characters, but they really are little else than diagrams, and do not convey the marvellous regularity and beauty of the tissues and organs of the tiny creatures he would represent. Among them we recognise many old friends from Ehrenberg's plates, and many more from the engravings to Dujardin's 'Histoire des Infusoires.' All these belong to a past era of microscopical research.

Likewise, when we turn to the representations of higher animals and of plants, we must say we are not better pleased with them. They are neither accurate nor clear. For instance, on Plate viii are figures of *Oscillatoria autumnalis* and of *Lyngbya muralis;* but the figure of one of these plants would do just as well for the other, no definite precise characters being given.

The author certainly apologises for his plates, saying that, to lessen expense, the figures have been drawn in pen and ink; and that, although rough, they will answer the purpose intended equally well. But we do not appreciate this apology. The observer of minute beings in water, unless finding merely boy's amusement in his observations, wants a reliable guide in the way of illustrations to interpret and define those he meets with. Moreover, if it was worth while to furnish illustrations of infusorial animals and of microscopic plants, it was desirable to make those illustrations accurate. Lastly, it was the duty of Dr. Macdonald, in undertaking to represent the microscopic particles or beings found in drinking water, to put before his readers what forms he knew, from his own research, to be found in it; and not to turn to Dujardin and one or two other authors on Infusoria and Algæ, and select from their engravings certain forms which would seem to have recommended themselves to his attention from their prettiness or their oddity.

Swedenborg's Christian Psychology, by Gorman.[1]—No one who has any acquaintance with the works of Swedenborg can do otherwise than confess that they indicate the product of a fertile mind, of one fond of metaphysical subtleties, and of a vivid imagination. But, notwithstanding the enthusiastic efforts of his believers and followers to promote their circulation and to secure for them the attention and veneration with which they themselves regard them, they seem to fall among a faithless and unbelieving generation, which will neither receive his doctrines in the natural sciences, nor in theology or psychology.

Unhappily, perhaps, for ourselves we belong to the category of unbelievers, who cannot accept mysticism for knowledge, nor acknowledge the claim advanced by Swedenborg himself of receiving special spiritual enlightenment by some mysterious relations with the unseen world. Resting upon this notion of special divine favour, he assumes the possession of the power to reveal not only the arcana of this world, but also those of a spiritual world, with its inhabitants and their doings. In the tractate before us he lays claim to the mission to recover mankind from its darkness and unbelief, on the ground, as expressed by himself, that "it has pleased the Lord to open the sight of my spirit, and to cause it to ascend up into heaven and also to go down into hell, and to display to its (mankind's) view the distinctive character of each."

It would, however, be entirely out of place in this journal to discuss Swedenborg's claim to special inspiration, or, indeed, to a position as a foremost expounder of natural science, or of religion or philosophy. Neither can we undertake to review the work before us, which, though stated in its title-page to be on psychology, has little indeed to do with that science, except so far as some mystic ideas about the human soul, influx and the spiritual sun, are concerned. To arrive at a distinct conception of what Swedenborg means in much that he says, would be a labour we are unwilling to undertake, and one not promising a reward. Perhaps Mr. Gorman would point us to his preface and notes to help us out of our difficulties. The notes and comments are indeed profuse enough, occupying 400 out of the 500 pages of which (excluding the preface) the work is made up; but from an examination of them we are not prepossessed with them. Speaking generally, they are notes of admiration or notes of exclamation. The former are applied liberally and uniformly to all that Swedenborg teaches; whilst the notes of exclamation are as liberally dealt out to all other philosophers, ancient and modern, the world has produced, but with one strain, that of surprise and

[1] *Christian Psychology: the Soul and Body in their Correlation and Contrast (a new translation of Swedenborg's 'Tractate de Commercio Animæ et Corporis'). With Preface and illustrative Notes.* By T. M. Gorman, M.A. London, 1875, pp. 502.

indignation at their darkness and folly. Now, Mr. Gorman may challenge for himself perfect liberty to believe in the supernatural endowments and wisdom of his master, but he should, even according to the maxims of the New Jerusalem Church, exhibit some love and esteem towards those not of his own household of faith. This he has not done, and his sarcasms and animadversions upon the great men of a past and of the present generation will not help his cause.

Original Communications.

I.—On the Occurrence of Renal Tube-Casts in Non-albuminous Urine.—By James Finlayson, M.D., Physician and Lecturer on Clinical Medicine to the Western Infirmary of Glasgow.

The habitual occurrence of renal tube-casts, without albuminuria, in cases of jaundice, while forming an interesting feature in this affection, raises some important questions as regards tube-casts themselves, and the significance to be attached to them as signs of renal disease.

In a "Report on Renal Cases" in 'The Glasgow Medical Journal' for January, 1874, I called attention to the occurrence of tube-casts in non-albuminous urine, citing a case of jaundice, a second case where the urine was loaded with urates and dumb-bell oxalates, and a third which seemed to resist explanation, but in which there was some suspicion of renal calculus. Since then my attention has been more or less constantly directed to the subject, and several cases throwing some light on the matter have come under my notice, partly in the hospital, but chiefly in private.

Before proceeding to submit the facts observed, it may be well to state that the tube-casts were usually searched for, in the first instance at least, with a low power of the microscope (No. 4 Hartnack), and on a slide with two or three drops of the sediment, without the intervention of a cover-glass. In this way they can be much more readily found if scanty, and when doubtful structures are seen the higher powers can usually be brought to bear on their resolution. Great care was taken to prevent fallacies from contamination of the samples by pipettes used for other specimens, and, indeed, the sediments were often examined without resorting to these instruments at all. Doubtful structures were occasionally seen (shreds of mucus, hyaline masses, fungi, &c.), but tube-casts were noted only when there seemed no reasonable doubt as to their nature, and confirmation by competent observers was often obtained as a check to prevent any fallacy in this respect. The tests for albumen relied on were those so long and so well tried in clinical work, viz.—(1) heat and cautious acidulation with acetic or nitric acid, and (2) nitric acid poured

gently to the bottom of the cold urine, care being taken to allow some time to elapse before the absence of albumen was pronounced. No attempt was made, at least systematically, to apply other tests for albumen in these cases, inasmuch as these tests, whatever their chemical value may be, have not been employed so long or so extensively as to afford complete confidence that their indications have the same clinical significance as in the case of the two well-known tests just mentioned.

The six following headings cover nearly all the cases which I have observed bearing on this subject; I have, however, met with tube casts once or twice under circumstances in which I could scarcely suspect any lesion of the kidney, or any other abnormality in the urine, but I am not prepared to discuss the question as to whether stray tube-casts may occasionally be found in perfectly normal urine and apart from any disease of the kidneys.

I. *Tube-casts may be found in the sediment of urine, which contains so little albumen that great care is required to bring out the reaction.*—As mentioned in my "Report on Renal Cases," the discovery of casts sometimes led to the re-examination of a sample which had been too hastily entered as non-albuminous, with the result of discovering the error committed. Cases of this kind need not detain us; indeed, they scarcely come within the scope of this article, except in so far as they lead us to understand that a very slight further diminution in the quantity of albumen present in such cases would result in its failing to give any reaction with the tests. Cases of this class, therefore, form a connecting link between markedly albuminous urines with tube-casts in their deposits and those cases, now under consideration, in which such casts are found apart from albuminuria. Without going into details, I may say, generally, that tube-casts associated with these minute traces of albumen occurred in a great variety of different diseases, and in urines in which the albuminous reaction seemed due to minute quantities of blood, as well as in other more ordinary cases.

II. *Tube-casts may be found in samples of urine passed at times when the albumen has temporarily disappeared.*—In a case, for example, of parenchymatous nephritis, resulting in contraction of the kidneys, I found that in the progress of the illness the amount of albumen diminished, and for a few days none could be made out; it reappeared, however, in small quantity for two or three weeks before death. *Tube-casts were present in the urine throughout, even when no albumen could be detected by the tests* ('Glasgow Medical Journal,' Oct., 1875, pp. 481-2). One of the most instructive cases of this class came to my knowledge thus. A patient had had severe convulsions, due, no doubt, to alcoholic excesses; the urine

was found to be albuminous to a considerable extent; no tube-casts were detected, but the presence of amorphous urates in great abundance might account for their not being seen; this was on September 15th, two days after the fits. The urine was examined occasionally, but only as to the presence of albumen, and this was noted to be slight on September 20th, and on September 28th the medical man in regular attendance found the urine non-albuminous. Two samples were sent to me for more careful examination, viz. that of the night of October 2nd and of the forenoon of October 3rd; during this very night, however, the progress of the case, hitherto perfectly steady and satisfactory, was interrupted by the occurrence of an attack of diarrhœa with some feverishness, but this seemed to be accidental in character and due to some error in diet. At any rate, the result was that, while the evening sample was clear and free from albumen, next morning's specimen was loaded with pink urates, and it was highly albuminous. This latter sample contained abundant tube-casts in the sediment. On careful search *a few tube-casts were found in the clear, non-albuminous urine of the previous night;* these were for the most part perfectly hyaline, but some contained a few glittering specks. The urine was frequently examined for the next fortnight (5th to 14th October), but no albumen was found, and morning and evening samples of the 13th and 14th October were sent for my own inspection, but I could find neither albumen nor tube-casts.

Another case, in the wards of the Western Infirmary, last summer, continued to yield tube-casts in the urine for several weeks after the albumen had disappeared. The case was one of dropsy, chiefly ascites, resulting apparently from hepatic disease brought on by intemperance, and complicated, while the patient was under observation, with several eruptions of purpuric spots, and a very marked attack of hæmaturia occurring after admission. The urine presented abundant tube-casts when the hæmorrhage began, and *the tube-casts persisted even when the last trace of albumen, as well as of blood, had disappeared for several weeks,* and when the patient, perfectly recovered from the dropsy and much improved in every way, seemed so well that he was dismissed from the infirmary. In this case the future recurrence of albuminuria (or of blood in the urine) was so probable that we may almost include it in the group covered by the present heading, but it forms a connecting link with the cases to be considered in the next section.

III. *Tube-casts may be found in urine which has been albuminous, but in which the albumen has disappeared for a considerable time, so that we may be in doubt how far the kidneys are restored to their normal state.*—An old lady (æt. 75), subject to chronic rheumatic arthritis, and suffering from severe pains in the left leg re-

sembling sciatica, was found, when I first saw her in January, 1874, to have œdema of the feet and to be suffering from very frequent micturition, with a sense of straining, and her urine was supposed to have been at times bloody. On examination of two samples I found a considerable sediment of pus in both; both were albuminous and acid to litmus; specific gravity 1018. Tube-casts were found in the sediments; some of them presented distinct fatty specks; pus-corpuscles, likewise, were seen, but no blood or crystals. The patient subsequently had a severe bronchitic attack, from which she recovered, although still tormented by her rheumatic pains; but I was surprised to find that the albuminuria had ceased in one so old, as I supposed the renal disease likely to continue till her death. She remained, with but little change, almost constantly in bed till November 16th, when she rather suddenly became very drowsy and confused, without any marked alteration in the pupils; indeed, she seemed to be sinking. Still impressed with the idea of renal disease, I procured a sample of the urine, but *I could not establish the presence of albumen;* although a very slight opacity occurred on heating and acidifying, no reaction was obtained with nitric acid in the cold. The colour was good, and the sp. gr. about 1020; under the microscope pus-cells were still found; *tube-casts, some granular and some distinctly fatty, were found without much difficulty.* She rallied from this alarming condition (the real nature of which remained doubtful), and still continues much as before. In January, 1875, the urine was found turbid; it still contained pus, but neither albumen nor casts were detected. Another examination in August last gave likewise a negative result.

A somewhat similar case was brought under my notice by a medical friend. An old lady, also over 75, and likewise affected with chronic rheumatic arthritis, was found in the springtime of 1874 to be out of health and to have considerable œdema of the feet; the urine was highly albuminous, and oily tube-casts were seen in the sediment. The prognosis seemed certainly very unfavorable, but during her stay at the coast that summer she improved greatly, and my friend reported to me that the albumen had disappeared. Remembering the facts just recorded with regard to the last case referred to, I procured a sample for my own examination (February 15th, 1875). The urine was somewhat turbid, but *no albumen could be detected* by heat or by nitric acid in the cold; *a few tube-casts, some with granular contents, were seen.* No crystals were found on the first examination, but in a couple of days uric acid was thrown down spontaneously.

IV. *Tube-casts are found occasionally in non-albuminous urines which are loaded with urates and urea.*—In my "Report on Renal Cases," already referred to, I gave one case coming under this description in which the urine was loaded with urates and octohedral crystals of the oxalate of lime, with a few dumb-bell forms; in this case tube-casts were found, although no albumen could be detected. Since then a remarkable case has come under my notice which may be placed in the same class. A young man was suddenly seized with an inflammation of the cæcum, and on the second day of the illness (June 26th) the urine was examined and *found non-albuminous, and, though frequently examined since then, no albumen has ever been detected.* The specific gravity, however, was high, 1035, the reaction acid, and crystals of nitrate of urea were readily formed on adding nitric acid to the cold urine; urates were deposited in abundance on adding a drop or two of acid; the colour was good, and there was no biliary reaction with nitric acid; in the sediment *numerous tube-casts were found,* either perfectly hyaline or with a few minute specks contained in them. On testing for sugar a very marked reduction of Fehling's solution was produced by a few drops of the urine. [As there were none of the usual signs of diabetes, and no intelligible explanation of the glycosuria, samples were submitted to Professor Ferguson, who by means of the bismuth as well as of the copper test, and more particularly by fermentation, placed the presence of sugar beyond doubt.] On June 28th the urine was high-coloured, and with a specific gravity of 1035; there was no marked sediment, but on adding a little acid a dense precipitate of urates occurred. The reaction with the copper test remained as before, and there was no albumen. Under the microscope no crystals were found in the sediment, but a few hyaline tube-casts were seen. On June 29th the urine had a specific gravity of 1026; only one or two casts were found; there was no obvious deposit, but on adding a little acid a sediment, less bulky than before, of uric-acid crystals fell down; there was still no albumen, and the reaction of sugar was established by the copper and bismuth tests. The urine of July 1st was examined by Professor Ferguson; its specific gravity was 1019·2, and the sugar, as determined by the copper-test, amounted to 150 grains per pint; the colour was good; there was no albumen; tube-casts were detected with difficulty (the urine was measured about this time and found to be less than two pints per day). On July 5th the urine had a specific gravity of 1032, the colour was good, and there was a spontaneous deposit of urates and uric-acid crystals; crystals of nitrate of urea appeared quickly on adding nitric acid; there was no albumen; a few hyaline casts were found; the reduction of Fehling's solution was obtained with difficulty. [From this date the sickness and diarrhœa, which had been troublesome, subsided, and a steady improvement was maintained.] The urine passed on the

night of July 12th and the morning of July 13th was examined. The former had a specific gravity of 1031; it had a spontaneous deposit of urates; there was a distinct reduction of the copper-solution, although very slight as compared with what it had been; there was no albumen. The morning sample had a specific gravity of 1027, with only a slight mucous sediment; there was scarcely any reduction of the copper-solution to be made out; no crystallisation occurred with nitric acid; on further keeping, the urine deposited, spontaneously, crystals of uric acid; no albumen could be detected; tube-casts were found in both samples. From this time the patient, who never presented any of the symptoms of diabetes, had recovered so well, that it was not deemed prudent to examine the urine too frequently, but in August the urine was examined repeatedly by a medical friend under whose care the patient then was; no evidence of sugar was found. On September 21st I verified the absence of sugar and albumen, and on October 21st a sample passed at night was examined, and a sample passed the following forenoon was likewise tested, but no sugar or albumen could be detected, and only one or two hyaline tube-casts were found after a prolonged search.

The question arises, how far was the glycosuria responsible for the presence of the tube-casts? In view of the case detailed in my "Report," already referred to, I was from the first inclined to regard the tube-casts as in some way related to the loaded state of the urine rather than to the sugar; but it may be proper to examine more critically the facts here noted. It is to be observed, then, that the tube-casts were most abundant on the first examination, when the specific gravity was highest, and that they were detected with difficulty when the specific gravity fell to 1019, although sugar continued to be present as before in considerable quantity; further, tube-casts continued to be found when the urine had ceased to affect Fehling's solution readily, or indeed at all. I had hoped in connection with this to direct some attention to the microscopic examination of simply saccharine urine to see if tube-casts usually, or ever, occurred in such cases, but I have not hitherto had favorable opportunities for doing so. Ordinary diabetic urine is so bulky that the search for tube-casts is not likely to be very successful, even if they were passed in moderate quantity. In one case of glycosuria, with paralytic symptoms, I procured a specimen, as I understood that the quantity passed was not excessive, but I found that it had greatly increased at the time I made the examination. The specific gravity was 1043, and there was no albumen; although two structures somewhat resembling tube-casts were discovered on prolonged examination, I was not satisfied that they were of this nature. In this case sugar had been passed for at least a year or two; and as epithelial cells of various kinds were seen in the sediment in considerable

abundance, and as the quantity of urine was not so excessive as is often the case, the examination may be regarded as having been made under favorable circumstances, and the result tends rather in the direction of negativing the presence of sugar in the urine as in itself a cause of tube-casts.

A case of obscure glycosuria, without the ordinary symptoms of diabetes, came recently under my notice through Dr. Gairdner, and in it I detected tube-casts in some abundance, but as there was a distinct trace of albumen also in this urine, it scarcely comes within the scope of our present inquiry.

V. *Tube-casts are found in the urine in cases of renal calculus and gravel, associated with blood, pus, and albumen in variable and sometimes very minute quantities, and occasionally with a complete absence of albumen, as judged by the ordinary tests.*—A lady was seized, for the first time, with the typical symptoms of renal calculus on the 7th of December, and the urine was found on the 9th to be loaded with urates, and to have abundant crystals of oxalate of lime; no blood-corpuscles were seen; the reaction for albumen was uncertain, only a slight cloudiness was detected on heating and adding acid. On the 15th a trace of albumen was entered in the notes; the urine was acid, and urates were thrown down on adding acid to the cold urine; the specific gravity was 1029; no distinct blood-corpuscles could be made out, but a few tube-casts were seen, chiefly hyaline, a few contained some renal epithelium; loose renal epithelium was present in considerable quantity.

[A small oxalate calculus was passed next day, and the illness came to an end, and the patient has been in good health since, and has gone through a second pregnancy without accident of any kind.]

On December 18th the urine of the previous day was alkaline when examined; a trace of albumen was noted; a precipitate still occurred on adding acids; hyaline tube-casts were still found, but the renal epithelium was much less abundant. On December 20th the urine was acid and had a slight mucous sediment; *no albumen could be detected;* the specific gravity was 1026; a few oval and reniform oxalates were seen, and also *several tube-casts,* some of these had a few cells of renal epithelium contained within them.

In this case it was difficult to be sure of the presence of albumen, even where it is noted in this report, on account of the faintness of the cloud produced by heating; while the precipitation of urates, on the addition of acids, destroyed the value of the supplementary test by nitric acid in the cold. The notes, however, are given as they stand in the memorandum.

Another case of renal calculus and uric-acid gravel, in a gentleman, came under my notice in May last. He had suffered several

times from this affection, and had passed a uric-acid calculus two years before. The pain began on May 10th, and became worse on the 12th and 13th. The urine was examined on the 14th, and was found to contain blood, as shown by the microscope; a trace of albumen was also discovered; uric-acid crystals were likewise found, and a considerable number of tube-casts. On May 16th the pain was worse, and the urine was found to be turbid when passed, this being due to uric-acid crystals; on adding a drop or two of acid to the urine a copious sediment of uric-acid crystals was thrown down; *no albumen could be detected on testing carefully with nitric acid* in the cold, and so it was presumed to be absent (although a very faint opacity occurred on heating and adding acid); *numerous tube-casts, some with fatty specks and some with a granular appearance, were found,* but no blood-corpuscles were detected. On May 20th abundant uric-acid crystals were still found, as also a few casts, but no blood-corpuscles were seen; no albumen could be made out; no opacity was produced above the level of the nitric acid added to the cold urine, but nitrate of urea readily formed as crystals. A slight opacity on heating still occurred, but this did not seem to be due to albumen.

The severity of the symptoms began now to abate, although some suspicion of a calculus in the pelvis of the kidney still remained, there being a certain amount of dull pain in the left side, and in the course of about two months a return of the pain in greater violence than before was followed by the passage of a small calculus. I cannot supply notes of the urine, as the patient was from home, and he was not then under my care or observation.

In the next case the diagnosis of renal calculus is not so well made out, although it is probable. Dr. Hector Cameron asked me to examine a sample of urine passed by a gentleman in almost perfect health, who, however, had frequently recurring attacks of hæmaturia, lasting usually for a few days and gradually subsiding. The bleedings seemed to himself to come on after some special exertion. He admitted having some pains in the loins, and perhaps over the bladder too. The sample brought was said to be passed just as the urine was beginning to lose its bloody appearance. On testing for albumen this seemed at first to be absent, but with care a distinct trace was obtained with nitric acid in the cold. Under the microscope abundant blood-corpuscles, and not a few hyaline tube-casts, were found; myriads of octohedral oxalates, some aggregated in little clusters, were seen, and also a few tailed epithelial cells. In a fortnight the urine was again examined, when the patient regarded the attack as off; there was no blood-tinge, and *no albumen could be detected* on the most careful testing; under the microscope no blood could be seen, *but a few tube-casts were found,* and also abundant oxalates, as before, with a few dumb-bell crystals.

Somewhat related to the foregoing, from the present point of view, is the case of a child, under the care of Dr. Joseph Coats, where recovery from invagination of the bowel took place by separation of the affected part. In the course of this case the urine, immediately on being passed, was noted to present some glittering objects which settled down and were found to be hedgehog crystals of the urate of soda. While examining these abundant tube-casts were readily found, and I had an opportunity of verifying their presence. The urine had been tested with some care and noted as non-albuminous; but on going over the reaction, by gently heating the urine so as to dissolve the turbidity from urates, I was able to demonstrate a slight opacity above the level of the nitric acid as poured gently into a test-tube. In the examination of subsequent samples this very minute trace of albumen was again found. The tube-casts persisted for some time, but disappeared along with these hedgehog crystals. In the second last examination the tube-casts are entered in the note without, unfortunately, any mention of the albuminous reaction, which is supposed to have disappeared; but without insisting on this the exceeding minuteness of the opacity produced by the reagents justifies the mention of the case in the present connection, as the peculiarity of these hedgehog crystals of the urates seems to be that they are, in many cases, formed within the kidneys, and certainly in the present case they were passed as crystals from the bladder.

VI. *Tube-casts are almost invariably found in marked cases of jaundice, and as a rule this occurs without albuminuria.*—In showing some typical specimens of this class to the Glasgow Pathological and Clinical Society in May, 1874, I stated that, so far as I had then examined the subject (twelve cases), my observations confirmed those of Professor Nothnagel as to the almost invariable presence of tube-casts in the urine if it were deeply jaundiced, and as to the absence of albumen in about two thirds of such cases. Since then other cases have come under my notice, and further observation confirms the statement made. The cases observed included many varieties of jaundice, so that one is forced to the conviction that the tube-casts are due to the jaundice itself, and not the special diseases producing it. In view of the facts and cases adduced in this paper, one is led to look for some mechanical (or, perhaps, some chemical) irritation of the renal tubules as a possible explanation of the presence of tube-casts thrown off in jaundice. When we remember that renal calculi and gravel, and even hedgehog crystals of the urate of soda give rise to, or at least are the only obvious cause of, tube-casts in the urine, and that such casts are found with the minutest possible traces of albumen, and, indeed, as we have seen, sometimes without any trace at all, we seem to have before us an

analogous phenomenon. For in the sediment of jaundiced urine we find, at times, little coloured particles composed, apparently, of the biliary colouring matter; a similar material has been found after death in the tubules of the kidneys themselves. Frerichs, in his chapter on Jaundice ('Obs.,' vi), speaks of the urinary sediment containing "brownish-black angular granules," and in the post-mortem report of the same case he says, "the little tubes were filled with a black brittle material," and that "nitric acid produced, in most of them (the deposits), the play of colours known to be characteristic of bile-pigment." It is worthy of remark that, in this case, although the secreting functions of the kidney were essentially impaired by the morbid process, no albumen had been found in the urine. These particles, perhaps from their size, perhaps from their nature, no doubt cause much less irritation than the renal calculi or gravel to which I have ventured to compare them, for we do not find hæmaturia to be one of the facts in jaundice; but they may suffice for the production of tube-casts, although they do not cause hæmorrhage, and but seldom albuminuria, just as the cases of renal irritation already detailed exhibited all the transitions from a profuse hæmaturia to an almost imperceptible albuminuria, or even to a total absence of any evidence of blood or albumen in the urine in which the casts were found.

II.—Observations on the Branches of the Third Part of the Subclavian Artery.—By J. Beswick Perrin, Demonstrator of Anatomy, &c., Owens College, Manchester.

In the preface to his very important work on the 'Anatomy of the Arteries of the Human Body' Quain says—"Several years have elapsed since I became impressed with the belief that the difficulties which have often occurred in the performance of those surgical operations in which the larger arteries are concerned have arisen in great part from want of sufficient acquaintance with the differences in anatomical disposition to which these vessels are subject, not merely in the deviations in the origin of large branches, which are usually named varieties, but other peculiarities of various kinds which are liable to occur, &c." Despite these remarks and the numerous examples recorded in the work of the irregularities of arteries, modern authorities do not attach that importance to the vagrancies of chief arteries and their branches which their significance demands. Take for example the branches of the subclavian artery. There is not a single one of its usual number of primary offsets that is not a vagrant. The vertebral, thyroid axis, internal mammary, superior intercostal, deep and superficial cervical arteries, as frequently present irregular disposition as they do the arrangement called average in anatomical text-books. There are two branches—the supra-scapular and the posterior scapular, and the latter more especially—which vacillate largely in their relations to the subclavian artery. Authors of anatomical text-books are divided in opinion as to the principal sources of origin of these arteries, some giving them the elevated position of primary branches, and others, again, degrading them to secondary branches. The majority, however, are familiar with the fact that these two, as well as other subclavian branches, are by no means constant in their origin, and one or both may be either primary or secondary branches.

The father of English "regional" anatomy, Ellis, in his seventh edition, says—"If there is a branch present on the third part of the (*subclavian*) artery, it is commonly the posterior scapular; if more than one, this same branch with the internal mammary; and if more than two, an offset belonging to the thyroid axis will be added."

In the sixth edition of Grey the posterior scapular artery is described as a branch of thyroid axis, frequently as a branch of the third part of the subclavian artery, and more rarely as a branch of the second part.

Knox[1] makes the transverse cervical artery the fourth largest

[1] 'A System of Human Anatomy,' on the basis of the 'Traité d'Anatomie Descriptive,' of M. H. Cloquet, 2nd edit., 1831.

branch of the subclavian, but sometimes arising from the inferior thyroid, while the supra-scapular as more frequently coming from the inferior thyroid rather than the subclavian, sometimes having a trunk common to it and the transverse cervical or internal mammary.

South (The 'Dissector's Manual,' 1828; also, the 'Edinburgh Dissector,' 1837) describes both the arteries as branches of the subclavian, but also occasional branches of the thyroid axis.

Jones Quain ('Elements of Anatomy,' 1834), describes the posterior scapular (transversus colli) as a branch more frequently derived from the subclavian itself than from the inferior thyroid; "the posterior scapular branch very frequently comes off singly from the subclavian in the latter part of its course, where it turns down upon the first rib."

Fyfe ('Compendium of Anatomy of the Human Body,' 1810) does not mention the posterior scapular or transversus colli. After describing those which he considered the more regular branches he says—"Others are frequently formed which are more variable in their origin and in their distribution in the neck."

Jenet, who copied James Benjamin Winslow almost word for word and praised him for his accuracy, and Douglas, who translated Winslow's 'Anatomical Exposition of the Structure of the Human Body' from the French original, have not so much even as mentioned the artery in question, nor led us to infer that there were other branches than those described, but inconstant in their direct sources of origin.

The 'Dublin Dissector,' 1831, describes both arteries as branches of the thyroid axis, also in many subjects as arising distinctly from the subclavian artery.

Cruveilhier ('Anatomie Descriptive') describes the posterior scapular (transversus cervicis, transversalis colli) thus :—"It arises from the front of the subclavian, sometimes to the inner side of the scaleni, sometimes between them, but most commonly on the outer side of those muscles; in the first case it often comes off by a common trunk with the inferior thyroid, and in the two other cases by a common trunk with the supra-scapular."

John and *Charles Bell* (the 'Anatomy and Physiology of the Human Body, 1829) describe the transverse artery of the neck as a branch of the inferior thyroid, and seem to attach much less importance to this vessel than to the supra-scapular, which "arises sometimes as an independent artery, and is so great that we wonder that it does not always do so."

In the article "Subclavian Arteries" (Todd's 'Cyclopædia of Anatomy and Physiology'), by McDowel, it is stated that the posterior scapular is frequently given off by the subclavian artery external to the scaleni.

There is a good deal of confusion in the old authors about the branches of the subclavian, especially the posterior scapular branch; and this confusion is rendered somewhat more complicated by the old-fashioned method of describing as axillary that portion of the subclavian between the scalenus anticus and the first rib. It is not to be wondered at, however, that these arteries should have been incompletely described, when we find even Monro the younger stating that "the irregularities as to the distribution of the subclavian arteries are more frequent than in any other part of the vascular system."

Quain ('Anatomy of the Arteries of the Human Body'), describing the transverse cervical, the superficial cervical, and the posterior scapular arteries, says—"A comparison of the best treatises on anatomy will show considerable discrepancies as to the nomenclature and the description of the arteries known under these names. These discrepancies are attributable to the fact that the arrangement of the vessels varies considerably, and probably, also, to the neglect of the writings of their predecessors on the part of the authors of some anatomical works."

The views of these authors are tabulated as follows:

Haller ('Icon. Anatom.,' fasic. 2, pp. 17 and 19) describes two arteries. According to his description one arises from the thyroid axis, and, after crossing the neck, ramifies in the muscles; this is named "ramus transversus colli." The other artery—which arises from the subclavian artery externally to the anterior scalenus muscle and passes through the nerves of the brachial plexus to be distributed, &c., is designated "cervicalis superficialis. Was this name (as M. Cruveilhier conjectures) applied in consequence of the comparatively superficial origin of the artery?

Murray ('Descript. Arter. Corp. Hum. in Tab. Redact.') describes likewise two separate arteries, and uses the same nomenclature.

Soemmering appears to recognise two distinct branches, and speaks of that given from the third part of the subclavian artery under the name "cervicalis superior sine superficialis."—'De Corp. Hum. Fabrica.'

J. Bell follows the same arrangement of the arteries as the preceding writers. He describes "the transverse artery of the neck, or transversus colli," as a branch of the inferior thyroid; and remarks, under the head "arteria cervicalis superficialis"—"The subclavian artery has got from under the muscle, and has passed the scaleni a little way before it gives off this superficial cervical."—'The Anatomy of the Human Body,' by J. and C. Bell, 7th ed., vol. 2, p. 163.

Bichat mentions but one artery, which he names "artère scapuaire postérieuse," and describes it as arising frequently from the

inferior thyroid, and occasionally from the axillary (the subclavian external to the scalenus muscle).—'Anat. Descript.'

Boyer describes but a single branch, naming it "artère cervicale transverse." He speaks of the origin in the same manner as Bichat, and in treating of the branches given at the side of the neck says —"Parmi ces rameaux il y en a ordinairement un plus considérable, auquel on pourrait donner le nom d'artère cervicale superficielle, parcequ'elle se perd dans le tissu cellulaire et dans la peau de la partie inférieure et latérale du cou."—'Traité Complet d'Anat.,' tome 3, p. 76, édit. 4.

J. F. Meckel applies the designation "oberflächliche oder gnere Nackenpulsader" to the single vessel which he describes, thus making the names superficial, cervical, and transverse cervical synonymous. —'Handbuch der Menschlich. Anat.,' B. 3, S. 148.

Tiedemann and *Weber* describe two separate branches corresponding in distribution to the third nerve of distribution mentioned in the text (Quain's).

In the works of these anatomists the small branch given from the thyroid is named "arteria cervicalis superficialis," and the larger branch which arises directly from the subclavian is denominated "arteria transversa colli, seu dorsalis scapulæ."—'Icones Arter.,' tab. 6 and 10, and F. Hildebrandt's 'Handbuch der Anat. des Menschen, besorgt von S. H. Weber,' edit. 4.

H. Cloquet, like Bichat and Boyer, mentions one artery only, and to designate it he combines the names used by them. This anatomist gives the distinctive appellation superficial cervical ("artère cervicale superficielle") to one of the branches, apparently following up the suggestion in the extract from Boyer's work, 'Traité d'Anat. Descript.,' t. 2, p. 462, éd. 5. Considering the influence which the treatises of Bichat and Boyer have exercised on the modern anatomical works of this country, it is very probable that the present manner of viewing the arteries in question has been derived from them, and that the change in the application of the name "superficialis colli" is due to one of them.

The following is transcribed from Quain's 'Anatomy of the Arteries:'

Transverse Cervical Artery.—This vessel may be regarded as the common trunk of the superficial cervical and the posterior scapular. In the bodies registered in the table it was found to arise—

Internally to the anterior scalenus muscle in	120	cases.
Under that muscle in	11	„
Beyond it in	6	„

The artery is generally derived from the thyroid axis, but the

origin was found connected to other branches than the thyroid; usually to the supra-scapular alone.

Occasionally to this branch and the internal mammary conjointly in	16 cases.
It arose from the subclavian in	14 „

Branches.—The ascending cervical artery is occasionally a branch of the transverse cervical. Four examples are noticed in the table.

Posterior Scapular.—The origin of this artery as a separate branch of the subclavian is noticed in the table as

Internal to the anterior scalenus muscle in . . .	1 case.
Under it in	43 cases.
Beyond it in	101 „

It usually extends as far as the lower angle of the scapula. I (Quain) have sometimes seen it end opposite the spine of that bone, and in such cases one or two of the posterior branches of the intercostal arteries were distributed instead of this vessel.

Branches.—Besides those usually derived from it, the "profunda cervicis" took origin from the posterior scapular in four cases.

Superficial Cervical.—The artery noticed under this name in the second and third of the modifications described above has occasionally some peculiarities in its origin. In the table it is stated to have arisen—

From the supra-scapular artery in	7 cases.
Directly from the subclavian in	2 „
Directly from the thyroid artery in	1 case

These are the percentage irregularities in 296 cases.

My friend and colleague Professor Morrison Watson tells me that he has invariably regarded the posterior scapular artery as a branch of the third part of the subclavian rather than a branch of the thyroid axis. Further, he has taught, during the many years that he has been a teacher, that the posterior scapular artery, on the average, was a derivative branch of the third part of the subclavian.

My own investigations when Demonstrator of Anatomy at King's College, London, did not lead me to infer the presence of this artery so frequently as in the subjects which I have met with at the Owens College, Manchester.

In fifty subjects examined in the dissecting room of the Owen's College Medical School, during the winter session 1873-74, I found variations in the origin and distribution of the branches of the subclavian artery in no less than thirty-seven subjects, nineteen males and eighteen females.

In ten (four males and six females) the posterior scapular artery

had its origin from the second part of the subclavian, on both sides, behind the anterior scalenus muscle. In the majority of cases the artery in its course backwards perforates the fibres of the middle scalenus muscle.

In four subjects (all males) the posterior scapular sprang from the second part of the subclavian on the right side only, and in four other subjects (two females and two males) on the left side only.

In eleven subjects (seven males and four females) the posterior scapular arose from the third part of the subclavian artery on both sides.

In seven others (two males and five females) on the right side only, and in four others (two males and two females) on the left side only.

In three of the subjects (one male and two females) the artery sprang from the second part of the subclavian on the left side and the third on the right side; in one subject (a male) from the second part on the right and the third on the left side. The transverse artery of the neck from the thyroid axis was wanting in about half the number of cases. When present it was as a muscular branch mainly to the trapezius muscle. In a few cases the transverse artery of the neck gave origin to the posterior scapular, and there was present also a posterior scapular from the third part of the subclavian, the latter aborting as it neared the scapular.

The supra-scapular artery was irregular in twelve subjects (five males and seven females); in five subjects it arose as a separate branch from the third part of the subclavian; in one (female) subject, on both sides; in four (two males and two females), on left side only; and in one (female) subject, on right side only. In another (female) the supra-scapular arose by a common trunk with the posterior scapular from the second part of the subclavian artery on the left side only. In two subjects (both females), it arose by a common trunk with the posterior scapular artery from the third part of the subclavian, in one case on the left side and in the other on the right side only.

When the posterior scapular artery springs from the second part of the subclavian artery it almost invariably passes downwards and backwards between the seventh cervical nerve and the cord formed by the union of the eighth cervical and the first dorsal nerves. But when it arises from the third part of the subclavian it was between the cords formed by the fifth and sixth cervical nerves on the one hand and the seventh cervical on the other. The nearer the artery approaches to the scalenus anticus muscle the more liable it is to follow the former course, and the further away from it the latter course.

During the winter session 1874-75, in the Owen's College dissect-

ing rooms, I found variations in twenty-four out of the thirty-six subjects dissected (ten males and fourteen females).

In eleven (four males and seven females) the posterior scapular had its origin from the third part of the subclavian on both sides. In two (both females) on the right side only, and in one (a male subject) on left side only.

In nine subjects (five males and four females) the posterior scapular arose from the second part of the subclavian artery on both sides. In two subjects (both females) on the left side only, and in one (male) on the right side only.

In one instance (a female subject) the artery sprang from the third part of the subclavian on the right side and from the second on the left. In a second case, exactly the reverse.

It will be readily seen from the foregoing statistics that at least two of the often-so-called branches of the thyroid axis have not their average origin from it, but from the parent trunk, the subclavian. I have never seen an instance yet in which the posterior scapular artery took its origin as an independent trunk from the subclavian artery internal to the scalenus anticus muscle. Quain mentions such a case. It is of little moment, however, for it might just as well spring as an independent factor from the first part of the artery as arise in conjunction with two others through the thyroid or rather scalene axis.

In a spare female subject the third part of the subclavian artery gave origin to the supra-scapular and posterior scapular arteries separately. Their veins—and the supra-scapular was very large—crossed the artery in their course to the subclavian vein. In several cases I met with an accessory profunda-cervicis artery, springing either directly from the subclavian, or the posterior scapular artery when it had its origin from the second or third part of the subclavian. The veins of this artery also cross over the subclavian. In one female subject the subclavian artery reached as high in the neck as the lower border of the cricoid cartilage. The supra-scapular artery was long and straggling and lay directly upon the subclavian artery throughout the whole of its third part.

With the exception of a slight degree of elevation or depression beyond the average, the subclavian artery possesses a fairly constant disposition.

I have only twice found it passing behind the trachea on the right side out of more than three hundred subjects examined.

The chief importance of the foregoing facts is in reference to deligation of the third part of the subclavian artery. Students are taught, and practitioners believe, that the subclavian rarely gives off any branch from its third part; further, that with the exception of the external jugular vein and its lower tributaries, the anterior relations of the third part of the subclavian artery are simple enough

to deal with in an operation for deligation. If the artery is average, if the patient has a long, thin neck, if the clavicle is fairly long and easily depressed, if the sterno-cleido-mastoid, trapezius, and other hyoid muscles are regular, if the artery is not surrounded by a plexus of veins or, what is quite as formidable an element of danger, crossed by several large venous trunks, then deligation of the third part of the subclavian artery is by no means a difficult operation to perform either on the dead or living subject. Unfortunately, however, these favorable conditions are the exception and not the rule.

Under the most advantageous circumstances the area of operation is limited, and important vessels are too closely adjacent to the artery for careless and reckless cutting. But in a considerable number of cases the subclavian space is materially encroached upon by the muscles bounding it. The average boundaries are too familiar to be described here. The lateral or muscular boundaries consisting of the sterno-cleido-mastoid and the trapezius have a strong tendency to differentiate and bridge-over the space. The clavicular portion of the sterno-cleido-mastoid is more frequently differentiated than the sternal, or than the anterior or clavicular portion of the trapezius. This differentiation presents itself under a variety of features. The simple form of the clavicular element of the sterno-mastoid is a strong, moderately broad band of fibres. In addition to this another muscle is oftentimes met with running parallel to it, attaining an occipital attachment, and known to anatomists as the cleido-occipital muscle. It presents various degrees of fission, from the complete one mentioned to the scarcely separable from the clavicular portion of the sterno-mastoid. The latter condition is by no means infrequent. Further, instead of there being one or two muscles, sometimes the differentiation is extended until three more or less complete portions of the clavicular factor are found. I have met with instances in which they were so isolated by well-marked areolar intervals that the pre-subclavian space was completely walled in by muscle.

Now, just as the clavicular factor of the sterno-cleido-mastoid is liable in certain instances to segmentation, so is the trapezius muscle, but not to so large an extent. Cleavage rarely co-exists in both muscles. The operative surgeon, then, should be prepared to meet with a far more formidable muscular layer, subsequently to section of the thin and scarcely appreciable cervical element of the panniculus carnosus, in the place of cellular tissue.

More deeply situated is another muscle, with more decided vagrant tendencies. It vacillates between its scapular origin and the clavicle. being attached sometimes to the one bone, sometimes to the other, and not infrequently to both bones. Occasionally I have met with it as a duplicate muscle. There can be no doubt that the omohyoid is a valuable landmark to the operative surgeon when it springs from the scapula and is disposed regularly; but when it springs from

the clavicle, or it possesses a double character, it is apt to mislead if great care is not exercised. But these are not the greatest difficulties which the surgeon has to encounter. Few good operators hesitate to divide muscle-tissue when it is requisite.

A careful dissection of the third part of the subclavian in the dead subject is almost enough to intimidate the boldest surgeon and to deter him from ever attempting to apply a ligature to the artery. In several subjects I have found the third part of the artery crossed, in addition to the usual structures, by the companion-veins of the posterior scapular artery, by a large distinct and fairly regular vein (the true posterior scapular), and by the cephalic vein which passed between the clavicle and subclavian muscle.[1]

In one subject, in addition to the foregoing irregular elements, there was an additional omohyoid muscle springing from the clavicle; and further, the posterior scapular artery was a branch of the third part of the subclavian. Under such circumstances it would be impossible to ligature the third part of the subclavian without injury to the veins, and, owing to the presence of a branch from the third part of the subclavian, the probabilities are that complete occlusion of the artery could not take place, and hæmorrhage, and most probably of a fatal character, must result.

Another and very rare addition to the complex relations of the extra-scalene portion of the subclavian I met with recently in a spare male subject dissected at the Owens College. The first dorsal nerve presented very unusual and remarkable dispositions. Instead of joining the eight cervical nerve directly after emerging from its intervertebral hole, it passed obliquely downwards and forwards, perforating the musculo-tendinous fibres of origin of the scalenus anticus close to their origin from the first rib. It then, changing its course, passed directly outwards to join with the inner branch of the dichotomous division of the isolated eighth cervical nerve, and lying in close relation to the inner and lower side of the third part of the subclavian artery, intervening between the latter and the subclavian vein. The first dorsal nerve, also the eight and seventh cervical nerves, remained as distinct trunks fully to the extent of a couple of inches. I have never before met with this peculiar arrangement. It is very instructive, inasmuch as it might very readily be included in a ligature passed round the artery, and illustrates how great care should be taken in applying a ligature to the artery, even when the superficial structures are regular.

Erichsen[2] says—"If the transversalis colli or humeri arteries,

[1] Occasionally the cephalic vein is found crossing over the clavicle to empty itself into the subclavian or external jugular, and sometimes the external jugular vein crosses the clavicle to empty itself into the axillary vein.

[2] 'The Science and Art of Surgery.' Sixth edition, 1872.

as occasionally happens, should inconveniently traverse this place, they must be drawn out of the way with a blunt hook." In the majority of subjects which I have examined one artery is certain to traverse the space inconveniently, and so inconveniently as to require, if any attempt to ligature the subclavian had been made during life, something more definite than a blunt hook.

Allan Burns ('Surgical Anatomy of Head and Neck') says—"We see that the connexions of the vessel (third part of subclavian) are not of such a nature as to render an attempt to pass a ligature around it improper. The situation of the artery is, nevertheless, such as will occasion considerable difficulty in the execution of this design."

My own views are just the reverse of the above. The situation is to be feared materially less than the associations of the artery.

The best description which I have met with of the surgical anatomy of the outer third of the subclavian artery is that by Morton and Cadge ('Surgical Anatomy of the Principal Regions of the Human Body,' 1850). The occasional wider attachments of the trapezius and sterno-cleido-mastoid muscles, and the clavicular origin of the omohyoid, are described. Further, they remark—"*For the most part*, the distal third of the subclavian gives no branch, but no arterial variety is more frequent than the origin of a large branch (usually the posterior scapular division of the transverse cervical), close to the scalenus; if such a branch were present and detected during an operation, the ligature would be placed above it *even though it required a partial division of the scalenus to effect this.*" I am of opinion that in all future operations on the artery in this, the third, also the second part of the artery, much greater care must be exercised in determining the presence of the posterior or other scapular branch from these two divisions of the subclavian. In cases of aneurism, surely scientific men can invent a more rational and less formidable means of cure than the heroic, and unsatisfactory in every sense, method of ligature. In lowly organised human beings the powers of physical endurance are not readily shocked, but the more highly organised and exquisitely sensitive deligation of an artery like the subclavian is almost as disastrous as amputation of the thigh.

I do not believe in the view, commonly accepted and generally believed, that variations in the distribution of the chief arteries, and their primary branches, are to be regarded as irregularities. It seems to me that we have much to learn as to the laws which govern these so-called irregularities. From many years' investigation of the muscles and arteries of man, and animals lower in the scale, I am led to believe that irregularities must be regarded from a much higher point of view. The constant recurrence of similar varieties of both muscles and arteries points most unmistakably towards definite type-forms. Further, I have no doubt that future more

extended investigations will enable us to determine what peculiarities of structure are present even from external configuration alone. However, the problem still requires asking, why one out of every so many cases shall have the vertebral artery on the left side springing from the arch of the aorta—why one class of human beings have the supra-scapular and transversus colli springing by a common trunk with the inferior thyroid, internal to the anterior scalenus muscle; in a second class the two latter arteries, either alone or in conjunction, spring from the subclavian behind the anterior scalenus muscle; in a third series external to it, above also, or in conjunction; and in a fourth series why the supra-scapular should be a fluctuating branch between the different varieties. Extended researches will inevitably show that there are distinct type-forms of human beings; and although their anatomy may not vary very much from one another, still they are as distinct varieties of the same species as the different members of the great whale family.

There is not a single departure in the disposition of the human tissues which is not of as much interest to the operative surgeon as to the philosophical anatomist. The anatomy of the human body for nearly a century has been regarded as a worn-out and barren field of research. Surgeons simply learn anatomy as taught in text-books, and as persistently ignore the researches which modern investigation most unmistakably teaches, viz. that all human beings have not an identical anatomical configuration. It would be an easy task to show that the little regarded eccentric dispositions of nerves, arteries, and muscles in operative regions could be extended with advantage to surgeon and patient. The time is not far distant when the operative surgeon will be required to have something more than a mere knowledge of the great arteries, nerves, and muscles of the human body.

Neither the elastic ligature, nor carbolic spray, nor the whole army of disinfectants and antiseptics, can bridge the great gulf of death which most undoubtedly exists between a comprehensive and a cramped average knowledge of anatomy.

III.—On the more Common Forms of Enlargement of the Lymphatic Glands.—By J. Warrington Haward, F.R.C.S., Assistant-Surgeon and Lecturer on Practical Surgery at St. George's Hospital; Assistant-Surgeon to the Hospital for Sick Children.

It has been so much the custom to regard enlargement of the lymphatic glands as the special characteristic of scrofula, that many glandular swellings, having no relation whatever to that disease, are frequently classed and treated as scrofulous. This is especially the case with regard to swellings of the cervical or submaxillary glands; yet it is certain that the majority of these enlargements are of a local and not of a constitutional character. The epithet "scrofulous" is, in fact, often applied very loosely, and seems sometimes to be used rather as implying some mysterious influence or peculiarity, than as indicating that a person is affected by a definite disease. Yet the symptoms of scrofula are sufficiently well defined, and enlargement of the lymphatic glands is but one, and that not a constant one, of these; and it is no more reasonable to call a child scrofulous because it has enlarged or even caseous cervical glands than it is to apply the term to a chronic inflammation of a joint in an otherwise perfectly healthy child, or than it would be to call a person syphilitic, because he had a periostitis of his tibia. An examination of any considerable number of cases of enlargement of the superficial lymphatic glands, will show the majority of these to have a local origin. The glands most often seen swollen are the cervical and submaxillary, and the greater number of such swellings depend upon inflammation of the scalp or gums. Slight cases of eczema, or impetigo capitis are exceedingly common in children, and are very frequently the cause of enlarged cervical glands; but the eruption being but trifling is often overlooked, and the surgeon's attention asked only to the condition of the glands. So also inflammation of the gums during teething, stomatitis, ulceration of the throat, and disease of the middle ear may give rise to swellings of the associated lymphatic glands. Glands affected in this way may attain a considerable size, but as a rule will recover their natural condition on the removal of the irritation. Usually several glands are affected; they are not distinctly isolable from the surrounding cellular tissue, nor are they freely moveable; they are, moreover, painful and tender; sometimes they suppurate. A peculiarly acute and painful inflammation of the posterior cervical lymphatic glands is occasionally seen in connection with scalp wounds; this usually runs a rapid course, and subsides without the formation of matter. Doubtless if any of the above-named irritations occur in a scrofulous person,

the glandular enlargement is prone to show an increase and a persistence, out of proportion to the severity or duration of the exciting cause, and thus it may pass on to caseation or necrosis; but this is by no means necessarily the case, for the lymphatic vulnerability varies greatly in scrofulous persons. Caseation must not be looked upon as the distinctive mark of scrofula, for almost any chronic enlargement of a lymphatic gland may result in caseation, and certainly this process may occur in an otherwise perfectly healthy subject. A single caseous, and in some parts cretaceous, gland was removed five years since from the neck of a boy who was the picture of robust health, and who I know remains so at the present time, and has never shown the slightest trace of scrofula.

Inflammation and chronic disease of a joint will cause indolent swelling of the associated lymphatic glands; and this in persons who are not in the least degree scrofulous. One of the earliest symptoms of disease of the hip-joint is often a slightly painful enlargement of the inguinal glands, and there are few cases of hip disease in which some swelling of these glands is not found. In disease of the cervical spine also, swelling of the posterior cervical glands often occurs, and it is important to remember that the stiffness of the neck in such cases may depend, not upon the painful glands, but upon the joint disease. The glands do not increase very greatly in size, but will remain for months swollen to about the size of filberts, and slightly tender to the touch; and as the joint disease subsides, they regain their normal condition; excepting the tenderness, they precisely resemble the amygdaloid glands of syphilis. In many robust persons this condition of inguinal or axillary glands ensues upon any severe exercise of the arms or legs, such as rowing, or prolonged walking, and seems to be quite unassociated with any delicacy or weakness of constitution.

The true scrofulous disease of the lymphatic glands is a slow and almost painless enlargement, usually of the superficial glands, and most commonly affecting those of the groin or neck. It commences simultaneously in several glands; these are at first soft, and surrounded by a little cellular swelling, so that the shape of the gland is not very well defined. As the enlargement increases the glands become firmer and more defined, in this respect differing markedly from Hodgkin's disease, in which, by their growth, the glands become fused together. In the course of time caseation ensues, and goes on either to cretefaction or to softening and abscess. Suppuration is much more rarely seen in the deeply situated, than in the superficial glands; and when it does occur, takes place slowly and with scarcely any pain; there is but little disposition to pointing, and the matter is ill-formed and mixed with caseous débris. The skin thus often becomes extensively undermined and ulcerated; and thus result the unsightly scars and

puckerings so often seen in scrofulous persons. An examination of a scrofulous gland reveals a general hypertrophy, with close packing of the cellular elements, leading, by a compression of its blood-vessels, to an anæmia, and consequent want of nutrition of its tissues. Fatty change soon ensues, and a subsidence of the swelling may take place; but usually the degenerated tissues either break down into cheesy material which eventually becomes calcareous; or suppuration takes place, accompanied by some little surrounding inflammation. Even if suppuration has occurred the abscess may not open, but may dry up, leaving only a little caseous matter unabsorbed; but this is very prone to become the seat of residual abscess, and thus to cause subsequent trouble; so that an abscess having once formed, its evacuation is to be desired.

In Hodgkin's disease the enlargement is usually at first confined to one set of glands, sometimes to a single gland. The affected glands can at first be felt firm and shot-like, and are perfectly defined and separable from the surrounding tissues. They are quite painless, and increase without any sign of inflammation. When somewhat larger, and while still firm, they closely resemble the syphilitic amygdaloid glands, and at this stage are quite indistinguishable from them. Growth, however, rapidly proceeds, and as the glands become larger, they also become softer and less defined, until they eventually become fused into one large lobulated mass, the skin over which remains unchanged. As the growth progresses it insinuates itself amongst the neighbouring structures, and may spread, by continuity, a long distance from the point of origin before the system generally becomes infected. Thus I recently saw a young lady who was dying from starvation from the occlusion of the œsophagus by a growth of this kind, which had commenced in the glands of one side of the neck, had made its way backwards across the spine and behind the pharynx, forwards into the anterior mediastinum, and upwards into the zygomatic and spheno-maxillary fossæ; yet, at this time, there were no signs of visceral infection. A man died in St. George's Hospital, in whom I found a growth which had originated in the glands of the neck, and had made its way through the sphenoidal fissure, whereat a portion the size of a hazel-nut projected into the right cavernous sinus and right side of the sella turcica. This kind of glandular enlargement, though at first much resembling the scrofulous, and exhibiting on examination only a general hypertrophy of the organ, differs from the scrofulous in that it is more vascular, and that as the gland increases all parts of it grow equally, and its vascularity increases in proportion; so that there is no overcrowding of the cellular elements, and no tendency to degeneration or caseation. MM. Cornil and Ranvier,[1] it

[1] 'Manuel d'Histologie Pathologique,' p. 259.

is true, mention in their excellent account of the morbid anatomy of this disease, that patches of caseous degeneration may be found in the affected glands, but this is certainly exceptional. I have examined post-mortem eleven cases of Hodgkin's disease, and the only instance in which I found caseation was one in which I had injected large quantities of tincture of iodine into the diseased glands, and the caseation existed only in the parts wherein the iodine had been injected. Caseation is an accident and not an usual occurrence, and is probably induced either by some inflammatory action having been set up (as by the iodine injections), or by hæmorrhagic infarctions, and the consequent interference with the nutrition of the part involved. Ordinarily, the vascularity of the tumour, and the coincident development of both trabecular and cellular elements, tend to aggressive growth rather than to degeneration. The tumours thus formed are, to the naked eye, very much like encephaloid cancer, soft, grey, cerebriform material, interspersed with hæmorrhagic or vascular patches, and yielding to pressure an opaque juice containing lymph-like cells.

Microscopically they exactly resemble, as has been shown by Dr. Sanderson, the structure of a lymph-gland which has undergone fibroid induration from prolonged irritation; that is to say, they consist of groups of lymph-like cells in the meshes of a delicately reticulated stroma. It is not necessary to give a detailed account of this remarkable disease, which has been fully described elsewhere;[1] what I wish chiefly to point out are the points which may help us to its early recognition, as it seems to me worthy of consideration whether the removal of the affected glands in the commencement of the disease, might not prevent or delay the general infection which usually ensues. The disease may, to its end, be limited to one part of the body, but in the majority of cases a general infection eventually takes place, and not only do other glands become affected, but similar growths occur in the viscera. The early diagnosis from scrofula is to be made chiefly by the greater mobility and isolation of the glands, which are generally also, in the commencement of the disease, of greater firmness and elasticity than those affected by scrofulous enlargement. Then there is the absence of other scrofulous symptoms and of any cause of irritation, the freedom from pain and tenderness, the rapid growth and coincident emaciation and anæmia, without increase of the number of white globules in the blood.

Syphilitic enlargement of the lymphatic glands is characterised by extreme indolence, as implied by the name "indolent multiple bubo." The glands most commonly affected are the posterior cer-

[1] See Hodgkin, 'Medico-Chir. Trans.,' vol. xvii, p. 68. Wilks, 'Guy's Hospital Reports,' vol. xi, p. 56. Trousseau, 'Clinique Médicale,' vol. iii, p. 555, second edition. Cornil et Ranvier, 'Manuel d'Histologie Pathologique,' p. 251. Murchison and Sanderson, 'Pathological Trans.,' vol. xxi, p. 372.

vical and the inguinal. They vary in size from a shot to an almond, seldom larger; are elastic, firm, moveable, defined, isolable, painless, and remain for a long time without alteration; they do not tend to caseation. Although therefore, on a single examination, they may closely resemble other forms of glandular enlargement, yet if they are watched they are seen to differ from these by their indolence; they do not grow like those in lymphadenoma, nor caseate like the scrofulous. Anatomically, they are characterised by a greater increase of the trabecular tissue than of the cells; and to the naked eye they present on section a somewhat cartilaginous aspect.

Glands affected by lardaceous disease are not easily distinguishable in the living subject from those of syphilis or early lymphadenoma; but then they are not usually affected until there are other obvious manifestations of the disease, which render the diagnosis easy. They increase slowly and do not attain any great size, are firm and semi-transparent, and yield on the addition of iodine the characteristic mahogany colour.

Cancer of the lymphatic glands is most often secondary, and the existence of the primary tumour will establish their character; but it should be remembered that the inflammation or ulceration of an innocent tumour may give rise to enlargement of the neighbouring lymphatic glands. Primary cancer of the lymphatic glands is rare, and probably most of the cases formerly described as primary medullary cancer of glands were instances of lymphadenoma.

The *treatment* of lymphatic glandular swellings must of course depend upon the diagnosis. The simple enlargements depending upon neighbouring irritation will, if left alone, subside upon the removal of their cause. I say, *if left alone*, for if the skin over them is irritated by the application of iodine, poultices, or blisters, they may be provoked, as one so often sees, into still further enlargement, or even suppuration. Nothing in therapeutics is more curious than the way in which some practitioners paint tincture of iodine over every imaginable kind of swelling; to some minds the mere existence of a tumour, seems at once to suggest the local application of iodine, and to these, painting with iodine seems their refuge in all cases of doubtful diagnosis, as though changing the colour of the skin were supposed to affect the character of the growth beneath it. Unfortunately the staining is not the only harm done by such applications, for they inflame the skin and thus keep up or increase the glandular irritation for the cure of which they are used, or render the parts unfit, for a time, for necessary operative treatment. An acute swelling of a single lymphatic gland may be sometimes rapidly cured by puncture. A narrow thin knife should be thrust into the centre of the gland and withdrawn, and the part then covered with a piece of cotton wool, the pain and swelling at once and quickly subside.

Single caseous or cretaceous glands, in healthy persons, should be removed if their position does not render the operation dangerous; when superficial, they are easily turned out, and the scar left is very slight. Gland-swellings in connection with diseased joints are of course an indication for rest. I have seen one case of hip disease, in which there was reason to believe that the joint affection was the result of suppuration spreading from the inguinal glands.

The scrofulous enlargements will be chiefly benefited by the constitutional treatment of the disease of which they are part; and for this, nothing is to be compared to the influence of sea air and cod-liver oil. Small doses of iodide of potassium, in combination with preparations of iron, may be advantageously given with the oil. This is far more efficacious than the syrup of the iodide of iron, which I believe to be an entirely useless preparation. The local treatment, as long as the glands are only swollen and tender, should consist in simply protecting them from cold, pressure, or other irritation, which is best done by covering the part with cotton wool. When, however, matter forms, or the caseous material softens and liquefies, a very small puncture should be made through the skin, and the contents of the abscess gently squeezed out, pressure being made by a pad of lint on each side of the opening. The puncture may require to be occasionally reopened with a probe; but by this means, adopted early, the integrity of the skin is preserved, and the unsightly scars and puckerings often seen in such cases are prevented. It is, moreover, very desirable, when possible, to get rid of the caseous products of inflammation, for they are otherwise liable to be the seat of constantly recurring suppuration, or may perhaps be the origin of a future tuberculosis.

The treatment of Hodgkin's disease is a much more difficult question. Dr. Hodgkin himself said that concerning the treatment of this affection, either curative or palliative, he had nothing to offer. We must still confess ourselves in much the same position. Certainly no medicine that has hitherto been used, has seemed to have the slightest effect either in retarding or arresting the progress of the disease. I have injected large quantities of tincture of iodine into the tumours, but without producing any benefit; parts of the tumour into which the injection was made underwent inflammation and caseation, but the disease progressed steadily to its fatal termination.

It is to be noted, however, that although in the majority of cases the morbid growth eventually ceases to be local, and becomes disseminated, yet in many this dissemination does not occur until late in the disease; and in some, the growth proves fatal by its pressure upon, and invasion of, important parts, without any secondary growths being found anywhere in the body. Wherefore it seems reasonable to ask whether any benefit would accrue from the early

removal of the primary tumour, when such an operation is possible, and whether, by so doing, the dissemination of the disease might be prevented or delayed.

I have had one opportunity of adopting this mode of treatment, but not at so early a period as I could have wished. The subject was a girl, 4 years old, in whom the mass of diseased glands occupied the whole of the left side of the neck, extending from the trachea in front to within an inch of the spinous processes behind, and from the mastoid process above, to the clavicle below. The growth measured six inches transversely, and four and a half vertically. It was lobulated, elastic, and moderately firm, and moveable upon the parts beneath; the skin covering it was natural. No enlarged glands could be felt anywhere else, and there was no increase in the number of white globules in the blood, neither could any visceral disease be detected. The child was very pale, but not emaciated. I removed all the discoverable diseased glands, in two operations; at the first clearing the anterior triangle of the neck, at the second the posterior triangle. The child recovered with great rapidity from the operation; she speedily gained flesh and colour, and was evidently much benefited by the removal of the growth. The improvement was, however, but temporary, for after about six weeks, some enlarged glands again appeared in front of the left sterno-mastoid, and subsequently in other parts of the body, and the child eventually died from general dissemination of the disease. It was notable, however, that no fresh growth took place in the posterior triangle of the neck, where the absence of important structures permitted a very complete dissection to be made, and it seems very probable that I may have left some glands in the early stage of disease, among the large vessels in the anterior triangle, where their small size may have prevented their discovery without a dangerous amount of dissection. The improvement in the general condition of the child was, however, sufficient to encourage me to repeat the operation, should I have the opportunity, at an earlier period of the disease.[1]

[1] For a detailed account of this case see a paper read before the Clinical Society, Dec. 10, 1875.

Chronicle of Medical Science.

REPORT ON PHYSIOLOGY.

By Henry Power, F.R.C.S., M.B. Lond.,
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Blood—Blood-Glands.

1. George Gulliver, F.R.S. *Observations on the Sizes and Shapes of the Red Corpuscles of the Blood of Vertebrates.* (In 'Proceedings of the Zoological Society of London,' June 15, 1875, issued Oct. 25.)
2. L. Malassez et P. Picard. *Recherches sur les modifications que prouve le sang dans son passage à travers la Rate.* ('Comptes Rendus,' t. lxxix, pp. 1511.)
3. M. Freyer. *Ueber die Betheiligung der Milz bei der Entwicklung der rothen Blutkörperchen.* ('Inaug. Dissert.,' abstract in 'Centralblatt,' No. 23, 1875.)
4. E. Neumann. *Neue Beiträge zur Kenntniss der Blutbildung.* ('Archiv der Heilkunde,' b. xv, pp. 441.)
5. E. A. Schäfer. *Note on the Intra-cellular Development of Blood-Corpuscles in Mammalia.* ('Proceed. Roy. Soc. 1874,' No. 151.)
6. W. Osler. *An Account of certain Organisms occurring in the Liquor Sanguinis.* (Collected papers, Physiological Laboratory, University College, London, 1874-5.)

1. Mr. Gulliver publishes a new table of measurements of the red corpuscles of the blood drawn to a uniform scale of $\frac{1}{4000}$th of an English inch. He observes that the corpuscles of Mammalia when dried slowly are apt to become misshapen and more or less irregularly contracted; but when dried instantaneously in single or very thin layers on a glass slide, their form is admirably preserved, and their size is a shade larger than in the wet state, especially when prepared in summer. The pyrenæmatous corpuscles, on the contrary, are generally somewhat contracted by similar drying. He states that he has many specimens prepared more than a third of a century ago, and they are still perfect. The smallest corpuscles amongst mammals occur in the Tragulidæ, the largest in elephants. In man the red blood-corpuscles have a transverse diameter of $\frac{1}{3200}$th, and a thickness of $\frac{1}{12400}$th. They are not always distin-

guishable from those of the dog; but are regularly larger than in any British land mammals, and are not known to be exceeded in size in eight or nine foreign species of this class, though closely approximated or even surpassed in some marsupials, edentates, rodents, cetaceans, feræ, and most monkeys. In regard to medico-legal questions, Mr. Gulliver remarks that though a very careful and practised observer like Dr. Joseph Richardson may distinguish stains of human blood from those of the sheep or ox by careful measurement of the red corpuscles, this kind of diagnosis, as Dr. Woodward has stated, would be unreliable in some probable cases.

2. According to MM. Malassez and Picard, the blood of the splenic veins differs in colour according to whether the splenic nerves are excited or not. When these nerves are excited, an increase in the number of blood corpuscles takes place, and with it there is increased capacity on the part of the blood to absorb oxygen. The difference between the blood of the splenic veins and arteries is only perceptible after section of the splenic nerves. When this has been done, the blood of the splenic vein exceeds that of the arteries in the number of the corpuscles and in the absorptive capacity for oxygen. This difference is the function of the spleen; for it does not occur in other kinds of blood, as, for instance, in that of the jugular vein after section of the sympathetic nerve. The authors then investigated the influence of section of the splenic nerves upon the number of the blood-corpuscles of the blood as a whole, and found an increase more or less considerable in amount in arterial blood shortly after the operation, which, however, soon disappeared. Under normal conditions venous splenic blood is richer in corpuscles than the arterial blood of the body generally. As a mean of four observations, the former contained 5,352,500 corpuscles, the latter 5,092,500 corpuscles in one cubic centimèter.

3. M. Freyer, who operated on Neumann's plan of puncturing the spleen with a capillary tube, found in more than 100 preparations from 19 human spleens of persons of various ages no transitional forms, that is to say, no cells which in the recent state are round, yellowish, and contain one or more colourless nuclei, which present on the addition of acetic acid a homogeneous fat-like appearance. It was only in embryos, or in children who had died shortly after birth, that he found a few nucleated red blood-corpuscles in the splenic venous blood. Hence it might be thought that the spleen aids in the formation of the blood only in the earliest period of life, and that it loses the power shortly after birth. But Freyer considers this view is untenable, since similar transitional forms may be found in equal or greater numbers in blood taken from any part of the embryo, and therefore their presence in the spleen is no evidence that they have been formed there. In the course of his observations he made the remarkable discovery that transitional forms of blood-corpuscles are present in the blood of the pig throughout life, which he attributes to the active formation of corpuscles in the medulla of the bones, from which they pass into the circulation. Freyer is thus opposed to the view that the spleen

is a blood-making gland, holding it to be a lymphatic gland in structure and function.

4. Neumann corroborates Freyer's statements. He has observed the gland-cells of the embryonal liver described by Kolliker and Schenk, and regards them as aiding in the development of the red corpuscles. The protoplasm of these giant cells includes colourless nucleated blood-corpuscles, and more or less coloured nucleate corpuscles, constituting transitional forms between the white and red corpuscles. The giant cells appear as outgrowths of the vascular walls, partly projecting into the lumen of the vessel, partly interposed between the proper hepatic cells, which they corrode at the points of contact. He thinks they proceed from the hypertrophied endothelium of the vessels.

5. Dr. Schäfer remarks that if the subcutaneous tissue of the new-born rat be examined under the microscope in an indifferent fluid, it is found to consist chiefly of a hyaline ground substance which is traversed by a few wavy fibres, and has a considerable number of exceedingly delicate more or less flattened cells scattered throughout the tissue. These cells are the connective-tissue-corpuscles, and are characterised by the extraordinary amount of vacuolation which they exhibit. The nuclei are clear and homogeneous, and contain one or two nucleoli. From these cells he maintains the blood-vessels of the tissue are formed, and within them red and perhaps also white blood-corpuscles become developed. Some of the cells may be observed to possess a distinct reddish tinge rather evenly diffused or in patches, not distinctly circumscribed. Others contain either one, two, or a greater number of reddish globules, consisting apparently of hæmoglobin; these soon assume a flattened form as they increase in size, and ultimately become red corpuscles. The cells enclosing them at the same time become lengthened, and in each a cavity is formed, within which the globules lie. The cavity is formed by the fusion of the vacuolæ. Such cells of fusiform shape represent a segment of a capillary, and, uniting by their extremities, gradually form a plexus.

6. Dr. Osler observes that in many diseased conditions of the body, occasionally also in perfectly healthy individuals, and in many of the lower animals, careful investigation of the blood proves that, in addition to the usual element, there exist pale granular masses which, on closer inspection, present a corpuscular appearance. These vary greatly in size, from half or a quarter that of a white-blood-corpuscle to enormous masses, occupying a large area of the field. Careful examination of the edges of such masses with high powers shows that the corpuscles are pale round discs, devoid of granules, and with well-defined contours. In water the corpuscles swell greatly; dilute acetic acid renders them more distinct, while dilute potash solutions quickly dissolve them. Their size varies from $\frac{1}{5000}$th to $\frac{1}{24000}$th of an inch. On watching them for some time, with the addition of a little serum, fine projections are seen at the margins of the mass, which soon exhibit a wavy motion, and finally break away. In addition, swarming granules are present in

abundance, and in the course of two hours there are numerous forms of moving corpuscles and filaments, some of which resemble spermatozoa. Dr. Osler is unable to explain either the origin or the destiny of these corpuscles, though he has observed that they increase considerably in magnitude.

Digestion.—Absorption.

1. Dr. E. Külz. *Steht das sogmannte Facialis centrum in Beziehung zur Speichel-secretion?* (In the 'Centralblatt. f. d. Med. Wiss.,' 1875, p. 419.)
2. C. Kuppfer. *On the Salivary Glands of the Blatta orientalis and their terminal Nervous Apparatus.* (In the 'Beiträge zur Anat. u. Phys.,' als Festgabe C. Ludwig, Oct., 1874.)
3. Olaf. Hammartsen. *Observations on the Digestion of Albumen in New-born Animals.* (In the 'Beiträge zur Anatomie u. Physiol.,' als Festgabe C. Ludwig, Oct., 1874.)
4. Joh. Korowin. *Zur Frage über assimilation der Stärkehaltigen speise bei Saüglingen.* (In the 'Jahrb. f. Kinderheilkunde,' Bd. viii, p. 4.)
5. D. Trifanowsky. *Ueber die Zusammensetzung der menschlichen Galle.* (In 'Pflüger's Archiv,' Band ix, p. 493.)
6. Drs. Pye-Smith and Brunton, and Mr. West. *On the Nature of the Intestinal Secretion.* (In the 'Report of the British Association for the Advancement of Science,' 1874, p. 55.)
7. E. Fleischl. *Von der Lymphe und den Lymphgefüssen der Leber.* (In 'Ludwig's Arbeiten,' b. ix, 1874, p. 24.)
8. MM. Martin, Ruge, and Biedermann. *Untersuchungen des Harns während der ersten* 10 *Lebenstage.* (In 'Centralblatt. f. d. Med. Wissenschaften.')
9. Dr. A. Fränkel. *Ueber den Einfluss der verminderten Sauerstoffzufuhr zu den Geweben auf den Eiweisszerfall im Thierkörper.* (In 'Centralblatt,' 1875, p. 739.)

1. Dr. E. Külz, of Marbourg, asks the question whether the so-called facial centre stands in relation to the salivary secretion. Eckhard, after practising the *piqûre* in dogs and rabbits, observed salivation. Nöllner, who noticed the phenomena more particularly in the dog, came to the conclusion that unilateral injury of the floor of the fourth ventricle causes salivation in *both* salivary glands as well as in the parotid of the injured side. The secretion is more abundant the more complete the destruction of the nerve nuclei and fibres belonging to the secretory nerves, whilst the increase is exceedingly small, or does not occur at all if the nerve nuclei or fibres accidentally escape being injured. Grutzner found subsequently, without being aware of Nöllner's observations, that irritation of the medulla oblongata when the chorda tympani was preserved intact, caused free secretion, but much less if the chorda were divided, and scarcely any if the sympathetic nerve also divided. Külz repeated the experiments of Nöllner and established their accuracy. It may therefore fairly be admitted that a centre for the salivary secretion exists in the floor

of the fourth ventricle, and this is in accordance with pathological observations. More recently Fritsch and Hitzig have shown that there is an area on the surface of the cerebrum which, on irritation, causes movements of the muscles supplied by the facial nerve, and though their observations have been repeated by many observers, Külz appears to be the first to draw attention to the circumstance that the so-called facial centre is of importance in regard to the salivary secretion. Külz used dogs which were advisedly not previously anæstheticised. Both submaxillary ducts were exposed, and a canula inserted into each. Irritation of the mouth with a little acid caused abundant secretion, and showed that both ducts were pervious. The part of the hemispheres of the brain containing the facial centre was then exposed and induced currents employed, gradually rising in intensity till the facial muscles contracted. When the excitation was of short duration, Külz was *not* able in any instance to discover an increase in the salivary secretion; if, however, a current of considerable intensity was transmitted for a long time, then violent contractions of the facial muscles, extending to those of the body generally, occurred, and coincidently with this, well-marked salivation was established. Külz therefore concludes that there is no special centre for the salivary glands connected with the facial centre.

2. C. Kuppfer, using very high magnifying powers, satisfied himself that numerous nerve fibrils penetrate into the peripheric cells of the acini of the salivary glands in the cockroach. The preparations were made by teasing the gland out with needles, after immersion for an hour or two in iodized serum, or in a two per cent. solution of potash. The gland had in some instances been exposed to the vapour of osmic acid.

3. M. Hammartsen macerated the gastric mucous membrane of animals newly born, immediately after death, in water acidulated with $\frac{1}{1000}$th of hydrochloric acid, the vessel being kept at ordinary temperature. The digestive power of the fluid was tested by means of fibrine and the white of egg, at a temperature of 100° F. He states that he found no notable difference in regard to the amount of pepsine present between the stomachs of fasting animals and those that had suckled. From the results of a considerable number of experiments, he finds that during the first week after birth the stomach of the dog contains no pepsine. It is only in the course of the second week that it begins to make its appearance. During the third and fourth weeks it contains as much relatively as the stomach of an adult. The stronger the animal the earlier does pepsine make its appearance. The question M. Hammartsen then proposed to himself was, how the albumen of milk was digested during the first period of life, and he replies by observing that experiments prove that the infusion of pancreas perfectly digests fibrine, as well as decomposes the fats, and it is by these means, therefore, that the digestion of the fibrinous and albuminous compounds is effected. The results of his experiments on cats were the same as on dogs, with the exception that he obtained

evidence of the presence of pepsine in the gastric mucous membrane of kittens only eight days old. In rabbits the formation of pepsine appears to take place at a still earlier period, for at the commencement of the second week the stomach contained notable quantities. The stomach of the new-born infant sometimes contains pepsine; but it contains in addition a ferment capable of coagulating milk, since the milk of woman is only coagulable with difficulty by acids alone.

4. Dr. Korowin has made a series of observations on the power of infants to digest farinaceous food. He washed out the mouth with distilled water and then introduced a perfectly clean sponge. The fluid extracted from this was added to starch paste, and after a while the mixture was tested for sugar by Fehling's test. In other instances he removed the parotid gland from recently dead infants, and having rubbed the gland tissue down to a pap with pure sand, macerated it with distilled water, and again tested its saccharifying power. Lastly, he performed the same experiment as the last with the pancreas. The results he obtained are as follows:—The secretion of the oral cavity in infants of a few days old may be obtained, but to procure the pure saliva is extremely difficult. The quantity of saliva increases considerably towards the close of the second month of life, and continues to augment from that time forwards. The oral cavity in infants, unless the greatest possible care has been taken in its purification, almost always reacts distinctly acid. After it has been thoroughly washed out and cleansed, the reaction may be either feebly acid, neutral, or (rarely) feebly alkaline. The secretion of the oral cavity of the infant, even within a few minutes after birth, has a distinct fermentative influence on starch; as age advances the power becomes more and more marked. The extract of the parotid gland always possesses a saccharifying power, whilst the extract of the pancreas for the first three weeks of life has no such action. At the commencement of the fourth week, however, it begins to possess it, and gradually becomes more marked, but it is still feeble at the close of the first year of life.

5. In Trifanowsky's researches on the composition of human bile, two quantities were experimented on: the first from subjects indiscriminately, without regard to the disease causing death; the other from a perfectly healthy subject. In both cases careful examination showed that after treating with potassium-nitrate, the bile contained sulphuric acid, thus proving the presence of taurocholic acid, though Jacobson had in a previous analysis been unable to demonstrate any trace of it. The amount of biliary acids present in the first portion was estimated at 2·845 per cent.; in the second, at 2·362 per cent. In the precipitate caused by ether, which chiefly consists of biliary salts and soaps, more nitrogen was found than was accounted for by the biliary salts present, and examination showed in the other precipitate a certain amount of neurine. It does not seem probable that the neurine is a product of the disintegration of lecithin, but rather that the neurine is contained as a saline combination with the biliary acids or fatty acids in the bile.

6. Drs. Pye-Smith and Brunton, and Mr. West, first give a *résumé* of the opinions held by physiologists in regard to the action of the nerves on secretion. They point out that the statements made by Pflüger, in regard to the mode of termination of the nerves in the gland cells, is generally accepted from a belief of its necessity, and from the corroborative experiments of Heidenhain, who showed that if the chorda tympani be paralysed by atropin, the stimulation of that nerve is still followed by dilatation of the blood-vessels and quicker flow of blood through the gland, yet that no saliva is secreted. They also found out that secretion of saliva may be excited by reflex irritation conducted through the sensory nerves of the tongue, and also by paralysis of certain nerves going to the gland. They believe that the process of intestinal secretion is performed in a very similar manner to that of the salivary glands. Because, 1. When the process of digestion is going on, and the food is present in the intestines, these vessels are fuller than at other times, just as they are in the salivary glands. 2. Stimulation of the mucous membrane of the intestines induces secretion of intestinal juice, just as stimulation of the mucous membrane of the mouth induces a flow of saliva. 3. Section of all the nerves going to the intestines produces a profuse secretion of intestinal juice, which resembles the paralytic secretion observed in the submaxillary gland after section of its nerves. The intestinal secretion can therefore be excited like the salivary one; 1st, reflexly, by stimulation of the mucous membrane of the intestine; and 2ndly, by division and consequent paralysis of all the nerves passing to the intestines. Unlike the salivary secretion, however, it has not yet been induced by direct stimulation of the secreting nerves, and, indeed, these nerves are not yet known; they are, probably, extremely short, and situated in the walls of the intestine itself. The stimuli which excite secretion, when applied to the intestinal mucous membrane, are of various sorts. Mechanical stimulation, such as tickling the surface of the mucous membrane, at once excites it. The application of dilute hydrochloric acid, and induced electrical shocks, have a similar effect. Sulphate of magnesia and other purgatives do so after a longer or shorter interval. The authors of the paper endeavoured to ascertain, first, whether other neutral salts have a similar effect to magnesium sulphate on intestinal secretion; secondly, whether any other compounds have the power of preventing such action; and, thirdly, what are the nerves which regulate this secretion during life. The salts they experimented with to test the first point were magnesium sulphate, potassium acetate, chlorate, ferrocyanide, iodide, sulphate, and phosphate, and tartrate of potash and soda, and they found that several of the other neutral salts possess a similar action to that of magnesium sulphate, though none are so constant or so marked in their action. The effects produced on similar portions of intestine with similar quantities of salts differed considerably in different experiments, perhaps dependent on the stage of digestion when the injection was made. Secondly, they tested the effects of various drugs in preventing this action of

neutral salts, and for this purpose took a saturated solution of magnesium sulphate, as that of which the action is the most constant yet ascertained; in some cases the modifying agent was mixed with the magnesium sulphate; in others it was subcutaneously injected. Atropine, methyl atropia, chloral hydrate, emetin, morphia, sulphate of quinine, tannin, and sulphate of zinc were mixed, and chloral and acetate of morphia injected subcutaneously. In none of these experiments was there any effect of the above drugs in diminishing the average amount of secretion produced by magnesium sulphate. There does not appear, therefore, to be any action analogous to that of atropia upon the secretion of the submaxillary gland. Direct ligature of the mesenteric veins produced profuse hæmorrhage into the loop of intestine without any apparent secretion. Lastly, they investigated the precise manner in which the nervous system influences secretion. In one out of three experiments ligature of the nerves supplying a portion of intestine produced, as in Moreau's experiments, profuse secretion, division of the splanchnics, and ablation of the semilunar ganglia, caused (with one exception, in eighteen experiments) no increase in the amount of secretion.

7. Fleischl observes that if the ductus communis choledochus be tied in a living dog, and shortly afterwards the lymphatics proceeding from the *porta hepatis* to the cisterna chyli be exposed, the naturally colourless contents of the lymphatics become stained of a yellowish colour. This led to his present investigation. He first ascertained that the yellow tint was due to bile. The bile, its natural course through the ducts being stopped by the ligature, readily entered the lymphatics of the liver, and from thence passed by the ductus thoracicus with the blood; when, however, the thoracic duct was also ligatured, the bile did not enter the blood, or entered it only in very small quantity. He describes in detail the appearances presented by the connective tissue accompanying the vena hepatica, and shows that it contains a close network of lymphatic vessels, and this he shows to be continuous with a firm plexus in the substance of the liver, the meshes of which enclose the biliary cells.

8. MM. Martin, Ruge, and Biedermann, in their researches on the urine during the first ten days of life, found, from observations made on seventeen infants, that the discharge of urine rarely takes place immediately after birth, but usually occurs at the close of the first day, frequently in the course of the second day, and sometimes even not till the third day. The first evacuation amounts to about eight cubic centimètres. During the following ten days the average of about twelve observations for each day gave the following numbers in cubic centimètres: 12, 12, 23, 39, 35, 55, 51, 55, 31, 61. The colour on the first day corresponded to number one or two of Vogel's scale; but it afterwards became nearly as pale as water. The reaction was almost uniformly acid, very rarely neutral. The specific gravity of the urine first discharged was, from an average of seven observations, 1010·5. After the second it gradually fell, till, on the tenth day, it only amounted to 1002·7. Every case

examined showed albumen in the urine; the albumen appearing in the first day in ten cases; five in an evacuation on the second, third, or fourth days; and three in every evacuation up to the end of the third day; seven showed albumen at a later period. The proportion of chlorides amounted to 0·088 per cent. on the average. The urea to 0·457 per cent., or, if exceptional instances were not reckoned, to 0·321 per cent. Uric acid was constant, the quantity increasing to the third day and then diminishing. Under the microscope the urine presented some epithelial cells, and masses of uric acid crystals. Half of the cases contained hyaline cylinders, sometimes slightly cloudy. In two cases there were large thick cylindrical structures.

9. In this paper Dr. Fränkel seeks to show that a diminished supply of oxygen to the tissues, however this may be effected, is constantly followed by an increase in the amount of urea eliminated; that is to say, it causes an augmented disintegration of albumen. His experiments were made on large dogs, into the tracheæ of which fistulæ were made, and canulæ introduced, by means of which the supply of air could be accurately regulated. The animals were in a condition of uniform nitrogen excretion. In one experiment, lasting six hours, the amount of urea eliminated, when the amount of oxygen admitted was reduced to the minimum, was doubled. He found the amount of urea was in a similar manner increased when the animals were exposed to the action of carbonic oxide poisoning. He draws the same conclusion from the increased amount of urea eliminated after bleeding, which was observed by Voit and Bauer, and also from the similar increase that occurs after the administration of various kinds of poisonous substances, as phosphorus, arsenic, and mineral acids, and perhaps alcohol, all of which have this in common, that they greatly increase the disintegration of the tissues (alcohol more slowly than the rest), while they lower the oxidising processes. Fränkel then discusses the conditions of oxidation in the economy at considerable length, and concludes with the observation, that the facts he relates demonstrate that urea is the result of a series of processes of disintegration and synthesis, and is associated with a comparatively small absorption of oxygen.

Nerve—Muscle.

1. Fleischl. *Sur la nature du Cylinder-axe.* (In 'Beiträge zur Anatomie und Physiologie,' als Festgabe C. Ludwig, zum 15, Oct., 1874.)
2. Dr. Woroschiloff. *Der Verlauf der motorischen und sensibilen Bahnen durch das Lendenmark des Kaninchen.* (In 'Ludwig's Arbeiten,' B. ix, 1874, p. 99.)
3. A. Bloch. *Sur la vitesse du courant nerveux sensitif de l'homme.* (In Brown-Séquard's 'Archives de Physiologie,' Sept., 1875, p. 588.)
4. L. v. Thanhoffer. *Die beiderseitige Reizung des Nv. vagus beim Menschen.* (In 'Centralblatt. f. d. Med. Wiss.,' 1875, p. 403.)

5. Dr. Pasquale Malerba. *Observations relative à la physiologie du Nerf vague faites sur l'homme vivant.* (Brown-Séquard's 'Archives de Physiologie,' No. 5, 1875.)
6. v. Urbantschitsch. *Zur Anatomie der Tuba Eustachii des Menschen.* ('Wien. Med. Jahrb.,' 1875, p. 39.)
7. Prof. Michel. *On the Mode of Radiation of the Fibres of the Optic Nerve in the Retina of Man.* (In the 'Beiträge zur Anatomie v. Physiol.,' als Festgabe C. Ludwig, Oct. 15, 1874.)
8. James Dewar and John McKendrick. *On the Physiological Action of Ozone.* (In the 'Proceedings of the Royal Society of Edinburgh,' Session 1873-74, p. 211.)
9. James Dewar and John G. McKendrick, M.D. *On the Physiological Action of Light.* (In the 'Transactions of the Royal Society of Edinburgh' for 1872-73, p. 141.)
10. Kronecker. *Sur la caracteristique du mouvement du muscle cardiaque.* (In 'Beiträge zur Anatomie und Physiologie,' als Festgabe C. Ludwig, Oct. 15, 1874.)

1. According to F. Fleischl, the cylinder axis during life is formed by a column of liquid, having a volume equal to more than half the size of the entire fibre, and contains a substance coagulating easily, but in a different manner according to the reagent employed. With alcohol the cylinder axis is thick and equable, with chromic acid it is thin and irregular.

2. Dr. Woroschiloff describes an instrument he has invented, by means of which accurate sections of any portion of any horizontal plane of the spinal cord may be divided—a kind of guillotine—and gives a number of photographic plates showing the parts divided, the description of the plates stating the effects produced. He finds, amongst other things, that the white posterior and anterior columns and the entire grey substance may be divided transversely on the same plane without apparent loss of the reflex connection between the lower and the upper portions of the body, or any disturbance in sitting or leaping. He also finds that the lateral columns increase from below upwards, as if they received a certain number of fibres from each turn, joining the cord, and conducted them to the brain.

3. Bloch has made some new experiments on the rapidity with which sensory impressions are transmitted in man. Schelske, it is well known, estimated the rapidity at 29·60 m. per second. Bloch criticises Schelske's and other methods of experimentation hitherto employed and devises a new mode. He considers that no method involving the will of the experimenter is reliable, since the duration of this factor is unknown and is probably variable. He points out that, from mere custom, impressions received at the tips of the fingers are registered with greater rapidity than those received by the skin of the arm. He maintains that the rapidity of the sensory current may be determined by attending exclusively to sensations without the participation of any other physiological phenomena.

His method is founded on the persistence of sensations for a greater or less period. If two shocks are received successively, one by each hand, and the interval between these two shocks is sufficiently short (one forty-fifth of a second, for example), the two sensations are perceived simultaneously. The only possible explanation of this is, that the sensation of the first shock persists with apparently undiminished intensity when the sensation of the second shock arrives. This persistence appears to augment in duration in proportion as the shock is more sudden, but in very small proportion. If in place of the finger the second shock be received on some part more closely contiguous to the sensorium—such, for example, as the ala nasi—apparent synchronism is obtained by allowing a longer period to intervene between the two shocks than when the two hands are experimented on. The difference of the two intervals measures the duration of the transmission of the impression from the hand and from the nose respectively to the brain. Conversely, if the shocks be received on the finger and on the toe, the interval required, in order to obtain apparent synchronism, ought to be diminished by the time representing the difference of duration of sensory transmissions from the foot and from the hand respectively to the sensorium. These calculations rest on an hypothesis which has been ratified by experience—namely, that the reception of an impression is equal for all the points submitted to shocks. Experiment shows that the persistence of a sensation augments in proportion as the sensibility of the region excited diminishes. The results of Bloch's observations rest on an approximation of two ten-thousandths of a second. They give as certain and incontrovertible conclusions the two following formulæ:—1. The rapidity of transmission is greater in the spinal cord than in sensory nerves. 2. The mean of all the experiments, without distinguishing whether the path pursued by the impression was in the sensory nerves or in the spinal cord, was 156 meters per second. Experiments, however, made with the nose, hand and foot, permit more precise deductions to be drawn, and give as final values, taking into account the length of the spinal cord and of the nerves, the following speeds:—In the spinal cord, 194 meters per second; in the nerves 132 meters per second.

4. Dr. Thanhoffer states that, actuated by zeal for science, his pupil, Ignacz v. Kovacs, made, and permitted to be made on himself, various experiments, to ascertain the effect of mechanical irritation of the vagus. The nerve of the right side alone was first stimulated on the inner side of the sternomastoid with the top of the left index finger, at the level of the external angle of the thyroid cartilage. A sphygmograph was applied to the right radial. When the pressure was made on one side the pulse at once became smaller, but the blood pressure did not (as Ludwig and Hoffa found to occur in animals) fall. M. Kovacs now made pressure on both sides, when, to Thanhoffer's dismay, the lever of the sphygmograph suddenly fell, and remained so for sixty-seven seconds, when it rose a little, and then remained motionless, the heart having ceased to

beat. Kovacs stared at him with glazed eyes without releasing the vagi from the pressure he had been applying, and, without speaking, v. Thanhoffer proceeded to remove his hands from the neck, which required considerable force, and the fingers remained bent. After a short period of loss of consciousness on the part of M. Kovacs, and of terrible anxiety to M. v. Thanhoffer, consciousness slowly returned: he, with a little assistance, washed his head and face, but he was unable to stand. His pulse was abnormal, but strong, and the heart's sounds clear and strong. On getting him into the fresh air he felt faint, and his head was heavy, and both it and his chest felt as if bound with iron hoops. Soon after he had a rigor and a slight febrile attack. During the rest of the day his pulse was about six beats per minute less than normal, and he experienced the sensation of nausea. He slept deeply, woke with a headache, and the following day recovered his usual condition.

5. Dr. Malerba's observations on the pneumogastric nerve were made on a captain under treatment for an abscess, which had exposed the deep parts on the right side of the neck. On making injections into the cavity, the patient complained of severe pain in the occipito-mastoid region, and of a peculiar sensation he was unable to describe, Dr. Malerba, suspecting that the vagus was irritated, felt the pulse, and on repeating the injection found that it suddenly stopped, but reappeared after an interval of a few seconds, the pulsations being at first stronger than natural, and afterwards becoming slower. He further noticed that the right temporal artery, with its ramifications, became larger and non tortuous, whilst that of the opposite side was scarcely visible under the skin, a phenomenon doubtless due to paralysis of the vaso-motor nerves of the same artery, which, a few minutes after the dressing had been applied, recovered its normal volume. The patient complained also of continual and harassing salivation, the secretion being abundant and very tenacious. Deglutition, especially of solid food, was performed with difficulty. The patient died from exhaustion. The following conclnsions are drawn by Dr. Malerba from his observations:—

1. The nerve, touched by the extremity of the syringe, was the right vagus. 2. The irritation was partly due to the temperature of the liquid (thermic excitation), and partly, though only to a small extent, by the force of the jet (mechanical excitation), and partly perhaps to the action of the sulphite of soda (chemical excitation). 3. The vagus nerve acts as a moderator of, or it inhibits the movements of the heart, because, were it an acceleration, it would be contrary to physiological analogies that irritation should retard or arrest its movements. 4. The vagus is probably more sensitive in man than in animals. 5. The peculiar sensation experienced by the patient may be explained by the momentary suspension of the circulation in the brain. 6. The augmentation in the volume of the temporal artery renders it probable that the vagus in the neck contains centrifugal fibres (fibres of Ludwig and Cyon), the excitation of which is capable of suspending the normal incitation,

which, originating in the cells of the vaso-motor centre of the medulla oblongata, confers tone upon the muscular fibre of the arterial coats. 7. The ptyalism that annoyed the patient demonstrates also that irritation of the vagus in the neck acts in a reflex manner on the trophic nerve of the salivary gland. 8. The pain experienced in the mastoid region was an excentric nerve symptom. 9. The contraction of the pharyngeal muscles was certainly occasioned by the alteration of the pharyngeal branches of the vagus.

6. Urbantschitsch describes some peculiarities in the attachment of muscles to the Eustachian tube. In regard to the cartilaginous part, he distinguishes the proper tubal cartilage, the tubal process, and accessory tubal pieces, which he describes in detail.

7. Professor Michel states that in the human retina the fibres issuing from the papilla and running outwards, are the fewest in number; those running inwards are more numerous, and still more those passing in other directions. As soon as the fibres leave the papilla they are grouped into small fasciculi. Those that run from above downwards, in place of being radiated, are slightly curved, and the curvation increases in proportion as they issue more externally to a line uniting the papilla with the macula. The chief peculiarity of the fibres of the optic nerve in the retina is that they present a plexiform character.

8. In Dewar and McKendrick's experiments the ozone was made by the passage of an induction current through air. The results of numerous experiments were, that the inhalation of an atmosphere highly charged with ozone *diminishes* the number of respirations per minute. That the pulsations of the heart are reduced in strength, and the organ is found beating feebly after the death of the animal. That the blood is always found in a venous condition in all parts of the body, both in cases of death in an atmosphere of ozonized air and of ozonized oxygen. That ozone exerts a destructive action on the living animal tissues if brought into immediate contact with them, but does not affect them so readily if they are covered by a layer of fluid. Lastly, that ozone acts as an irritant to the mucous membrane of the nostrils and air passages, as all previous observers have remarked.

9. Drs. Dewar and McKendrick consider that they have experimentally proved—1. That the impact of light on the eyes of members of the following groups of animals, viz., mammalia, aves, reptilia, amphibia, pisces, and crustacea, produces a variation amounting to from three to ten per cent. of the normal electromotive force existing between the corneal surface and the transverse section of the optic nerve. 2. That this electrical excitation may be traced into the brain. 3. That those rays that we regard as most luminous produce the largest variation. 4. That the alteration of the electrical effect with varying luminous intensity seems to follow very closely ratios given by the psychophysical law of Fechner. 6. That the electrical alteration is due to the action of light on the retinal structure itself, and is independent of the anterior portion of the eye; therefore, the natural supposition that the contraction of the iris

might produce a similar result cannot be sustained. 6. That it is possible by experiment to discover the physical expression of what is usually called, in physiological language, fatigue. And, 7, that the method employed in this research may be applied to the investigation of the special organs of the other senses.

10. M. Kronecker observes that one contraction of the heart leads to another, whilst an arrest of the heart's action renders a recurrence of the contraction more difficult. The temperature and the nature of the liquid by which the heart is surrounded influence its contractions. Its maximum of irritability appears to be when it is immersed in serum, at a temperature of about 77° Fahr. At a higher temperature it responds less easily to excitants, but on cooling it recovers its excitability, unless it has been heated to 107·3° Fahr. At this temperature it loses it entirely, and becomes rigid. If the heart were previously cool, and were then rapidly heated to about 86° Fahr. it becomes rigid. On cooling, the movements of the heart become slower, supposing it to contract every tenth second: if it be irritated, say, at the end of every five seconds, the excitations remain without effect, and it only contracts every ten seconds. M. Kronecker discusses the question whether the heart is capable of exhibiting true tetanus, and arrives at a negative conclusion. A heart exhausted by fatigue can be reanimated by the injection of serum with a few drops of blood mingled with it, and its beats may become more vigorous than at first if the heart contained only serum without the blood. If the serum be replaced by a weak solution of sodium chloride, the heart rapidly stops in diastole, and remains insensible to the strongest stimuli; on injecting an oxygenated fluid, however, it slowly recovers its contractile power. The heart is capable of producing work in a very economical manner from any pabulum that may be presented to it; but if there be no pabulum it cannot borrow from its own proper substance: it ceases to work.

REPORT ON SURGERY.

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Surgical Pathology and Therapeutics.

1. *On the Antiseptic as compared with the "Open" Treatment of Wounds.* By Dr. R. U. Krönlein, of Berlin. ('Archiv für Klinische Chirurgie,' Bd. xix, heft 1.)

The author, in concluding an elaborate analysis of comparative results, obtained in the surgical clinics of Zurich, Leipsic, and Halle, from the antiseptic and open treatment of wounds, deals with questions which, though secondary in importance to that of the

comparative mortality attending these two methods, need to be taken into consideration before we can decide as to the complete advantage of one over the other. He discusses whether in carrying out the antiseptic treatment, and discontinuing the open method of dressing, the patients are cured more rapidly and effectually, and with less inconvenience and expense, and whether the former method is generally applicable.

With regard to the duration of treatment, there can be no doubt that the method of treatment of wounds which favours primary union and immediate or speedy healing, ought to be preferred to a method which does not in all instances aim at the so-called *prima reunio*, or which renders such a mode of healing impossible. The antiseptic treatment is practised with the former aim, the open plan of treatment with the latter aim. The motto of the former is *cite et tuto*, that of the latter *lente sed tuto*. Tables of cases collected by the author show that in amputation wounds, dressed by the antiseptic method, the period of healing was much shorter than in those left exposed. In amputations, both of the upper and lower extremities, the duration of the antiseptic treatment to that of the open treatment is found to show a proportion of one to two. The author states that the results of the antiseptic treatment after amputation in the above-mentioned hospitals have been brilliant, and he asks, "With what other treatment of such cases could cures be effected in fourteen and nineteen days after amputation of the thigh, in eleven days after amputation of the leg, and in fourteen and twenty-one days after amputation of the arm?" The best results, he states, were obtained from Professor Thiersch's treatment (with salicylic acid). In four instances amputation wounds of the thigh and leg healed after two dressings only, and in one case complete healing was effected after three dressings. In consequence of this abbreviation of the period of treatment, the advantage of the old over the antiseptic treatment of dressing, as to cost of material, is neutralized, as are also the trouble and consumption of time required in the latter method. Again, with a speedy union of a large wound there is less secretion, and less loss of albuminous material by the patient. The pernicious influence of a long sojourn in a hospital, the so-called hospital cachexia, which even the healthy organism is incapable of resisting, is much reduced by shortening of the period of treatment.

The question as to the superiority of one over the other plan of after-treatment, with regard to the subsequent condition and capacity of the wounded member must, according to Dr. Krönlein, be answered in favour of the antiseptic plan; inasmuch as this plan brings about this healing by primary union, which, of all modes of healing, corresponds most to the requirements of the surgical art. As Volkmann states, "A stump which has healed by primary intention is free from all the defects inherent in one which has become cicatrized after a prolonged process of healing by granulation. The former is beautifully rounded, the bone is well covered by soft parts, is not thickened by callus, and not, as is so often the case in the latter

kind of stump, adherent to depressed and retracted skin; consequently neuralgic pains and excoriation of the scar seldom occur."

The question as to which method causes the less pain cannot be so easily answered, since we have to depend more upon the patient's sensations and objective signs by which these are manifested, which signs are generally deceptive and variable according to age, sex, breeding, race, &c. Analgesia during the course of healing is an advantage attendant both on the open and on the antiseptic plan, since in both these the wound is protected from the contact of a number of irritating agents, which in other plans of treatment are being constantly applied, and which cause pain and intensify the inflammatory action set up by the injury or operation. The irritants excluded by the open plan of treatment are mechanical, whilst those secluded by the antiseptic plan are chemical. In consequence of an almost total exclusion of excitants of inflammatory action from the region of the wound and the raw surface, not only is there an absence of local inflammation, but there is very often an absence of traumatic fever. Volkmann reports that a very large number of patients with severe wounds, due either to injury or operations, who, under any other plan of treatment would certainly be very feverish, do not, when treated by the antiseptic plan of dressing, present any signs of elevation of temperature. Professor Thiersch also reports that he has frequently observed this absence of fever after some operations, where the wounds had been dressed with a solution of salicylic acid. Dr. Krönlein, however, states that such observations of absence of fever soon after major operations, and very severe local injuries, are not exceptional in cases that have not been treated by the antiseptic plan. Three years ago he reported six cases of amputation (one thigh, one arm, three fore-arms, one foot), in which the stumps were left open and exposed to the air, and in none of these was the temperature ever during the process of healing above 100°. The great fault in Lister's plan is the use of carbolic acid as an antiseptic, as this agent not only irritates raw surfaces, but often acts as a poison, sometimes a fatal one, on the general organism. A case has been reported by Volkmann in which, after amputation of the penis and some diseased inguinal glands, the application of a three per cent. solution of carbolic acid to the resultant wounds was followed by prostration, coldness of the surface, vomiting, and a tarry appearance in the urine. The same surgeon believes also that death after resection of the hip was due in one case to carbolic acid poisoning. A temporary elevation of temperature has been sometimes observed after an application of the carbolic acid spray to a wound during a prolonged dressing, which elevation of temperature Dr. Krönlein attributes to irritation of the raw surface by the caustic antiseptic agent. Dr. Krönlein is disposed to believe that fatal cases of poisoning by carbolic acid, applied externally as an antiseptic agent, have of late occurred with greater frequency than is generally supposed, and he regards many of the deaths after amputation with Lister's treatment, which have been reported as due to anæmia, shock, and

exhaustion, to have resulted rather from acute carbolismus. He asserts that salicylic acid is an agent which, whilst not inferior in its antiseptic properties, is free from the poisonous and irritating properties of carbolic acid.

2. *On Hospital Gangrene.* Professor Von Nüssbaum, of Munich. ('Archiv für Klinische Chirurgie,' Bd. xviii, hft. 4.)

The author gives a graphic account of a severe and prolonged attack of hospital gangrene in the State Hospital of Munich, and testifies very warmly to the great benefit to be derived from practising Lister's antiseptic dressings in the treatment of this affection. In the Munich Hospital, which for many decennial periods had not been free from pyæmia, this form of gangrene appeared for the first time in 1872, when twenty-six per cent. of all the patients with open sores or wounds were attacked. In the following year fifty per cent. of the patients suffered, and in 1874 no less than eighty per cent. The first case, in 1872, was that of a journeyman, who, before his admission into the hospital, had been treated for a slight stab in the chest, under very unfavorable conditions, and in a dark and damp cellar. The wound, which was foul, and covered by a greyish deposit, soon became clean on the application of a mercurial lotion, and the man in the course of a few days was cured. The subjects of wounds and ulcerations lying to the right and left of this patient's bed soon became affected, however, with local sloughing and gangrene, and during the following three years the hospital was never free from this complication. The form of the gangrene and the severity of the local lesions varied at different periods, but there seems to have been a constant and gradual tendency in the disease to extreme intensity and rapidity of action. In the first year the signs consisted merely of a greyish deposit on the affected wound or ulcer, and an erysipelatous redness of immediate parts, and the constitutional reaction was very slight. In 1873 and 1874 the local changes consisted in diphtheritic deposits, necrosis of the surface of the wounds, hæmorrhagic suffusion, widely spread erysipelatous redness of the surrounding parts, rapid destruction of fascia, tendon, and muscle, exfoliation of superficial parts of bone, and occasionally ulceration of a large artery. In these latter and more severe cases of hospital gangrene there were considerable constitutional disturbance and a rise of temperature, which generally preceded by a few hours the manifestation of the local phenomena. When the evening temperature of any patient under treatment for wound or ulcer had been found as high as 104·8° or 105·8°, the appearance of hospital gangrene on the following morning was always confidently anticipated. Notwithstanding this priority of the febrile phenomena, Professor Von Nüssbaum holds that hospital gangrene is a strictly local affection, and states, in confirmation of this view, that it frequently happened with patients under treatment for multiple wounds, that one only of those took on bad action after a rise of evening temperature, or that the gangrene had ceased in one wound before attacking another. It was found that no protection against hospital gangrene could be afforded by age, sex, or constitution. During the three years of the prevalence of this

affection to so great an extent in the wards of the Munich Hospital, Professor Von Nüssbaum did not meet with a single instance of this form of gangrene in his extensive private practice.

Some details of much practical interest with regard to the preventive and curative treatment of the local affection are given in this paper. In 1872, the first year of its appearance in the hospital, the gangrenous condition of the wounds in those attacked was always readily and successfully controlled by the local application of lotions containing nitrate of silver, corrosive sublimate, or carbolic acid; but as the distinctive changes became more and more acute, it was found necessary to have recourse to more active means, and to apply caustic pastes and the actual cautery. Energetic applications of the latter agent proved the most efficacious, and a perfectly successful result of such treatment was usually indicated by a previous fall of the patient's temperature. During the prevalence of the gangrene many different attempts were made to protect healthy wounds and sores from contagion. The continuous water-bath and applications of ice, moist warmth, and lotions of carbolic acid, salicylic acid, chlorine water, &c., were tried, but without any good results. At last Lister's antiseptic plan of dressing was practised most strictly, so that no open surface was dressed save under the carbolic-acid spray, and no instruments or dressings used save after careful disinfection. The hospital gangrene at once ceased, and not a single case, Professor Von Nüssbaum states, has been observed in his ward since the adoption of this plan of dressing, although at the period of its first use eighty per cent. of the surgical patients had been affected. Professor Von Nüssbaum asserts that he feels it his duty to testify to the efficacy of Lister's method as a prophylactic against hospital gangrene. He insists, however, upon the necessity of carrying out this plan of dressing in all its details. He holds that the secret of its great success in this instance lay in a pedantic exactness in its mode of application, and he expresses it as his opinion that the surgeon who allows a wound to remain for one second open to the air, and unprotected by the carbolic-acid spray, cannot reasonably expect any good results from his practice of Lister's method.

3. *On the Etiology of Acute Purulent Infiltration of Cellular Tissue after Lithotomy.* By Dr. Jaesche, of Nischni-Novgorod. ('Archiv für Klinische Chirurgie,' No. xviii, hft. iv.)

The author states that stone in the bladder is a very common affection in some parts of Eastern Russia, where it prevails endemically, and in almost every instance commences in infancy or childhood. In most cases where subjects are adults, the affection has existed from an early period of life, and has not been brought under surgical notice in consequence of ignorance, indolence, or dread of operative treatment. In these latter cases the stone has generally acquired a large size, and incurable degenerative changes have taken place in the urinary organs. To these results of neglect, viz., a large stone and diseased bladder and kidneys, is to be attributed the frequent fatal result of lithotomy when performed on middle-aged and old

patients. The results of the author's operative treatment of young subjects is reported to have been very successful, and quite in accordance with the recognised surgical dictum that the younger the patient the more likely is lithotomy to be followed by a favourable result. The operation, however, is not invariably free from fatal results in young subjects, but in these death is generally due not to any chronic visceral disease, but to the effects of a phlegmonous erysipelas in the cellular tissue of the lower part of the pelvis, which passes with great rapidity into suppuration, and spreads upwards, usually on the left side, along the sheath of the psoas muscle. In this inflammatory process, which primarily is extra-peritoneal, the serous surface of the peritoneum becomes at last involved. The author was led to investigate closely the etiology of this somewhat rare complication after lithotomy in early life, in consequence of the almost simultaneous occurrence of two cases during the spring of 1870. The idea of an epidemic diathesis was excluded in consequence of the fact that, at the same time, there were lying in the same ward occupied by the two affected patients, two other young patients recently cut for stone, both of whom, although the operations had been performed with difficulty and application of much force, recovered without any bad symptoms. On inquiring into the clinical history of his two unsuccessful cases, the author found that the early symptoms of the pelvic cellulitis came on very soon after an arrest of the flow of urine through the perineal wound, and a consequent accumulation in the bladder. A third case occurred shortly afterwards, in which early but well-marked symptoms of pelvic cellulitis observed to be almost coincident with retention of urine after lithotomy in a child, disappeared after the urine had been drawn off through an elastic tube, which was allowed to remain in the wound. Other cases are reported of purulent infiltration of the pelvic cellular tissue after lithotomy, from the details of which it may be fairly assumed that this serious complication is often, if not always, a result of retention of urine. The author, in conclusion, gives the following explanation of the pathological connection between cause and effect: when it happens that the surfaces of the operation-wound, especially their more external portions, adhere together soon after the operation, and that at the same time the flow of urine through the urethra is prevented through swelling of the mucous membrane or some other cause, the efforts of the patient's will, as may be readily conceived, force the urine into the meshes of the cellular tissue involved in the perineal incision, and so give rise to destructive and spreading inflammation. This is the more likely to take place after lithotomy, as the urine of a subject of stone is usually of abnormal composition.

4. Gosselin.—*On False Abscesses of Long Bones, and on the Neuralgic Osteitis which accompanies them.*

M. Gosselin read an important paper on this subject at a sitting of the Academy of Medicine, October 12, 1875.

After having reviewed the principal memoirs on painful abscess of bone, particularly those of Brodie and Cruveilhier, M. Gosselin

showed, by six observations of his own, that pain is not always the sign of an abscess, and that it is also proved in those cases of false abscesses formed by a cavity not enclosing fluid pus, and situated in the centre of a condensing osteitis. The pain would be due to a neuritis by propagation. It is well known, from the researches of Kobelt and Kölliker, that there are nervous filaments in the proper tissue of bone, which becoming inflamed first causes compression, and then inflammation of the nerves, and so sets up pain of a long duration. It is the same process probably which produces the neuralgia of the fifth nerve in dental caries, and also that noticed after simple fractures.

When this neuralgia disappears, often after many years' duration, it is because the osseous canals of the callus enlarge, and the compression ceases. M. Gosselin counsels in these cases *trephining*, and for the following reasons:—

1. Because the diagnosis is impossible between those cases where there is or is not pus, since a dozen times out of two-and-twenty surgeons affirm to have found pus, and are induced to believe in its existence. When found it must be evacuated, for it is probable that it always acts as a foreign body; and even supposing there is none, we should still operate, for by so doing the patient is sometimes relieved.

2. Because there is no danger attending it; in fact, there are few vessels in condensed bone, and therefore less chances of consecutive inflammation. Moreover, there are dressings which still further diminish these chances.

3. Because trephining acts almost always with certainty upon neuritic osteitis. This amelioration may perhaps be explained by the revolvent movement which acts as in the cases of fracture first alluded to. Perhaps there is at first a recrudescence of the osteitis, but the resolution manifests itself rather later on. To sum up—

1. In long bones condensed by an old osteitis, cavities may exist which are not abscesses, and the neuralgic pains attendant are not due to them.

2. Neuralgic osteitis may exist even without any accidental cavity, but always in hypertrophied bone by an old-standing osteitis.

3. Trephining may be useful, and is not dangerous in those cases of hyperostoses with osteo-neuralgia.

5. *Sub-mucous Epithelial Tumour in the Sigmoid Flexure of the Colon.* ('Le Progrès Médical,' Sept. 4, 1875.)

This case occurred in the practice of Dr. Laveran, of Val-de-Grâce, and is of considerable clinical interest, since it presented great diagnostic difficulties; and it is still more interesting in its relation to the history of tumours in general, for it is an example of a kind rare and hitherto ill defined.

The patient, a soldier, æt. 25, a weak, emaciated-looking man (who had had syphilis), had a tumour in the left iliac fossa, just above Poupart's ligament, hard, nodulated, and about as large as an apple, adherent to the deep tissues, but not to the walls of the abdomen, not painful on pressure, non-pulsating, and presenting no

souffle audible by the stethoscope. The growth had been noticed for about two months. Though itself painless, it caused pain along the course of the anterior crural nerve, chiefly whilst walking. There was no interruption of the circulation in the limb, the inguinal glands were not enlarged. The intestinal functions were normal, the testes were in the scrotum, and no peculiarities existed in the pelvic skeleton or vertebral column. The temperature was high, there was great loss of appetite, and the diagnosis formed was that of a scrofulous or tuberculous tumour of the iliac ganglia.

A month later, however, the tumour became the seat of intense sufferings, the flexor muscles of the thigh contracted, and the limb took the position noticeable in psoritis. An abscess formed just above Poupart's ligament, which was opened, and two days afterwards the patient died. The *post-mortem* examination, made the day after death, showed that it was impossible to isolate the tumour, which adhered to the large intestine; and on raising the mass, a large collection of purulent matter, situated behind it and occupying the entire left iliac fossa, was exposed. The fascia iliaca had almost entirely disappeared, as had also the psoas muscle, in which lay the anterior crural nerve, still preserving its natural colour. The large intestines seemed to enclose the mass, and there was no notable distension of it above the tumour, or any accumulation of fæcal matter. The tumour was developed in the wall of the intestine, but only on one side, and had in nowise constricted its calibre. The mucous membrane was a little puckered, but not ulcerated. The tumour itself was irregular, nodulated, tolerably grown, and about three inches in diameter, and in appearance very like encephaloid. There was no cancerous deposit in any organ. *Histological examination* showed, (1) a fibrous stroma, dense in some places, and very delicate in others, circumscribed, its meshes very irregular in form, some long and large, others small, and rounded. (2) The wall of the lacunæ or cysts was lined by cylindrical epithelium, and in the small ones the epithelium seemed younger, and was ciliated. (3) In the interior of the lacunæ were irregular amorphous masses, partly made up of granular cells, which stained yellow by picrocarminate, whilst the epithelium and the connective tissue became a more or less deep rose. The process of development was easily seen, the new cyst with a regular layer of vibratile epithelium surrounding a small orifice; the larger cyst, with mucus accumulated in its centre, and by pressure causing the breaking down of the epithelial layer, the reunion of several cavities by rupture of the walls, and the edges of the cyst presenting a series of hollows and indentation lined with epithelium. The tumour was very slightly vascular. The crural nerve was the seat of inflammation, well characterised by the proliferation of the connective tissue, and by the varicose condition of the nervous tubes.

Dr. Laveran proposes the term submucous epithelial tumour, instead of myxoid epithelioma, which expresses but incompletely the true nature of the affection.

6. *On Psoriasis Palmaris and Plantaris due to Syphilis.* (By Dr. Emanuel Kohn.)

Syphilitic psoriasis of the palm and sole is nothing more than a gyrated form of a lenticular papular syphilide, the modications presented being dependent on the anatomical conditions of the seats. This form of psoriasis is usually met with on both hands, and when it affects the hands is almost invariably present on the soles of the feet. It has considerable diagnostic significance, as it is never observed, save in the subjects of constitutional syphilis.

There is but one affection that can be well confounded with it, viz. eczema usually localised in the palms and soles. In the syphilitic lesion the efflorescences presented by the same individual are always of the same type; the rash is not pruriginous, and consists of more or less resistent new formations deposited on the skin, over which deposits loose scaly plates are laid; the desquamation is dry, and the "plaques" hard and tough, and sometimes horny; the plaques have a tendency to circular grouping. In eczema palmæ manus, on the other hand, the efflorescences are usually polymorphous, the eruption is pruriginous; it presents the hepatic condition of eczema, and does not produce any new formations; the desquamation is dry in some places, moist in others; horny plates are never formed, and there is no tendency for the patches of eruption to become grouped in circles.

Operative Surgery.

1. *Extirpation of a Tumour from the Bladder.* Billroth's Clinic: reported by Dr. Gussenbauer. ('Boston Med. and Chirurg. Journ.,' No. 93, July 3, 1875.)

The following case of myoma of the bladder deserves attention, as the tumour was correctly diagnosed and extirpated with an unexpectedly good result; also as the method of operating has never heretofore been employed; and, further, microscopical examination proved the tumour to be of a variety rarely occurring in the bladder.

On June 3, 1874, D. J., a boy twelve years of age, was admitted to the clinic of Professor Billroth, suffering, according to his father's statement, from stone in the bladder. He had been troubled for ten months. The first symptoms were pain after passing water, localised in the glans penis and in the region of the bladder. After a while a severe attack of painful micturition set in, which in the course of ten months became more frequent, and often came on so suddenly, that the boy could not prevent a sudden discharge of urine. At the time of admission the patient was obliged to pass his water every ten minutes (a small quantity each time), with frequent and severe pain, partly in the region of the bladder and partly in the glans. The urine was feebly acid, slightly cloudy, but contained nothing characteristic on microscopical examination, except a moderate quantity of pus-corpuscles and a few cells of bladder epithelium.

On examination a tumour was noticed in the region of the bladder, to the left of the median line. It was to be felt through the abdominal walls; it was apparently about the size of the fist, was hard and somewhat sensitive on pressure, slightly movable, attached apparently to the bladder. Per rectum the tumour was also felt. On introduction of the sound it was found to slide over an uneven surface. On careful examination it was noticed that the beak immediately on entering the bladder was pressed forward; and on attempting to move it from one side to another it always slid over an uneven tumour before reaching the back of the bladder.

The combined examination with sound and finger, per rectum, proved clearly that a tumour connected with the back of the bladder hindered the movement of the sound. The consistence of the tumour was that of a fibro-sarcoma, and the size that of a small fist. The rapid growth of the tumour demanded energetic treatment, as it threatened by the suffering which the patient then endured soon to end his life.

The operation was performed on June 15 in the following manner:—After the patient was narcotized, the lateral incision for removal of stone was made. The finger introduced into the bladder showed immediately that a tumour nearly of the size of the fist, with an uneven surface, projected from the posterior wall and extended towards the top of the cavity of the bladder. Owing to its size, it was found impossible to extract the tumour with the finger from the perinæum. A suprapubic incision was then made, without injury to the peritoneum; and, to give sufficient room, both recti muscles were cut across at their insertion; a transverse incision into the bladder was also made. Professor Billroth soon came to the conclusion, after examination with the finger, that the use of the *écraseur* was not practicable or desirable, as the tumour possibly might be already adherent to the peritoneum, in which case the latter would have been so injured as to delay healing. He therefore decided to tear the tumour with his finger near its base, and to cut out the remainder from the wall of the bladder, after passing a ligature round to check bleeding. The extraction of the torn pieces of the tumour was not so easy, in spite of the large size of the incision, as would have been supposed. In dissecting out the pedicle, it was necessary to turn the bladder partly inside out. It then appeared that the tumour took its origin from the muscular coat of the bladder, but had not attacked the outer coat of the peritoneum. The plan was, in case the peritoneum had been opened, to close the hole with sutures. Two arteries were tied, and the ligatures brought out through the upper incision in the bladder. The wound in the bladder was not closed, but a drainage-tube was drawn through the bladder, and brought out at the incision in the perinæum. There was no subsequent difficulty, and in one month the patient was discharged perfectly well. The tumour was found to be a well-marked mixed tumour, principally a myosarcoma, but in places a myocarcinoma.

2. *On the Method of Rendering the Female Urinary Bladder accessible, and on Probing the Ureter in Women.* ('New York Medical Journal,' October, 1875.)

Professor Simon, of Heidelberg, in a very able clinical lecture, translated in the above journal, calls attention to the important connection of these methods with the local treatment of a whole range of diseases, which heretofore could be but incompletely cured, or were totally incurable, and which may now be undertaken with a certain view of success.

With regard, in the first place, to the methods of rendering the female bladder accessible, there are two plans, namely, bloodless dilatation of the urethra, and vagino-vesical section. After reviewing the proceedings of the former English and foreign authors, especially Mr. Heath's, he details his own method of exploration of the bladder, which consists of three *acts*: (1) The slitting of the orifice of the urethra, (2) the dilatation of the urethra itself by means of plug-shaped specula, and (3) the subsequent bimanual digital palpation of the bladder.

Concerning the first act he widens the orifice by small slits. He commonly makes two lateral incisions of a quarter of a centimètre in the upper margin, and one downward of a half centimètre in depth. By these slits several considerable advantages are gained, above all, a forcible intrusion, which, as he has frequently noticed, causes a considerable laceration of the edge of the orifice and sometimes of the mucous lining situated above, is obviated, besides, it allows the finger to enter deeper into the bladder. A further dilatation by the finger is impossible without applying force fraught with danger of lacerating the parts, and therefore a closer examination of the bladder is generally abstained from. This disadvantage is overcome by dilatation, after slits of the above-named depth have been made; the orifice becomes wide enough to admit even the thickest finger without the exercise of any force. If the slits are at first not made sufficiently deep, so that neither speculum nor finger can advance, the stretched tissue is easily split a little more by using them as conductor. In consequence of these slits the finger can go up higher, by the length of the incisions, that is, from a quarter to half a centimètre, to which extent the urethra has been shortened. The small lesion produced by these slits need not be taken into account in diseases where the palpation of the urinary bladder is indicated; and a disadvantage, so far as continence of the bladder is concerned, cannot result from them, as there are very few if any fibres of the muscular coat cut through. The slits skin over and remain permanent. This is favourable in those cases in which repeated explorations or operations in the bladder have to be performed. In these cases explorations can be made easily and without chloroform, as they cause next to no pain. The slitting is best performed by means of scissors.

Dilating the urethra itself with plugs forms the second act. He prefers in all cases the successive use of plug-shaped specula. The finger, being commonly very much wrinkled, has to overcome more

frictional resistance, and does not so easily pierce the urethra as the smooth plug. At narrower points, therefore, such as under the pubic arch, the mucous lining is frequently torn and the muscular coat becomes not only dilated, but also displaced and bruised by the advancing finger. This undue traction is apt to become so sufficiently considerable as to interfere with continence. The favorable results with regard to continence, after proceeding in his method, are due to the small traction exercised on the muscular coat of the urethra, by the successive dilatation with smooth plugs. With plugs the dilatation increases in a perfectly fixed and steady progression, each larger size of the plugs being one millimètre larger in diameter than the one preceding it. These plugs are specula, which are made of hard rubber, cut off straight at the point and shutting with a rounded mandrin. Together with the mandrin each forms a round, smooth plug, which can be readily introduced within the urethra. He makes use of seven different sizes for dilatation. The smallest size is three quarters of a centimètre, the largest two centimètres in diameter. After the thickest has been used, he introduces the finger into the bladder. The dilator-plug offers at the same time the advantages of a speculum.

In the third act the finger is passed through the urethra into the bladder, and the interior of it is examined. The great desirability of a thorough palpation of the bladder seems to have escaped most authors. To this end he finds two manipulations of great service: First, on passing the forefinger through the urethra, he introduces at the same time the middle finger into the vagina, then advances with the forefinger into the bladder until the margin of the septum urethro-vaginale presses against the commissure of the two fingers. If this direction is neglected, and the middle finger doubled into the hand, it will press against the labia majora, and the forefinger will not advance as far by at least one centimètre. The second manipulation consists in pushing the apex of the bladder against the exploring finger with the other hand. The apex is thereby inverted, its mucous surface reached and directly palpated with the point of the finger. Only those lateral parts which are attached to the bone are not so easily reached, but an operation requiring the most accurate control, such as the extirpation of a foreign growth, can be easily performed.

By the method described even the narrowest urethra can be dilated, without the least force, and in a few minutes, to that width which is warranted by the individual circumstances.

But, however easily the operation can be carried out, and however small the lesion attached to it may be, like former methods it might be followed by an injury, incontinence of urine, if certain precautions were neglected.

In order to obtain the advantage of the proceeding, without incurring its risk, it is important to decide the question, To what degree may dilatation be carried without danger of permanent inconvenience?

And after many experiments and operations he arrives at the con-

clusion that, in the adult, dilating-plugs of 6 to 6·3 centimètres in circumference, i. e. 1·9 to 2 centimètres in diameter can be used without danger. In extreme cases, i. e. where the disease justifies a rather more daring course, dilatation may be increased as far as from 6·5 to 7·0 centimètres in circumference. In girls, 4·7 to 6·3 centimètres in circumference are the measures inside of which the surgeon has to keep according to each individual case.

A degree of dilatation in which incontinence need not be feared is perfectly sufficient in a majority of cases, where dilatation is desirable for diagnostic or therapeutic purposes. The dimensions of 2 centimètres in diameter (= to 6·3 centimètres in circumference), are also those of the largest speculum, which is a width sufficient to admit the largest finger into a urethra, without any exhibition of force. The thin stem of an instrument may then be placed at the side of the finger. The increase in circumference must be according to the size of the finger, and must not exceed 0·5 to 1·0 centimètre, so that stem and finger together never have a circumference of more than 7 centimètres. In cases in which dilatation may have to be carried further, in order to make the bladder accessible, the other method, that of vagino-vesical section, would have to be carried out.

Tne second mode of rendering the cavity of the bladder accessible consists in the performance of vagino-vesical section. This is brought to bear in those cases where the dilatation of the urethra has attained no results, or would have to be carried to an undue extent. This operation is attended with a wound, and sometimes with a considerable hæmorrhage; and though the chances of healing after the reuniting of the wound are very good, a vesico-vaginal fistula may remain, which requires another operation.

Professor Simon has carried this mode of rendering the cavity of the bladder accessible still farther on the cadaver, and, by a peculiar direction of the cut, has succeeded in controverting the bladder through the incision into the vagina and even into the vulva itself, so that it can be laid open not only to palpation, but to inspection also.

By a long incision through the whole length of the urethra, to the mouth of the uterus, the bladder could not at all or but very little be introverted. The inversion of the vertex and the upper part of the fundus, however, was obtained after a transverse incision of three centimètres in length, into the anterior vault of the vagina, one fourth to one half centimètre in front of the anterior lip of the os uteri, and still better when, beside this transverse incision, a second one was made at right angles directly toward the urethra, so that a T-shaped incision resulted. By the exercise of some traction in the direction of the vagina, with a fine double hook, inserted into the mucous lining of the bladder, and a simultaneous pressure above the pubic arch on the apex of the bladder, introversion was produced. The incisions are safest carried out by dilating the vagina with the dilatation instruments, or with Bozeman's speculum, the wall of the bladder being well extended; or after drawing down the womb and the upper part of the vesico-vaginal wall. The hæmorrhage which is to be expected in the living patient can be stopped by torsion or ligature.

After the purpose for which the incision through the wall of the bladder was made has been attained, the opening must be closed again. If the wound can be closed when fresh, it has only to be sewn up, but if the borders are cicatrized, the regular operation for vesico-vaginal fistula must be performed.

After the vesico-vaginal section, especially the T-shaped incision, the inside of the bladder is made so completely accessible by inversion, that the most complicated and most difficult operation can be performed with the same facility as on the surface of the body.

In regard to the dilatation of the urethra, the indications are given in a great many diseases as well for diagnostic as for therapeutical purposes, and they are as follows:

1. For the diagnosis of the diseases of the mucous membrane.
2. For the diagnosis of foreign bodies and stones, which can be found even when they are very small.
3. For the extraction of such bodies.
4. In cases of inveterate catarrh of the bladder, for the purpose of applying strong caustics.
5. For the cure of fissures of the urethra.
6. For diagnosis of defects in the vesico-vaginal septum when the vagina is closed up.
7. For the diagnosis of the seat and extent of growths of tumours in the vesico-vaginal septum.
8. For the extirpation of tumours, especially papilloma, starting from the mucous surface of the bladder.
9. For the discovery and subsequent extraction or excision of renal calculi from the vesical part of the ureter.
10. For the opening of hæmatometra when puncture is impossible, also dangerous, between the bladder and rectum.
11. For the cure of colo-vesical or entero-vesical fistula by cauterizing the ostium vesicale of the fistula.

The indications for the vesico-vaginal section are more limited, and are

1. In cases of very large stones, and great sensibility of the bladder.
2. To allow of the direct escape of urine in cases of inveterate catarrh of the bladder, with ulceration of the mucous lining.
3. Extirpation of tumours and excrescences which are situated so high in the lateral part of the bladder that they cannot be made large enough of access through the dilated urethra alone.
4. Operation for colo-vesical or entero-vesical fistulæ, which could not have been cured by cauterization after the urethra had been dilated.

Probing and catheterization of the ureter can be performed by the means described, thereby levelling the path for future diagnostic and therapeutic assistance in diseases of the ureter and kidneys.

After referring to Dr. Tuchmann's lately published article on the possibility of closing one of the ureters by means of an instrument similar to a lithotrite, Professor Simon describes his own method of introducing the probes or sounds into the ureters, and which seems to depend upon a nice touch and thorough knowledge of the anatomical characteristics, and normal measurements of the female bladder.

Professor Simon considers that catheterization of the ureter, as a method of diagnosis, or as a remedy, will be of the greatest advantage in diseases of the kidneys and the ureter.

The diagnosis of these diseases will especially be promoted, because the pelvis of the kidney can be entered with the catheter. Stones, for instance, will become palpable in the whole course of the ureter, and even in the pelvis of the kidney. The probing will most likely be very much easier in cases of nephrolithiasis, because the ureter will have been dilated by stones which have passed in former times. For the diagnosis of unilateral diseases of the kidney the catherization undoubtedly surpasses Tuchmann's experiment, on account of the ease with which it can be performed, and its certainty of success. The anatomical waymarks are more easily to be found by the end of the finger than by Tuchmann's instrument, and the control which we have over the instrument, as regards the correct application, can here be carried out with the greatest safety. A particular advantage may be seen in the fact that the urine comes directly out of the kidney, and does not necessarily pass the bladder, as happened in Tuchmann's experiment.

The probing promises to become of great use, not only for diagnosis, but also for therapeutics. Professor Simon presumes that stones which are felt in the bladder part of the ureter can be extracted or cut out. If the stones should lie in the neighbourhood of the pelvis of the kidney, they could perhaps be pushed back. Strictures could perhaps be dilated, and those hydronephroses, where the ureter is perfectly intact, and is only closed by a kind of valve at the upper end, could be emptied by the catheter.

3. *On Irreducible Dislocation of the Metacarpo-phalangeal Joint of the Thumb backwards.*—M. Marcano ('Bulletin Générale Thérapeutique,' September 13 and 30) has an able article on the anatomical characteristics of the several dislocations of the metacarpo-phalangeal articulation of the thumb backwards; and as the resources furnished by pathological anatomy are so few, authors have been obliged to resort to experiments, and as these present very great difficulty, it is easy to see how they have arrived at very opposite opinions. M. Marcano has arrived at his conclusions after inducing these lesions on the dead body, freezing the specimens, and subsequently dissecting them; experiments which were suggested and supervised by M. Dolbeau. After making a number of experiments, the results of which corresponded with those of other writers, it was found that there was no difficulty in reduction when the ligaments remained intact. On inducing the accident by the exertion of great violence, it was found that the anterior ligament, in the most constant and significant lesions and in the reducible form, was almost invariably torn in two directions, sometimes at the level of the sesamoid bone, and more often on the level of its phalangeal insertion; and that point at which this takes place plays an important rôle as regards the possibility of reduction. In the form of reducible luxation produced by violence, the anterior ligament was found torn on a level with its insertion,

lying in front of and separated from the dislocated phalanx by the antero-posterior diameter of the head of the metacarpal bone. When it took place at the level of the sesamoid bones, the superior fragment lay in a plane anterior to that of the inferior, but there was no interposition of the either laminæ; the sesamoid bone in the inferior lamina could be readily returned and was never interposed.

In cases in which the reduction is attended with difficulty, the superior attachment of the anterior ligament is torn, but the inferior portion is entire; or if not, it accompanies the phalanx, and lies between the articular surfaces, becoming horizontal, and the articular surfaces of the sesamoid bones look upwards and backwards. In one variety where the lateral ligaments are either partly or totally preserved, so as to "bridle" the articulation, the difficulty of reduction is still greater.

In the difficult reduction, the lateral ligaments are most frequently ruptured, although they may remain intact, and in cases where they are so, the luxation is most difficult to reduce.

The muscular "buttonhole" is, of all the cases cited by authors, the most difficult to understand. The forces which produce the luxation may rupture the insertions of the short flexor, and sometimes there are more irregular ruptures, which involve the skin, and do not confer any character to the dislocation. The "buttonhole" does not render reduction impossible. This would appear to be in direct opposition to the teaching of Demarquay, and M. Marcano has induced this state of things after luxation, with rupture of the inferior insertion of the anterior ligament, and the metacarpal bone has been readily made to pass through the muscular mass without any difficulty. The long flexor almost invariably occupied its normal position, and rarely lay external to the metacarpal bone.

As a result, then, of these experiments, the lesions present only one constant factor, the rupture of the anterior ligament; should this occur on a level with the phalanx, it gives rise to a reducible luxation; and if at the level of the metacarpal bone, to one presenting difficulties in proportion to the injury.

To summarise. The first variety does not present any peculiar anatomical characters; it may be complete without any ligamentous rupture or with the breaking up of all the ligaments, as also the anterior, but with the restriction above mentioned.

The second variety is that which presents difficulties in reduction. This form is more often due to the interposition of the anterior ligament between the articular surfaces, and separated from its metacarpal insertion, and must be reduced according to its anatomical peculiarities, whether by simple propulsion or bending backwards with propulsion. Supposing these methods to fail, fresh attempts must not be made at haphazard; but after due consideration of the kind of luxation, and as it is often noticeable that surgeons consider a dislocation irreducible, after having attempted one method of reduction solely.

Should the reduction be impossible, it then belongs to the third

variety. First and foremost are old-standing luxations when there are false joints. Published cases in which reduction has been attempted are numerous enough to show that they should not have been done, since the knife itself is incapable of removing the obstacle. Moreover, the movements reappear, and the thumb is rendered useful. Recent irreducible cases are rare, and must be considered as a separate type. This category would be confined to those in which the surgeon should use his knife; but what obstacle is to be attacked by it? In the worst and most stubborn cases "induced" by the author, the displacement was not very considerable. The anterior ligament presented the rupture of irreducible luxations; at the same time the lateral ligaments were intact, they were almost transverse, and the phalanx drew strongly towards the metacarpal bone.

This double arrangement, the interposition of the anterior ligament and the forced coaptation produced by the lateral ones, seems to be the key to the phenomenon, and it would appear that the lesson gained is to divide the lateral ligaments directly, to get rid of their traction, and to attempt reduction by the disengagement of the anterior ligament. The method to be adopted in cases of dislocation backwards of the metacarpo-phalangeal articulation of the thumb should in the first place consist in determining whether it be irreducible or not, and that the latter conclusion should not be arrived at unless all the methods of reduction have been attempted; and if shown to be irreducible and recent, the articulation should be opened by dividing the lateral ligaments, so as to facilitate the subsequent division of the anterior ligament.

4. M. Farabœuf ('Bull. Gén. Thérap.,' Sept. 15, 1875) has also published an important memoir on this subject, based on experiments and dissections made on upwards of one hundred bodies, and has arrived at the conclusion that the principal rôle is played by the sesamoid bones, and especially by the external. The dislocation presents three varieties: the first incomplete where the phalanx solely is dislocated, and there is no displacement of the sesamoid bones; the second is characterised by the displacement of the sesamoid bones, which are located on the back of the metacarpal bone; and the third variety, or complex, is a variation of the second, and may be the result of complicated injury, but is more frequently produced by the efforts at reduction, of the surgeon or patient, in pulling on the thumb, the sesamoid bones are not replaced, and it merely returns to its abnormal position.

REPORT ON TOXICOLOGY, FORENSIC MEDICINE, AND HYGIENE.

By Benjamin W. Richardson, M.D., F.R.S.

I. Toxicology.

An officinal multiple Antidote.—Dr. Du Vivier proposes an officinal multiple antidote for poisons. His proposition, which was read by Dr. Jeannel before the Medico-legal Society of France, on the 8th of February, 1875, supplies the following as the formula of his antidote:

Solution of sulphate of iron (sp. gr. 1·45)	100	parts.
Water	800	„
Calcined magnesia	88	„
Purified animal charcoal	40	„

The iron solution is to be kept separately; the magnesia and animal charcoal in a bottle with water. When required for use the iron solution should be poured into the bottle, and the whole shaken well together.

1. This mixture should be administered in consecutive doses of from 50 to 100 grammes.

2. The experience of the author has proved that this antidote renders preparations of arsenic, of zinc, and of digitalis, completely insoluble; that it does not render oxide of copper entirely insoluble; that it leaves in solution a notable quantity of oxide of mercury, and an appreciable quantity of morphine and of strychnine; that it does not decompose nor precipitate the cyanide of mercury; that it thoroughly saturates free iodine; that it acts partially only on solutions of alkaline hypochlorite.

3. The author's experiments upon living animals justify him in arriving at the following conclusions:—That the proposed antidote is perfectly efficacious in the case of arsenical preparations in the proportion of 120 grammes of antidote to five decigrammes of arseniate of soda. It retards the poisonous effects of the sulphate of strychnia, and would perhaps give time for the administration of salutary evacuants. It has been seen to act efficaciously against digitalis injected into the intestines in doses of one decigramme.

4. This formula is certainly preferable to the officinal hydrate peroxide of iron, as this undergoes, by the action of time and of a temperature higher than 15° C., a molecular modification, which renders it inefficacious against arsenical preparations. This formula, containing peroxide of iron, extemporarily prepared, of magnesia hydrate, and of animal carbon, is a satisfactory antidote in a great number of demonstrations. It is, however, not efficacious against alkaline minerals, phosphorus, the hypochlorites, the cyanides, and tartar emetic.

5. The protosulphate of iron, dissolved and mixed with magnesia and sulphate of soda, is of absolute chemical efficacy as regards the salts of copper. As regards bichloride and cyanide of mercury it seems preferable to the extemporised hydrated peroxide of iron; but it is inefficacious against arsenical preparations, the sulphate of strychnia, and possibly the other alkaloidal salts.—*Annales de la Société Méd.-Chir. de Liége*, May, 1875; and *Annali di Chimica*, N. 3, September, 1875.

Action of Cotarnine and Hydrocotarnine.—Dr. Pierce states, as the results of a few preliminary experiments on this subject, that a well-marked difference has been established in the action of these two bases on different animals. Cotarnine was given (by subcutaneous injection of known weights of a base dissolved in just sufficient hydrochloric acid) to several kittens and rabbits, without producing the slightest noticeable effect, even in doses up to 5 decigrams. On the other hand, equivalent quantities of hydrocotarnine given similarly to animals of the same sizes and ages produced marked results. Doses of $2\frac{1}{2}$ to 5 decigrams produced in kittens, rabbits, and guinea-pigs rapid and well-marked tremors, passing into severe epileptiform convulsions, accompanied apparently by more or less affection of the sensory organs. Great muscular prostration and salivation ensued. With kittens a dose of 0·5 gramme proved fatal in ten minutes; with a guinea-pig the same result ensued with half that dose. 0·5 gramme, however, did not kill a full-grown rabbit, the symptoms being somewhat less severe (though identical in kind) than those in the other cases (owing to the larger size of the animal?).

It hence results that the addition of hydrogen to cotarnine converts a base which is apparently inert into a very active substance, the change in physiological action being far more striking even than the alteration brought about in the physical and chemical properties. —*Journal of the Chemical Society*, July, 1875.

On Poisoning by Codeine.—C. Ambrosoli relates a case of a child, two years of age, which was poisoned by codeine. Ambrosoli was called in the evening, in November, 1874, to the child, when it was apparently in the act of dying. The face was pale, the extremities were cold, the pulse was small, the abdomen was distended with flatus, the eyes were glassy and immovable. In the course of some hours there had been administered to the patient for broncho-pneumonia a mixture containing 0,01 of codeine, with 150 grammes of julep and 30 grammes of syrup of ipecacuanha. This mixture had been ordered a second time. Ambrosoli immediately had the child wrapped in hot cloths, had bottles of heated water placed at the feet, and kept up this treatment until three in the morning, at which time the pulse became distinct. The pupils dilated and fixed and the fever remained, indicating a cerebral inflammation. The application of ice to the head, the local abstraction of blood, and the administration of calomel, were followed by complete recovery in twenty-five days. The accident was attributed to an error in the preparation of the medicine, by which the child got an over-dose of codeine at one time.—*Gazzetta Lombardo*, t. 8, h. 6, 1875.

Poisoning by Dhatura.—Surgeon R. C. Sanders, M.B., records two cases of poisoning by dhatura. They are interesting, from the *post-mortem* appearances being exactly similar, and from the peculiarity that the most marked venous congestion extended from the lips, along the larynx and trachea, to the bifurcations of the bronchial tubes. This congestion was uniform in its whole extent, but did not pass down the œsophagus. The history is as follows: A sweeper, and his two children, aged 19 and 9, were sitting near their house, when they were accosted by a traveller (a Mussulman), who, after talking to them for some time, gave them some coarse sugar to eat with their rice, and left. The father ate a small quantity, and gave the greater to his two children; he quickly became insensible, and remained so until evening, when consciousness returned, and he found the two children dead. The bodies were sent in for examination by the police.

I. "Mussamat Badamo," a young, fat, well-developed woman, 19 years of age, has a large number of zinc ornaments on the body; face swollen and congested; eyes intensely congested, chemosed, pupils widely dilated; bloody froth issuing from the nose; mouth and tongue dark blue from congestion; clothes covered with fine dust; hands clenched, nails blue; on the left buttock and right leg skin rubbed off in large patches; wounds discoloured by dirt. Rigor mortis persistent.

Head.—Scalp much congested and blood effused over the occipital region. Brain membranes gorged with blood; sinuses full of dark blood. Brain substance much congested. Each lateral ventricle contains one drachm of serum; choroid plexus turgid from congestion; medulla oblongata much congested.

Lungs.—Adherent on right side, much congested posteriorly; tongue, fauces, larynx, and trachea blue from a congestion which extended to the smaller bronchial tubes.

Heart.—Healthy, substance extremely flabby.

Stomach contained a little food, mixed with which were seeds of the *Dhatura alba.* All other organs healthy.

II. "Buju," a young, healthy, well-developed boy, about 9 years old; bloody froth issuing from nostrils; face congested; eyes chemosed, pupils widely dilated; lips vivid; body covered with fine dirt; on the left buttock and calf of left leg, as also on right leg, skin rubbed off in large patches; wounds dirty; hands clenched; nails blue. Rigor mortis persistent.

Head.—Scalp much congested, blood effused posteriorly, membranes of the brain much congested, brain congested. Each lateral ventricle contains serum; choroid plexus intensely congested, erect, and turgid.

Lungs.—Congested posteriorly; mouth, tongue, fauces, larynx, and trachea much congested, blue in colour. The congestion extends, without intermission, from the lips to the smaller bronchial tubes.

Heart.—Empty, flabby.

Stomach contains semi-digested food, and dhatura seeds mixed with it; organ healthy. All other organs healthy.

These two bodies were examined within twenty-four hours of death; in each large swollen seeds were found in the stomach, and it was not until the seeds had dried that they were recognised as dhatura, when they exactly resembled the figures, in Taylor's work, of *Dhatura alba*. The girl's stomach contained seventeen, and the boy's eleven seeds.

Urgent thirst is mentioned as one of the symptoms of dhatura poisoning. This might be accounted for by the extraordinary congestion of the mouth, throat, and respiratory tract shown in the above cases, but the author has not in any work seen an account of this symptom; he would be glad, therefore, if in future cases of dhatura poisoning it were specially looked for; so that, if constant, it might be placed on record.

The seeds were verified by the chemical examiner.

Dr. Sanders accounts for the large superficial wounds on the buttock and legs, in each case, and for the blood effused beneath the scalp posteriorly by supposing that the bodies had been violently convulsed. The clothes of the woman, as also the body of the boy, were covered with fine dust, a fact which would also point to the conclusion that the bodies had rolled over and over upon the ground.—*Indian Medical Gazette*, June 1, 1875.

II. Forensic Medicine.

The Microscopic Test for Blood Stains.—Dr. Joseph G. Richardson, of Philadelphia, communicates an improved method of applying the micro-spectroscopic test for blood stains. He also forwards us an interesting and instructive photograph, showing the difference of human blood-corpuscles from those of the pig. The corpuscles of each, magnified 650 diameters, are shown on the same plate, and the distinction between the two is most definite. On the subject of the micro-spectroscopic test for blood stains Dr. Richardson remarks, that the value to medical jurisprudence of spectrum analysis as employed for the detection of dried blood is so fully established by the researches of H. G. Sorby, Dr. W. B. Herepath, Prof. A. S. Taylor, W. Preyer, and others, that it seems unnecessary to do more than state that the demonstration of the two dark bands in the green caused by scarlet cruorine (hæmoglobin), such as that contained in a recent blood stain, enables experts to discriminate positively blood from other red colouring-matters soluble in water, whether mineral, vegetable, or animal, except an extract of the red feathers from the *Turacus albocristatus*, a bird found in the East Indies, and quite unknown on the continent of America.

Valuable as this test is thus seen to be, there are, unfortunately, several circumstances which limit its general application, as, for example, the changes in the constitution of hæmoglobin which occur from prolonged and frequently from comparatively brief exposure to the air, the modification of the absorption-bands, caused by the presence of other substances, and last, but not least in many in-

stances, the difficulty of procuring sufficient material for experiment. The insuperable nature of this latter obstacle will be at once appreciated when it is mentioned, that whilst the smallest amount which Sorby, Herepath, and Taylor furnish directions for testing is a spot "one-tenth of an inch in diameter, or a quantity of the red colouring-matter amounting to no more than one thousandth part of a grain." The important stain upon an axe-handle supposed to have been used in a murder which he, the author, is now investigating, probably weighed less than one three-thousandth of a grain when entire and uninjured.

The exigencies of this case have led Dr. Richardson to seek out some other method than that of Mr. Sorby, who recommends that a solution of the suspected colouring-matter should be made in a few drops of water contained in a cell composed of a piece of barometer-tube one half an inch long and one seventh of an inch in diameter. After numerous experiments the author contrived the following plan, which, on trial, proved satisfactory beyond his most sanguine expectations, enabling him to reveal the presence of blood in a quantity of matter only one one-hundredth the amount directed by Mr. Sorby:

Procure a glass slide, with a circular excavation in the middle, called by dealers a "concave centre," and moisten it around the edges of the cavity with a small drop of diluted glycerine. Thoroughly clean a thin glass cover about one eighth of an inch larger than the excavation, lay it on white paper, and upon it place the tiniest visible fragment of a freshly dried blood-clot (this fragment will weigh from one twenty-five thousandth to one fifty thousandth of a grain). Then with a cataract-needle deposit on the centre of the cover, near the blood-spot, a drop of glycerine about the size of this period (.), and with a dry needle gently push the blood to the brink of the microscopic pond, so that it may be just moistened by the fluid. Finally, invert the slide upon the thin glass cover in such a manner that the glycerined edges of the cavity in the former may adhere to the margins of the latter, and turning the slide face upwards, transfer it to the stage of the microscope.

By this method it is obvious we obtain an extremely minute quantity of strong solution of hæmoglobin, whose point of greatest density (generally in the centre of the clot) is readily found under a one-fourth-inch objective, and tested by the adjustment of the spectroscopic eye-piece. After a little practice it will be found quite possible to modify the bands by the addition of sulphuret of sodium solotion, as advised by Preyer.

In order to compare the delicacy of the author's plan with that of Mr. Sorby, a spot of blood one tenth of an inch square may be made on a piece of white muslin, the threads of which average one hundred to the inch. When the stain is dry, ravel out one of the coloured threads and cut off and test a fragment as long as the diameter of the filament, which will of course be a particle of stained fabric measuring one one-hundredth of the minimum-sized piece directed by Mr. Sorby. When the drop of blood is old, a larger amount of

material becomes requisite, and it may be necessary to moisten it with aqua ammoniæ, or with solution of tartrate of ammonium and protosulphate of iron; but in the criminal case referred to, *five months* after the murder, the author was able from a scrap of stained muslin one fiftieth of an inch square to obtain well-marked absorption-bands, easily discriminated from those produced by a solution of alkanet-root with alum, and those caused by infusion of cochineal with the same salt.

In cases of this kind, where the greatest possible economy or even parsimony of material is needful, the following is the mode of procedure for proving and corroborating the proof of the existence of blood, so that its presence in a stain may be affirmed with *absolute certainty*.

From a suspected blood spot upon metal, wood, leather, paper, muslin, or cloth, scrape with a fine sharp knife two or three or more minute particles of the reddish substance, causing them to fall near the middle of a large thin glass cover. Apply in close proximity to them a very small drop of three-fourths per cent. salt solution, bring the particles of supposed blood clot to its edge, and proceed as already directed.

After thus examining the spectrum of the substance, the observer may generally, by rotating the stage, cause the coloured fluid to partly drain away from the solid portion, wherein, under favourable circumstances, should the specimen be blood, the granular white blood-globules become plainly visible, as do also cell-walls of the red discs. Among the latter, if the mental and physical vision is keen enough, he can by the aid of a one twenty-fifth immersion lens and an eye-piece micrometer measure a series of corpuscles accurately enough to discriminate human blood from that of an ox, pig, horse, or sheep.

Lastly, to make assurance triply sure, lift up the thin glass cover, wipe off the tiny drop of blood solution and clot under examination on the folded edge of a thin piece of moistened blotting paper, let fall upon it a little fresh tincture of guaiacum, and then a drop of ozonized ether, which will at once strike the deep-blue colour of the guaiacum-test for blood.

In this way Dr. Richardson has actually obtained these three kinds of evidence, to wit, that of spectrum analysis; that of the microscope; and that of chemical reaction, from one single particle of blood, which, judged by a definite standard, certainly weighed less than one fifteen-thousandth, and probably less than one twenty-five-thousandth, of a grain.

Although Mr. Sorby claims to be able to demonstrate the absorption-bands from a single red blood-corpuscle, yet, as his instructions for detecting *blood stains*, quoted above from the 'Quarterly Journal of Science,' vol. ii, p. 198, are reiterated in his paper for the 'Monthly Microscopical Journal' of July, 1871, p. 9, and seem to be those solely relied upon by Dr. Herepath, in the 'Chemical News,' 1868, vol. i, p. 124; by Prof. L. S. Beale, in his 'How to Work with the Microscope,' London, 1868, p. 222; by Dr. W. B. Carpenter, in

The Microscope and its Revelations,' 5th ed., London, 1875, p. 121; and by Prof. A. S. Taylor, in 'Guy's Hospital Reports,' 1869, p. 274, and in his 'Principles and Practice of Medical Jurisprudence,' 1873, vol. i, p. 542; and since W. Preyer ('Die Blutkrystalle,' Jena, 1871, S. 114) advises no more delicate mode than making and examining a solution in a watch-glass, the author feels justified in offering his method to microscopists and medical jurists, as an improvement in the ordinary and facile application of spectrum analysis to blood stains, by which this important test is rendered at least one hundred times as delicate as it has hitherto been when employed according to the directions of the highest British or Continental authorities. Thus a recent blood spot on white muslin covering one ten-thousandth of a square inch, and forming a speck scarcely visible to the unassisted eye, may be detected.—*Paper read before the Biological and Microscopical Section of the Academy of Natural Sciences, Philadelphia*, 1875.

Medico-legal Returns from Civil Surgeons in the Bengal Presidency.—Dr. Harvey continues his valuable, and they are indeed valuable, reports on the above-named returns. We embody a series of facts, in what follows, on cases which are of the most novel and curious character, and which, certainly, have never before been collected with so much care by any medico-legal observer.

Death after the Bite of a Rat.—An extraordinary case of death following the bite of a common musk rat is thus circumstantially detailed by Surgeon T. Matthew in the Monghir return for May, 1871: "A Dhanúk boy, aged 10. Case sent in by police as death from the bite of a rat. The rat (a common musk) was sent in. Father's story is that the boy was bitten during the night, and sprang up, shouting 'I have been bitten!' The rat was seen to run into a hole, was got out with a stick, and killed by the boy himself with one blow. Immediately after he dropped dead. Body well nourished, rigor mortis set in, no marks except a very small punctured wound beneath the skin over right patella. Bloody fluid exuding from mouth and nose. Tongue caught between teeth. *Chest.*—Right lung normal, left slightly congested, and bound down by recent adhesions of pleura. An ounce of serum in pericardium. Right heart full of fluid blood, left empty. *Abdomen.*—Stomach full of recent food. All viscera, solid and hollow, healthy. *Cranium.*—Brain surface and membranes found congested. *Opinion.*—Death could not have been caused by the bite of a rat or anything else in so short a time. I conclude the boy died of fright."

The case was filed as one of death from fright, no doubt after inquiry into the truth of the father's statement.

Deaths from Bites of the Crocodile.—Seven cases, all fatal, are ascribed to these reptiles. Five are from the Dacca; two from the Dinapur Circle. Barrisal furnished three of the cases, and Tezpur, Gowhati, Monghir, and Rajmahal one each. The wounds are always terribly severe, great masses of flesh being frequently torn away.

The finding of a dead body thus wounded is, however, no proof that death was caused by the bites, as crocodiles, though preferring

fresh meat, are believed to feed on carrion, and the injuries may have been inflicted after death from other causes; while by the maceration of the body in a running stream all evidence that they were inflicted during life may have been washed away in cases where death was actually due to *maggar* bites. If nothing is known of the facts of a particular case a guarded opinion should be given. The Civil Surgeon of Monghir refused to certify the cause of the death of a woman, whose decomposed body was found in a river with the "front of chest and abdomen, and contents of both completely torn out. Right arm, flesh of nates and thighs apparently gnawed." The case seems to have been accepted as one of crocodile bite. A man at Gauhati lived 24 hours after having his lower extremities "dreadfully mangled" and part of his scrotum torn away. The marks of the teeth were quite distinct in places.

Deaths from Stinging Insects.—A case of death from the irritation caused by wasp stings is given by Surgeon-Major Loch, M.D., in the Bareli return for July, 1871. The body, which was that of a boy four years of age, examined 34 hours after death, was somewhat decomposed:

"Face appears swollen and blue; scalp œdematous; brain perhaps slightly congested; right lung somewhat inflamed; right side of heart contained a white clot, left empty; liver rather pale, spleen normal; stomach contained a brown fluid; intestines normal; kidneys very pale; bladder empty. Cause of death, cannot say."

Deaths from being "Bitten by Wasps" is the endorsement of the police, founded no doubt on sufficient evidence; and the swollen face and œdematous scalp are just what would be found after numerous stings. Another child, two years old, was stung to death at Gaiah, in October, 1872. Black spots from some of which wasp stings were extracted were found on the head and upper part of the body. The brain and its membranes, with the lungs, liver, and kidneys, were congested, and coagulated blood was found in the right heart.

Deaths from the Sting of the Scorpion.—Six fatal cases of scorpion sting are recorded, and as little is known from the appearances to be looked for after death from this cause, we give one at length:

"*No.* 3.—*Partabghar, May*, 1870.—A Hindu girl, aged $9\frac{1}{2}$, said to have died from the effects of a scorpion sting in three hours. The scorpion, which was also brought by the police, measured $2\frac{1}{2}$ inches from head to tail. Sting broken, but hanging. The body of the child presented a curious appearance. All the superficial veins of the head, neck, and upper extremities were distended with blood, so much so that an anatomist would have been spared a dissection; they contrasted strongly with the olive hue of the body; while the tips of the fingers and palms of the hands were quite black.

"On the ball of the left great toe there was a small puncture, and on dissecting it up nothing in the shape of ecchymosis or effusion could be found in the areolar tissue. The lower extremities presented nothing abnormal. The face was greatly swollen, and blood was oozing from the mouth and nostrils. The eyeballs were prominent, and there was a look of great anxiety on the features. On

cutting into the scalp blood flowed freely, appeared black, and had no tendency to coagulate. The sinuses of the dura mater contained fluid blood. On removing the meninges the surface of the brain was found to have its veins full. Brain surface appeared softer than normal, and on section many bleeding points presented themselves. Venticles contained about a drachm of serum. Every part seemed to have become highly congested. Cerebellum and medulla oblongata also highly congested. Spinal cord also highly congested; there was no effusion at the base of the brain. *Thorax.*—Lungs pale and ex-sanguine, nothing abnormal in them; pericardium contained half an ounce of serum. Both sides of the heart contained a small quantity of fluid blood, the right a little more than the left. *Abdomen.*—Stomach contained a small quantity of half-digested food, but was apparently healthy. Intestines distended with *post-mortem* gases; liver healthy; spleen very large, ½ oz. (pound ?) in weight, very soft and friable. Kidneys very much congested. Bladder was distended with healthy urine." Reported by Mr. J. Hart. Case filed.

The cases are too few, Dr. Harvey says, to draw conclusions from, but will serve for future comparison. The absence of infiltration at the part stung, in all those where the fact is noticed, is in accordance with the writer's experience of numerous non-fatal cases. It is worthy of note that all the subjects whose age is mentioned were children, yet cases of adult deaths are recorded by Chevers, and the sting of a black scorpion, six or seven inches long as some are, would probably be dangerous to any one. The appearances in Mr. Hart's admirably reported case, the swelling of the face, turgidity of the veins, blackness of the fingers, blood oozing from mouth and nose, prominent eyes, and anxious face, are commended to the notice of those who consider these signs strong evidence of strangulation. They were probably due, in part at least, to commencing putrefaction, evidence of which was obtained from the state of the bowels. —*Indian Medical Gazette, Oct.* 1, 1875.

III. Hygiene.

Report on Trichinosis.—Dr. George Sutton, of Aurora, Indiana, reports on the disease Trichinosis as it was observed in Dearborn County, Indiana, in 1874. The animal food that was the cause of the outbreak was uncooked pork, eaten in the form of German sausage. The symptoms were not at first recognised, but after a time it was discovered by Mr. Adam Whehe that the sickness occurred amongst those only who were having their meals at the house of a Mrs. Trenant. Knowing that Mrs. Trenant had been using the fresh meat of two hogs which she had butchered a few days before the commencement of the illness, Whehe suggested that the pork was the cause. Dr. Lamb, who was in attendance, thereupon invited Dr. Sutton to examine the pork with him, and the result was the discovery that the flesh was charged with trichinæ. The mode in which these infested animals had been prepared for food is worthy of notice, and is thus described:—The hogs were killed in the

morning of 5th of January. The meat was cut up and salted, and sausage made the same day. The pork, which had all the appearance of being perfectly healthy, having a thick layer of fat over the hams, was cut up, salted, and placed in a barrel in the usual manner. None of the salted meat had been used. The sparerib, backbones, trimmings, and sausage were the only meat used up to the time of the sickness. Two kinds of sausage were made: one the common meat sausage, the other what the Germans call liverwurst. The meat sausage was made in the usual way: lean portions of the back tender loin, trimmings from the hams, &c., were chopped up fine, highly seasoned with salt, pepper, sage, and other herbs, and forced, uncooked, into the prepared intestines of the hog. The liverwurst was made from the liver, lungs, and fragments of meat: this was boiled or cooked, then chopped up fine and highly seasoned with onions, sage, pepper, salt, &c., and also enclosed in skins. These sausages were immediately hung up in the smoke-house, and kept over a dense smoke for several days. From the day the hogs were killed the meat had been used, fried and cooked in the usual manner. On the evening of the 9th, four days after the hogs were butchered, portions of this smoked meat sausage, which by this time was well smoked, was placed upon the table uncooked, and eaten—a mode of using this form of sausage common to the Germans. Whether it was eaten by all the children is not now known, as no attention at that time was directed to the subject. That night, about six or eight hours after the sausage had been eaten, Mrs. Trenant and her son Henry were taken unwell; other cases of sickness followed, all traced afterwards to eating this meat.

The symptoms of the disease induced by the trichinæ are well displayed in the case of Mrs. Trenant, age forty-one, who had been daily eating the cooked pork from the time the hogs were killed, January 5 up to the 9th. That evening, January 9, the two kinds of sausage uncooked were placed upon the supper table; Mrs. Trenant ate a portion of the uncooked meat sausage to try its flavour. About five or six hours afterwards she was attacked with vomiting and purging. The next morning, January 10, Dr. Lamb was called in. The doctor informs us that she then had diarrhœa, discharges thin and yellow, occasional vomiting, low fever, coated tongue; these symptoms continued up to the 24th, and were treated upon general principles with tonics, alteratives, diuretics, and sedatives. At this time Dr. Sutton saw the patient. As he had detected trichinæ in the meat that had been eaten, and regarded this sickness as arising from trichinæ, the symptoms were now more closely observed. There was diarrhœa alternating with dysenteric symptoms, discharges mostly thin and yellow; occasional vomiting, coated tongue, red around the edges; temperature 98½°, varying to 100°; pulse small, ranging from 85 to 95; thirst; no appetite; tenderness over the bowels; but little if any pain, except slight soreness of the muscles of the extremities; urine healthy although rather high coloured; skin occasionally moist, at other times dry with increased temperature; mind clear,

but with much anxiety which partly arose from the illness of the family. She complained of great weakness. As he could find no satisfactory treatment recommended in our medical works for trichinosis, Dr. S. concluded to try different remedies upon the different patients. In this case he continued the quinine, and gave anodynes as the diarrhœal symptoms required. He also gave a solution containing the sixteenth of a grain of corrosive sublimate every four hours, thinking it might possibly destroy the parasite. This course of treatment he continued until the 1st of February, six days, when he discontinued the corrosive sublimate, as there was no marked improvement in the case, and continued the quinine or stimulants: he also gave anodynes as the diarrhœal symptoms required.

The appetite slowly returned, the diarrhœa and dysentery gradually subsided, and seven weeks from the time she was taken ill this patient was pronounced convalescent, and has since wholly recovered. There was in this case but little œdema, and only slight soreness of the muscles. The prominent symptoms were those of gastro-enteritis.

Eight other persons partook of the sausage and three died. The symptoms in one of the fatal cases were as follow. Mrs. Beuter, thirty-four years of age, wife of the minister of the German Methodist Church of Aurora. This lady went to the assistance of Mrs. Trenant and her family, and while attending on the sick she ate heartily of both the meat and uncooked meat sausage. On the 15th, the next day after being at Mrs. Trenant's house, she was attacked with vomiting and diarrhœa. A German physician was first called to attend this case, but after a few days' attendance he was dismissed and Dr. Lamb was called in. On the 28th, Dr. Sutton also saw her with Dr. Lamb; she then had diarrhœa, also dysenteric symptoms; nausea and occasional vomiting; the discharges were thin and watery, changing to mucus when there were dysenteric symptoms; the tongue was coated, and red around the edges; thirst; skin hot; she was occasionally bathed in profuse prespiration; temperature varying from 98° to 104° F.; pulse ranging from 95° to 116°, generally full; mind clear. It was now ten days from the time she was taken unwell, and her face, limbs, and whole body were œdematous; the skin pitted on pressure; there was an intolerable itching over the face, hands and arms, with slight eruption; there were inflamed patches from an inch to two inches in diameter on the chin, side of the face and breast, resembling erysipelas; the conjunctiva was injected; there was hyperæsthesia of the surface of the body, and soreness of the muscles of the extremities—this painful sensation of the muscles increased to such an extent that she could scarcely be moved.

The doctor, when first called, gave this patient small doses of calomel and Dover's powder, three grains each; after this he administered a solution of carbolic acid, five drops every three hours; also small doses of quinine and opium, as symptoms required, to allay the tenesmus. This treatment was continued four days. About this time, her husband, hearing that sulphur was a remedy

for this disease, the carbolic treatment was discontinued, and the system saturated with sulphur. This treatment was continued three days, when, as it was evident the patient was growing worse, this treatment was discontinued, and the patient was put upon a solution of corrosive sublimate, one sixteenth of a grain every three hours, with opium and quinine. This treatment was continued until February 6th, when the mercury was omitted, as no good effects were observed from it. The quinine and opium were continued. She continued to sink. On the morning of the 8th the pulse became almost imperceptible, and she died about 1 p.m., the mind remaining clear to the last.

The post-mortem appearances in two of the fatal cases were those of intense inflammation of the mucous lining of the alimentary canal, with enlargement of the mesenteric glands. Trichinæ were not found in the bowels, but some were detected in the muscular system. In the case of Mrs. Beuter large numbers were detected in the gastrocnemius muscle; few were seen to be encysted.

In some experiments made on dogs by feeding them with the infested pork, it was found that the animals were made ill with signs of irritation of the bowels; but they recovered, and when they were killed no signs of trichinæ could be found in their muscular organs. Dr. Sutton remarks that he also made examinations of the flesh of other animals. He examined the flesh of four cats, all of them presenting the appearance of being diseased before they were killed. Three out of four were found to be swarming with trichinæ, and the trichinæ from one of them were seen to be in motion. The flesh of forty-two rats was also examined, all apparently healthy when caught —no trichinæ were found. Also the flesh of eight moles, they appeared healthy when killed—no trichinæ were found. He merely presents the facts as they occurred in the investigations, but not as evidence that trichinæ are never found in flesh of these animals.

From the facts which have been presented Dr. Sutton draws the following, amongst other, conclusions:

That it is only by thoroughly cooking the meat that the vitality of trichinæ can be destroyed, and that eating smoked or dried pork uncooked in any form, or the partially cooked ham used in the form of sandwiches, common in eating houses, is attended with danger.

That in south-eastern Indiana from three to sixteen per cent. of the hogs are affected with trichinæ.

That in the western states there is put annually upon the market forty-four millions two hundred and ninety-six thousand eight hundred pounds of diseased meat, every ounce of which, under favourable circumstances, is capable of producing disease.

That ninety per cent. of disease produced from eating trichinous pork appears either as gastro-enteritis, or as diarrhœa or dysentery, and not more than ten per cent. as the fully developed form of trichinosis in which the muscular system becomes affected.

That as diarrhœa, dysentery, and enteritis rank high as causes of mortality in the United States, these diseases causing thirty-one thousand one hundred and fifty-three deaths in 1870, it is more than

probable that trichinæ have a much greater influence in the etiology of this class of diseases than has been recognised by the profession.

That "hog cholera" and trichinosis, supposed to be the same diseases, are, beyond all doubt, entirely distinct.— *Report on Trichinosis, reprinted from the Transactions of the Indiana State Medical Society*, 1875.

IV.—Summary.

De La Genèse du Cholera dans l'Inde et de son mode d'Origine. Par J. D. Tholozan. Lecture to the Academy of Medicine at Paris. June, 1875.—Dr. Tholozan in this lecture passes beyond the history of the genesis of cholera and includes the whole question of the origin of the communicable diseases. He considers that the theory of specific germs as causes of these maladies is not sufficient to account for the phenomena, because in order to explain the diversity of the phenomena it is necessary to refer to the epidemic influence. This is a second hypothesis superposed upon the first, and it is therefore requisite in order to explain the whole constitution of epidemic disease to admit "epidemicity" as well as contagion. These two words probably only correspond to one and the same fact derived from the same source.

Prevention and Treatment of Scarlatina. By J. Brakenridge, M.D. Edinburgh, Machlachlan and Stewart, 1875.—The author of this essay proposes to treat scarlet fever by what he designates the internal administration of disinfectants. The disinfectant he employs is the sulpho-carbolate of sodium in doses varying from five to thirty grains a day, according to age. He administers the same to those who are exposed to the poisons of scarlet fever and diphtheria, though they may not be suffering, and in this manner he thinks he prevents the infection of the disease. Dr. Brakenridge is unfortunate in the use of the term disinfectant in connection with this treatment. It has long been known that ammonia, which is the most active of all antiseptics, is the nearest approach to a specific for scarlet fever. But the connection of the antiseptic action with the curative action has not as yet been demonstrated.

Presence of Copper in the Animal Organism. MM. Bergeron and L. L. Hôte. *Comptes Rendus*, lxxx, 268.—These authors have revived the experiments of Orfila on the question of the determination of copper in the animal organism. They examined the kidneys and livers of fourteen human bodies, and discovered that in eleven of these sufficient of the metal could be obtained to be weighed. The amount ranged from 0·7 to 1 milligramme, and in one subject who had attained the age of 78 as much as 1·5 milligramme was obtained. In two instances out of the fourteen copper was detected, but not in sufficient quantity to admit of its being weighed. The experimenters are of opinion that copper continually finds entrance into the human body, being derived from the copper vessels and utensils that are in daily use and also from copper coins. The metal is ordinarily thrown off from the system by the excretory channels, but some little, under all circumstances of age, sex, and condition of life, is retained. To

prevent errors in their research, the experimentalists employed iron vessels and apparatus, excluding every copper vessel, &c., from the laboratory.

How We Die in Large Towns. By Balthazar Foster, M.D. Reprinted from 'Public Health,' 1875.—Dr. Foster republishes in this pamphlet a lecture he delivered at the Temperance Hall, Birmingham, in February last. It places in simple language the lessons taught by the statistics of the comparative mortality of Birmingham with other large towns of the kingdom, which statistics he prepared for the sanitary conference held in Birmingham earlier in the year. The working men of this country are, the author says, the great sufferers by the past neglect of the rules of sanitary science. "The time for all this to end has now come if the people of this country who have created so much of its wealth will only insist on such legislation as will give to themselves and their children healthier, purer, and happier lives."

On Lung Disease from the Inhalation of Dust. By John T. Arlidge, M.D., F.R.C.P. Pamphlet, 1875.—This admirable essay from one of our ablest observers and authorities on the subject upon which it treats describes, as briefly as clearly, many practical points of great interest and importance. Dr. Arlidge sets aside conclusively the physiological doctrine of the impeding action of the cilia to the entrance of foreign particles into the lungs, and in a few pages places the different opinions that have been offered on this matter in a clear light. He directs attention to the effects of different kinds of dust upon the lungs, and to the variations of diseased conditions which are thereupon excited. He closes with a notice of the diseases of the chest peculiar to potters.

The Geographical Distribution of Heart Disease and Dropsy, Cancer in Females, and Phthisis in Females. By Alfred Haviland, M.R.C.S. London: Smith, Elder, & Co., 1875.—We cannot continue our summary without drawing attention to this remarkable essay, remarkable equally for its originality of character and for the industry with which it is composed. The work is illustrated by six small and three large coloured maps. The object of the author is to put forward, first and foremost, the facts of the geographical distribution of disease; to show in a glance at one of the maps where a malady gives the highest and where the lowest mortality. Then the author endeavours "to sift" the facts so as to be led to correct conclusions as to particular causes for particular series of phenomena. The method is crucial, and must elicit truth.

Milk in Health and Disease. By A. Hutchinson Smee. Separate volume, 1875.—In this little volume, which is ably constructed, the author gives a number of analyses of cows' milk. He comes to the following conclusions:

1st. That milk from individual cows is liable to considerable variation.

2nd. That it is possible for good average milk to be watered to a limited extent without detection.

3rd. That the casein, under certain circumstances, undergoes modification both physical and physiological.

4th. That the food of milch cows affects the quality of the milk.

5th. That milk can be the vehicle of contagion—

By direct communication of the contagion, either by the water used for purposes of adulteration, or by the vessels in which it is stored being cleansed with impure water.

By the absorption of the contagion by the exposure of milk to deleterious gases.

That in extreme instances power to communicate disease is produced in the milk itself, probably from an altered secretion of diseased animals.

6th. That the methods now employed by public analysts are not sufficiently delicate to detect the minute physiological changes which may at times take place in so complex a fluid as milk.

Analyses of Milk. By N. Gerber. 'Bulletin Soc. Chim.' and 'Journal of the Chemical Society,' December, 1875.

Estimation of water.—15 to 20 c.c. of milk are mixed with 1 to 3 grammes of calcium sulphate, evaporated carefully to dryness, and the residue dried at 105°—110° and weighed. The author prefers to mix the above quantity of milk with 10—20 grammes of washed sand instead of calcium sulphate, with which substance he has obtained very satisfactory results.

Estimation of the butter.—Having coagulated the milk in the usual manner, filter, and wash the coagulum first with water, then with strong alcohol, and lastly with ether, until it is exhausted. The casein must not be allowed to become too compact by standing, before treatment, with alcohol and ether.

Estimation of albumin.—It is necessary not merely to treat the coagulated and filtered milk, but to evaporate it to one half of its bulk, in order to insure the precipitation of all the albuminates. The pellicle formed on the surface of boiling milk contains, besides casein and fat, coagulated albumin.

The author adopts the following arrangement for the estimation of the butter in milk :—The funnel and filter containing the coagulum of casein, &c., is fixed vapour-tight into a small flask which is half filled with ether, and the upper portion of the funnel is connected with an upright condensing arrangement. The heat of a water bath volatilises the ether, and rapidly and effectively exhausts the casein.

Acute Tetanus successfully antagonized by Inhalation of Nitrite of Amyl. By William S. Forbes, M.D. Essay read before the College of Physicians at Philadelphia, April 7th, 1875.—In the case of tetanus described, Dr. Forbes followed out the method we originated of administering the nitrite of amyl, by inhalation, until the spasmodic attacks were subdued.

The cause of the tetanus in Dr. Forbes' patient was a burn, and the symptoms commenced on the fourth day, advancing with great rapidity. In forty hours the temperature rose to 102° F., the pulse to 133, and the respirations to 32 per minute. There was trismus

and tetanus with opisthotonos and repeated painful spasms. The patient was kept under the influence of the amyl nitrite at intervals for forty-six days and made a perfect recovery. Two facts of great practical interest attach to this case and to its recovery. The one fact is that during the whole course of the treatment no other remedy was used except the nitrite of amyl, so that the remedy stands alone as the means of cure. The other fact is that on the morning of the eighth day of the attack the nitrite of amyl in the hospital ran out, and for sixty hours the administration was suspended. During this time the symptoms which had already been subdued by the remedy commenced to reappear, the temperature rose and the spasmodic seizures returned, but on again obtaining and administering the nitrite the symptoms were overcome, and forty-six days after the first dose was inhaled the patient was completely restored to health. He inhaled altogether one ounce of the fluid.

The Chlorine Test for Morphia. By H. S. Wellcome. 'American Pharmaceutical Journal,' vol. ii, p. 305.—Mr. Wellcome gives the following reagent for detection of morphia. Two ounces of fresh chlorinated lime are to be added to a pint of water, and after being left to stand for a few hours the clear solution is to be decanted.

Morphia in powder gives a deep red colour on the addition of a drop of this solution.

With a solution of one grain of morphia in one thousand grains of water the test gives a bright red colour; and a drop of the mixture evaporated on a porcelain plate leaves a deep red ring; on the addition of ammonia or any other strong alkali the solution becomes dark brown.

The test gives a distinct orange colour to a solution of one grain of morphia in five thousand of water, and with care may be made to show plainly in a solution of one grain in ten thousand. Excess of the chlorine decolorises these solutions, and the orange colour cannot be restored. Excess of an acid decolorises them, but the colour reappears on adding excess of an alkali. These reactions are the same in the presence of all other alkaloids with which the author has experimented.

If a few drops of chlorine water be added to morphia in powder, and, after solution, a drop or two of ammonia, beautifully red star-like spangles will form. This test is best performed on a porcelain plate or crucible cover.

In testing for morphia the solution must be neutral or alkaline, excess of chlorine must be avoided, and no substance should be present which will give a red colour with alkalies.

Detection of Ammonia in the Air. By T. Schloesing. 'Comptes Rendus,' lxxx, pp. 265-8.—Schloesing, in order to discover the amount of ammonia in the atmosphere, employs a large bell jar, closed at its bottom by a disc of platinum, pierced with 300 holes of half a millimètre in diameter. By means of a caoutchouc tube filled with water, this jar is fitted air-tight into a large dish containing 300 centimètres of acidulated water, which, when a strong current of air is forced below the disc, is driven through the holes and con-

verted into a mass of bubbles; contact of air and liquid being thus ensured along an enormous surface, the air is deprived of its ammonia in less than two seconds.

The current of air is obtained by the injection of a jet of steam into a tube, and can be controlled by measuring the water condensed and the pressure of the steam.

A Powder for the Artificial Production of Ozone. By LENDER. 'Annali di Chemica,' No. 5, November, 1875.—In order to produce artificial ozone Lender makes use of equal parts of peroxide of manganese, permanganate of potash, and oxalic acid. When this mixture is placed in contact with water ozone is quickly generated. For a room of medium size two spoonfuls of this powder, placed on a dish and occasionally diluted with water, would be sufficient. The ozone developes itself; it disinfects the surrounding air without producing cough.

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APRIL, 1876.

Analytical and Critical Reviews.

I.—Jaccoud's Clinique Medicale.[1]

Those of our readers who during the last ten years have studied in the French School of Medicine, or have taken interest in French medical literature, will require no introduction to the distinguished physician and teacher whose latest contributions to clinical medicine form the subject of this review. M. Jaccoud is not only a most zealous worker in the field of practical and scientific medicine, but also well acquainted with the writings of English and German physicians, references to which are of constant occurrence in his pages.

The book before us contains thirty-one lectures, in which some of the most important pathological questions of the day are discussed at considerable length. It has not only much theoretical but also practical value, and both as to the diagnosis and treatment of disease contains many fresh observations and useful suggestions.

With these prefatory remarks, we proceed at once to a general examination of its contents.

After a short exposition of his method of instruction—that of a careful physiological and pathological analysis of the phenomena presented—and after dwelling on the necessity of gathering light from every source and information from every country, he proceeds to the study of that obscure malady characterised by general hypertrophy of the lymphatic glands.

M. Jaccoud points out that spontaneously occurring disease of this kind coincides with a twofold condition of the blood. In one group of cases the number of red globules is reduced in amount, and that of white globules greatly increased. To this form, recog-

[1] *Leçons de Clinique Médicale, faites á l'Hôpital Lariboisière.* Par S. Jaccoud. 1873.

nised in the first instance nearly simultaneously by the late Professor Bennett, of Edinburgh, and Professor Virchow, of Berlin, the names of *leucocythæmia* and *leucæmia* have been attached. In the second group, observation has never detected any excess in the number of white globules in the blood. To this class various designations have been given, according to the point of view whence the complaint has been studied. The late Dr. Hodgkin, of Guy's Hospital, first called attention to it in the 'Medico Chirurgical Transactions' for 1832; and considering that the normal proportion of red globules in the blood is diminished, and that this condition originates in disease of the lymphatic system, gave it the name of *lymphatic anæmia*. Dr. Wilks has since written upon the subject under the same appellation. Regarding it from an anatomical point of view, the titles of *general hypertrophy of the lymphatic glands*, proposed by Cossy, or *multiple lymphadenoma* given by Wunderlich, are unexceptional. The term of *adénie*, or *adenoid disease*, assigned to it by the late M. Trousseau, is rejected by M. Jaccoud as not involving any clear idea. It has, however, the advantage of simplicity, and points to the most noticeable seat of the complaint. Lastly, looking at the subject historically, and with the wish to perpetuate the name of its first describer, the phrase *Hodgkin's disease* has recommended itself to some for adoption.

That the two groups or twin forms of the disease are allied is generally admitted; but M. Jaccoud goes further, and asserts their essential identity, and on these grounds:

They are alike in their slow and insidious *début*; in the organs attacked—lymphatic glands, spleen, liver, and occasionally the intestinal glands; in the gradually increasing weakness and emaciation which mark their progress; in the cutaneous eruptions which appear in their course; in the œdema or hæmorrhage which occur in their later stages; in their invariably fatal termination, and in the modes in which this takes place—asphyxia from pressure of enlarged glands on the air-passages, intercurrent inflammation, or some cerebral accident.

There are some differences, however. Taking typical examples, the anatomical constitution of the glandular swellings is not the same in each variety. In pure *leucocythæmia* the new growth is altogether expended on the cellular elements; but in *Hodgkin's disease* the capsule and connective tissue are more or less hypertrophied and increased in density. But there are, as might be expected, intermediate forms in which these conditions are variously blended. And, as before stated, the character and composition of the blood is very dissimilar in the two affections. Can any explanation be offered of this variation? M. Jaccoud throws out three suppositions. It may be, he thinks, that leucocythæmia is not due to an excessive formation of white globules, but to some defect, or

arrest in the process of their transformation from white to red. Or, it is possible that the thickened and indurated condition of the enveloping and connecting tissues of the glands in Hodgkin's disease may compress their efferent vessels, and thus prevent the passage of white globules into the blood. Or, lastly, that some light may be derived from the observations of Neumann and Bizzozero, who have shown that the osseous medulla contains colourless corpuscles like white blood-globules, as well as cells with coloured nuclei. Hence, it seems not improbable that alterations in this tissue may have some influence in producing leucocythæmia. Bearing upon this we may recall to mind that in four out of twelve cases M. Trousseau noted the coexistence of chronic coryza, or otitis, or lachrymal tumour.

As regards the nature of this affection, M. Jaccoud maintains that it is unique in character, and distinct from scofula, tubercle, syphilis, or cancer. The habit of body in which it originates he styles the *lymphogenic diathesis*, which may exist either with or without leucocythæmia.

Clinically considered, one of the most important questions bearing upon the probable course and duration of the disease relates to the state of the intrathoracic ganglia. When these are largely involved death is an early consequence of the gradually increasing respiratory and circulatory embarrassment which they produce. Upon the etiology of the complaint M. Jaccoud can tell us nothing, and his treatment of it has not been more successful than that of other physicians. Iron and iodine, mercury and arsenic, quinine and cod-liver oil, have been tried again and again. In some cases good has appeared to result, for a time, from the use of one or other of these agents; but, in the end, all of them have proved equally powerless in preventing a fatal result.

The subject of aphasia, or loss of the power of speech, is interesting to the psychologist, the physiologist, and the physician. Neglected for many years, it has of late attracted much notice, and has been variously regarded. Dismissing theoretical considerations, M. Jaccoud studies it as a question of philosophical and practical medicine, and bases his classification of the forms of the disease upon cases actually under his observation. The words aphasia or alalia he regards as generic expressions, embracing all pathological, non-congenital disorders of speech, and of these he recognises five varieties.

The *first* is seen in the confused and indistinct utterance following an attack of hemiplegia, where the muscles of the affected side of the tongue have not recovered their power. There may be no impairment of memory or intelligence, no difficulty in swallowing; the relation between thought and expression is undisturbed, but one half of the tongue is more or less paralysed. To this form M. Jaccoud gives the name of *glossoplegia*.

The *second* is also found as a more or less remote consequence of a cerebral attack. The power of speech, which had been lost, is partially regained, but the articulation is faulty. Words are wrongly pronounced, and are uttered with a tumultuous precipitation which renders them hardly intelligible. The intellect is unaffected, the action of the tongue and lips is perfect, and the ability to write, where previously possessed, remains. The seat of the disease is in that part of the nervous centre which presides over the association of complex movements, such as articulation.

This want of co-ordinating power, as applied to the organs of speech, M. Jaccoud styles *glossoataxia*.

The *third* variety answers more strictly to its generic designation. Here the power of speech is absolutely lost, a simple meaningless monosyllable being probably all that can be uttered. But the intelligence and memory of words remain; impressions made upon the auditory or visual nerves are recognised and understood. Such a patient may be able to write, or mentally read, but he cannot transmit a verbal formula externally. This arrest of transmitting power has received the name of *logoplegia*.

A *fourth* group of cases is characterised by loss of memory of words. The attack may be sudden, and without paralysis or loss of consciousness. But there is usually, at first, a degree of intellectual stupor, and loss of the power to read and write. The title of *verbal amnesia* responds well to this condition.

The limits of M. Jaccoud's *fifth* variety are less well marked than those of the preceding divisions. Joseph Frank, long ago, described a form of mental depression met with in cerebral disease, in which abolition of speech is consequent upon abolition of thought. This is classified as aphasia from *hebetude*.

Leaving unnoticed certain exceptional forms of aphasia, let us consider the succession of acts which occur in completed speech. We find—1st. The idea or thought. 2nd. The investiture of this in the formula of language. 3rd. The transmission of corresponding impulses to the motor apparatus. 4th. The harmonious co-ordination of this action. 5th. The execution of the requisite muscular movements. These five successive processes may be reduced to the three acts of the *formation*, the *transmission*, and the *expression*.

As a deduction from this, M. Jaccoud offers the following pathogenetic table of asphasia :

I. Disorder of the process of translating ideas into words	Hebetude. Verbal amnesia.
II. Disorder of the process of voluntary transmission.	Logoplegia.
III. Disorder of the process of expression . .	Glossoataxia. Glossoplegia.

Both physiologically and pathologically there is an exact parallel between the functions of speech and that of voluntary motion, and akinesia may be subdivided and classified in the same manner as aphasia.

The question now arises—Have the forms of aphasia, sketched above, corresponding anatomical expressions? Has each group of cases a definite seat as to the lesions producing it? Speaking generally, the reply is affirmative.

In the first variety (*glossoplegia*) the affection of the hypoglossal nerve may be due to some textural change between its apparent origin and its distribution; or it may be in the medulla oblongata; or, and this is most frequently the case, it may exist at any point in the course of the connecting filaments between the bulbar origin of the nerve and the cortical layer of the opposite hemisphere of the brain. M. Jaccoud makes an important practical observation, bearing upon our knowledge of these conditions. When the nervous trunks or their medullary origins are implicated, deglutition is difficult, and the reflex movements of the paralysed parts are abolished, but this is not the case when the disease occupies any part of the higher fasciculus of fibres in its course through the brain.

In *glossoataxia* the anatomical relation is clearer and better defined. Schroeder van der Kolk (in 1858) ascribed to the olivary bodies a regulating influence over the muscles concerned in the production of voice; and consequently in them and in their connecting filaments he placed the anatomical centre of diseases in which this function is disturbed. But the more recent facts recorded by Romberg, Cruveilhier, Meynert, Chrastina, Rosenthal, and others, prove that this formula is too narrow and exclusive, and that in this variety of aphasia there may be disease in the olivary bodies, in the peduncles of the cerebellum, or in any part of the commissural system existing between the cerebellum and the medulla oblongata.

In the third subdivision (aphasia in its most restricted sense, or *logoplegia*) special care is needed to avoid erroneous and precipitate conclusions. The cases which have been collected range themselves in two distinct groups. In one the form and seat of the disease are various. There may be atrophy, or softening, or hæmorrhage, and this may exist in one or other of the corpora striata; in the centrum ovale of Vieussens; in the anterior, middle, or posterior lobe of either cerebral hemisphere, or in the island of Reil; in the second group, which is also the larger, the evidences of structural change are limited to the origin of the third left frontal convolution of the brain (the first of most German anatomists). It is upon these cases that M. Broca has founded his now celebrated theory. Commenting upon them, M. Jaccoud considers, first, the question of region (third frontal convolution), and secondly, that of side (the

left). As to the *first*, he concludes that, without deliberately neglecting many carefully recorded observations, it is impossible to assign to the disease a constant seat. He admits that morbid appearances are more frequently met with in some situations than in others; structural changes being most common in the corpora striata, in the island of Reil, and in the frontal convolutions, especially the third. And this he regards as the simple and natural consequence of the anatomical relations of the cerebral hemispheres and the spinal apparatus. By the optic thalami centripetal impressions reach the grey matter of the brain. By the corpora striata centrifugal motor impulses are transmitted to the spinal cord. Whatever the starting-point of the impulse, whether in the anterior, middle, or posterior lobe, there is no other channel but this, the corpus striatum and the island of Reil; hence structural changes of these parts (M. Jaccoud calls them *insular lesions*) produce aphasia, not by implicating any supposed seat of the faculty of language, but by interrupting the continuity or permeability of the channel between the hemispheres and the spinal cord. In explaining the production of aphasia by disease of the middle or posterior lobes, M. Jaccoud reminds his readers of Valentin's comparison of the cortical substance of the brain to a mosaic, the compartments of which are united by anastomosing filaments; and granting our utter ignorance of their specific functions, he thinks the hypothesis admissible, that the cellular groups of these regions have relations with the process which clothes thought in words, and thus that an alteration in the connecting white matter may induce abolition of speech by the same mechanism as in disease of the anterior lobes. Or, it may be that in these cases an influence analogous to that of reflex action is transmitted through the general antero-posterior commissures of the brain.

As to the *second* question, that of *side*, the attempt to establish as a law the dependence of aphasia upon disease of the third left frontal convolution has completely broken down. Many examples have been recorded of aphasia with left hemiplegia and structural change on the right side of the brain. It is undeniable, however, that in the great majority of cases the left is the side attacked. How is this to be explained? Rejecting the far-fetched theories of Gratiolet, Broca and Meynert, as to the greater influence and earlier development of the left hemisphere of the brain, M. Jaccoud points out that most frequently the lesion discovered is softening produced by embolism of the Sylvian (or middle cerebral) artery. Now, this is much more frequent on the left than on the right side. Out of fifty-one examples, Meissner met with only eight instances of obstruction on the right side. And this, as is well known, is simply owing to the mode of origin of the left common carotid from the arch of the aorta. As to the last two varieties of aphasia, *verbal amnesia*, and that proceeding from *cerebral hebetude*, facts are wanting to

warrant their association with disease in any particular part of the brain; all that is allowable is, to refer them to alterations in the cortical layer of the hemispheres.

The foregoing paragraphs give in a condensed form the most important of M. Jaccoud's observations on aphasia, upon which we now offer a few comments. His classification, although probably the most complete and correct which has been given, has some deficiencies. A division is wanted to comprise the not infrequent and multiform cases of transient and functional loss of the power of speech, as those produced by strong moral emotion, or fright, or occurring after fever, or, in hysterical and insane patients, or in some cases of lead poisoning.

His first group might, as M. Jaccoud himself remarks, be expanded so as to include those cases where speech is prevented in consequence of spasm affecting the tongue, or muscular trembling, as in paralysis agitans. The name of *glossal dyskinesia* might then be assigned to it.

The second is a limited and well-defined variety. There is no hemiplegic paralysis, no loss of the power of writing, no impairment of mental power, but a chorea-like condition of the muscles concerned in speech from want of co-ordinating power exists. Such cases as the remarkable one recorded by Dr. Osborn ('Dublin Journal of Med. and Chem. Science,' vol. iv. p. 157) find here their appropriate place.

M. Jaccoud's third class is a very comprehensive one, and might advantageously be subdivided. It embraces cases of aphasia with paralysis and without; where the mental powers are slightly and seriously impaired; where the ability to understand what is said or read exists, and where it does not; where silent reading can be followed, and where it cannot; also the rare instances where reading aloud can be practised, but not speaking in other modes. Again, as to writing, there may be more or less complete inability, or the power to copy words may exist, or the still more rare ability to write from dictation. The only explanation which can be given of this diversity is, that, of the various channels by which motor influences reach the co-ordinating centres of the apparatus of speech, whether downwards from the cerebral hemispheres, or from impressions made on the nerves of sight or hearing referred to their respective centres, and reflected thence, one or more may be closed. Something also may be due to the energy and tension of the impulse, and thus one of emotion may be transmitted where volition fails.

Although future researches may modify some of these conclusions, there is every reason to believe that the main lines of inquiry have been accurately traced, and M. Jaccoud's contribution to the work is an important one.

Passing over M. Jaccoud's lectures on hemiplegia with the remark that they are models of close and careful reasoning, we come to his discussion of some interesting questions connected with chorea. And, first, as to its treatment. Recent studies of the disease have raised much doubt as to the power of remedies to overcome it, or even materially shorten its duration. Thus, the average period in Messrs. Gray and Tuckwell's eighteen cases—no drugs being given, but fair hygienic conditions being secured—was ten weeks and three days; in Gray's seven cases treated with arsenic and steel, ten weeks and two days; in Tuckwell's eight cases where arsenic was given in gradually increasing dose, ten weeks and six days; in Hillier's thirty cases, "about ten weeks"; in Sée's one hundred and seventeen cases, nine weeks and six days.

Hence, in any plan of treatment where the complaint is recent, if improvement only manifest itself after the lapse of two months, it is fair to presume that it is as much due to the naturally favourable evolution of the disease as to the remedies employed. But the efficacy of medication may be better estimated in chronic chorea, and if, after an existence of three months, the disorder gradually subsides under any system, it may claim the credit of the cure. M. Jaccoud has tried in several cases, with encouraging success, the mode of treatment recommended by Lubelski and Zimberlin. It consists in the application of ether spray over a space of from two to three inches on each side of the vertebral column. At first it should be employed twice daily, and for not longer than three minutes at one time; but if the case be severe, after two or three days it may be used three or four times daily, and for five or six minutes.

It appears to have a marked and speedy influence in reducing and controlling the violence of the irregular movements of chorea, where it fails, as it sometimes does, to effect a cure. The constant electric current is another agent of which M. Jaccoud thinks highly in the treatment of chorea, and according to the observations of Remak and Benedikt its effects are speedy and certain. The current should be weak, ascending, and not employed for more than one to three minutes. As to other remedies, M. Jaccoud only gives his own testimony in favour of bromide of potassium.

The relations of chorea and rheumatism have been, and still are, the subject of considerable difference of opinion. The connection did not escape the notice of many physicians, English and foreign, of the last generation; but the late Dr. Bright, of Guy's Hospital, was, we believe, the first to regard chorea as induced, not by articular rheumatism, as such, but by the serous inflammations with which it is so often associated.

The late Dr. Senhouse Kirkes advanced this doctrine another step, and concluded from numerous observations that in the

production of chorea, it is the heart, whether with or without rheumatism, which is in fault. Cyon, who wrote a careful memoir on the subject, in 1865, regarded rheumatism as but mediately connected with chorea, through the supervention of peri- or endocarditis. Shortly after this date Spitzmüller again claimed for simple rheumatism a large influence as a cause of chorea. He made an important remark, the justice of which has been confirmed by Benedikt. Irregularity in the heart's rhythm, and even alteration of its sounds, occurring in chorea, are no sufficient proof of structural change. They may be due to in-co-ordination of the papillary muscles, and may be produced by the chorea itself. Hence, when the patient's medical history is not complete, the importance of watching a case to its close before pronouncing upon the existence of actual heart disease. M. H. Roger, the well-known and distinguished physician of the Children's Hospital in Paris, affirmed (in 1868) his belief in the existence of an interdependence between the three affections. He viewed chorea as the result of a rheumatic diathesis, and equally with peri- or endocarditis as one of its manifestations. He also thought that it might be the first evidence of this, and precede any heart or joint implication. A valuable communication to this journal in the same year, by Dr. John Ogle, proved that in fatal cases fibrinous deposit on the valves or lining membrane of the heart had been found by him in ten cases out of sixteen. Of non-fatal cases the pre-existence of rheumatic fever was noted in eight cases out of eighty. Lastly, Steiner, a careful observer of the disease at the Children's Hospital in Prague, was led to conclude that rheumatism had but little influence in its causation; out of 252 cases, in four only did chorea show itself during the course of a rheumatic attack.

With these facts before us, we cannot refuse to accept M. Jaccoud's statement of the question—that the influence attributed to rheumatism is really due to implication of the heart; but this relation, although frequent is, he thinks, not sufficiently so to be regarded as a law. Another interesting problem for solution is, what is the nature of the relationship between chorea and the affections of the heart to which reference has been made?

Dr. Kirkes, in 1863, supposed that fine molecular particles of fibrin washed from an inflamed endocardium and arrested in the capillaries of the nervous centres produced chorea, by setting up "irritation" there. In support of this view he appealed to pathological anatomy, and adduced examples of fatal cases of chorea, where white softening of the brain or spinal cord, or both, had been found on examination. A modification of this theory has been widely accepted both in this country and in Germany. It is objected to, by M. Jaccoud on the following grounds:—He observes, firstly, although without attaching any great weight

to the argument, that it is only applicable to fatal examples of the disease, and that in the much larger number of curable cases it is hard to admit the existence of capillary embolism of the corpus striatum or optic thalamus. It must be admitted, of course, that we really know nothing of the amount of tolerance which these bodies possess, or of the limits within which very circumscribed lesions may be repaired; so that a negative position may be as fairly assumed as a positive one. But to come to closer quarters: the new theory contains two propositions—one as to the nature of the brain condition (capillary embolism); the second, as to its seat (the corpus striatum or optic thalamus).

Now, the first is certainly not constantly true. There are many recently reported notes of necropsies where neither heart disease nor embolism was found. And in some older records, as those of Rokitansky, Demme, and Eisenmann, the only evidences of structural change were spinal.

On clinical grounds, also, there are objections to this view. In many cases there is no proof of the existence of heart disease, and here it would be gratuitous to speak of embolism.

Again, cerebral embolism is much more frequent on the left than on the right side; hence, if the theory were well founded, unilateral chorea should be on the right. But it is undeniable that it is most frequently found on the left side.

The second proposition is no less disputable than the first. On studying the pathological anatomy of the disease we find that in many instances there is no evidence of morbid appearances in the cerebral ganglia, and in others there is proof that the spinal cord has undergone structural change. The clinical history of chorea is equally opposed to the exclusive limitation under discussion. The dorsal pains dwelt upon by Stiebel, and the choreic vertebral points to which Triboulet called attention, clearly indicate spinal implication in the cases in which they occur.

Again, hemichorea, although the most frequent, is not the only form of the disease. When the complaint is double, are we to assume that alterations exist in the corpora striata and optic thalami on both sides? Or how explain the fact that the disorder is sometimes limited to the limbs, and does not affect the face? Or that occasionally the eyeball and larynx are involved, or the mind disturbed? No other inference seems admissible than that the organic seat of the disease varies, and that it may occupy any part of the great motor co-ordinating apparatus which extends through the whole cerebro-spinal axis. We may remark also that a more careful classification would remove much of our doubt and difficulty. The irregular muscular contractions due to reflex action, set up by a tapeworm in the intestines, or by uterine or ovarian irritation, ought surely to receive a different place and

designation from those which are produced by a tumour in the spinal cord or by softening of the nervous centres.

We append M. Jaccoud's conclusions upon the subject of chorea, which we think our readers will now be prepared to accept:—(1) The relations of chorea with articular rheumatism are less close than hitherto believed. (2) The causative influence of rheumatism is really due to the coincident inflammation of the heart, and especially of the endocardium. (3) When these lesions exist, they may in some instances produce chorea by giving rise to capillary embolism. (4) It is impossible to consider the cardiac affection as the only cause of chorea. (5) Moral influences and bodily conditions, both direct and reflected, have an important share in its production. (6) The organic seat of chorea cannot be fixed in the corpus striatum, but rather in any part of the bulbar-spinal co-ordinating apparatus, its extent and situation being exactly proportionate to the distribution of the peripheral symptoms.

To the subject of tuberculosis and pulmonary phthisis M. Jaccoud devotes several lectures. He commences by an interesting historical sketch, in which he shows how the more correct ideas of Portal and Baillie at the close of the last century were displaced by the opinions of Bayle and Laennec; how the notion of phthisis became fused with that of tubercle, and both with that of scrofula; how for thirty years these views were all but universally accepted, until brought back by the teaching of Graves and Addison in this country, Autenrieth, Schonlein, Reinhardt, and Virchow, in Germany, the doctrine of the duality of phthisis has been re-established, with corresponding changes in our estimate of the etiology, prognosis, and treatment of consumption. By the term just employed it is intended to express that pulmonary phthisis is sometimes linked with tuberculosis, or initial granular tubercle; sometimes with pneumonia and cheesy transformation of the effused inflammation-products. Opinions still vary much as to the relative occurrence of these forms; some authorities regard them as about equally common; others, as Slavjansky, Sangalli, and M. Jaccoud, believe that pneumonic phthisis is much the more frequent.

But as the subject of tubercle and consumption will shortly be again discussed in the pages of this Review, we shall not attempt to follow M. Jaccoud through his careful examination of the pathology and differential diagnosis of the two varieties of phthisis, but simply select for notice a few points characterised by special interest or novelty. And first, as to the diagnosis of acute tuberculosis. Practical physicians are familiar with the difficulty of arriving at positive conclusions in these cases. Miliary granulations, whilst solitary and not accompanied by catarrh, may give rise to no abnormal sounds, hence the attendant fever has again and again been mistaken for typhoid, an error all but inevitable in such a case as one narrated

by M. Jaccoud, which is sufficiently interesting to be transcribed here. A young man who had recently arrived in Paris was suddenly seized with epistaxis, slight dyspnœa and cough, fever, and diarrhœa. Enlargement of the spleen, and on the tenth day of his illness the appearance of a rose-coloured eruption on the chest and abdomen, confirmed the opinion that the disease was typhoid fever. Not until the fourteenth day were any bronchial râles or other signs of lung disease, detected. On the fifteenth, opisthotonos and trismus supervened; on the eighteenth, the tetanoid spasms became general, and were accompanied by delirium, and death ensued on the twentieth. At the necropsy no intestinal lesions were found; the spleen was large and soft; the lungs contained granular tubercle throughout; and a fungous growth, of the size of a filbert, springing from the under surface of the tentorium cerebelli, explained the convulsive phenomena. Although in this case life was cut short by the cerebral complication, its history illustrates well some of the perplexities of practice. In forming a diagnosis in these puzzling cases, M. Jaccoud believes that much aid may be gained by bearing in mind the following points:—(1) The personal and family history of the patient should be carefully interrogated to ascertain if there have been any previous gradual impairment of the health, or if there be any hereditary tendency to tuberculosis. (2) If the initial symptoms be not followed in forty-eight or seventy-two hours by undoubted physical evidence of catarrhal or lobar pneumonia, tuberculosis may be suspected. (3) The same conclusion may be formed if the temperature rise at once to 103° or 104°, and remain continuously high; the morning remission being very slight. (4) From repeated observations, M. Jaccoud attaches considerable value, as a diagnostic symptom, to the occurrence of pain in the hypochondria, sharp, persistent, or frequently renewed, and increased by pressure or deep inspiration. It is probably referable to some tuberculous deposit on the serous membranes, or to some low and circumscribed inflammation. (5) Acute tuberculosis of the lung is very often accompanied by deposit elsewhere—in the intestines, on the peritoneum, or in the brain—hence groups of additional symptoms which may throw light on the character of the disease. (6) To this we must add that Oppolzer states, in his 'Clinical Lectures,' that in an ophthalmoscopic examination of the eye we have a means of forming a right diagnosis even in the most difficult cases. Cohnheim, Von Gräfe, B. Frankel, Leber, Bouchut, and others, affirm that in acute tuberculosis miliary tubercles are always to be detected in the choroid membrane.

Another interesting question upon which opinion has fluctuated much is the relation to tuberculosis and phthisis, and the clinical value of broncho-pulmonary hæmorrhage. We speak, of course, of hæmoptysis occurring independently of heart disease, or aneurism, or cancer. Morton, Hoffman, and Cullen considered it as not infrequently

giving rise to phthisis. Laennec, whose views were accepted by Louis and Andral, regarded it as a sign of already existing tuberculous disease; it was the effect, and not the cause. This doctrine was very generally received both in this country and on the Continent, but of late years the old belief has gained ground. The late Professor Graves, of Dublin, to whom practical medicine is so deeply indebted, was one of the first to call in question Laennec's teaching. He pointed out that suppurative pneumonia may follow lung condensation produced by hæmorrhage, and that fatal cases of pulmonary apoplexy are met with without the existence of a single tubercle in the lungs; at the same time he granted that bronchial hæmorrhage does result from tuberculous deposit. The views of Oppolzer, of Vienna, were very similar; he affirmed that in a considerable proportion of cases hæmoptysis must be deemed, not a consequence, but a cause of phthisis, and that this is seen, not only in weakly persons, but in individuals of good constitution, and, to all seeming, previously in robust health.

But it is to the writings of F. Niemeyer that the revulsion of opinion is chiefly due. He maintains that bronchial hæmorrhage occurs more frequently than is generally admitted in persons who are neither consumptive at the time nor become so subsequently; and that if it be the forerunner of phthisis in some cases, there may be no causal connection, but both may be referable to a common condition, that of vascular weakness. He believes that the non-expectorated blood of a broncho-pulmonary hæmorrhage often becomes a source of irritation and inflammation, and that although this sometimes ends in resolution, it often passes into cheesy degeneration, ulceration, and phthisis. He thinks that the cases are rare in which hæmoptysis is the consequence of such pneumonic inflammatory changes as lead to consumption in their progress. He admits that blood remaining in the pulmonary cells, and with pneumonic infiltration undergoing cheesy transformation, may give rise to an eruption of miliary tubercles; also that attacks of bronchial hæmorrhage are met with more frequently in the course of pulmonary phthisis than before it.

These views were combated by Skoda, but they have been largely adopted both in this country and France by those who have made a special study of consumption. M. Jaccoud receives them with some reservations, believing that they are too narrow and exclusive.

The effects of extravasated and retained blood seem to vary in different cases. It is only occasionally that after an attack of hæmoptysis a residue of blood in the pulmonary cells gives rise to inflammation and disorganization. Some additional influence appears to be needful to bring about this result. At present the nature of this can only be surmised; M. Jaccoud calls it a "predisposition;" further information is wanting.

In the mean time it must be admitted that a decided advance has been made in our knowledge. It is clear that the old doctrine of Laennec must be abandoned. Hæmoptysis is not necessarily an indication of already existing tuberculous disease, nor is it indissolubly linked with subsequent phthisis. Moreover, when the connection does exist, the phthisis is pneumonic and is due to the effused blood, and the inflammatory afflux which caused the hæmorrhage.

M. Jaccoud occupies intermediate ground between the opinions of Laennec and Niemeyer. He admits the existence of a double relation between broncho-pulmonary hæmorrhage and lung disease; it may be primary, and occasion it; or secondary, and be a consequence of it. In the former case it produces phthisis by initiating pneumonic processes followed by cheesy degeneration and ulceration. In the latter it is generally due to the rupture of some pulmonary artery, dilated and denuded by advancing ulceration. In his account of secondary hæmoptysis we need not follow M. Jaccoud, as he simply reproduces the statements of Rokitansky and Rasmüssen, with which our readers are doubtless familiar. But his observations on tricuspid dilatation are novel and deserve quoting. How is it, he inquires, seeing that the conditions favourable to the occurrence of secondary hæmorrhage are so common, it does not take place more frequently? It is owing, he believes, to a gradual diminution in the amount of blood sent through the pulmonary artery, owing to insufficiency of the tricuspid valves, and consequently reduced pressure. For some years, on examining after death the bodies of phthisical patients, M. Jaccoud has minutely investigated the state of the heart, and has found without exception that, whenever absence of secondary hæmorrhage coincided with large pulmonary ulceration, there existed a more or less considerable dilatation of the tricuspid orifice. In some of these cases the insufficiency was not recognised during life; in others it gave rise to a systolic murmur, heard under the xyphoid cartilage; in a third group both to murmur and evident venous reflux.

Want of room prevents our following M. Jaccoud in his admirable lectures on the management of phthisis. A short summary must suffice.

He observes that one of the most valuable results of the changed opinion as to the nature of the disease is the greater hopefulness with which its treatment may be undertaken. On the curability of the pneumonic form his experience is very encouraging. As to tuberculous phthisis the question must be reserved; with the exception of Lebert's cases, data are wanting. He is a warm advocate of a supporting and even stimulating regimen, and a firm believer in the efficacy of cod-liver oil and iron. In chronic cases, where there is no special indication for the use of steel, he gives arsenic,

and, by preference, in the form of granules containing one sixtieth of a grain; of these the dose is gradually increased to eight or ten daily, which are taken with food. He is under the impression that it calms vascular and nervous excitement, has a beneficial influence on the nutritive processes of the body, and that it is useful in combating any tendency to evening fever. Where the disease is merely imminent or in its earliest stage, he has great faith in the good influence of a suitable change of climate, both for winter and summer; but where the evidence of alteration of structure is more decided, he believes that the fine season of the year should be spent at some mineral spring. Especially should this be done if there be any signs of the patient having suffered from scrofula, or if he be predisposed hereditarily or otherwise to gout or rheumatism, or that badly defined habit of body which our neighbours call herpetism. It is, in his greater reliance upon the use of mineral waters, and in his employment of arsenic that M. Jaccoud's practice differs chiefly from that of English physicians.

In his lecture on mitral stenosis M. Jaccoud offers an explanation of the mode of production of the two murmurs characterising it which seems simple and sufficient. In most cases the presystolic murmur and the murmur with the second sound exclude each other; hence the controversy on the subject, some admitting as evidence of mitral contraction a diastolic murmur as its sole proof. But each or both may occur, the interpretation in the latter case being the coexistence of valvular insufficiency. Why in mitral contraction is a murmur with the second sound so rare? Doubtless, owing to feebleness in the pressure of the column of blood which begins to pass from the auricle into the ventricle at the moment of the sudden expansion of the latter. If the pressure be notably increased, the blood passes with more force, and a murmur results. This is coincident with the second sound of the heart. But a passive flow being established, it remains silent until a new power comes into play, the contraction of the auricle, and a presystolic murmur may be produced. Thus both, as in the case upon which M. Jaccoud founded his comments, may coexist. Now, in mitral inadequacy a portion of the blood which should pass into the aorta flows back into the auricle, and an increase of pressure is created in proportion to the amount of reflux. When the diastole occurs it again descends suddenly into the ventricle, with an increased initial force in exact relation to the amount of tension. Hence vibration and a murmur, but this effect may be only momentary, as is the excess of intra-auricular pressure.

Under the term parenchymatous atrophy of the liver M. Jaccoud groups several forms of hepatic disease, attended sooner or later with wasting of the organ and suppression of its functions. Thus, he includes the destructive hepatitis giving rise, as a primary affec-

tion, to acute yellow atrophy, or occuring secondarily in the course of typhus and other diseases. The shrinking consequent upon the fatty degeneration of tuberculosis and old age, or produced by some poisons, or the result of pressure, are also embraced. But this classification is open to the objection that its characteristic does not apply at all times, and that in the early stages of some of the forms of disease which it comprises the opposite state may exist. More or less completely in these cases the functions of the liver are damaged or destroyed; the blood suffers by the non-separation of those elements which furnish the materials of the bile, and a state which M. Jaccoud calls *hepatic asphyxia* is induced. Hence the occurrence of hæmorrhages, convulsions, delirium and coma.

In the management of hydatid cysts of the liver M. Jaccoud recommends the complete withdrawal of the liquid, by means of the aspirator. After the operation he advises that the patient should be kept upon his back for at least three days; that ice should be constantly applied over the right hypochondriac region; that slight pressure should be maintained there by a bandage, and that if pain arise it should be quelled by large subcutaneous injections of morphia. After the lapse of a few days, if satisfactory progress be made, iodide of potassium may be given in large doses, commencing with half a drachm daily, and gradually increasing the quantity until a drachm and a half be taken in the same space of time. As regards the latter proceeding, it may be stated that the utility of the employment of iodide of potassium in these cases has been seriously called in question both by Frerichs and Murchison. Good evidence has lately been produced of the value of the electrolytic treatment of hydatid tumours of the liver, but of it M. Jaccoud has had no experience. In the treatment of uric acid gravel, and catarrh of the urinary mucous membrane as a consequence, M. Jaccoud warns his readers against the employment of terebinthinate and balsamic remedies, which he thinks likely to aggravate the evil. And in renal obstruction and diminished secretion from the same cause he strongly recommends that the patient should be placed for a time on an exclusively milk diet. In a case of this nature three and a half pints of milk taken in eight portions during the twenty-four hours, in the course of six days, increased the daily secretion of urine from twelve to fifty-four ounces, with complete removal from it of the deposit of uric acid. A mixed regimen, containing a large proportion of milk, is also advised for habitual adoption by the subjects of this diathesis. The maintenance of the urine in a diluted state explains the good effects of this proceeding.

Believing that the great value of milk as a therapeutic agent is insufficiently recognised in this country, we notice more fully M. Jaccoud's lecture on this subject. Milk may be employed

as food, as a renal hydragogue, or as a sedative. Estimated from the first point of view, from its emollient and soothing influence, it is especially useful in ulceration of the gastro-intestinal mucous membrane; so it is also in chronic enteritis either of adults or of children, when accompanied by diarrhœa, and in chronic dysentery. Milk alone, being for those who have passed the age of childhood an insufficient food, it has on this ground been recommended by Weir Mitchell in the treatment of obesity, and by Pécholier in that of hypertrophy of the heart. But upon its quality as an unirritating hydragogue rests the special medicinal value of milk. Hence its suitability for the whole class of dropsies, and affections of the kidney attended with albuminuria. In acute general dropsy from cold, whether with or without the presence of albumen in the urine, in most instances, when three or four days have passed, and the fever has subsided, no other treatment is needed than the adoption of a purely milk diet; and the same remark applies to dropsy after scarlatina. In anasarca from heart-disease, a palliative action only can be expected, but milk is often a most useful resource, and fails altogether only in old cases where the heart's action is feeble, or its muscular structure has undergone degeneration. In catarrhal nephritis its use is distinctly curative, and in all the varieties of Bright's disease, the dropsy may be removed by it for a time. Whether more than this can be effected must depend upon the form and period of the malady. When chronic from its commencement the intervention of art can but mitigate the evil, but in this respect the worth of a milk regimen is often great. Such is also the case in catarrh of the urinary mucous membrane, whether specific or otherwise. Lastly, in acute pleurisy with large effusion, both M. Jaccoud and his colleague M. Siredey have found the diuretic action of milk most serviceable. If the amount of liquid be not so great as to call for paracentesis, this mode of treatment may be had recourse to, with a confident expectation of success.

In his management of cases of typhoid fever, M. Jaccoud has for some years adopted a special mode of proceeding, with, as he believes, unusually favourable results, in proof of which he appeals to statistics. Taking the records of hospitals for several years in some of the capitals and large cities of Europe, and grouping the whole, he finds the rate of mortality to average from nineteen to twenty per cent. In his own practice, at the Maison de Santé and the Hôpital Laribosière, from January, 1867, to August, 1872, excluding the period of the siege of Paris, and also deducting the instances in which the patient sunk within five days after admission, twenty-eight deaths occurred in 282 cases, or, as nearly as may be, in ten per cent. Now, although the value of this evidence is somewhat diminished by the fact that the fatality of fever varies at different

times, and that M. Gaultier de Claubry found that taking the whole of the mortality in the several epidemics which swept over France between 1841 and 1852, the percentage of deaths was about fifteen only; still the reduction is sufficiently considerable to merit a careful consideration. Other advantages are also claimed for his mode of treatment, amongst which may be mentioned diminution or prevention of meteorism; absence of secondary gastric disorder; rareness of bed-sores or of thrombosis; the substitution of a decided and complete declension for the more gradual fall which generally characterises the subsidence of fever; and shortness of the period of convalescence.

Whatever theory be adopted as to the nature of typhoid fever, it is certain that the action of the poison has a very prostrating influence. Also, excluding abortive and speedily fatal cases, that it has a longer course than most acute diseases. There is an excessive production of heat, and a rapid consumption of the body. The complaint is very liable to be complicated by the supervention of broncho-pulmonary disorder. Hence, three paramount indications: to sustain the patient's strength; to diminish calorification; to ward off or check congestion of the lungs or air-tubes. These M. Jaccoud endeavours to fulfil as follows. He abjures from the commencement all debilitating measures, such as the purging until lately in vogue in the Paris hospitals. If needful, a mild laxative of Seidlitz water is given. During each twenty-four hours the patient takes a cordial mixture containing two or three scruples of extract of Cinchona bark, two drachms of tincture of cinnamon, an ounce of syrup of orange-peel, three ounces of red Bordeaux wine, and an ounce of old brandy, which may be increased, as occasion requires, to three or four ounces. The main constituent of the food is beef-soup given at least twice daily, and which is thought to be much more nutritive and useful than our English beef-tea; about eight ounces of old Bordeaux wine are allowed, and this is taken alternately with the stimulant mixture just mentioned. The ordinary beverage is lemonade, containing one fourth of red wine. If it appear needful, all this is supplemented at the end of the second week by nutritive enemata twice daily. This mode of alimentation is stated to be well tolerated by the stomach, and there is no note of its having any tendency to disturb the bowels.

When the patient's temperature reaches 102° Fahr., with a rise in the evening not above 103°, M. Jaccoud directs that twice during the day the whole body should be well sponged with aromatic vinegar. If the temperature rise higher, three, or even four, of these applications are made daily. They are continued, more or less frequently, until the close of the fever, and both comfort and benefit are derived from their refreshing and cooling effects.

Should any threatening of bronchial or pulmonary implication

arise, this routine is not altered, but dry cupping is freely employed as a derivative. Its influence being very transient, it should be repeated at short intervals. So long as the indication for its use exists, M. Jaccoud advises that night and morning from forty to sixty cupping glasses should be applied over the base of the chest and over the lower limbs.

Although details may vary, we believe that the principles on which this treatment is based are sound, and that they are those which now generally govern English practice. Much has lately been written, especially by the physicians who served with the French and German armies in the late war, in praise of the cold-water treatment of fever. It is hard to see, however, that at any period of the complaint its good effects can be more than subsidiary, whilst, in the later stages of the disease, its use does not seem free from risk. The free sponging advocated by M. Jaccoud, involving no exertion on the part of the patient, appears to be a safer and more commendable practice.

In the management of idiopathic erysipelas, M. Jaccoud also claims to have met with more than ordinary success. Out of sixty-seven cases, many of them of great severity, which passed through his wards in about five years, he had but one death. This is certainly a highly favourable result, and merits our notice. The following is the system pursued. Compresses kept moistened with infusion of elder-flowers are applied to the inflamed surface. A mild laxative of Seidlitz water is given if needful. The administration of the Cinchona-bark wine of the French codex is then commenced at once. To a patient of average constitution, and in whom there is no wandering of the mind, from four to six ounces are given daily. If there be mild and chiefly nocturnal delirium eight ounces are prescribed, and the proportion is increased to twelve or even sixteen ounces if the delirium be constant and violent. To a subject accustomed to the use of stimulants, from one to three ounces of brandy with a few drops of laudanum are allowed daily in addition. These remedies are steadily continued until the fever subsides. The food is chiefly beef-soup, and lemonade with wine is given as a beverage.

Want of space, not want of matter, enjoins that we now bring these remarks to a close, and we do so, warmly recommending M. Jaccoud's book to the study of our readers. They will find much information in it, conveyed in a style easy and flowing, and singularly clear.

II.—Localisation of Nerve Disorders.[1]

The localisation of lesions in the nervous centres is by no means merely a point of pathological or anatomical refinement. It is a question which presents itself to the practitioner of medicine every time that he is called upon to advise in any one of the almost countless forms of paralysis. The problems of the nature, seat, and extent of the lesion in any example of such affection, have ever been objects of the deepest interest, and at the same time of great perplexity, to all but the mere routine practitioner.

It is the complicated structure of the nervous centres which renders this question one of such extreme difficulty. The different and multiple relations of any given nerve, not only in its course before it reaches its primary centre, but the varied connections also of the nerve centres themselves, all add to the apparently impregnable condition of the point to be carried. Let us take, for instance, the hypoglossal nerve. A patient presents himself with loss of power of movement in the tongue; putting aside the various points at which this nerve may be affected before it is traced to the medulla, when it has reached that point, the lesion may be found in the nucleus of the nerve. Further, we know that in common with other spinal nerves this hypoglossal is connected with the corpus striatum, and not only with the corpus striatum, but according to Hitzig, Fritsch, and Ferrier, with certain points in the cortical grey matter of the cerebrum. Thus the cause of the loss of movement of the tongue, in such a case, may be either in the medulla, at the nucleus, or in the fibres which connect the nucleus with the corpus striatum; in the corpus striatum itself, in the fibres radiating between that point and the cortical grey matter; or, finally, in the cortical grey matter itself.

The question of localisation is still further complicated, in cases where the affected nerves are purely spinal, by the decussation which takes place in the anterior pyramids. From the time of the discovery of this decussation, by Mistichelli, a paralysis of one side of the body has been regarded as due to a lesion of the opposite side of the brain. Latterly, however, cases recorded by men who cannot be considered as likely to be mistaken, seem to qualify this view. Attempts to explain away the phenomena of these apparently contradictory cases must in the view of Brown-Séquard[2] be regarded as futile.

Many as are these difficulties, the attempts which have been

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[1] *De la Localisation dans les Maladies Cérébrales.* Par le Dr. R. Lépine. Pp. 160. Paris, 1875.

[2] 'Lancet,' January 1, 1876.

made to surmount them may be said to be almost as numerous. To show, however, the ignorance of the seat of disease of the brain at the beginning of the present century, we quote from the essay of M. Lépine, the following propositions by M. Sandras,[1] in 1829, in answer to the question which he put in a thesis: "An peculiarium encephali et medullæ spinalis, partium læsionibus suæ sint peculiaria signa?

Sint ne signa læsionum substantiæ corticalis?
Nulla.
„ „ substantiæ medullaris?
Nulla.
„ „ loborum anteriorum?
Nulla.
„ „ loborum posteriorum?
Nulla.
„ „ corporum striatorum?
Nulla.
„ „ nervorum opticorum?
Nulla.
„ „ loborum mediorum?
Nulla.
„ „ ventriculorum?
Nulla.
„ „ partium mediarum?
Nulla.
„ „ ceterarum partium?
Nulla.

"Si peu que nous sachions localizer, il ne sera, je pense, pas difficile de localiser plus que Sandras," adds M. Lépine. Sandras was led to deny the possibility of localisation of lesions in the brain, although he had met with the observation of Bouillaud upon the association of loss of speech with lesions of the anterior lobes. This causal connection he met with denial or neglect. The physiological experiments of Flourens led to further researches in this direction, and eventually Broca propounded the theory that has been associated with his name.

M. Vulpian[2] observed that the theory was wanting in support, while with reference to the grey matter there was no evidence of the presence therein of motor centres. The discovery however of such centres, it is alleged, has been made.

MM. Fritsch and Hitzig produced movements of muscles by the application of a galvanic current to the cortex of the brain of a dog. They, however, had been anticipated in their conclusions by Dr. Hughlings Jackson, who insisted that the existence of motor centres in the cortical grey matter of the hemispheres was proved by his own clinical observations. In 1873, Fritsch and Hitzig published a second memoir, and in the same year Ferrier made his

[1] Theses quas deo faventi, in saluberrima facultate duedica Parisiensis, 1829.
[2] 'Leçons sur la Physiologie du Systeme Nerveuse,' p. 719.

experiments. Notwithstanding efforts which have been made to disprove the conclusions arrived at by these experimentalists, very little doubt remains, but that the nerve cells in the grey cortical substance of the anterior portion of the cerebral hemispheres are, to a large extent, centres whence originate voluntary movements. The posterior portions of the hemispheres, when stimulated by the electric current, were not found to produce movements. These portions of the hemispheres have, in consequence of this fact, been supposed to be the seat of sensibility. At present, however, few facts have been adduced in proof of this theory. All that is at present known upon this point is of a negative character.

That the anterior portions are absolutely necessary for the production of voluntary movements is, as already observed, not admitted on all hands. On the contrary, several cases have been published which seem to prove that many, if not all, the voluntary movements can be carried on as well when this portion of the brain is destroyed as when it is perfect. Among the most convincing of these, are those collected and published by Dr. Henry Day,[1] in an article on "Brain Injuries." The general nature of these cases will be best seen from the following quotation from the narrative of Mr. W. Roberts, of Carnarvon. He says:

"Just seven years ago, I was sent for to attend a young man who had met with an accident in the extensive slate quarries of Mr. Ashton Smith, in this county. While stamping a rock the powder ignited, and the blast went direct to his face; both eyeballs were shattered to pieces, the scalp on the forehead was much lacerated, and above the inner canthus of the left eye was a small hole in the *os frontis*, fairly through into the brain. Upon my arrival, I found the person who attends the men at the quarries introducing a grooved director through the hole, and scooping the inside of the skull, bringing out some blackish sludge and a good deal of brain, both cortical and medullary. I must confess I had no great hopes of the patient's recovery, and thinking I could do no more mischief than had already been done, being also anxious to know if the brain possessed any sensitive power, I took the director, and passed it in a direct line until it touched the *os occipitis*, and then turned it round in different parts, the young man being quite conscious all this time, assured me it gave him no pain; and it was only at the hole, in drawing out the instrument, that he did feel pain. It may be necessary to observe there was no pressure of the skull on any part of the brain, and no fracture, with the exception of the hole in the *os frontis*. After dressing the wound, and giving directions as to after treatment, I took my leave and was not sent for again; the patient got well without any unfavourable symptoms, and it so happened that I did not see him from that period until a few months since. The person already alluded to assured me that

[1] 'Transactions of the St. Andrew's Medical Graduates' Association,' 1872-73.

he repeatedly introduced the director afterwards, but it was not at my desire. From the manner in which the instrument was used at different times, I am satisfied both hemispheres of the cerebrum must have been broken down, and made a regular puddle of. Nevertheless, the young man has been in good health ever since, and not only that, but all the faculties of his mind are perfect, as well as his hearing, taste, smell, and feeling; his sight is of course destroyed. I shall content myself with merely stating the facts of this case. What I have here stated can be attested by living witnesses. The young man himself lives to tell the tale; his name is Griffith Jones; he resides at a farm called Ty-du, within half a mile of the old inn, on the banks of the celebrated Lakes of Llanberris."

The above is only one of the cases collected by Dr. Day: it is associated with others equally well attested, and which, if considered from the Hitzig and Ferrier point of view, certainly present many points difficult to be explained.

There is no necessity here to give details of the experiments made by Hitzig, Fritsch, or Ferrier: they have elsewhere been detailed with sufficient frequency. The opposing arguments of Burdon Sanderson, Carville, and Duret, and others, are also well known. Replying to these, Dr. Ferrier writes as follows:[1]

"It is contended that the electric currents employed by me for irritation are conducted from the surface of the brain to the basal ganglia, and that thus the movements are called forth. It is urged that if the cortical centres are motor, they ought to behave in the same way as the lower motor centres under the influence of anæsthetics, such as chloroform and ether; and it is urged that the cortex of the brain cannot contain motor centres, on the ground that in the state of complete narcosis, no results are obtained on the irritation of the hemispheres, while the lower ganglia and the motor nerves are still capable of excitation by the same stimulus. . . . . Were the results simply due to conduction, it would be expected that the motor phenomena would be manifested by the application of the electrodes to any part of the surface of the hemispheres. This, however, is not the case, for there are certain parts of the brain, the irritation of which gives rise to no external phenomena whatever."

The same author goes on further to say, that the use of the term motor centre has given rise to some misconception of his views of the motor functions of the hemispheres. By "motor centres," as applied to the cortical substance,

"I have endeavoured to signify the fact that this part is in direct communication with the motor tracts and their ganglia, and that its function is to excite co-ordinated muscular action of a definite kind of the nature of which we call voluntary."

[1] 'West Riding Lunatic Asylum Medical Report,' vol. iv, 1874.

Thus far, then, for the motor function of the cortex of the hemispheres as indicated experimentally.

For pathological support of the above theory we turn to the works of Dr. Hughlings Jackson. Many of these are we fear lost, so far as regards any practical use, since the author of them has seen fit to scatter the fruits of his own highly endowed brain almost broadcast throughout the medical literature of the day, by this means rendering the complete study of his views a matter of great difficulty, if not impossibility. The pamphlet [1] now before us is perhaps the first instalment of the collection of Dr. Jackson's published writings.

In the preface we find the following remarks with reference to the subject now under discussion:

"It is asserted by some that the cerebrum is an organ of the mind, and that it is not a motor organ. Some think the cerebrum is to be likened to an instrumentalist, and the motor centres to an instrument: one part is for ideas, the other for movements. It may then be asked, how can discharge of a part of a mental organ produce motor symptoms only? I say motor symptoms only, because, to give sharpness to the argument, I will suppose a case in which there is unilateral spasm without loss of consciousness. *But of what 'substance' can the organ of the mind be composed unless of processes representing movements and impressions; and how can the convolutions differ from the inferior centres, except as parts representing more intricate co-ordination of impressions, and movements in time and space than they do?* Are we to believe that the hemisphere is built on a plan *fundamentally* different from that of the motor tract? What can (the anatomical substratum of) an 'idea'—say of a ball—be, except a process representing certain impressions of surface and particular *muscular adjustments?* What is recollection but a revivification of such processes, which in the part have become part of the organism itself? What is delirium except the disorderly revival of sensori-motor processes received in the part? What is a mistake in a word but a wrong *movement*—a chorea? Giddiness can be but the temporary loss or disorder of certain relations in space, chiefly made up of muscular feelings. Surely the conclusion is irresistible, that 'mental' symptoms from disease of the hemisphere are fundamentally like hemiplegia, chorea, and convulsions, however specially different. They must all be due to lack or to disorderly development of sensori-*motor* processes."

Dr. Jackson proceeds to say that believing as he does the above facts, he thinks that they will tend in some degree to explain the results obtained by Hitzig and Ferrier. For himself he says:

"I had been driven to the conclusion that the convolutions *must*

[1] 'Clinical and Physiological Researches on the Nervous System,' (Reprint); No. 1, 'On the Localisation of Movements in the Brain,' by J. Hughlings Jackson, M.D., F.R.C.P., from 'Lancet,' 1873.

represent movements and impressions long before their experiments were made. So far as I can imagine, there is nothing else they can represent."

Dr. Jackson then entirely agrees with the deductions of Ferrier and Hitzig, and this because he had himself not only previously arrived at the point which they now profess experimentally to have proved, but had gone farther, and had applied the results which they obtained later to the elucidation of the nature of the processes of "mentation," which, he says, is the function of the cerebrum. The facts which he has employed to arrive at these conclusions are derived from his clinical study of the human brain, as observed in epilepsy, chorea, hemiplegia, &c.

The acceptation, then, of these views as to the functions of the convolutions, renders localisation of lesions therein a matter of less difficulty than would have been imagined. For example, let us suppose that a large quantity of blood, sufficient to affect all the anterior portion of one of the hemispheres has been effused. In a case of this nature we should expect to find a very complete form of hemiplegia, as all the motor nerves are here represented. Paralysis of co-ordinated groups of muscles may also be attributed to a lesion restricted to one, two, or three convolutions.

We now come to the fact that one at least of these centres has been specially pointed out, and had its rôle in the drama of life clearly assigned to it. Several years since Broca, founding his conclusions on the appearances he discovered in two autopsies, localised the function of articulate speech in the third frontal convolution of the left side. Since then many more cases have been brought forward in support of this theory. In fact, too many cases have been reported, and some of them in a slovenly manner.

M. Meynert[1] has extended the territory of language, and included in it the insula.

M. Lépine publishes a case in which the insula was affected, and not the third frontal. In connection with this case, although it does not seem to have much bearing on the subject, we would notice that the lesion is simply described as "un foyer du volume d'un noisette." Here, as in many other cases, there had been no microscopical observation either of the diseased portion, nor indeed of any other portion of the brain, the remark therefore with which he concludes, "La troisieme circonvolution frontale était absolument saine," is in our view open to question.

The evidence in favour of this localisation of language, in the neighbourhood at least of the convolution where Broca first placed it, is of a very strong nature, and has been by many pathologists accepted as conclusive.

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[1] 'Zeitschrift der Gesellschaft der Aertze,' 1860.

Dr. Ferrier, in his experiments, has found that this part of the brain when irritated produces movements in the mouth, tongue, and other muscles concerned in the production of language.

Dr. Hughlings Jackson agrees with Dr. Ferrier, and makes the conclusions arrived at by him to agree with his own theory, that the anatomical substrata of words are to be found in the anterior convolutions, and are motor processes.

Dr. Bastian,[1] speaking of aphasia, says:

"Looking to the records of carefully sifted cases there can remain little doubt, I think, as to the importance of the third frontal convolution on the left side, in regard to the power of intellectual expression. And though we need not regard it as the seat of any supposed faculty of language, we can easily imagine that the volitional stimuli destined to incite the motor acts of speech would pass off from a part of the brain which was in intimate functional relationship with many other regions of the hemisphere.

"There must be some one spot from which such motor stimuli habitually pass over."

After quoting Dr. Broadbent as to the anatomical relations of the third frontal convolution, he proceeds:

"The third frontal therefore seems to be a convolution having those complicated relations with other parts of the brain which we should have a right to expect would exist in the event of its being immediately concerned in the determination of the motor acts of speech."

The question now arises: How is it that aphasia, or more correctly, disturbances of the function of articulate language, are not so frequently met with when the third frontal convolution on the *right* side is attacked? Must we conclude that this convolution on the right side has functions distinct from that of language? or must we consider the fact of our more frequent use of the right hand, and therefore of the left side of the brain, as rendering that side of the brain more adapted for all sorts of work. M. Broca holds this opinion, and asserts in proof thereof, that the left side of the brain is heavier than the right.

Dr. Bastian, treating of this question says:

"The functions of the two convolutions are doubtless the same, and in all probability they habitually act in unison by means of their commisural connections (through the corpus callosum), the only difference being that the outgoing or volitional motor stimulus for speech seems, in right-handed individuals at least, to pass off from the convolution on the left side."

Dr. Ferrier,[2] arguing from his experiments, writes as follows:

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[1] 'Paralysis from Brain Disease,' p. 207.

[2] 'West Riding Lunatic Asylum Medical Reports,' vol. iii, p. 76.

"From the experiments related, there is every reason to conclude that the two hemispheres are symmetrical. If this be so, it would be unphilosophical to say that the third left frontal convolution is the seat of the faculty of articulate speech. I agree with those who regard the hemispheres as of unequal preponderance in the initiation of voluntary movements. Just as more people are 'right-handed,' and therefore 'left brained,' so it may be argued, the left hemisphere is the driving side. The occurrence of aphasia with lesion of the left lower frontal convolutions is to be explained by anatomical reasons, for we find in the lower frontal regions centres which regulate the opening and closing of the mouth, the tongue, and the accessory muscles on both sides. Physiologically it may be explained by the fact that in most persons the left hemisphere is the one which is usually selected, or selects itself, in the performance of voluntary acts."

Dr. Hughlings Jackson enunciates the hypothesis that the left side of the brain is the leading side for words, while the right is the automatic side. At present, however, this is not borne out by clinical experience.

With regard to localisation of lesions in the corpora striata, there seems no doubt that these ganglia are in connection with, or have to do with movement. Up to the present time, however, the recorded facts do not permit us to differentiate with accuracy between lesions of these centres and lesions of the thalami optici. Nor do the results obtained by Ferrier and Nothnagel, in experiments on animals, find their parallels in human pathology.

Dr. Bastian is of opinion that there is more impairment of sensibility in lesions of the optic thalami than in lesions of the corpora striata, but does not consider that our present state of knowledge leads to any certain conclusion on which to base a diagnosis of injury in the corpus striatum. The thalami optici, although beset with much the same difficulties as regards the recognition of disease localised in them, have had more light thrown upon their functions than have the corpora striata.

Among those who have worked at this part of our subject are Nothnagel, Meynert, Ferrier, and Crichton Browne.

The views of Nothnagel as drawn from experimental research on rabbits, with regard to the function of the optic thalami, are of a negative character only, they are: 1st. The course of conduction of voluntary motor nerve power has nothing to do with the optic thalami. The destruction of these does not cause voluntary motor paralysis. 2nd. The course pursued by sensorial impressions to their centre of perception has very little, if any, connection with the optic thalami.

Meynert, arguing from anatomical data, says:—

"The optic thalami are the centres of unconscious reflex movements, which are produced in consequence of peripheric irritation. But the results of these irritations are also elaborated by the optic thalami, and by means of a centripetal course of fibres are transmitted to the cortex of the cerebrum, where they are fixed on the nerve cells under the form of images of produced movements. These images henceforth are the starting points of voluntary movements. So that if an animal could be born without, or deprived at birth, of his optic thalami he would for ever be incapable of voluntary movement."

Dr. Ferrier[1] says, "from the absolute conformity of negative results obtained by stimulation of the optic thalamus, I conclude that this part has no motor significance." Nor can it be stated that the irritation caused pain, for the animals never gave any outward manifestation of such sensations.

Dr. Ferrier explains Meynert's views by saying, that the paralysis of motion, produced in discharging lesions of these bodies, is due to the implication of the motor strand of the cerebral projection system passing downward from the corpora striata, and through and underneath the optic thalami to the crura cerebri.

Dr. Crichton Browne,[2] in an elaborate article, discusses the results he has obtained from disease of these ganglia, and concludes that their function is either that of inhibition on the reflex centres below them, or that between them and the spinal reflex centres there is a special inhibition centre. His reasons for this inference are, that in cases where one optic thalamus is entirely destroyed, there is complete loss of reflex movement on the opposite side to the lesion, and that when it is only partially affected the reflex movement of the subtending side is only partially affected. From these facts he argues that the lesion, whatever it is, either by the irritation it causes, stimulates the ganglion to excessive inhibitory action, or, by depriving it altogether of its function, allows some inhibitory centre below it, which in health is itself inhibited by the optic thalamus, to exercise an almost unlimited inhibitory action on the spinal reflex centres.

Be it as it may, the opinions arrived at by nearly all who have studied this point admit of the assumption that the function of the optic thalami is to a considerable extent that of presiding over reflex action.

We are therefore justified in asserting that, in cases where one side has lost its power of reflex action, the thalamus opticus on the reverse side is the seat of some lesion. The nature, however, of that lesion is not so clear, as we cannot during life, in the present

[1] 'West Riding Lunatic Asylum Reports,' vol. iii, p. 63.
[2] Ibid., vol. v.

state of knowledge, decide whether its nature is destructive or irritative.

The corpora quadrigemina would seem, according to the anatomical descriptions of the course of their fibres by M. Vulpian, to be the point from which the optic nerves take their origin.

M. Flourens experimentally found that sight was weakened when these were destroyed. According to Adamück, the movements of the eyes are superintended by these bodies. Dr. Ferrier, by exciting them, has produced dilatation of the pupil, but the most marked effect produced by this stimulation was opisthotonos, and this of a very sudden and severe kind.

Dr. Bastian has observed, in a case where there was softening of the two anterior tubercles, that the more marked symptom was polyuria.

It appears then safe to conclude from the above that lesions of these bodies may be set down as causing defects both of vision and of the co-ordinated movements of the eyes.

In order to learn the symptoms of disease in the pons Varolii, or in the crura cerebri, we are compelled to rely on clinical data; these portions of the brain being beyond the reach of experiments of any other kind than those made by disease.

"When a central lesion of the pons is slight in extent the patient after a time recovers from the first shock of the injury, and consciousness is gradually regained. We find, however, a generalised paralysis more or less equally distributed over the two sides of the body; sensibility also may be very notably diminished or perverted in one or more of the limbs. When, in addition to such signs, there is well-marked but irregular paralysis about the face involving eyelids, mouth, and tongue—and when there is also a difficulty in deglutition, associated with a decided impediment in articulation or actual speechlessness, not of the aphasia kind—we may be pretty sure that we have to do with a lesion involving the central parts of the pons Varolii."

Dr. Bastian, in his work on 'Paralysis from Brain Disease,' thus speaks of the symptoms produced by lesion of the pons Varolii:

If the injury is not exactly central, we have what is called alternate hemiplegia. But if the lesion is in the lower part of the lateral region, the paralysis of the face is on the same side as the lesion; this not being the case when the lesion is in the upper lateral half. The reason of the difference being that in the latter case the seventh nerve is affected before the decussation of its fibres in the pons.

Dr. Bastian's views with regard to the crura cerebri may be briefly expressed as follows:—In consequence of the close relation of the upper or cerebral surface of each crus to the optic thalamus, it is not

easy to distinguish lesion of this portion from lesion of the centre. If, however, the lesion be situated on the lower or basal surface of the crus, the peculiar symptoms are more marked. The most distinguishing symptom is the complete paralysis of all parts supplied by the third nerve. There is, of course, accompanying this a hemiplegic condition.

Having thus shortly reviewed the functions of some of the most important parts of the brain, with the view of aiding in determining the localisation of lesions, we must now take cognisance of another obstacle which presents itself to the inquiry in a case of hemiplegia. It is the fact of the decussation in the anterior pyramids. As before observed, until very lately all cases of hemiplegia have been referred for their cause to lesions of the opposite side of the brain. Instances contravening this view have either been disregarded or disbelieved. Henceforth, however, this summary mode of procedure can no longer be adopted. Within the last two or three years, doubts have sprung up in the minds of men whose opinions carry weight, and these doubts have been strengthened by reliable records of cases; and lastly, in the present year, Dr. Brown-Séquard[1] has expressed his opinion that there are and have been numerous cases of hemiplegia in which the lesion has been on the same side as the paralysis. How this fact may be explained yet remains to be seen. It is an open question whether, after all, we shall find that Dr. Brown-Séquard is wrong in his conclusions, and that in the cases he has adduced there must have been some overlooked lesion on the side where, according to received views, it ought to have been; or whether the correctness of the inferences that he has drawn will be proved by additional anatomical discoveries.

Little as is at present accurately known with regard to brain function, there is no doubt that during the last few years vast strides have been made towards a more perfect knowledge of the subject; and we are fully justified in looking forward to a still more complete interpretation of the problems belonging to it, as a result of the precise and minute examinations of the brain, both in health and in disease, now made by nnmerous most competent observers.

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[1] 'Lancet,' January 1, 1876, *et seq.*

III.—Nurses and Nursing.[1]

THE acquisition of several books and brochures on nursing suggests the expediency of dealing with them collectively, and thereby to display the present state of opinion respecting sick nursing and the position and qualifications of sick nurses.

To Miss Nightingale must be assigned the merit of having given nursing its form and substance. Prior to her own bright example as a nurse, and to the appearance of her 'Notes on Nursing,' the intelligent tending of the sick was a matter that had very imperfectly taken hold upon the public mind and was as imperfectly appreciated. The methodical attendance upon the sick, under the influence of Christian dictates and personal piety, was first exemplified in the institution of the Catholic order of Sisters of Charity, and the example of the devotion of the 'Sisters,' and the recognition of the value of their services to the sick and suffering, aroused at length the Protestant populations to the duty of imitating it, and of establishing an organised plan for training women for the work of nurses. Doubtless, the need of trained intelligent women as nurses had long impressed the minds of many, both lay and professional, with the necessity of properly educating nurses for their work; but the credit of giving such impressions form and substance is due to the Pastor Fliedner, who commenced a training school for female nurses, in 1836, at Kaiserwerth, near Dusseldorf, and thus identified the obscure German village with the development of a philanthropic movement which ranks high among its like in this age so fertile in works of humanity and philanthropy.

In England, as in other nations advanced in civilisation, the public mind was prepared at once to receive the lesson presented. On the

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[1] 1. *Lectures on Nursing.* By WILLIAM ROBERT SMITH, Resident Surgeon, Royal Hants County Hospital. London, 1875.

2. *A Manual for Hospital Nurses and others engaged in attending on the Sick.* By EDWARD J. DOMVILLE, L.R.C.P., M.R.C.S. Eng. London, 1872.

3. *Handbook for Nurses for the Sick.* By ZEPHERINA P. VEITCH, late Head Surgical Sister of King's College Hospital. London, 1876.

4. *Notes on Surgical Nursing;* being a short course of Lectures. By J. H. BARNES, Surgeon to the Liverpool Workhouse Hospital.

5. *Remarks on the Training of Nurses.* Read originally as a Report before the American Medical Association. By S. D. GROSS, M.D., LL.D. Philadelphia.

6. *Volunteer Hospital Nursing* (a Paper read before the Social Science Association). By ELIZABETH GARRETT, L.S.A.

7. *National Association for Providing Trained Nurses for the Sick Poor.* Report of the Sub-Committee of Reference and Enquiry. 1875.

8. *Thoughts on Nursing.* A Paper by EDWARD H. SIEVEKING, M.D., F.R.C.P.

9. *Reports of the Committee of the Order of St. John of Jerusalem.*

part of many worthy individuals there were longings and strivings after improved nursing; and medical men especially had constantly to deplore the want of assistance in the sick chamber to carry out the treatment and management they prescribed; consequently not many years elapsed before the admirable example set by Pastor Fliedner was followed in England, as well as in various parts of Germany; the first training school for nurses founded in this country being that of St. John's House and sisterhood in London. Other institutions devised for the same purpose quickly followed; some modelled upon the like basis of a Sisterhood with religious sanctions, and others on the broader principles of association, excluding religious bands. But it is needless to follow up the history of the rise and progress of such institutions; for whatever good work they had accomplished, or were in course of achieving, at the time of the Crimean war, it required that war, with its hospital incidents and experience, to give the final impulse, whereby the hitherto immature and somewhat incoherent endeavours made should become effective in educing and giving consistency to the right principles and practice of sick nursing; in convincing the public of the necessity for the training of those who undertake the duty, and in placing the position of nurse upon a much higher platform than it had ever before enjoyed.

Probably in no subject affecting their interests and welfare have the public become so readily and speedily instructed as in that of the requirements of nursing and of nurses. The history of the Crimean hospitals and the brilliant example of Miss Nightingale and her noble band of Christian women, produced at once a vivid perception throughout the civilised world of what could be effected by constant, and intelligent, and sympathising guardianship and tending of the sick; and when the 'Notes on Nursing' appeared, the little book was everywhere received as an oracle, and its aphoristic teachings lovingly followed.

Anteriorly to the publication of that small treatise no attempt of importance had been made towards imparting instruction to those occupied in nursing, with one exception, however; namely, with regard to attendants upon the insane. For, as Englishmen, we may be proud of the teachings of Dr. Conolly in all that relates to the care and management of lunatics; teachings pervaded by the best principles of humanity and a clear insight of the character and wants of the insane, and such as must ever continue to be valued and acted upon.

To proceed. If no adequate attempt to instruct sick nurses had been made before Miss Nightingale's book appeared, nothing since has been published calculated to replace that book as a manual for nurses. The treatises and brochures before us simply develope certain of its lessons and add to details; or, taking cue from prin-

ciples laid down, furnish supplementary matter and enlarge on collateral subjects of information.

Miss Veitch furnishes a capital little handbook, restricted in the compass of its teachings, but replete with useful hints, of primary importance to nurses, and bearing the impress of practical observation and experience, and we are glad to find a second edition of it called for. Mr. Barnes, surgeon of the large workhouse hospital at Liverpool, in his 'Notes on Surgical Nursing,' has, from the medical point of view, contributed most valuable information for the guidance of nurses. Besides an introductory chapter there are four other chapters, severally devoted to "observation," "general duties," "injuries and accidents," and "fractures and operations."

The 'Manual for Hospital Nurses' by Mr. Domville takes a general survey of the position and duties of nurses, laying down concise rules and directions for their guidance in most of the details incident on attendance upon those labouring under sickness or submitted to operations. It is precisely what it pretends to be—a manual of lessons that every nurse should be well up in; set forth in as few words as practicable, unencumbered by extraneous matter and by tiresome talk about the weight and dignity of nurses, their duties, cares, and responsibilities.

The reader of this little volume will be sensible how much Mr. Domville owes to the teaching of Miss Nightingale both in manner and matter; at the same time he will be ready to admit the fitness of the disciple for reconveying the doctrines of his teacher, and for extending and applying them.

The next book before us, and the most recent, is that of Mr. W. R. Smith. It consists of a dozen lectures addressed professedly to the nursing staff of the Hants County Hospital. In extent it is the largest and at the same time the most pretentious of the number. We do not happen to know anything of Mr. Smith; but, as he attaches no medical qualification after his name, and only returns himself as "resident surgeon" of the hospital, we infer that he is a young man qualifying himself more thoroughly by prolonged hospital work for practice. The inference is strengthened by the fact stated in the preface—that the lectures were originally prepared for delivery at the Huddersfield Infirmary, where he presumably held a similar office to that he now occupies.

An examination of the volume is farther confirmatory of our opinion. It is ambitious in character, and neither in matter nor manner exhibits characteristics of the experienced practitioner or mature thinker. But what is worse, it is remarkable for plagiarism. The author has written with the manual of Mr. Domville before him; has copied the division of subjects and a multitude of paragraphs and of detached precepts,—at times verbally transformed more or less, without the smallest acknowledgment and without

reference to their source. We cannot encumber our pages with parallel quotations in evidence of this charge, which can be substantiated by any one, in a few minutes, who will be at the pains to compare the instructions given by the two writers in what relates to the actual duties of nurses. Likewise when we look to the contents of the book, other than the information more concisely and pertinently conveyed by Mr. Domville, and what may be collected from approved surgical treatises respecting dressings and kindred matters, we come upon a vast amount of detail only indirectly connected with the author's subject. He enters largely on anatomy, descriptive and microscopic, and must have rather confused than instructed his hearers with the anatomy and physiology of the valves of the heart, and of the sounds produced by their action.

He refers to Lister's mode of dressing, but the account he furnishes gives us the impression he knows practically nothing about it.

It is, however, quite needless to minutely criticise this volume: its defects are patent, but as a compilation from reliable authorities upon nursing and on the branches of medical knowledge discussed in its pages, it necessarily contains much useful information. But we would advise Mr. Smith rather to pursue for awhile his course as a learner than be ambitious to become a teacher.

The pamphlets remaining for notice require few words. The brochure of the highly esteemed American surgeon, Dr. Gross, of Philadelphia, was a 'Report of the Committee on the Training of Nurses, made to the American Medical Association.' The necessity and value of training and the wretched state of nursing in many institutions in the United States constitute the subject-matter of the report. The following paragraph gives a sad picture of nursing in some places. After noting the want of knowledge and the unfitness of those called on to nurse in private life, he goes on to say: "In public institutions the same ignorance prevails, superadded, not unfrequently, to the basest moral delinquencies, as intemperance, indifference to duty, and positive disregard of the orders of the medical attendant. Male nurses are everywhere notoriously bad and incompetent. Few, even in our large towns and cities, are qualified for their business. Drunkenness and male nursing are almost synonymous terms in the experience of the American physician" (p. 5).

Miss Garrett's essay was read before a meeting of the Social Science Association, and was directed against the encouragement of lady volunteer nursing; the employment of women drawn from the lower middle-class, and adequately remunerated for their services, being preferred. To the general adoption of this lady's views Dr. Sieveking appears as an opponent in his 'Thoughts on Nursing,' read at the annual general meeting of "the order of St. John of Jerusalem."

There remain for examination the reports of the "order" just named and of the "National Association for Providing Trained Nurses for the Sick Poor." These pamphlets are largely concerned with schemes for district work, and the opinions on nursing advanced in them will call for reference hereafter; at present we will only note that in the latter report there is a very important appendix of Suggestions on Nursing, and on training nurses, from the experienced pen of Miss Nightingale, which will command special attention on all hands, and be viewed with something like reverence even by those who cannot entirely endorse them.

Having thus far indicated the character of the materials before us, we will venture on an examination of the opinions which now prevail respecting the duties, qualifications, and training of sick nurses. To form a right opinion respecting these matters it is necessary to possess a distinct notion of what sick-nursing is.

Now, it may be asserted of nursing generally that it is an integral part of medical treatment; and as essential to efficient and successful treatment as the therapeutical agents prescribed by the medical man. Indeed, without proper nursing the value of the best and surest remedies may be imperilled or destroyed, and a curable case be converted into an incurable or fatal one. Nay, further, the natural history of some maladies shows that attentive, discreet nursing comprehends wellnigh all the measures called for in their treatment. At the present day these remarks will be accepted without controversy. Every one who reflects for himself, and has any acquaintance with sickness and its treatment, is prepared to admit their truth.

But although nursing be a portion of treatment, it is that division of it which usage and the necessities and convenience of the case have taken out of the hands of medical men; of those charged with the responsibilities of diagnosis, of continuous medical observation and instructed inference as to the course and phases of disease and of the application of the requisite remedial measures. The medical man cannot efficiently and appropriately play the part both of doctor and nurse. It is neither good for his patient nor for himself, either medically or socially, that he should undertake the double duty.

True it is that the duties of doctor and of nurse mutually overlap; and that a rigid line cannot be drawn between those which rightly devolve on the former and those which more seemly attach to the latter. The medical man always has the advantage of a much wider acquaintance with disease and its incidents, and with the measures requisite for its relief and useful for the solace of the sufferer, and consequently may be generally assumed to be a more competent attendant in sickness than a nurse. On this ground it is for him to fix for himself the point at which his personal attend-

ance and aid to the patient shall cease, and at which the assistance of the nurse shall be called for, and in every case the doctor should be the absolute director and referee in all the details of nursing the sick.

As a matter of course, the extent of the medical man's action, in the department of treatment usually relegated to nurses, will be governed by many considerations, arising out of personal relations, the nature of the malady or injury and the social condition of the patient, and other surrounding circumstances; and he must be allowed to fix his own limits. His judgment and discretion must be, charitably at least, presumed to be equal to his knowledge and skill; although we are free to confess that some members of the profession are too officious and fussy, and worry both patients and nurses. This, however, applies only to individuals and does not affect the broad lines of action to be pursued.

It seems pretty well settled that women make the best nurses for the sick, and the present tendency is to hand over to the female sex the whole domain of nursing. It is even proposed to replace male attendants on male lunatics by females. In selected cases this course may be practicable and even advantageous, but we are not at all prepared to endorse its utility for the greater part. The conditions of the patients are so unlike those of persons with bodily sickness only, that the arguments for female nursing cannot be justly applied; whilst, on the other hand, there are numerous objections against the employment of women in the capacity of attendants on insane men. This question, however, is connected with a side issue with which we have at present nothing to do.

If by general consent women be recognised as the most fitting agents for the work of nursing, it does not follow as a necessary corollary that all women are fit instruments. As in all other occupations so in this—and, indeed, from a higher necessity by reason of its many peculiarities—there must be a talent for it. There must be a special aptitude for it, a proper combination of moral and mental qualities, and therewith besides a healthy body. Philanthropy, sympathy with the distressed, Christian love, innate impulse to be useful, and all like motives are very good appurtenances to a would-be nurse, but not one half of the possessions she needs to make her successful in her calling. Contact with the actual duties will often cause these excellent motives to droop and to perish as useful incitants.

Miss Veitch very pertinently writes:

"To ladies who intend to make nursing a profession, I would say—Do not undertake the work with any romantic ideas of being a 'ministering angel,' moving about your wards in a very becoming hospital dress, and followed wherever you go by loving looks, and murmured blessings, from grateful patients, or you will never have

courage to face the reality of finding yourself always a hard-worked, often a weary, worn, and sorely harassed woman."

This advice and warning is most true, and often necessary. There are many probationers but much fewer nurses; a result due both to the influences intimated in Miss Veitch's remarks, viz., a romantic adoption of the office of nurse, and to the absence of the requisite innate aptitude for the duties entered upon.

Several of the essayists under notice have well described the general qualifications necessary to a nurse, insisting on kindness, forbearance, discretion, sympathy, power of observation, and ability to record what is observed, readiness in difficulties, and the possession of manual dexterity.

Of the necessity of such qualifications there cannot be two opinions. They should be natural gifts, but they should also be cultivated by training. Then alone will they qualify their possessor for the duties of nursing, and for the learning and right performance of those many other offices which experience must teach. There are, in fact, two departments of the nurse's art, one consisting in the general duties of watching and tending the sick, not calling for any distinctly medical knowledge; the other involving more or less acquaintance with disease and with various means and appliances for its relief. The two departments merge the one into the other, and the second into that of the medical man. Watching, the administration of food and medicines according to instructions given, cooking and preparing certain articles of invalid diet; washing and changing the patient, bed-making, poulticing, fomenting and some other simple therapeutical expedients, are duties and operations that fall within the compass of an intelligent woman, of some experience with the sick, acting under the immediate control of a medical man. Observing and recording the phenomena of disease may likewise be undertaken by such a woman if instructed what to observe. On the other hand, the duties and operations ranging under the second division of a nurse's labours require distinct and special training and instruction. Such are, the dressing of wounds, bandaging, the application of splints for fractures, the use of the thermometer, of the catheter and tourniquet; and it is with reference to this class of operations that the question arises as to the extent to which they should be entrusted to the nurse for their performance. Moreover, on the determination of this question largely hangs the solution of another; namely, the extent of special medical training and education to be given to nurses.

There is no escape from the fact that, at the present day, nurses do "magnify their office," and that they do make inroads upon what has heretofore been considered the peculiar province of pro-

fessional men. The strong current of popular sentiment during the last twenty years in favour of nursing has, by its impulse, driven its practitioners beyond all former boundaries, within which their capabilities and functions had been previously restricted, and has fostered aspirations after further fields of medical usefulness and concomitant importance attaching thereto. It rests now chiefly with the members of the medical profession to determine the boundary line beyond which the education of nurses in medicine and surgery, and their practice of those arts, cannot be advantageously extended. The question is not one of ability of women to acquire the necessary knowledge and to usefully put such knowlege into practice. Such ability cannot be gainsaid But the problem is whether it will be for the public good to further advance the medical education of women, and in fact, create a lower grade of medical practitioners—for medical practitioners a considerable number of them would inevitably become;—and whether, in connection with private and with hospital nursing, such extended medical training be wanted and be useful.

It is our opinion that the boundary line, beyond which their medical education should not be pushed, has been already reached, if, in some instances, not actually overpassed.

It cannot be denied that, *cæteris paribus*, the more medical knowledge an individual possesses the better attendant upon the sick should he or she be; consequently, as remarked previously, a discreet, well-informed medical practitioner would make the best nurse did not professional considerations, usage, and various so-called matters of propriety, render his acting in that capacity at large undesirable. Nevertheless nursing is an integral part of treatment, and a prudent medical man will retain in his own hands more or less of it according to circumstances; much more, indeed, when in charge of private cases than in hospital practice. In private practice he almost invariably leaves what we have classed as the general or non-medical department of nursing to the hired nurse, acting under his instructions and observations: but, at the same time, he reserves to himself the medical and surgical duties, consisting in the use of instruments, in dressings and bandaging, &c., and the investigation of symptoms and variations of diseased action; and we may accept it as a legitimate inference, that when duties of this nature are devolved upon nurses, it is generally because the physician or surgeon wants time and opportunity to personally undertake them. In other words, the allotting those duties to nurses is an admission of deficiency of medical aid. Now such deficiency is inevitable at times in private practice, particularly by reason of the distance of patients' homes from the residence of their medical attendant; but it ought to be quite the exception in hospitals and infirmaries. In these institutions there ought to be

found resident medical officers and pupils; and to the latter it is of the greatest importance that a wide field of experience should be supplied in all those lesser details of the medical art to which we are referring. If those practical details are to be handed over to the trained ward nurses for performance, where is the training of our young surgeons and physicians to be obtained?

Unhappily, it must be admitted, much more work, especially surgical work, is required from the nurses in many provincial hospitals than should be, by reason of the absurd and mischievous regulations of the examining bodies, which tend to centralise all education and training in the hospitals of the metropolis and of a few of the largest towns of the country, and thereby to denude of pupils by far the larger number of provincial infirmaries. This state of things is to be deprecated on all sides as injurious pre-eminently to the profession, and in a lesser degree likewise to the public; were it remedied the demand for highly instructed medical nurses would be lessened as well in hospitals as in private practice; because the existence of instructed pupils in every provincial hospital would furnish practitioners with useful agents for the carrying out of the minor operations of surgery, or assisting them therein, in cases where their own agency was impracticable or needed supplementing.

The foregoing considerations lead us to this general conclusion: that where there is a sufficient supply of professional aid there is no legitimate demand for nurses elaborately instructed in medical and surgical practice; but that inasmuch as professional assistance is often deficient or unattainable, the employment of such nurses may under these conditions be advantageously resorted to. But granting that nurses medically trained are at times called for, the problem remains, what shall be the extent of medical education given them?

At this stage of our remarks it will be well to turn to the pamphlets and books before us to see what qualifications are pronounced to be requisite for nurses, and what medical education shall be required of them.

Dr. Gross summarises the whole under the five following heads:

"1st. To do justice to her vocation, or to perform her labour with alacrity and efficiency, a nurse must be of sound constitution, of good muscular strength, and of great powers of endurance, capable of bearing up manfully under fatigue and loss of sleep.

"2ndly. Her age, at the commencement of her career, should not be under twenty-two or over thirty-five.

"3rdly. She should possess a certain amount of common education; at all events she should be able to read and write, be of a gentle and refined disposition, courageous, patient, temperate, punctual,

cheerful, discreet, honest, sympathising, and ever ready and willing to perform every service, however menial or disagreeable. Her moral character should be of the purest kind, and she should be willing to devote herself unreservedly to the duties of her vocation.

"4thly. She should possess the faculty of observation, in order that she may be able to notice with advantage the character of the secretions and excretions, and the changes in the skin, pulse, countenance, eyes, tongue, mind, and temperature of the patient.

"5thly. Her education should embrace a knowledge, 1st, of the principles of hygiene, especially of ventilation, clean linen, temperature, and the nature and use of disinfectants; 2ndly, of the methods of preparing food and drinks; 3rdly, of the administration of medicines and of the doses of the more common articles of the materia medica; 4thly, of the application of leeches, blisters, bandages, and other dressings, as cataplasms, unguents, and lotions; and, 5thly, of making up beds, changing sheets, and handling patients exhausted by disease and injury."

This list of qualifications is sufficiently comprehensive and complete, and collation with it of the requirements enumerated by the other writers before us exhibits no material difference, unless it be in respect of the educational qualifications mentioned in the last paragraph. Excepting in Miss Nightingale's paper, the educational requirements are set forth in general terms. Mr. Barnes writes: the nurse

"Should also have a broad idea of what the patient is, or anatomy; of how the body grows and is sustained in life, or physiology; of the nature of food and sanitary conditions, or hygiene; of the laws of relationship between similar atoms, or chemistry. . . . . Amongst the arts subservient to the perfection of nursing may be named the art of cooking; so much of the art of surgery as is involved in the adaptation of appliances, bandaging, and dressing wounds; the art of mechanics as far as will enable a nurse to meet any sudden, unexpected difficulty, and render her particularly useful in small emergencies."

In harmony with these views, Mr. Barnes instructs his pupils—the workhouse infirmary nurses—in some rudimentary symptomatology, particularly of eruptive fevers, in the general composition of the atmosphere, and in the leading principles of ventilation, in the rules of diet, in the plan of resuscitating patients from drowning and from the effects of anæsthetics, in the leading varieties of wounds, in applying ligatures and sutures, in the course of the great blood-vessels, the varieties of hæmorrhage, and the use of the tourniquet, in the application of splints and bandages, in the use of the thermometer, &c.

The propositions of the committee of the order of St. John assume a somewhat more defined character. They recommend

"Systematic instruction for *not less than six months in an hospital;*

after preliminary education or industrial training should be made imperative for the nursing profession. Instruction by private practitioners, though to a small extent allowable and available, should not be permitted to supersede the period of hospital instruction. It is very desirable that there should be a scientific school of nursing, as at St. Thomas's Hospital, where nurses, pupils, and probationers may receive, at stated periods, medical instruction (including the elements of physiology and hygiene), and be periodically examined by proper authorities as to progress, &c. The examiners should be authorised to grant certificates of competency or efficiency *after* a sufficient course of instruction and a satisfactory examination."—(Report, 1873, p. 8.)

We cannot cite Mr. W. R. Smith as an authority in this matter of the width of medical instruction needed, although his 'Lectures on Nursing' may be taken to indicate the direction and force of the current of public opinion with regard to the matter. In our apprehension he has gone beyond the necessary limits, and ventured within the range of instruction more especially fitted for students in medicine. For, not content with noting the principal arteries liable to injury, he has discoursed on the structure and physiology of the heart and its valves; on the minute structure and chemical composition of bone; and on the skeleton, noting the distinctive characters of the several vertebræ of the spinal column. But we need no longer delay to mark his estimate of the education to be required of nurses, for there remain for consideration the definite and decided principles laid down by Miss Nightingale, which challenge careful examination. This able and experienced authority indicates in her "suggestions" two classes of trained nurses; the first engaged in training others, and the second those trained for immediate service. The rules we quote first apply especially if not exclusively to the former class. They are followed in the training school for nurses at St. Thomas's Hospital, "under the Nightingale Fund." The ward "sister" is the recognised trainer of the probationers in her ward, and is herself under the direction of a matron "who superintends the training of the probationers, in addition to her other duties, for which the 'Fund' pays her a salary irrespective of her salary as matron of St. Thomas's Hospital."

Moreover several of the professors attached to the school, "voluntarily and without remuneration, give lectures to the probationers on subjects connected with their special duties, such as elementary instruction in chemistry, with reference to air, food, &c.; physiology, with reference to a knowledge of the leading functions of the body, and general instruction on medical and surgical topics."

Further, besides lecturers there is a specially appointed 'Medical Instructor,' paid by the 'Fund,' whose business it is to instruct the probationers in the various duties of a medical and surgical character, other than those to be taught by ward training properly so-called.

Such being the educational apparatus, let us now quote the directions as to the instruction to be acquired:

"*To train* to *train* needs a system—a systematic course of reading laid down by the medical instructor, hours of study (say two afternoons a week), regular examinations by him, themselves cultivating their own powers of expression in answering him. Those who have to train others are the future leaders; and this must be borne in mind during their year's training. (Our period of training is one year for a nurse, but we should much prefer giving two years to train those who have to train others in their turn.)

"Careful notes of lectures, careful notes of typecases, and of cases interesting from being not types but unusual, must be kept by them; their powers of observation must be improved in every way. To illustrate the cases they are nursing in the wards, descriptions of these cases must be pointed out to them at the time in the books in their library.

"They must be encouraged to jot down afterwards, but while still fresh in the memory, the remarks made by the physicians and surgeons to their students in going their rounds, they must be taught, both by the ward sisters and the medical instructor, to know not only symptoms and what is to be done, but to know the "reason why' of such symptoms, and *why* such and such a thing is done. Else how can they train others to know the 'reason why'? Time must be given them for this, otherwise they are too likely to degenerate into drudgery in the wards.

"They must write out their jottings afterwards in the Home; without some such system, it is but too easy to potter and cobble about the patients for a year without ever learning the reason of what is done, so as to be able to train others.

"They must, of course, be able to read the 'cards' on patients' bed-tickets readily.

"A case paper, with printed heading, such as 'temperature,' 'pulse,' 'respiration,' to be taken morning and evening; 'sleep,' 'nourishment,' urine,' 'stools,' to be noted every twenty-four hours, and other such heads, should be regularly kept by each probationer who is to be a head nurse and future trainer of other; the cases to be thus kept to be selected by the ward sister.

"If the medical instructor has beds, as it is most desirable that he should, in the hospital, it is important that such probationers should pass under him in his ward, late in their year, so that he may check their case-taking at his own beds."

The extent of instruction for probationers is more briefly set forth:

"They are required to keep a diary of their ward work, in which they write day by day an account of their duties. They are also required to record special cases of disease, injury, or operation, with the daily changes in the case, and the daily alterations in the management, such as a nurse requires to know. Besides these books, each probationer keeps notes of the lectures.

"All these records kept by the probationers are carefully examined, and are found to afford important indications of the capabilities of the probationer."

We may remark, in passing, that the line of demarcation between the two classes of nurses in the matter of the extent of medical education is shadowy, when their future career is considered, unless the probationer who is to be developed into a nurse, and the nurse who is to train, can ever after be kept distinct. But how can this end be brought about? For the nurse, on the completion of her training, is open to engagement at any hospital as a ward-nurse; and, as such, it will probably become her duty to train probationers. It is impossible and impolitic to reject a trained nurse duly qualified and ambitious to have charge of a ward. It is neither practicable nor desirable to maintain two orders of nurses in hospitals, so that one only shall be eligible for the post of ward-nurse, and the other be ever debarred from rising to that position. But if the distinction cannot be maintained in practice, there seems no good reason for making it in education. Nor will it be found practicable, on the elevation of a nurse to the charge of a ward, to send her back to a training school for the more advanced technical medical instruction deemed requisite for her new position. On the other hand, we must infer that a nurse must learn in the very discharge of her duties, and so fit herself for a position of greater responsibility; and the conclusion seems to be, that if some further guarantee of fitness be required of a nurse when raised to the position of a ward-nurse and called upon to train others, it must be attained by examination.

Some of our readers may consider Miss Nightingale's estimate of the office of nurse as sufficiently high, if, indeed, not too high, and the curriculum she lays down for the education of nurses as too extended, too ambitious, and too professional; but neither the one nor the other, so far as her writings before us sanction, rises to the level at which others would fix it.

For instance, the sub-committee of the National Association for Providing Trained Nurses for the Sick Poor strikes a higher chord, and, at p. 24 (Report, 1875), thus writes *à propos* of employment for gentlewomen:

"Grave objections are entertained to women entering the medical profession; but who could object to women making *nursing* a profession? It is closely allied with the medical profession, equally honorable; useful, and, if they are properly qualified, perhaps hardly less scientific. Even the improved training for hospital nurses given as the Nightingale School does not supply the comprehensive education and training that would elevate nursing to the rank of a scientific *art* for educated women like the medical profession; for it does not profess to train 'private nurses,' although the training given to its hospital nurses and superintendents is the

only systematic one in England, and is more practical than that given in any hospital abroad. To give nursing the social rank and standing that would make it a profession fit for women of cultivation, a more comprehensive education and training would be necessary; and this would secure to its members the social position and material rewards that belong and are generally given to those who combine a scientific education with a useful calling."[1]

But even this elevated platform is overtopped by that set up by Dr. Howard, professor of the theory and practice of medicine at Montreal, who thus writes:

"Such an art (nursing) would imply, in my view, a liberal preliminary education at least equal to that now required of the medical student, assigning, however, a first place to natural science and a lower one to the classics. And, second, a professional education extending over three full years, and embracing the following scheme of subjects: anatomy, physiology, chemistry, materia medica, pharmacy, dietetics, hygiene, and clinical instruction in nursing the sick and wounded, in dressing wounds, and applying splints, &c." [for which education they would, of course, pay as medical students do]. "Such nurses might receive a diploma, upon examination, entitling them to practise the art of nursing, and to charge fees in rates proportionate to our own. Such a body of trained nurses would supply the greatest want we have as physicians, and would open up a career of usefulness and honorable employment to our sisters, who would then be not only the helpmates but the *complementa* of the medical profession."

These views of the Canadian professor are quoted approvingly by the "Sub-Committee of Reference and Enquiry" of the "National Association for Providing Trained Nurses for the Sick Poor." They may be assumed, therefore, as representing the opinions of that committee, and as supplementary to those presented in the extract from their report above given.

Now, both in the current lay publications of the day and in professional essays and papers the value of trained nurses and the need for their education have been pet subjects. Indeed, they are such as can be written about *currente calamo*, provided various underlying problems, of much importance to the medical profession, aye! and to the public also, are kept out of sight. Some such problems have already been foreshadowed in the preceding pages of this article. They deserve attention; and although their thorough discussion would carry us beyond the limits and purpose of this paper, a few further comments may be allowed; sufficient, we trust, to awaken the attention of our contemporaries of the medical press, with

[1] We may by the way observe that a considerable portion of the report here quoted has appeared verbatim as an original article in a recent number of 'Good Words,' from the pen of Miss Florence Lees, who therefore may be supposed to have dictated much that that report advances in respect of the position and education of nurses.

whom the discussion of such questions is, on the whole, more cognate.

It remains for the profession at large to settle the question whether it requires the services of nurses of advanced medical knowledge such as proposed in the schemes of education we have quoted? If medical men want women to "take" their cases for them, to record the daily features of disease, to reduce fractures and dislocations, to apply bandages and splints, to use the tourniquet and catheter, and to treat wounds of all sorts, it would seem to us that in so doing they desire to abrogate their own functions. It would look as if they regarded the minor details of their profession as unworthy of their individual care, whatever importance they may in words assign to the right observance and attendance on those details. It would imply that medical men assume the care and treatment of a larger number of patients than they can individually and efficiently attend to; or, in other words, that there is a deficiency in the supply of medical aid to meet the public requirements.

Our education and experience lead us to opinions and practice opposite to all these implied conclusions. We have learnt to look and to act for ourselves in all those minor matters. We prefer our own observation of the varying features of disease, of temperature, of the appearance of evacuations and discharges, and of wounds and eruptions, and all such matters. We believe that when we examine into these changes for ourselves that we do so with more edification and information than we can extract from the recorded examination of others. We believe in seeing with our own eyes; and that a nurse, however well trained, cannot see so well for us, because we cannot suppose her to follow in the same groove of thought and inference. On the other hand, the nurse, if trusted to for observation and information, may lead us unintentionally iuto error, for her reports may be blemished by preconceived notions and prejudices. Indeed, it is quite within the range of probability that the highly trained nurse may be a more treacherous observer and reporter than an ordinary discreet woman, by reason of pride of her medical education, but which, by its incompleteness, leads her to erroneous conclusions as to the nature, course, and character of a patient's disease, and colours all her observations. Or, again, on the other hand, the erroneous judgment of the self-confident nurse, particularly if expressed and acted upon, will be a source of annoyance, and vexation, and mistrust between the medical attendant and the patient's friends; for it is impossible, under such circumstances, for the divergence of opinion between those in constant attendance in the same house to be concealed.

These remarks call up before the mind some disadvantages that

may accrue from the employment of the class of nurses in question; disadvantages in reference to the relations existing between doctors and their patients. What these may be will at once occur to every one who reflects on the position a woman, laying claim to medical education, hospital training and experience, can take up in a family, if her folly or vanity tempt her to it. This result may not have presented itself to the minds, or happened in the practice, of the distinguished hospital physicians who have so warmly advocated the extended medical education of nurses, but we cannot shut our eyes to it as both possible and probable in the case of many less conspicuous, though equally able practitioners.

As we indicated some pages back, nurses with extensive medical acquirements are not needed by the private medical practitioner who recognises his responsibility and his duties to his patients, and does not undertake more work than he can accomplish by himself, or with the assistance of his partner or qualified assistant. On the other hand, he may by their employment, as already noted, expose himself to certain annoyances. Further, in hospitals, as before pointed out, the need of such women is not evident, if there be provided, as there should rightly be, a sufficient staff of resident medical officers aided by pupils. It is in workhouse infirmaries alone, where, in our opinion, the class of nurses alluded to can really be required; for in such institutions parsimony brings about medical insufficiency and inefficiency; and, in consequence, it may be legitimate in the absence of sufficient medical aid to supplement it by what is the next best substitute, the employment of medically trained nurses.

In all that we have written we have contended not against the training of nurses, but against an evident disposition on the part of some enthusiasts in the cause to overshoot the mark in respect of the medical education and requirements of sick nurses; we believe that many, if not the majority, of our readers will consider that tendency established by the extracts we have presented in previous pages from the documents before us. But we feel that some few words are yet necessary with regard to the professional status laid claim to on behalf of nurses in the extracts in question.

In the opinion of the sub-committee of the National Association nursing is a profession. Now, no offence need really be taken at the present day, when words formerly signifying social distinction have lost their meaning, by calling nursing a profession; but to say that nursing is to be made "a *scientific* art like the medical profession," and that it is not only closely allied with that profession, but "equally honorable and useful, and, perhaps, hardly less scientific," is an arrogant and exaggerated assertion, and one that would involve, if accepted and acted upon, the subversion of the relations between the medical man and the nurse, and the

elevation of the latter to an unfit position wherein her usefulness would be diminished.

And when we find appended to these advanced notions of the professional status of nurses the elaborate scheme of Dr Howard (of Montreal), to require of them a preliminary education "at least equal to that now required of the medical student," "a professional education extending over three full years," clinical instruction in hospital, and an examination for a diploma, we have in fact displayed before us a project for the conversion of nurses into medical practitioners.

We have here no concern with the question of the desirableness and usefulness of the admission into the profession of women, educated in accordance with the curriculum demanded of all who would enter it, and who are ready to submit to the usual examination for a license to practise. Entrance by such a portal is legitimate and straightforward. But we would earnestly contend against the medical education of nurses according to the plan proposed with the assigned object of giving them a professional status on a level with medical men. To do so would be to introduce by a side wind a new class of practitioners, of middling information and skill, who would become a constant cause of annoyance and embarrassment to the regular practitioner, male or female, and would confer no advantage on the public. We should in England repeat, but in a worse form, the experiment of an inferior grade of medical practitioners, which has in France, in the institution of the class of "*officiers de santé*," been found to operate so prejudicially to the public good.

It may be answered that the women so trained and adorned with diplomas are only to serve as nurses; but what security is there for such limitation of practice? Experience tells of the reputation held for medical skill by discharged hospital porters, and by superannuated hospital nurses of the old pattern, and of the amount of illegal practice carried on by these and other ignorant persons whose only recommendation is, that they have at some period of their existence had something to do with doctors and with drugs. With how much more reason, and valid reason, too, might the professional nurses of the new style lay claim to minister to the ailments of their fellow citizens? It may be said again that such practice is illegal, and therefore would not be carried on. But every day observation shows that the sentiment of illegality offers no bar to the practice of physic where willing clients are to be found. The public at large do not debate the question of legal right to practice; when they need treatment they will accept it from the hands of any who will offer it. Neither are they competent or disposed to judge of qualifications for practice: were they, we should not see around us such countless examples of quackery and knavery.

And when the public are thus minded, laws against illegal practice are impotent and worthless. Consequently it would be equally vain to look for protection from existing laws, or to call for further stringent measures against unqualified practitioners, were the race of semi-qualified medical nurses of the approved pattern called into existence. In fact they might advance plausible claims to be allowed to practise medicine within certain bounds; and we should have such claims entered among the rights of women and become the object of popular agitation.

The principal argument in favour of the medical education of nurses is, the necessity for their knowing "the reason why" in reference to all the details of management and treatment which fall under their cognisance. But this is an argument which may readily be overridden. It must be acknowleged that, in nursing, intelligence and the power of observation are qualities of great price; but at the same time it does not seem to us that it will materially add to the nurse's usefulness, considered as the aid to the doctor in manual duties under his guidance and in observing according to his directions, to give her clinical instruction in the nature and treatment of disease: to call for from her reports of cases, particularly those of an unusual character, and to impose upon her a systematic study of medical books. "The reason why" of half of our actions and of half of the phenomena we see around us cannot be comprehended or told; and yet our actions may be very useful, and our power in regulating and controlling phenomena very advantageous. To endeavour to grasp the reason why of all that we do and observe is to endeavour after universal knowledge; and it is incompatible with practical usefulness and the efficient fulfilment of the ordinary duties of life to delay action until all its antecedents and consequences have been fully inquired into and comprehended, and the more so when such action is performed under the guidance of one acquainted with its application and utility, or otherwise with the sanction of the experience of mankind at large.

To attempt to give nurses instruction as to "the reason why" the surgeon or physician considers this or that expedient necessary would be, in the majority of instances, to inflict a heavy task upon them and to lift them more or less out of their proper sphere, possibly at the risk of withdrawing them from due attention to their less intellectual but equally useful functions. To give them more than an insight into it is to demand for them complete education as medical practitioners and to transform them from nurses into doctors—a consummation assuredly not to be desired.

Here we must leave this discussion. We have entered on it from a persuasion that, in the hands of some parties, the medical education of nurses is pressed on too fast and too far, whether for the good

of nurses themselves, or for the well-being of the medical profession, or for the true interests of the public. There are fashions in opinion as well as in dress, and there are enthusiasts in practical as well as in theoretical matters; and it is from time to time needful to show the folly or distortions of fashion and to put the curb upon enthusiasm.

IV.—Recent Researches on Inflammation and Suppuration.[1]

The literature on the subject of inflammation and suppuration has grown to such an extent within the last few years that a writer has some difficulty in selecting what is reliable and trustworthy. The idea of defining any exact limits to the process, or, in fact, of giving any general conception of its essential nature, seems to be further than ever from our grasp. Nay, the more one compares the healthy process of nutrition and growth with inflammatory conditions, the more one is tempted to ask in what, if in any, respect the inflammatory phenomena differ from those met with in normal nutritive processes, and whether, in reality, the difference be not merely one of degree. If we admit, on the one hand, that a division of pre-existing protoplasmic structures constitutes the essential factor in the production of the newly formed elements, are we also to grant that the division of the primitive mass of protoplasm in the embryo is of an inflammatory nature? Or, if we admit that the exudation of colourless corpuscles from the blood-vessels is a normal process, how can we construct a theory of inflammation having this for its groundwork? We cannot define the process on either basis, for, if it be said that the inflammatory process is an exaggeration of either the one or the other of the above conditions, the question arises, how much cell proliferation or how much exudation constitutes an inflammation, and how much can be said to be normal?

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[1] 1. *Noch Enimal die Keratitis.* J. Cohnheim. 'Virchow's Archiv,' vol. 61, p. 289.

2. *Experimentelle Untersuchungen über die Enstehung der Eiterkörperchen bei der traumatischen Keratitis.* Prof. Arthur Boettcher in Dorpat. 'Virchow's Archiv,' vol. 58, p. 362.

3. *Experimentelle Untersuchungen über die Entzundung der Hornhaut.* Von Prof. Axel Key und Med. Lic. C. Wallis in Stockholm. 'Virchow's Archiv,' vol. 55, p. 296.

4. *Recherches Experimentales sur le passage des Leucocytes à travers les parois vasculaires.* Par M. le Docteur v. Feltz. 'Journal de l'Anatomie et de la Physiologie,' 1870-71, pp. 33 and 505.

5. *Anatomy of the Lymphatic System.* Dr. Klein. (Parts I and II.)

6. *Untersuchungen uber den Eiterungsprocess.* L. Stricker. 'Oesterr. Med. Jahrb.,' heft 3 und 4.

Where does the one process begin and the other end? It, accordingly, seems that the old idea of having one general rule for the inflammation of all organs and tissues must be abandoned, and that one must consider each process on its own individual merits without regard to one theory or another.

It is a matter of some regret that the inflammatory changes have been worked out in so few organs and in such limited spheres. Why is it, for instance, that even yet we know almost nothing of the inflammatory process experimentally demonstrated in the liver, pancreas, spleen, or nervous system? All are equally capable of being acted upon; but, so far as we are aware, few or no investigations of primary importance have been carried out in these organs. That they do not offer such favorable grounds for investigation as the cornea or serous membranes we are not prepared to admit. The changes to be met with in them are, we believe, quite distinct, and would, we feel convinced, well reward careful experimental inquiry. The nervous system seems to us especially to offer some of the most beautiful and extensive fields the for observation of inflammatory manifestations. What is a myelitis, for instance? What appearances characterise it, and how can we distinguish it from the many other affections of the spinal nervous centres? What do we even mean by a hepatitis? It is questionable whether any of our best pathologists at the present day could give us a description of either the one or the other process, notwithstanding that they are of such primary importance. The supposition that the changes which occur in the cornea or mesentery are typical of all inflammatory processes seems to be far too wide for acceptance. Every one who has paid minute attention to experimental evidence on such subjects knows how deceptive such statements are, and how much safer it is to confine oneself to facts as they exist instead of indulging in generalisations which only tend to mislead.

Even at the present day, after so many admirable investigations have been conducted on the subject, the cornea remains one of the unsettled battle-fields. What constitutes keratitis? Is there any one essential process which can be considered typical of its inflammation? We are inclined to believe that there is not, but that the phenomena vary according to circumstances.

Before taking into review the numerous recent observations on the traumatic inflammatory conditions capable of being produced in the cornea, it may be as well to glance at one or two points in regard to its structure, for on this depends greatly the interpretation of the inflammatory appearances. It was not until the publication of Todd and Bowman's valuable contribution to the anatomy of the cornea that its true *laminated* structure was recognised ('The Physiological Anatomy and Physiology of Man,' 1845-7). The subsequent investigations of Henle (Canstatt's 'Jahresbericht' für 1852, Band 1)

confirmed these observations, and maintained that the lamellæ were over three hundred in number. The fibrillated structure of these lamellæ has also been much more fully inquired into of late years by Rollett ('Sitzungsberichte der Wiener Akademie; Band xxiii, 1859), Schweigger Seidel ('Sitzungsberichte der K. Sächsischen Gesellschaft der Wissenschaften,' Math. Phys. classe), and others; so that, with such very reliable authority little or no doubt can exist that the matrix or ground substance of the cornea is composed of a fibrillated structure. These fibrillæ run in decussating straight bands, and can be easily isolated by means of various reagents. A point of greater difficulty is the question whether there really exists a series of *straight* channels between these straight and interlacing fibrillæ. Bowman succeeded in injecting a series of canals of this description with mercury (loc. cit.), and similar canals have been described as existing in fresh corneæ by Schweigger Seidel (loc. cit.) and Boddart (Centralblatt, 1871, No. 22). They can be filled by puncture injection of various substances suspended in oleaginous media, and, under such circumstances, appear as interlacing dark lines running at different angles to one another and on different planes. Whether these are real channels seems to be a disputed point among anatomists, for while the above-mentioned authors believe in their existence in the normal corneæ, Rollet (Stricker's 'Manual of Histology') maintains that they are merely splittings of the fibrillar substance of the matrix. Sometimes they lose their linear character and become more diffuse, but this is not so commonly met with, and only occurs in certain animals.

The well-marked system of plasma canals differs in contour in different species of animals. Within them are contained the fixed cornea corpuscles, whose shape does not vary much in different animals; but, under certain circumstances, their processes may be retracted, so that we see a more or less oval or rounded protoplasmic body filling up the star-shaped plasma space.

Under normal circumstances the corpuscles and their processes probably do not entirely fill up the plasma spaces, and consequently do not correspond to them in contour as was previously held by His ('Beiträge zür normalen ünd pathologischen Histologie der Hornhaüt,' Basel, 1856). Still, in certain instances, they do present very much the same contour, but as the contained corpuscle has the power of contracting under stimulation, it is possible that there may be an unoccupied interval between the branching plasma spaces and the contained cornea corpuscle. It still seems to be a matter of doubt whether these plasma spaces have any proper wall or are merely structureless deficiencies in the matrix. That the wall is not membranous we think will not be disputed, but that an endothelial lining exists within the star-shaped cavities, constituting a wall, we believe to be of much more frequent occurrence than is

usually admitted. Hoyer (Reichert and Dubois Reymond's Archives, 1865, p. 214) has described this system of endothelial plates in the posterior laminæ of the cornea of the kitten. Rollet (loc. cit.) has only found them in young animals. With this, however, we cannot agree; for while we admit that in young animals these endothelial plates are more abundant than in animals of maturer age, still, in well-stained silver preparations of the adult cat, we have seen the most beautiful endothelial tracings running across the star-shaped plasmatic spaces. This endothelial lining, as will be seen from what follows, may be held to be of the utmost importance in the consideration of certain appearances met with in the inflamed cornea. The wandering cells are more or less abundant in different corneas. Where they come from, whether they are merely the so-called fixed corpuscles which have taken on a wandering action, or parts of the same, the result of division, does not seem to be quite settled.

Since the publication of Cohnheim's second work on inflammation ('Neue Untersuchungen über die Entzündung,' Berlin, 1873) in which he still maintained the ideas formerly enunciated by him in regard to the formation of pus-corpuscles, he has again contributed to the subject, more especially in regard to the inflammation of the cornea (loc. cit.). He found, formerly, that in a keratitis produced by drawing a thread through the cornea, the regular star-shaped figures of the cornea corpuscles were always to be found regularly distributed between the pus-corpuscles; what he then found he has frequently since confirmed in the corneæ of frogs and mammalia. In gold-stained corneæ of spring frogs, twenty-four hours after central irritation with nitrate of silver, and from eight, ten, to twelve hours afterwards in rabbits, nothing else is noticed in the zone close to the slough than the elements of the part badly stained and in a state of *vacuolation*. Around the slough is a dark stained line, and, at the outer border of this, begin the regular contours of the so-called cornea corpuscles with their nuclei. They reach uninterruptedly to the periphery, where, in peripheral keratitis, numerous pus-corpuscles are seen lying between them, especially in the anterior lamellæ. As this period closes, however, another commences in which entirely different appearances are found.

The vacuolar figures are paler and with greater difficulty recognised because they are covered over with two kinds of elements; one set is spicular and usually thickly set together at different angles; another is composed of the ordinary pus-corpuscles, rounded or more commonly spindle shaped, only to be distinguished from the spicular bodies by the fact of their containing one or more nuclei which are always wanting in the latter. In frogs the spicular bodies are much more frequent than the pus-corpuscles, but in the rabbit they are not so common, the pus-corpuscles being more

generally met with. From the commencement these are arranged in rows, and it is curious that they often run at first alongside the nerve-fibres. In every case Cohnheim has found the star shape of the corpuscles in all the zones unaltered, although pus-corpuscles may be found lying close by them so thickly as partly to obscure their contours. In construing the history of these appearances, he says that the spindle-shaped bodies and pus-corpuscles clearly, in this case, do not wander in from the vessels of the periphery. He has repeatedly seen that injection of the border of the cornea and peripheral cloudiness often completely fail in keratitis. The explanation of the presence of the pus-corpuscles, in this case, he finds in the fact that the elements of the conjunctival fluid are increased, and that it is these which wander into the cornea through the lesion at the centre. During the first twenty-four hours, the corneal tissue is unbroken, corresponding to the period in which there are few or no pus-corpuscles in the part. They cannot gain admittance until the cauterised portion becomes vacuolated and destroyed at the borders, and the epithelial covering lost. The conjunctival fluid freely bathes the surrounding parts, and, by the action of the eyelids, is driven into the canals in the matrix which become distended and widened. As it mixes with the "Kittsübstanz" the more or less spicular figures are produced.

In the normal conjunctival fluid there are always fat, lymph-cells, and a slight amount of epithelium. In injury of the cornea the cellular elements of the conjunctival fluid are much increased in number, and these also press forward into the distended interfibrillar channels in a linear manner. Immediately after the separation of the slough the new formation of epithelium commences, the entrance of new corpuscles into the substance of the cornea is prevented, and those which have already wandered inwards spread themselves outwards towards the periphery.

The description so far as it goes is almost, if not quite, in coincidence with most of the descriptions by different authors of the appearances found in gold-stained corneæ in which a circumscribed keratitis has been set up. The interpretation of the appearances, however, we are by no means disposed to admit *in toto*. As may be seen, Cohnheim still denies that the fixed cornea corpuscles take any part in the formation of the pus. He admits, however, that they are not unchanged, for while he states that they do not divide to form new elements, he compares the metamorphosis which they undergo to a retrograde condition in which they become vacuolated and break up into small pieces. This process of vacuolation of the cornea corpuscles is a condition much dwelt upon by all pathologists who have worked on the subject of keratitis, and has been more especially considered in the very able paper by

Professor Axel Key and C. Wallis (loc. cit.). They say that after cauterisation of the frog's cornea with silver certain changes resembling the vacuolation of the central slough are seen in its close proximity, and that, although they have been noticed by Stricker and Norris, Cohnheim, and others, they have not met with sufficient consideration from these authors. They would place the limits of this vacuolation at a much greater distance from the side of the slough than Cohnheim seems inclined to do, and indeed there are practically no limits to its extension, as it depends entirely on the degree and extent of the cauterization whether or not it can affect the whole cornea as far as the periphery.

Boettcher (loc. cit.), however, although he grants that a certain amount of vacuolation goes on in the corpuscles nearest the point of irritation, still adheres to the opinion of Stricker and Norris that this is not the case in all instances, but that a true division of the corpuscles in reality occurs in the outer limits of the cornea, and that the divided portions wander in towards the point of irritation, giving rise to the spear-shaped bodies so often met with. He recognises two modes in which the new elements may be formed from the fixed corpuscles. These are either by endogenous development of young cells within the larger protoplasmic masses, which, although not nucleated at first, subsequently become so, or by fissiparous division. This is virtually a mere confirmation of Stricker and Norris' original observations on the subject. We think that, taking all the evidence on the subject into account, there can be little or no doubt that, although a certain amount of vacuolation does go on, many of the corpuscles are destined for a higher sphere of action, and actually give rise to new elements, either by fissiparous division or by endogenous growth, and that these new elements resemble pus-corpuscles in their general outline. Nay, Cohnheim himself would readily grant (loc. cit.) that in certain instances divided true nuclei are seen, even in great numbers, within the cornea corpuscles, but asks what signification this has for the fact of their forming isolated individual pus-corpuscles? He cannot see that this is a formative process, but would look upon it in much the same light as that condition which Fleming has seen in atrophy of fat-cells (Virchow's Archiv, vol. 52, p. 568, and Schülze's Archiv f. Mik. Anatomie, vii, p. 328-371). This seems to us to be going in the face of the most common facts in pathology; namely, that whenever we have active processes of growth going on, the cellular structures are in great number and almost invariably polynucleated. We cannot, moreover, help thinking, after reading this last contribution from Cohnheim, that his whole method of reasoning is somewhat exclusive and far-fetched, and that he is explaining facts on theoretical grounds, instead of using them for the building up of doctrine.

Turning now to a different set of experiments, we find in Stricker's latest observations on the subject of keratitis (loc. cit.) certain very valuable, and apparently not much appreciated facts which have been worked out by him since the publication of his first article in combination with Norris (Stricker's 'Studien,' Vienna, 1870). It seems a somewhat curious circumstance that in the preparation of *inflamed corneæ* gold has been almost invariably used as the staining reagent. The pictures which are obtained in the *inflamed cornea* by the employment of silver are entirely different from those seen when gold is the staining reagent used, and as so much depends on the staining of the cornea for its proper interpretation, we have thought it necessary specially to emphasise this.

Stricker's method of producing the inflammation is to make a small slough in the centre of the cornea with silver in the usual manner, and then, at the different periods required, to stain it with a two per cent. solution of nitrate of silver while the animal is alive, or to rub over the surface simply with the solid nitrate, also while the animal is alive, but, of course, in both instances, narcotised. The latter procedure he regards as preferable to the former. He states that the appearances seen with the silver staining are much more fully brought out when the cornea is stained in the living condition. In from twenty-four to forty-eight hours after irritation of the cornea of a young animal, what appears as a dark granular ring with low powers is seen at some distance from the slough, either entirely or partially encircling it. This ring is composed of pus-corpuscles; is, in fact, a microscopic abscess. The same observer also holds that the fixed corpuscles on each side have their processes retracted and are in a state of division. A thick network composed of the thickened and retracted processes, also in a divided state, is seen on each side of this suppurative area. The divided portions become rounded and nucleated and form pus-corpuscles. The ground substance in this region seems relatively small. Further out from this are found little protoplasmic clumps divided into two. three, or more portions. He regards all these appearances as further evidence of tissue pyogenesis. The facts which he states can be verified, and it will be found that appearances such as he figures are almost invariably produced, in some instances more pronounced than in others.

We are inclined to regard the appearances, however, as quite different from those seen when the cornea is prepared with gold. In the latter case the cornea corpuscle is seen in a state of division within the stellate-shaped plasma canal. When, on the other hand, the cornea is stained with silver, the outline of the plasma canals is brought prominently into view, while the cornea corpuscle, although it can be plainly seen, is not nearly so distinct as in gold preparations. To imagine that the segments seen by this silver

method are the result of division of the fixed corpuscles, one must suppose that these latter entirely fill up the plasma spaces with their body and processes. Although this is to a certain extent true, yet it is not invariably true, the plasma spaces being generally wider than the contained processes of the fixed corpuscle, and not always corresponding to it in form. On the contrary, we have satisfied ourselves that, apart from the appearances dwelt upon by Stricker, the cornea corpuscles can be seen underlying the well-marked outlines, and that they are also divided and many-nucleated, but present an entirely different aspect from that specified by him. The well-known action of silver in staining endothelium is nowhere better marked than in the cornea. In the great majority of animals, both old and young, but especially in young animals, if the cornea be prepared in the manner recommended by Professor Stricker, a well-marked endothelial lining is visible in the stellate spaces and certain of their processes. Indeed, it is a matter of conjecture whether the entire aspect of the plasma canals, as seen in silvered corneæ, may not be due to the presence of an endothelial lining, or, at least, of a condensed cement substance capable of precipitating the silver. Now, what is it that one sees at the side of the "suppurating area" described by Stricker? The processes of the plasma spaces are thickened, and, in their interior, are numbers of more or less rounded or angular corpuscles placed in close juxtaposition, in each of which is seen a granular nucleus. At the same time, however, the normal endothelial outlines are lost, and, in their place, we see an endothelial cell broken up into three, four, or more rounded or angular portions, corresponding to those seen in the area of greatest suppuration. These are distinctly nucleated, in fact, do not only contain one nucleus, but, frequently, two or three; and the explanation one would give of their formation is that they are not the result of division of the fixed corpuscles, but of the endothelium lining the cavities. It is notorious that the appearances are better seen in young than in old animals, and it is precisely in these that the endothelial markings are best observed. To any one who is familiar with the division which takes place in the endothelium of the mesentery seen with the silver method of staining the appearances are especially striking. We believe that what Professor Stricker has described in this paper is an entirely different state of things to that formerly dwelt upon in his first work. The figures which he gives in his latest contribution are perfectly true (being taken from photographs), but, we believe, he has put an interpretation upon them which is untenable. According to this, then, we have another and what seems to us, judging from the analogy of the mesentery and endothelial surfaces generally, very probable source of proliferation and formation of pus. In the one and same cornea it is often quite possible to demonstrate

all the three alleged sources of suppuration :—the spear-shaped bodies more abundant towards the periphery and lying over normal cornea corpuscles; the divided and manynucleated true cornea corpuscles; and, lastly, this other condition described by Stricker and which we look upon as a division of the pre-existing endothelium of the plasma canals. We cannot, therefore, believe that the purulent area in a circumscribed traumatic keratitis is the result of any *one* special process, but rather incline to the idea that it is generally the result of two factors—division of pre-existing elements and infiltration of wandering cells from without. Whether these wandering cells are capable of entering the cornea through the conjunctival fluid or not we will not at present discuss. We would, however, merely suggest that the explanation, in the face of the experimental evidence of Boettcher and others, seems somewhat far-fetched. If wandering cells are capable of finding their way into the centre of the cornea from the periphery, under normal conditions, we cannot see anything very extraordinary in the fact that under conditions of abnormal stimulation they should be found much more abundantly. On the other hand, we cannot grant that they are necessary for a keratitis. We believe, on the contrary, that a keratitis in which abundant pus-corpuscles are produced can take place without either the presence of cells which have wandered in from without, or of those which have been formed by a division of fixed cornea corpuscles, and, in this case, we think that a very probable source of those new elements is to be met with in a division of the lining endothelium of the plasma canals.

By far the most important work which has been done on the serous membranes is that by Dr. Klein in his two most valuable contributions to the anatomy of the lymphatic system (op. cit.) We cannot too highly eulogize the care taken in the preparation of these two volumes, and, to us, it seems that an entirely new era is opened up in the history of inflammation and the allied processes by the author's accurate description of the normal and pathological conditions of the peritoneum and pleura. The unbiassed and truly scientific manner in which Dr. Klein has gone about the investigation gives one confidence in his statements, a matter not so readily accepted in regard to the work of many other *inflammatory pathologists*. The more one peruses these two most excellent volumes, the more one is struck with the great similarity between the appearances which normally exist in the different structures, and the same under inflamed conditions. One is tempted again to ask where do the normal processes end and the abnormal begin? Is the abnormal not, in great part, merely a difference in degree rather than in the development of essentially new features?

There exist in the normal peritoneum of animals areas of

greater or less extent in which the endothelium is in a state of germination, the individual proliferated cells being attached by a stalk-like process or forming giant cells. These germinating endothelial areas are much increased in size and number under acute, but, more especially, under chronic inflammatory conditions, and, curiously enough, they have generally a true stoma in their centre. Besides these areas of germinating endothelium, there exist in the ground substance of the omentum of rabbits numerous lymphangeal structures. These are either made up of patches of more or less flattened and branched cells, or patches and tracts, the matrix of which consists of a reticulum whose meshes contain a variable number of lymphoid corpuscles. The latter are usually provided with an abundant supply of blood-vessels. The blood-vessels here, as in other parts, are enclosed in an invaginated lymphatic vessel, and between the two is seen a reticulum of adenoid tissue. The stomata are of two kinds, *true stomata*, or veritable openings lined with small endothelial cells, and which communicate with an underlying lymphatic vessel, and *pseudostomata*, which are smaller spaces between the endothelial cells closed up by one of the processes of an underlying branched connective-tissue-corpuscle. These openings are most abundant on the diaphragmatic peritoneum in mammalia.

If ammonia, iodine, or toxic fluids are injected into the abdominal cavity of a warm-blooded animal, it is noticed that in twenty-four to forty-eight hours the membranes are very red, the endothelium has been shed in great masses, and there is present a fluid containing an abundance of isolated membranes, consisting of endothelial cells. The individual cells, adherent and cast off, are swollen, and their protoplasm occupied by a greater or less number of variously sized granules. In place of the normal endothelium we find large roundish or oblong drop-like elements, which take the place of the cement substance. The nuclei of the endothelial cells are distinctly divided. Should, however, the inflammation be less, as when caused by threads, entrance of air, injection of a mixture of starch and oil, &c., the endothelial plates are detached here and there, but not so extensively as before. The endothelium also becomes more strongly stained with silver, the individual cells are swollen, and the nucleus is abnormally prominent. *The proliferation is very extensive where germinating areas have previously existed in the healthy state.* The proliferated cells arrange themselves into cords stretching over the surface of the membrane. In chronic peritonitis, as from artificial tuberculisation, the germination of endothelium is still more marked, especially round the stomata, which become much more evident than before. The germinating endothelial cells sprout forth from a stalk, and, what seems a most curious fact, is that this stalk represents the centre of a germinating area and corresponds to a pseudo-stoma.

Indeed, the author says (p. 68) that if we trace one of the cord-like proliferated portions viewed on edge, we can see that the germinating polyhedral, or even conical cells of the surface, are as much due to this last cause as to a proliferation of endothelium, and the cells produced from both sources are exactly alike. The stomata are frequently filled with a plug of fibrin, and may be obscured from this cause, clearly showing their function to be that of absorbent openings. This is further borne out by the fact that in such a preparation the lymphatics can be seen to be filled up with round cells. These round cells are partly formed by the changes in growth of the superficial endothelium extending deeply, and partly by the young cells falling into the lymphatic vessel. He has not the slighest doubt about the extensive alteration in the cellular elements of the matrix. These appearances are best seen after injection of pleural and peritoneal septic fluids. In consequence of the extensive œdema so produced, the cellular elements of the matrix are brought prominently into view. The lymph-canaliculi are seen to be swollen, the canals or cell processes reduced in number, less branched, and, finally, the nuclei of the cells are in the act of division (p. 75). Budding and endogenous growth similar to that described by Norris and Stricker take place in the branching elements. In the omentum of rabbits numerous migratory cells were met with in the neighbourhood of the vessels, in the lymph-canalicular spaces, and in the lymphatic nodules a distinct increase of migratory cells could be made out, especially round granular cells, which may be taken, in all probability, for emigrated colourless blood-corpuscles. The author's observations on the formation of fibrous bands from the intercellular matrix, and the transformation of vacuolated cells into lymphatics and blood-vessels, are points of the utmost importance and of the greatest interest. In fact, the two volumes are so full of matter of the first importance that in a short notice of this kind it is impossible to do them justice. Work of this kind is always truly valuable, and it is matter of great regret that other branches of pathology have not been worked out in a similar thorough, unbiassed, and scientific manner. The bearing of Dr. Klein's researches on the subject of tuberculosis in man we cannot dwell upon here. It is a subject on which we may hope to get a great deal more valuable information from the author of these present two volumes.

Arnold's investigations ("Ueber die Beziehung der Blut und Lymphgefässe zu den Saftkanälen," Virchow's 'Archiv,' vol. lxii, pp. 157 and 487) go to show that there is a tolerably free communication between the blood and lymphatic vessels by means of the lymph-canaliculi. In minute blood-vessels which have been injected with a dilute solution of nitrate of silver, small, round, dark markings with a clear centre are seen between the endothelial plates. These

he looks upon as veritable openings, and calls them stigmata in contradistinction to the stomata found on lymphatics. In the frog's web the relations between the lymph-canaliculi, blood-, and lymph-vessels are well seen after injection and removal of the epithelium, or splitting of the web. Colouring matters may be injected subcutaneously into the lymph-sac which lies over the small bones of the foot, or under the skin of the toes; or diapedesis may first be produced, and then the injection forced through the blood-vessels. The blood-vessels are found to split up into a fine network, from whose sides come off numbers of small branches communicating with those of branched protoplasmic cells. They also communicate with the branched pigment-cells of the web. He states that the lymphatics could also be filled by injecting the blood-vessels in this way, clearly showing that there exists a free communication between the two. Blood-corpuscles may pass out of the blood-vessels through the stigmata into the adventitia and lymph-canaliculi of the connective tissue, and from these into the true lymphatic vessels.

Picot ("Recherches Experimentales sur l'inflammation suppurative et le passage de leucocytes à travers les parois vasculaires," 'Journal de l'Anatomie et de la Physiologie,' 1870-71, p. 465) has tabulated a series of very accurate observations on the mesenteries of different animals. His system of observation was to map out the field into several divisions, and to note carefully, from time to time, over a lengthened period, what occurred in each. He says that, after some time, new globular elements form in the intervascular spaces without having been seen to proceed from any neighbouring vessel. They appear as little clear points at first, but gradually enlarge and take on a granular aspect, and exhibit amœbiform movements. Similar elements appear in the same way along the edge of the vascular wall. One would imagine that they had originated from the inside of the vessel, if one had not previously counted the contained corpuscles. At no time has he ever seen the corpuscle shut up in the lumen of the vessel, leave it, and wander outwards; but, nevertheless, the interspaces of the peritoneal tissue become filled with new elements. He cannot account for these newly formed elements otherwise than by supposing that they are leucocytes derived from the vessels, although he has never seen them pass through the wall. It seems to us that this is rather too wide a conclusion to be accepted from the facts presented, for when we remember the active division which the endothelium undergoes in the inflammatory conditions, and the action of the pseudostomata in enlarging (Klein, loc. cit.) and forming globular corpuscles, it seems somewhat rash to admit that they *must* all be leucocytes passed out from the interior of the vessel. Cohnheim ("Ueber Entzündung und Eiterung," Virchow's 'Archiv,' vol. xl, p. 1) has himself described corpuscles as appearing in the

endothelial interspaces, but ascribes them to wandering leucocytes, which, having escaped from the vessels, make their way under the endothelium, and then come to the surface between the individual plates. Feltz ("Recherches Experimentales sur le passage des Leucocytes à travers les parois vasculaires," 'Journ. de l'Anatomie et de la Physiologie,' 1870-71, p. 33) has been unable to confirm a great many of the statements made by Cohnheim, especially in regard to the exudation of leucocytes in inflammation. He believes that a good deal of the appearance of the blood-corpuscle separating from the outside of the vascular wall is due to an optical effect. One sees, apparently, that a corpuscle has passed through the vascular wall because there is what looks like a clear space intervening between the two. This, however, is only an optical delusion, and can be proved to be so by altering the focus. In his researches on the peritoneum Feltz found that leucocytes were apparently produced in the interspaces of the tissue. On careful measurement, however, he discovered that these were larger than the leucocytes within the vessel. In twelve hours or so he found that the endothelium had nearly all undergone degeneration, and in its place were a number of large elements of the nature of leucocytes, especially in the neighbourhood of the vessels and for some distance round them. He concludes that in the peritoneum the endothelial elements degenerate or melt away, and that, in their place, we get structures which are not the same as those within the vessels. These observations, although perhaps somewhat one-sided, appear to us of considerable importance in explaining the presence of numerous leucocytes found in the inflamed mesentery of the living animal. Although not for a moment doubting that the leucocytes do exude from the vessels under observation, it has always appeared to us as if the immense numbers produced within twenty-four hours could not be all accounted for in this way. Picot observed (loc. cit.) that many of those in the interspaces appeared without his being able to trace their origin to extruded leucocytes. They first showed themselves as small globular points on the surface, which afterwards enlarged and became granular. We cannot see why many of these were not produced by germination of the endothelial nuclei or pseudostomata, especially as in the silvered preparations this germination can be easily demonstrated.

The inflammatory affections of the nervous system are only beginning to attract the attention which they deserve from pathologists, but, as yet, no very definite conclusions have been arrived at. Dr. Jolly, of Strassburg (Stricker's 'Studien,' 1870), in his experiments found that the earliest changes in traumatic encephalitis were to be traced to the blood-vessels, in whose walls were found many oil-globules and compound granule-cells. These compound granule-cells were of two kinds, large and small. The former were

of irregular shape, and contained numerous oil-globules. The latter were about the size of a colourless blood-corpuscle, and were seldom entirely filled with oil-globules. The origin of these cells he traces to exuded leucocytes through the wall of the blood-vessels themselves. These cells resemble those described by Virchow as of frequent occurrence in the encephalitis of children, but Virchow avers that they make their appearance secondarily to an enlargement and increase in number of the neuroglia cells, and supposes the former to be derived from a fatty degeneration of the latter. They are, we suppose, similar to what are so commonly found in all nervous tissues under certain morbid conditions. We cannot, however, recognise them as typical of an inflammatory process, but would rather incline to the belief that they, in great part, result from the nutritive changes which may have been caused either in making the experiments or by the subsequent inflammatory action. Their accumulation round the vessels does not present anything remarkable; as, in the resorption of the fatty products, the blood-vessels and their probably lymphatic sheaths would above all others be the places where we should look for them. The observations of Arndt on the brain, spinal cord, and ganglia ('Archiv f. Pathol. Anat. und Physologie,' B. 50, S. 511), comprise a description of certain lesions which seem to us more typical of inflammatory action. Chief among these is a morbid condition of the axis cylinders by which the cells are converted here and there into smooth, shining, faintly granular masses contained within the nerve sheath, and holding two to three large granules as nuclei. A similar condition of the axis cylinders has been referred to by Leyden ('Klinik de Rückenmark's Krankheiten, 1874). The nerve-fibre is frequently two to three times larger than normal, and the axis-cylinder takes chief part in producing this result, while the medullary sheath disappears or is atrophied. The cortical part of the axis-cylinder is transformed into a broad band, and, as a rule, irregularly varicose and fatty granules are seen in it. The same changes are observed on the processes of the nerve-cells and on the single contoured central nerve-fibres. The processes of the nerve-cells under such circumstances resemble a condition described by Meynert in syphilitic brains. The more the inflammatory conditions of the nervous centres are inquired into the more is doubt thrown on the fact of the nerve-cells actually participating in the formation of new elements, as asserted by Meynert and others. Ceccherelli ("Ein Beitrag zur Kenntniss der Entzündlichen Veränderungen des Gehirns," 'Oesterr. Med. Jahrbüch,' heft 3 and 4) out of many experiments on the brains of rabbits and fowls met with this condition only in one instance. Hayem ('Compt. Rend.,' 78, No. 4) confirms these observations in the spinal cord. It is probable, therefore, that, in the truly inflammatory processes, the ganglion-cells seldom or never participate in the production of newly formed elements, but that

they undergo a retrograde metamorphosis, either of an œdematous (Meynert) or fatty nature.

Certain inflammatory affections of arteries have been described lately by Heubner ('Die Luetische Erkrankung der Hirnarterien,' 1874), and Friedländer ("Ueber Arteritiis Ohliterans," 'Centralblatt,' 1876, No. 4,) which seem to be of great interest. The former is probably the cause of the local paralyses which ensue in the course of constitutional syphilis. The two affections are, however, of such interest and importance as to be worthy of a separate notice.

V.—The International Sanitary Conference at Vienna in 1874.[1]

The excitement produced among a certain school of epidemiologists, by the rapid diffusion of cholera over the northern shores of the Mediterranean in 1865, had not diminished materially when the International Sanitary Conference assembled the following year, at Constantinople, to devise measures to ensure Europe from future visitations of that disease. That Conference concluded that cholera was indigenous in India, and that it was carried from that to other countries by man, and recommended that rigid quarantine should be established whenever there seemed a chance of stopping its germ in transit.

The conclusions of this Conference did not receive much weight at the time in this country, or even in several parts of the Continent, and the experience derived from the course of cholera, both in Europe and India, since 1866, had impressed the Continental epidemiologists so strongly with the belief that the views promulgated at Constantinople required modification, and that great amelioration might be introduced in the practice of quarantine without risk, that, in 1874, Austria proposed that another Conference should be held to reconsider the subject. The other powers of Europe having received the Austrian proposal favorably, delegates were sent to Vienna, and the Conference was opened on 1st July, 1874. Nineteen European states, together with Persia and Egypt, were represented at it, each state by one or more medical men; and in the case of Austria, France, and Sweden, members of the diplomatic service were associated with them. Among the medical delegates were some of the best known epidemiologists of the Continent, and four of them, Drs. Fauvel (France), Leuz (Russia), Bartoletti

[1] *Procès-Verbaux de la Conférence Sanitaire Internationale ouverte a Vienne, le* 1 *Juillet*, 1874. Vienne Imprimerie Impériale et Royale, 1874.

(Turkey), and Dickson (Great Britain), had been members of the Conference at Constantinople in 1866.

The primary object for which the Conference was assembled was the settlement of quarantine regulations with reference to cholera, and a secondary one was the formation of an International Commission for the study of epidemics and for devising means for checking their spread. With reference to the former it is obvious that the members would require to arrive at a common understanding, so far as our present information permitted, as to the nature of the immediate cause of cholera, and the mode in which it became diffused, before they could venture on concerting measures to prevent that diffusion or to render the cause inert. With this object the official programme set forth the following points for consideration, preliminary that of quarantine properly so called:

"1. Does Asiatic cholera capable of spreading (epidemic) become developed in India only, and does it always come from without when it appears in other countries? Or does it assume the endemic character in other countries as well as India? If so, what are these countries?

"2. Is cholera transmissible by man?

"3. Can cholera be propagated by articles brought from an infected locality, and which have been in use there, especially by those who have been in communication with cholera patients?

"4. Can it be propagated by provisions?

"5. Can it be imported by living animals?

"6. Or conveyed by merchandise?

"7. Or, finally, by the bodies of persons who have died of cholera?

"8. Can cholera be propagated at a distance through the medium of the air alone?

"9. Does the free exposure of the matters giving rise to or propagating cholera to fresh air from without, or does the seclusion of those matters from the external air, have any influence on the contagious character of cholera?

"10. Supposing there be contagion, what is its period of incubation?

"11. Are there any means or processes of disinfection by the employment of which the exciting cause or contagion of cholera can with certainty (or with any chance of success) be destroyed or deprived of its intensity?

"If so, what are these means?" (Pp. 398, 399.)

These questions embrace points of much interest, not only for their bearing on quarantine, but also for all who are in any way concerned in obviating the ravages of cholera. As there is still great divergence of opinion regarding them, however, we will lay before our readers the conclusions of the Conference on each, and

endeavour to show how far these are in accordance with what we know of the natural history of this disease.

In reply to the first question, then, the Conference came unanimously to the following conclusion:

"Asiatic cholera capable of spreading (epidemic) becomes developed spontaneously in India, and it always comes from without when it appears in other countries.

"It does not assume the endemic character in other countries than India." (*Relevé des conclusions adoptées, Procès-verbaux*, p. 379.)

When the Conference concluded that cholera is developed spontaneously (*se développe spontanément*) in India, it of course implied that the immediate cause of the disease, that which determines its character as the variolus virus does that of smallpox, is produced there in quantity under the influence of climatic and local causes, whether it be a germ which vegetates altogether external to the human body, as believed by Dr. Bryden, or one which finds a fitting soil mainly in living animals and most frequently in man; or whether it be of the nature of what, for want of more precise information as to its intimate nature, has hitherto been included under the term malaria. In short, there must be an active principle concerned, which is generated according to fixed laws, and the grand object of epidemiologists is, by careful study of the natural history of the disease, to eliminate gradually every accessory though non-essential circumstance, so that they may arrive at last at what really are the conditions necessary for its production; and, finally, what is its precise nature and properties.

India has now for many years been subject to severe and widespread visitations of cholera. These occur at pretty regular intervals, and pursue courses which, while not absolutely identical for the different epidemics, nevertheless present a general resemblance which cannot be overlooked. Between these visitations there are vast districts which are for months, or even years, quite free from cholera; while there are others, more limited in extent, in which sporadic cases, or even small groups of cases, are not infrequent, and, it may be, occur every month between the cessation of one epidemic and the commencement of the next. The Conference concluded that cholera was endemic in certain parts of India, and consequently that its immediate cause must be in permanence in these, though presenting very variable states of activity at different times; but one point is involved in this admission, the bearing of which does not seem to have suggested itself to any of the members; if the Conference claim the sporadic cases and small outbreaks in the intervals between the epidemics as evidence of endemicity, it must admit that these cases are essentially the same as those which constitute an epidemic

(just as sporadic cases of smallpox do not differ from those met with during an epidemic of that disease), and not the milder form designated cholera nostras, which it has been assumed is altogether different in its nature from malignant cholera. It is in evidence from several sources in India that many of these cases arise without any known communication with previous cases; that they present every characteristic symptom of the malignant form of the disease; and that, had they occurred during an epidemic, they would have been classed without hesitation as well-marked examples of it. Without, therefore, asserting dogmatically that cholera nostras and malignant cholera are altogether identical in their nature, we think our present information does not sanction any distinction being drawn between cases of malignant cholera, whether they occur sporadically or during an epidemic; and we further believe the Conference was quite justified in concluding that the malignant cholera, which at times assumes the character of invasive epidemics, is really endemic in certain localities in India.

As malignant cholera, therefore, may and does appear sporadically in certain localities in India, it may be asked to what is its epidemic extension from time to time attributable? To a potentiality in the immediate cause of the disease, which is latent in the intervals between the epidemics, but which on becoming active leads immediately to their development, as the deliverance of the Constantinople Conference would lead us to infer? Or to other factors coming into operation which do not alter the nature of the immediate cause, but only favour its rapid development in localities where it was previously to be met with, and, it may be, which at the same time increase the susceptibility of the residents to suffer from exposure to it, and which exercise a similar influence over extensive districts where previously there was no indication of the disease? The latter seems more in accordance with what we know of other diseases; no one, for instance, claims for the virus of smallpox, when the cases are few and scattered, different infective powers from those it manifests when the disease is epidemic; yet every epidemiologist will admit that in the latter case some factor or factors must have come into operation to cause unusual extension of the disease. which, in the previous instance, were in abeyance. It appears to us the Constantinople Conference, when it added "invasiveness" to its definition of malignant cholera, committed the error of mixing up two things essentially distinct in themselves, though frequently operating together, and that the confusion so produced is not only to be traced through its conclusions, but has continued to influence medical reasoning injuriously ever since, and to prevent it taking that line which it must do before many of the obscure questions surrounding the origin and propagation of cholera can be resolved.

The Vienna Conference concluded that "Asiatic," but which it may be better to designate "malignant" cholera, as involving no hypothesis, does not assume the endemic character in other countries than India; the Constantinople meeting adopted a similar conclusion, but with the qualification that it was necessary to except secondary foci of more or less duration. What number of cases, and for what length of time they must continue, to appear to justify the application of the term endemic, neither Conference has defined; nor did that at Constantinople indicate the period it considered might elapse between the cessation of the original outbreaks and the recrudescence of the disease in the secondary foci attributed to them, so it is difficult to state with precision whether any given instance would come within their definition or not. That localities are not to be found in Europe which continue to present a nearly constant series of cases between the termination of one epidemic and the commencement of the next, for long periods of years, as is the case in India, is quite true, but the evidence which has been made available since 1866, by Drs. Pelikan and Arkhangelsky, is conclusive as to malignant cholera having never been absent from the eastern parts of Europe from 1847 to 1856, and again from 1865 to 1873. The interval between the great epidemics of 1847-49 and 1852-55 was characterised by a great diminution in the number of the cases, and the reduced area over which they occurred, but still there was a succession of cases of the malignant form of the disease, and the same was observed between the epidemics of 1865-67 and 1869-71. If there have not been the same continuance of cholera in western Europe, still it has been observed, since the epidemic of 1848-49, that in the warmer months every year a few cases of cholera are usually met with, both in this country and in France, presenting every characteristic of the malignant form of the disease. These facts prove beyond doubt that whether we accept the conclusion that malignant cholera does assume the endemic character in Europe or not, it is incontestable that the immediate cause of that form of disease can be evolved in many parts of it under favorable circumstances, so as to develop the disease in the most unmistakeable manner.

The Vienna Conference concluded further, as that at Constantinople had done previously, that malignant cholera capable of spreading always comes from elsewhere when it breaks out in Europe. No doubt several epidemics have reached Europe in this way, which could be traced step by step from India or Persia, but, as pointed out by Tholozan, that of 1852-55 did not arrive in that manner, a fact that has been confirmed by Arkhangelsky. Pelikan also shows clearly that there is no ground for concluding that the epidemic of 1869-71 was imported into southern Russia, for it commenced distinctly in Kiew, in the interior of the country, while all the districts

to the south, as far as the Black Sea and Persian frontier, were quite free from it. Here are two examples, then, of severe epidemics arising in Europe itself without direct importation; of course the same may have occurred before, and may be met with again, and the position of the Conference cannot be maintained as it now stands.

It thus appears that, in India, malignant cholera is met with in certain localities as an endemic, and that from time to time it becomes developed into an epidemic, which usually displays highly invasive powers and spreads far and wide; in Europe, if not a strongly marked endemic, still typical cases present themselves nearly every year in variable numbers, and in two instances, at least, these led up to severe invasive epidemics, without fresh importation, exactly as occurs in India. Pelikan regards the epidemics of 1852-55 and 1869-71 as recrudescences of the material left by the previous epidemics, but, even if this be admitted, some factors must have come into operation previous to the new development of the material, which had until then been quiescent, the influence of which was necessary to bring about that development. These factors can be no other than those which ensure the same development in India itself, and, in fact, they were in active operation there in 1852-53, as shown by the following ratios per 1000 of deaths from cholera in the European and Native troops in Bengal from 1850 :

	Europeans.	Natives.
1850	1·52	·17
1851	3·23	·88
1852	5·88	1·97
1853	12·09	1·59

In 1853 the Bombay Presidency and Ceylon were affected, Mauritius began to suffer, and cholera prevailed from Persia through the south of Europe, in the Northern Antilles, and the south part of the United States. In 1868, again, there was no great prevalence of cholera in India, the deaths from it among the civil population having been 25,253 only in the Bengal Presidency and in the Central Provinces, but in 1869 the deaths from it in the same districts rose to 167,922, a mortality nearly seven times greater than that of the previous year. In 1867-68-69 cholera had been very active in Persia to the south and east of the Caspian, but there was very little towards the Russian frontier south of the Caucasus. From these illustrations (and many others of like character might be given) it is obvious that the factors in question may embrace very large portions of the earth's surface at the same time, but whether, in 1869, for example, they were active from Bengal to Russia, or were in force merely in those districts in which cholera manifested itself, our want of information as to their nature does not permit of a decision.

To the second question the reply was, unanimously, to the following effect:

"The Conference accepts the transmissibility of cholera by man coming from an infected locality; it considers man can become the specific cause only beyond the influence of the infected locality; further, it regards him as the propagator of cholera when he comes from a place where the germ of the disease already exists." (*Relevé des conclusions,* p. 379.)

After its conclusion on the first question, it was not to be expected the Conference would reply to this in terms very different from those just given. Pettenkofer, who believes that man carries the germ, but that it is developed in the locality itself, which thus becomes the focus of the disease, stated that observations made at Munich, Heilbronn, and Spires, had failed to establish a correlation between the first indications of the disease and the arrival of sick from an infected locality, but he was told that instead of clearing up the question he rather introduced a regrettable confusion. In fact, the members were so satisfied with the view set forth in the reply, that they seemed to think no other deserved consideration. It will occur to most of our readers, who have reflected on the bearings of the preceding remarks, that the reply of the Conference does not afford by any means a satisfactory solution of the question submitted for its consideration, or one in accordance with the evidence which has been referred to above, but it may be well to particularise the conditions which have to be complied with to ensure success in such an inquiry.

Whenever there is a question whether cholera have arisen from a contagion which has been imported or from causes existing in the locality in which it has shown itself, it is clear that, to establish the fact of the importation having been the efficient cause of the subsequent manifestation, it is necessary to show that causes operating in the locality at the time could not have produced it. Now, as the occurrence of cases merely affords evidence that the causes of the disease are in operation at the place, it does not, of itself, enable us to say whether these be due to the sick who have arrived, or to circumstances existing at the time in the locality, and unconnected with the sick. Yet by far the greater number of instances brought forward to establish the importation of cholera are of this description, and are quite insufficient to prove either the one or the other view to be the correct one, for the simple reason that from the way in which they are associated with the arrival of sick on the one hand, and with local circumstances on the other, the influence of either cannot be excluded by any independent evidence, though the disputants generally decide the point for themselves according to their previous bias. This, however, is not the method of investigation

to be pursued if we desire to remove the uncertainty which now obscures this question.

It was mentioned above that sporadic cases of malignant cholera had continued in eastern Europe between the epidemics of 1847-49 and 1852-55, and also between those of 1865-67 and 1869-71, and that such cases were met with, more or less every year, both in this country and France. A few instances in which groups of cases appeared in this country when the disease was not epidemic may be noticed to illustrate the point under consideration. In 1837, when cholera was epidemic in Malta, Sicily, Italy, south of France, and to a less degree through Germany up to the Baltic, four cases of malignant cholera occurred in the Dreadnought Hospital Ship in the Thames in August, two of which died; of the antecedents of these there is no information. The disease was absent during September, but, on 8th October, a man who had been under treatment on board for about a week with an affection of the head was attacked with cholera, and up to the 28th of the month twenty cases in all had occurred, and of these twelve died. The man attacked on 8th October had left Dantzic on 8th September, where some cases had appeared, but neither he nor any of the crew of the vessel he came in were affected. The disease in this instance attacked patients on all three decks of the Dreadnought (an old three-decker), and also the boatswain, who lived under the forecastle separate from the hospital decks altogether. No source of importation seems to have been detected.

A few months afterwards cholera appeared among the inmates of the House of Industry at Coventry, in the centre of England, and between the 7th January and 5th February, 1838, caused fifty-five deaths. No trace of introduction from without seems to have been detected in this instance, and, considering the distance of the place from the sea-board, and that the disease was not epidemic in the country at the time, it affords a striking illustration of the possibility of malignant cholera breaking out without previous importation by man.

In 1859, when cholera was epidemic in Belgium, the western parts of Germany, and south of Sweden, there were three small isolated outbreaks in this country, viz. at Woolston, a coastguard station on the Southampton water; at Glass Houghton, a village near Pontefract, in Yorkshire; and at Wick, in the north of Scotland. In the first, diarrhœa had prevailed at the station from 3rd to 17th July; the first case of cholera occurred on the 18th, and up to the 30th, of about forty persons at the station, three had cholera, of whom two died, three had choleraic diarrhœa, and nearly all the remainder had diarrhœa. The registrar of deaths, the collector of customs, the quarantine officer, and the dock-master, at Southampton, all declared they had not heard of another case of cholera

in the locality during the summer. The outbreak at Glass Houghton was somewhat later; here, in a small village with about 200 inhabitants, twelve deaths from cholera (out of nearly thirty attacked) took place from the 4th to 17th October, and fifteen died at Castleford and Whitford, in the vicinity, of cholera and diarrhœa. There was no importation traced in this instance. At Wick the outbreak commenced on 8th September, during the herring fishery, when large numbers of people are collected there from various parts of the coast, and the place much crowded. There was a good deal of diarrhœa as well as cholera; but as there was a dispute as to whether the disease were imported, and as the evidence does not admit of this question being decided, it is needless to refer to it further here.

In 1865 again, when attention had been for some time particularly directed to the mode in which cholera was disseminated, there were some outbreaks which were investigated very closely, and which afford some most important lessons on the point under consideration. As many of our readers may recollect, cholera made its appearance at Mecca early that year, and as the pilgrims were on their return for the southern coast of the Mediterranean cholera appeared at Alexandria on 2nd June; it was at Malta on 20th; at Marseilles it was officially notified on 23rd July, but there seem to have been suspicious cases as early as 9th June, and finally the epidemic was recognised at Poiteaux, near Paris, on 18th September, the nearest point to England it had reached up to the end of October. While this advance of the epidemic was taking place a fatal case of cholera presenting the characteristic symptoms of the malignant form of the disease proved fatal in the Borough on 28th June, and there was another case under treatment in Guy's Hospital in the beginning of July. At Southampton, a woman aged 23, who had suffered from diarrhœa for a week, which she attributed to having eaten crab, presented unequivocal symptoms of malignant cholera on 2nd August, but recovered. At Southampton again, on 22nd September, a man was attacked with cholera and died. At Weston Common, two miles from Southampton, there was one attack on 23rd September and two on 26th, and at Bitterne, also two miles from Southampton but in a different direction, there were two attacks on 27th and another on 29th, while in Southampton itself there was a second attack on the previous day, the 28th September. Of sixty cases in all which occurred in Southampton or its vicinity up to 4th November, thirty-five died, and there was no doubt among the medical attendants they were malignant cholera. Dr. Parkes, who took great trouble to ascertain all the facts connected with this outbreak, admits "its origin by importation is deficient in precision of evidence;" in short he failed to establish importation after exhausting every source of information within his reach.

While these were occurring at Southampton a Mr. Groombridge and his wife, from Theydon Bois, in Essex, who had been seventeen days at Weymouth for the benefit of their health, returned home on 25th September by railway, passing through the station at Southampton on their way, but it is not known that either of them left the station while the train remained there. Mr. Groombridge had had diarrhœa, sickness, and cramps, on 23rd and 24th, while at Weymouth, but was better on 25th, and, though not well, was still able to travel. Mrs. Groombridge, aged 50, complained of pain in the back and some discomfort of the stomach and bowels on 25th, which she attributed to the shaking of the train; on 26th, when at home, diarrhœa commenced, on 28th sickness and cramps were added, and on the 29th collapse ensued. The following day reaction commenced, secondary fever followed, and she died on 11th October. On 30th September a daughter, aged 8, was attacked with cholera and died the same night in collapse; and from the 1st to 7th October there were six attacks of cholera in persons connected with the family, Mr. Groombridge himself being one (in him the characteristic symptoms appeared on the 6th), and in two others who had communication with them. On the 10th October a woman was attacked who had assisted to lay out the body of one of the above who died on the 7th, but who had no communication with Mr. Groombridge's house or family; she died on the 11th, and finally, a child aged 3, in the neighbourhood, grandson to the last person, was attacked on 31st October, and died the following day. Of twelve cases in all, nine died. On examination subsequently it was discovered that at Mr. Groombridge's house the soil pipes from the water closet leaked into the well, the water from which was used for domestic purposes, and hence, no doubt, the extreme virulence of the disease in his family; but an additional most important fact is, that "two or three cases of common sporadic cholera had taken place in Epping district about the time of the outbreak," though it is added none of them proved fatal or presented any peculiar symptoms. Their occurrence, however, is an indication of unusual activity of the causes of that form of disease in the locality at the time, which, in the present state of the inquiry, cannot be overlooked.

In the summer in 1865, too, cholerine and diarrhœa were more common than usual in Copenhagen. In the southern districts of Sweden, also, there was great frequency of cholerine and diarrhœa that summer, and among them a few isolated cases of cholera, which are reported as resembling perfectly "cholera Asiatica," though no epidemic broke out.

If, now, these details be summarised it appears that in 1837 and early part of 1838, when cholera was epidemic from the Mediterranean through Germany to the Baltic, there were two

distinct though circumscribed outbreaks of the malignant form of the disease in England. In 1859, when it was epidemic along the eastern shore of the North Sea, there were again two small but still distinct outbreaks in this country, and in addition the returns of the Registrar-General show a considerable increase of mortality from cholera and diarrhœa over the previous year, not only in London, but throughout England. And in 1865, when the epidemic of cholera had advanced as far as Paris only, cases of malignant cholera were met with in London, at Southampton, and in Essex, also in south of Sweden, while the Registrar-General's returns show a marked increase of cholera and diarrhœa both in London and in England generally over 1864, and similar features were displayed both at Copenhagen and in Sweden. In all these instances no importation was made out, yet in each diarrhœa and the milder forms were more frequent than usual, while single cases or groups of cases of malignant cholera cropped up of so marked a character as to leave no doubt of their nature. The conclusion is inevitable, therefore, that these various affections must have arisen from causes operating at the time in the various localities where they appeared, whether, in the case of the maligant cholera, from the recrudescence of a material left on former occasions, as Pelikan and Arkhangelsky believe, or from matters originating at the spot, the evidence does not enable us to decide. In either case, however, the co-operation of the same general factors previously alluded to will be wanted to account for increased activity manifested by these local causes, exactly as where the epidemic was actually prevailing. We thus arrive at this position, that the influence of these factors may be experienced a long distance in front of the point the advancing epidemic has reached, or to either side of it (the aura of the epidemic, as Dr. Bryden has called it), under which sporadic cases and small outbreaks occur, while from unsuitableness of the localities at the moment the manifestations stop short of an epidemic. With such a combination of circumstances preceding and bordering an advancing epidemic for great distances, of what value are the great majority of proofs adduced in favour of importation? Unless the influence of the factors here alluded to, both local and general, can be eliminated, any inference as to cholera having been communicated by a person from an infected locality to others about him in one hitherto free can rest only on a surmise.

In reply to the third question of the programme the Conference decided unanimously that—

"Cholera may be transmitted by clothing and other articles (*les effets à usage*) brought from an infected locality, and especially if they have been used by persons with cholera; also it results from certain facts that the disease may be carried to a distance by these

articles if excluded from free exposure to the air." (*Relevé des conclusions*, p. 380.)

The evidence on which transmission by clothing, bedding, and such articles, is founded is of very much the same nature as that adduced in support of transmission by persons; it is assumed for instance that if, at a distance from where it is epidemic, cholera follow the reception of an article of dress worn by a person who has had cholera, or even only a slight relaxation of the bowels, and so been merely suspected of having had it in its incipient stage, this is to be taken as sufficient proof that the disease was so conveyed. Were such occurrences frequent, and pretty regular under every variety of circumstances, they would undoubtedly become convincing; but as, on the contrary, they are comparatively rare, and are much influenced by time and place, the inference drawn from them is for the most part open to the same objections as have been shown above to apply to the importation by persons, and is not entitled to confidence as the true explanation of the facts. To arrive at conclusions worthy of confidence all such cases should be interpreted in accordance with the principles inculcated above, when we have little doubt most of them will be abandoned as inconclusive or open to a different explanation.

In answering the fourth question it was considered necessary to separate food and beverages. To the former the reply by a majority of eleven to seven was—

"The Conference, not being in possession of conclusive proofs of the transmission of cholera by food (*par les aliments*), does not consider itself authorised to come to a decision on this point."

To the latter the following was adopted unanimously:

"Cholera can be propagated by beverages, particularly by water." (*Relevé des conclusions*, p. 380.)

The Swiss delegate, Dr. Zehnder, who had laid before the Conference a memorandum on cholera following the employment of ox feet as food, in a circumscribed district in the neighbourhood of Zurich (given at length in the *Procès verbeaux*, p. 432) submitted the following for their consideration:—"That the transmissibility of the germ of cholera by eatables is probable," and he wished to add, "especially if those eatables be subject to putrefaction." From his account it appears that at Zurich the feet of oxen are separated from the carcases at the slaughter-house, where they are boiled, the hair then removed, and so prepared they are placed under water for the night in the basin of a fountain. The following day they are sold publicly, and a portion is bought by people from the country, or by middlemen who hawk them through the district above alluded to once a week. In either case the food seems to have

been consumed the day it was received, apparently as part of a salad. It was observed that on these occasions cholera appeared in different families, commencing generally the second day after having used this food. Though the transmission of these feet to the country did not occupy more than a few hours, yet, as the weather was hot (August 23rd to September 13th), Dr. Zehnder thinks that, naturally prone to spontaneous decomposition, this process had already commenced on their reaching their destination. To account for their leading to cholera he says the water supplying the fountain at the slaughter-house came from the basin of another fountain about a hundred yards off, before an auberge frequented by the lower classes. He thinks it very probable these people washed clothes at this fountain, and, as there were some cases of cholera here *subsequent* to the occurrence of those in the country above mentioned, he concludes there might have been some *before* their appearance, and that the germ of the disease derived from their discharges had got into the water of the fountain, and passed on to that in the slaughter-house, and so infected the ox feet placed in it. There are too many important blanks in the evidence in this case to allow it to have any weight in establishing Dr. Zehnder's conclusions; the essential points seem to be that, while cholera was epidemic, certain persons who had eaten articles commencing to putrefy were attacked by the disease, a fact by no means new nor peculiar to any particular kind of food.

The use of water contaminated with cholera discharges is certainly apt to be followed by an aggravated form of the disease, but does such water contain the germ of cholera, as is usually supposed, or does it merely contain matter which, when the other factors above mentioned are in operation, is capable of exciting that disease, and may a similar matter not then be found in water containing ordinary sewage? In India Drs. Lewis and D. Cunningham produced an affection in dogs, which they describe as "hæmorrhagic gastro-enteritis," by injecting cholera discharges in various conditions into their veins, but they found that a solution of fæculent matter from a healthy man produced results undistinguishable from those caused by the cholera material, only in a smaller percentage of the cases. Experimenters have hitherto considered the disease following these administrations or injections of cholera material in the lower animals as cholera; but Drs. Lewis and Cunningham state the lesions and attendant phenomena so produced are not identical with those of cholera in man, an opinion meriting the consideration of those who think they had produced cholera by administering cholera discharges from man to animals; as, on the one hand, if correct, the whole of their inferences and the superstructure which has been based on them necessarily fall to pieces; while, on the other, if this affection be identical with cholera, then,

as it can be equally produced by healthy fæculent matter, it cannot depend on a specific material peculiar to cholera discharges.

If from animals we revert to man, similar evidence is to be obtained occasionally. When, for instance, did Mrs. Groombridge contract the cholera of which she ultimately died? If previous to her arrival at home on 25th September, then she had not been exposed to any one with cholera, or to their discharges in any way, so far as is known; if after her arrival, then she had come into a district where several cases of common sporadic cholera occurred about this time, showing there was a disposition to that form of disease then existing there; and, as was subsequently discovered, the water of the well which supplied the family was much contaminated by soakage from the water-closets, though up to this time it contained no cholera discharges. Under these circumstances Mrs. Groombridge was affected with diarrhœa on the 26th, with symptoms of cholera on 28th, followed by collapse on 29th. Thus, under the operation of influences at work in the district, of which there was independent evidence, and on commencing the use of water contaminated with ordinary sewage, first diarrhœa, and subsequently confirmed cholera, ensued, without exposure in any way that has been discovered to choleraic discharges. After this first case it is in conformity with other experience that the contamination of the water with its discharges had much to do with the subsequent virulence of the disease in that family, but the whole tendency of the evidence is in favour of the view that this was not owing to any thing specific in the cholera discharges, and confined to them, but, as shown by Drs. Lewis and Cunningham's experiments, it equally exists in those of healthy persons, though less active in form, and, under favouring circumstances, will no doubt lead to similar results.

In reply to the fifth question the Conference affirmed by ten votes to two, six delegates declining to vote:

"No fact proving the transmissibility of cholera from animals to man is known, but it is quite reasonable to admit its possibility." (*Relevé des conclusions*, p. 380.)

The Conference replied to the sixth question as follows, thirteen voting in favour, and five abstaining:

"While admitting unanimously the want of proof in support of the transmission of cholera by merchandise, the Conference admits its possibility under certain circumstances." (Ibid., p. 380.)

To the seventh question the following answer was given unanimously:

"Although it has not been established by satisfactory evidence that the bodies of those who have died of cholera can transmit the disease, it is prudent to consider them dangerous." (Ibid., p. 381.)

After the comments on the previous replies these call for no remark. To the eighth question of the programme the Conference adopted the following answer unanimously:

"Up to the present there is no fact to prove that cholera can propagate itself to a distance by the atmosphere alone, in whatever condition it may be; besides, it is a law without exception that an epidemic of cholera has never extended from one point to another in less time than is necessary for man to travel.

"The surrounding air is the principal vehicle for the generating agent of cholera; but the transmission of the disease by the atmosphere, in the immense majority of cases, remains limited to a very short distance from the point of emission. As to the statements of transport through the atmosphere to the distance of one or many miles, they are not satisfactorily conclusive." (Ibid., p. 381.)

It has long been a favourite argument with those who believe that man is instrumental in spreading cholera that it never travels faster than he does, and occasionally the inference "therefore he carries it" is added. The Conference at Constantinople, misinterpreting the occurrences in 1865, announced that the chances of propagation were augmented in proportion to the increased rapidity and frequency of locomotion; but that of Vienna, with nine years' more experience, has seen cause to limit the proposition to its original terms. Without questioning the rate at which epidemics of cholera proceed, we cannot admit that it sanctions the inference which lies behind it. The remarks already made indicate a series of factors as concerned in the evolution and spread of epidemics altogether independent of man, and their rate of progress is determined in the main by these, and not by the rapidity or frequency of his wanderings. The Conference seems to view the atmosphere simply as a vehicle for the transport of the so-called cholera germ, but there may be other conditions of it concerned in the generation of epidemics which science has not yet made us acquainted with, and it argues a want of prudent foresight, while we admit our present limited information, to assume that greater enlightenment is not to be looked for hereafter.

To the ninth question the Conference returned the following answer unanimously:

"It results from the study of facts that on free exposure to air the generative principle of cholera loses its morbific activity rapidly; such is the rule, but, under certain peculiar conditions of confinement, that activity can be preserved for an indeterminate period.

"Cholera may be transmitted by clothing, bedding, &c., brought from an infected locality, and especially if they have been used by persons affected with cholera; and likewise it appears from certain facts that the disease may be carried to a distance by those articles if packed so as to prevent free exposure to the air.

"The great deserts are a most efficacious barrier to the propagation of cholera; there is no example of this disease having been carried to Egypt or Syria, across the desert, by caravans from Mecca." (Ibid., p. 381.)

The opinions expressed in this reply are to a large extent deductions from views adopted by the Conference as to the nature of the immediate exciting cause of cholera and its mode of dissemination. If, as we believe has been shown above, these views be erroneous, the deductions from them must partake of the error and cannot form a safe rule for our guidance.

On the tenth question the Conference displayed considerable difference of opinion, but ultimately came to the following conclusion by thirteen votes to one, while four abstained from voting:

"In nearly every case the period of incubation, that is to say, the time lapsed between the moment when an individual came under the choleraic intoxication and the commencement of the premonitory diarrhœa or of confirmed cholera, does not exceed a few days. All the instances cited of a longer incubation refer to cases which are not conclusive, or in which either the premonitory diarrhœa has been comprised in the period of incubation or the contamination might have taken place after the departure from the infected locality.

"Observation shows that the duration of the premonitory choleraic diarrhœa—which must not be confounded with other forms of diarrhœa which appear while cholera prevails—does not exceed a few days.

"The instances cited as exceptional do not prove that those cases of diarrhœa which continue beyond that are choleraic and capable of transmitting the malady, when the individual affected has been protected from any cause of contamination." (Ibid., p. 382.)

During the discussion exception was taken to the want of precision of the expression "a few days" (in the original "*quelques jours*"), but it was shown that the Constantinople Conference, which had previously employed it, had added in a parenthesis "a week at most," and the phrase was adopted at Vienna with this limitation, though the general impression seemed to be that the incubative period was usually from two to five days. One delegate cited an instance to prove that the period might extend to eighteen days, but he counted from the day the subject of it last saw a case of cholera, without taking into consideration the possibility of his having been exposed to other sources of the disease afterwards.

Though the necessity for separating premonitory diarrhœa from ordinary diarrhœa in reckoning the period of incubation was pointed out by several of the delegates, none of them ventured to describe how these could be distinguished from each other. The former is held to be but the preliminary stage of cholera, and may communicate the malignant disease, while the latter is harmless, yet no one can distinguish the one from the other in many cases until it go on to the

fully developed disease. A person from a distant place, where cholera is prevailing, may arrive at one where it has not yet appeared, and either on his arrival or soon after, have a slight bowel complaint scarcely requiring treatment to remove it; a laundress washes some of his clothes, and in a few days she and some of those about her are seized with cholera; forthwith the traveller's diarrhœa is called premonitory, and he is said to have introduced the disease; had no cholera followed his arrival his complaint would have been considered ordinary diarrhœa. This is no idle tale, but is really a fair representation of several of the instances adduced in favour of importation by those who hold that view; cases of cholera having occurred in a certain locality, the contagion must have been brought from somewhere else, and it is immediately assumed that any one who has arrived from an infected locality with diarrhœa, or in whom it arises within a few days subsequently, is the individual to blame, and his complaint, whatever may have been its character, is assumed to have been premonitory; while had a case presenting the character of malignant cholera appeared at the same place a month sooner, without any trace of importation it would have been considered as totally different in its nature from those met with during the epidemic. Such are some of the inconsistencies habitually met with in this subject, and we can never hope to remove the obscurity in which it is at present involved until we introduce more rigour in the selection of evidence and more precision in drawing inferences from it.

There was some difference of opinion as to the form in which the eleventh question of the programme should be put, and it was ultimately agreed that a vote should be taken on it, omitting the words "or with any chance of success." Should this be negatived it was to be submitted again with these words replaced, but striking out "with certainty." The Conference rejected the former by twelve noes, while seven abstained. The latter was adopted by thirteen ayes to five noes. To the further question, "What are these means?" the following reply was given unanimously:

"Science has not yet indicated sure and specific means for disinfection; in consequence, the Conference recognises the great value of hygienic measures, such as ventilation, flushing, cleansing, &c., combined with the employment of substances regarded as disinfectants." (Ibid., p. 383.)

There was much difference of opinion as to the value of disinfectants. Dr. Dickson cited the statement of Dr. Budd as to their efficacy at Bristol in 1866, to the effect that while the deaths from cholera there in 1832 were 626 and in 1849 1979, yet after the hygienic measures adopted in 1854, especially the improvements in the sewerage and the introduction of an improved supply of water, with the addition of carefully applied means of disinfection in 1866,

the epidemic of that year caused twenty-nine deaths only. Dr. Drasche, on the other hand, stated that the commune of Vienna, in 1873, expended 400,000 florins and employed 53,000 quintals of sulphate of iron for disinfecting purposes; the result on the mortality from cholera is shown by the following ratios per 1000 of the population, in the epidemics of that and previous years:

In 1849 deaths per 1000 were	2·6	No measure for disinfection in force.
„ 1850 „ „	2·2	
„ 1854 „ „	4·6	
„ 1870 „ „	4·6	Disinfecting measures employed.

M. Hirsch also stated that in Germany, in the epidemic of 1849, when no means of disinfection were in use, the mortality from cholera was 3 in 1000, and in 1865, when these were employed assiduously the ratio of mortality was the same. M. Marcovitz, the delegate from Roumania, while admitting that the results for Vienna were striking, remarked that at Jassy there had been a very violent epidemic of cholera in 1866 and a very mild one in 1872, though no means of disinfection had been employed during either. M. Marcovitz's remark shows how necessary it is to ascertain how far one epidemic falls short of another in force before the reduced mortality which may accompany the use of disinfectants be taken as the measure of the advantage derived from these. Had Dr. Budd done this we scarcely think he would have been so satisfied that the smaller death-rate at Bristol in 1866 was due to any material extent to the employment of disinfectants. Thus, taking the deaths from cholera in the counties of Somerset, Gloucester (including Bristol), and Monmouth, which surround Bristol, for the years 1849 and 1866, from the Registrar-General's Report on the epidemic of 1866 (pp. 10—15) they stand—

	1849.	1866.
Somersetshire	929	68
Gloucestershire	1467	39
Monmouthshire	777	204

These show the incidence of the disease was very much less in Somersetshire and Gloucestershire in the latter period than in the former, and the reduction in Monmouthshire would have been much greater but for the occurrence of 122 of the 204 deaths in 1866 in the single district of Bedwelty, which embraced a large mining and manufacturing population, in a very unhygienic condition.

We had intended to give an outline of the views of the Conference as to quarantine, and the proposition to establish a permanent international commission for the study of epidemics. The length to which these remarks have extended, however, precludes our doing more than stating, in general terms, that the Conference recommended that quarantine by land should be abolished, as it interfered most injuriously with traffic, and could not be made effective, and,

for the same reason that quarantine on rivers should not be maintained. As to quarantine by sea, the Conference recommended its continuance on the Red Sea and the Caspian, but when Europe is invaded that a system of medical inspection, similar to that now carried out in this country, should be adopted; but inasmuch as some of the countries in the south are not prepared to trust to this measure alone, a series of regulations to form the basis of a quarantine system were agreed to by all the countries represented except Spain, the delegate from which abstained from voting.

As to the Permanent Sanitary Commission, it was to be composed of delegates from each nation, to sit at Vienna, and its principal object was the study of the etiology of cholera and the measures for its prevention, though other epidemics might be included. It was to draw up a scheme for uniform researches in all the contracting states on this subject, and to collect information from all countries, to send missions to places where it was important to observe the course of cholera and at which there were at present no competent observers, and its transactions were to be published. Such a commission, no doubt, would be useful in indicating many of the points which it was desirable to investigate, over a large portion of Europe and the neighbouring continents, at the same time, in arranging a common mode of observation, and in putting the information so obtained in a form that would be generally available; but the proceedings at the Conference at Vienna itself sufficiently show that in such assemblies national feeling and the views of the governments which joined in them necessarily influenced the delegates to such an extent that, though they might agree in some international arrangement to meet the convenience of the different states, they are quite unsuited for settling doubtful points in science.

VI.—Pulmonary Consumption in the British Army.[1]

Modern doctrines as to the nature and affinities of pulmonary consumption have given a new direction to treatment and a new reading to statistics. If phthisis is to be regarded as almost or altogether a preventable disease the main object of the physician must be to investigate in minute detail the circumstances which concur either with its exceptional prevalence or its exceptional rarity.

Such an inquiry is the more to be commended when, as in the

[1] *The Nature and Varieties of Destructive Lung Disease included under the term Pulmonary Consumption as seen among Soldiers, and the Hygienic Conditions under which they occur.* By Francis H. Welch, F.R.C.S., Surgeon Army Medical Department, Assistant Professor of Pathology at Netley.

treatise before us, it concerns a community with no marked predisposition to the disease, and where a common discipline orders the general routine of life upon one uniform plan. Professor Welch has fulfilled his task with great ability and at a great expense of labour. His careful treatise, in striking contrast with some other productions mainly devoted to personal assertion and the vaunting of secret remedies, is hardly capable of compression. We shall confine ourselves, therefore, to the general conclusions of the author, and especially to considering the use which is here made of army statistics in support of the pathological views of the school of Niemeyer.

Professor Welch arranges phthisis anatomically under a number of headings, which are not always easy to identify. Phthisis, no doubt, is related to many diseased states, but the precise manner of the association is not always apparent. To subdivide phthisis into "dysenteric," "diabetic," and "syphilitic" varieties, as though post-mortem inspection served always to distinguish those several forms, is to classify beyond actual knowledge. Such terms may indeed appear appropriate when, after death, the whole circumstances of an individual come under review, and a special life-history is found to correspond with special anatomical lesions, but the means of diagnosis will not admit of any such classification beforehand. The syphilitic and the diabetic are apt to die of other forms of phthisis than those set apart for them. Contracting either disease does not ensure either that the lung shall suffer or that it shall suffer after a particular manner. In the surrender of tubercular and inherited phthisis (a surrender so complete with Professor Welch that he quotes with approval the statement that people "are very exceptionally, if ever, born to die of phthisis") there remain a multitude of external agencies capable of producing the disease. It is quite possible to select the wrong one. Exposure to weather, intemperance, and syphilis are apt to concur in the same individual, while his phthisis may be due to none of them, but to the inhalation of vitiated air. To say nothing of the living, can it be affirmed, even after death, that the character of the lung destruction will decide to which particular "morbific influence" or combination of influences it owes its origin? Anatomical description must confine itself to such names as the pathologist is able to confer from inspection of the organ without knowledge of its history.

It must be admitted that our author, who is above all things careful and precise in his statements, does not insist upon the complete separability of his groups. He seeks practically to distinguish but two phthisis—an affection of purely local origin and extent from phthisis associated with other disease. Regarding the pulmonary affection "as the result of morbific influence on the histological elements of the air-vesicles," he sets himself to discover what

may be the nature of that influence in one section of the community —the army. There is the remarkable fact to be accounted for— a fact verified by statistics—that amongst a class of men carefully selected on the ground of their freedom from disease, and especially from pulmonary disease, there is, as compared with the civil population, "a decided and great excess of consumption;" in the words of Dr. Parkes, "a large amount of consumption is generated in the army."

The affection thus fostered by something or other in which the soldier is placed at a fatal disadvantage, the essayest proceeds to show, has all the characters of a local disease. "The cases do not conform to the rule of inherited diathesis." They are, in fact, purely of inflammatory origin.

"We do not often," he admits, "have the opportunity of tracing the unquestionable caseous mass from the unquestionable inflammatory product. The major part of all lung inflammation has hitherto been regarded as exemplified by acute sthenic pneumonia, equally as all yellow cheesy products have been considered due to tubercular growths, but because preconceived views have hidden from us the antecedent links there is no sufficient reason from negativing the connection of the lung destruction with prior inflammatory phases."

That it is not "preconceived ideas" alone that have hidden these intermediate links will appear presently from the statement that the process itself is concealed or "latent." The author goes on to affirm that inflammatory idiopathic consumption, whether "pneumonic" or "bronchitic," is beyond comparison that form of phthisis by means of which soldiers at least (it may be otherwise with the civilian) are destroyed. The frequency of this form of disease is represented by 73·81 out of 99·79, while "specific textural phthisis," the next in order of frequency, is represented by 11·28.

The occurrence of hæmoptysis as the first element in this idiopathic form of lung destruction is much insisted on. Cautiously, as befits one who rejects antecedent diathesis or constitutional defect, mention is made of "a delicacy of the capillary walls, far removed from a normal state, a want of molecular textural coherence"—we may not call it morbid—"which allows of rupture under causes of congestion." It is further explained that "the structural delicacy of the apex and the pathological tendency evinced by it to prior involvement under deteriorating influences explain the general site of origin of the outpour." The congestion, it is immediately added, is mechanical, due to "compression of the chest-walls from the tunic and accoutrements" as well as to alcoholism and foul air. In the same connection the "irritative action of blood-clot" is dwelt upon in terms familiar to the readers of Niemeyer, while arguments are adduced from comparative anatomy in support of the "delicacy and extra

susceptibility of the apex organization in man." However these allegations may bear the test we would apply to them, it needs only to insist here that the blood clot is not more irritating nor the lung apex more delicate in the army than out of it. Whatever is accomplished by putting "delicacy" and "vulnerability" in place of morbid predisposition, it is obvious that there is less to be got out of the soldier in this way than out of other men.

We consider next the agency of bronchitis. "The mass of army consumption of the bronchitic variety," Mr. Welch believes, "is due to a bronchial lesion which, becoming permanent, progresses backwards to the lobules, which it infarcts with cellular elements, ultimately caseating." "A protracted capillary bronchitis is common in the army." Its termination in tissue disorganization is stated in the words of Niemeyer "to be met with most in weakly and delicate individuals with feeble powers of resistance"—to be met with least, we repeat, upon that hypothesis, in a body of men so selected as the army.

Lobular consolidation being thus reached, whether by the avenue of bronchitis or of latent pneumonia, "such are the surrounding conditions of army life, that the disease once commenced seldom fails to produce disorganization of tissue." "It generally commences in the delicate apices and is bilateral." "A period of latency and abeyance follows the primary destruction." Thus "the bronchitic and the subacute pneumonic variety are the too dominant forms of pulmonary consumption in the army, equally chronic, the former tubal and generally dependent on atmospheric vicissitudes and exposure, the latter vesicular and originating under atmospheric organic foulness."

With this definite statement we reach the main conclusion of the essay and the point to which we would direct special attention. In the army hospital statistics adduced by the author, but which cannot here be quoted, he finds evidence for asserting, first, that "the only forms of disease arranged under the tubercular series numerically important in causing sickness are phthisis and hæmoptysis, the combined remainder being utterly insignificant;" and secondly, "that the cause of lung destruction in the army arises from other conditions than inherent defects, that it is, in fact, gendered." The phthisis of soldiers is a purely local disease, directly produced and fostered by the circumstances of army life.

What, then, are these circumstances? Mainly, says the professor, the impurity of barrack atmosphere. Although other influences are not to be neglected, as dress, food and occupation, yet "nearly a half of army consumption is connected with vitiated barrack atmosphere entirely to be eliminated." "This is the one condition, common to all, capable of explaining the prevalence of lung disease." "A barrack-room atmosphere may be regarded as moist

and foul, with the occasional adjunct of warmth. It combines pulmonary and cutaneous exhalations, products of combustion from lights, fires and smoking, organic emanations from meals, particles of material from clothing, furniture, &c., and inorganic matter from cleansing of accoutrements, dust of floors, &c."

From such morbific influences within doors the change is to the dangers of chest constriction on sentry, where the organic accumulation is aided by imperfect expansion. "A soldier never knows a healthy home until he commits some crime which brings him into the thoroughly ventilated cell of a military prison."

The picture is a gloomy one. It represents the individual at ceaseless warfare with his surroundings and endows with a power for mischief unthought of until lately the fire that warms and the food that was thought to nourish, while the very clothing to obtain which is often the main object of enlistment becomes a source of danger. The British soldier, chosen on the ground of health and subjected to a strict physical training and discipline, which, if it cannot wholly control his conduct, regulates at least tne routine of his life to a degree impossible with the civil community, is the unconscious victim of a set of circumstances arranged for his destruction and from which the only means of escape is by crime. It is matter for surprise that the Netley professor should have to complain that the source whence such grave conclusions are drawn—the Army Medical Blue Book should be "as though it were not."

In commenting upon these statements of Prof. Welch it is not our intention to question his data or impugn his fairness. Yet while the bare fact that pulmonary disease (perhaps even that destructive pulmonary disease or phthisis) is exceptionally prevalent in the army may be capable of demonstration, the precise cause of that event can hardly be assigned positively without great risk of error. With so wide a range of choice as is afforded by the complex surroundings of every human life, this cause or that will be laid hold of by the particular observer in obedience to his pathological bias. To one a special danger will be seen in frequent and abrupt temperature changes, to another the inhalation of organic particles will appear competent to produce just that sort and degree of irritation which by a foregone conclusion is credited with exciting a latent inflammation. In other words, if we are set to seek for a cause of disease, and especially for a cause of phthisis, which shall satisfy our preconceptions of its nature and origin, it is certain that we shall not seek in vain. It is true, indeed, that Prof. Welch makes out a case so far as to show that bad lodgment is a prominent feature of army life, and entitled therefore to be considered in relation with its prominent disease; but it is also true that this author and his school, rightly or wrongly, lay a special stress on the evils likely to accrue

from breathing a vitiated air. Whatever the influences to which the soldier is exposed, it is manifest that we must come to an agreement (and this not upon hypothetical grounds or mere statement) as to the measure of harm of each of these before we can say it is the soldier's barrack which gives him phthisis, or it is his sentry work, or his cross-belts. Given any one of these, I can construct its mode of operation; but in presence of them all I know not which to prefer until I know under which pathological school I am to serve. There is the reason—*my* reason—why a particular agency should act in a particular way, and there is the fact that it does or does not so act.

And suppose the fact made out. Grant that intemperance, or bad dwelling, or night duty, one or other of them, is specially prejudicial. Causes like these are complex. If resolved into their elements the bewilderment is only made the more apparent. Suppose, for instance, that alcohol is a cause of phthisis, and even (which, in fact, it is not) of a special form of phthisis. Does it become so from its direct action as a poison, or from impairing nutrition and promoting tissue degradation, or, incidentally, from the mere accident that the drunkard is reckless of exposure and insensible to external impressions? How much is due to the alcohol, and how much to the conduct which the alcohol is apt to produce?

It is quite fallacious to reply that the harm of alcohol is the sum of all these separate harms to which it exposes its victims. The subjects of "alcoholism," as no one in this country needs to be told, admit of considerable subdivision. Between the youthful drunkard whose attacks are acute and periodic, impressing profoundly for the while the nervous system, and casting the sufferer on the pathway or into a ditch, to undergo unconsciously the process of "surface cooling," and the cautious old tippler, who is never drunk and never sober, there are many gradations. It can hardly be that men who take their liquor in such divers fashions will all suffer from it similarly. As regards the disease we are considering, either the reckless exposure, which belongs to one form of drinking, or the interference with nutrition, loss of appetite, and gradual poisoning, which belong to another form, will be the chief means by which alcohol is capable of mischief. Not the mere fact of drinking, therefore, but the particular kind of drunkard has to be ascertained, before the source of the evil can be traced. It is highly probable that one mode of intemperance is especially prejudicial to one person and another to another.

It is the same with uniform dress and vitiated barrack air. As contributing to lung disease these agents may be adduced under a number of aspects. To credit the former with a tendency to produce "an accumulation of effete dead particles," and the other with supplying these particles, is only one view of the matter.

It may be said, indeed, as our author says, army life is conspicuous for the vitiated air of barrack rooms; it is conspicuous also (and *pari passu*, comparing one branch of the service with another) for the generation of phthisis. There is a probability that these two things are related, that the foul air is *in some way or other* a cause or part cause of the lung destruction. It is one thing, however, to conclude upon sufficient evidence that the cause of excessive phthisis resides somewhere in a complex set of circumstances, and another thing to help oneself to one factor in the combination and upon *à priori* grounds to assert that the sum of the mischief is due to it. It is necessary for what follows to lay stress upon this distinction, which, however obvious when stated, is often lost sight of in the anxiety to appropriate material for pathological use.

Abandoning, then, all preconception, and accepting the bare fact that pulmonary disease is gendered *somehow* in the army, we are set to seek the special circumstances which distinguish military from civil life. Is the breathing of vitiated air thus special? However noxious the soldier's barrack atmosphere, he does not breathe it throughout the twenty-four hours. Comparing his lot with that of the mass huddled together in our great towns, he is at least better off than his brother artisan, who exchanges the poisoned atmosphere of a crowded lodging for the dust-laden air of the factory or warehouse. True, Professor Welch finds in this very exchange a further source of mischief, and shows how the accoutrements and fit of tunic, "with active exertion on parade, go hand-in-hand with barrack-room impurities in predisposing the parenchyma to minor degrees of ordinary irritation;" yet taking each agency by itself, which seems the only feasible method, it is fair to insist that the less of each the better. The soldier's condition, then, in reference to the particular influence which is credited with the greatest power for mischief is certainly not the worst.

Nor is it altogether irrelevant to test this matter by an appeal to that common knowledge of a common disease—inexact, indeed, and not reducible to figures—yet which may still assert itself in face of statistics derived from a special case. If foul air be so potent a cause of phthisis, many things should happen which do not. Phthisis would come to reside in certain quarters of the town; with a fixed population it would become, indeed, here and there endemic, just as, to the discredit of our civilisation, typhoid is endemic. Moreover, the women confined in this bad air throughout the day in household duties would suffer more than the men who went abroad. Nothing of the kind is observed. It is not necessary, therefore, to deny the evil effects of vitiated atmosphere, only to prevent the argument derived from it from being overstrained and made to tell more for one branch of the community than for another, to bring death to the soldier in England

while it is innocuous to the Italian peasant, "who breathes freely," says a countryman of his own, "in air that stifles other men."[1] It is to be remembered at the same time that many of our own indoor trades which are conspicuous for phthisis—steel grinding, carding cotton, hackling flax, do not so suffer from confinement *per se*, but in a way of their own from the direct spoiling of delicate lung tissue by a material agent.

We have yet to consider, therefore, whatever is *special* to military life—those duties which the soldier has to perform in an exceptional way, with less freedom of choice and more regard for a pattern than his neighbours. In the treatise before us this point is not overlooked. At least sufficient emphasis is given to the evils likely to arise from chest constriction due to a regulation dress and the consequent "accumulation of effete dead particles." But this is no more than a partial view of the matter. The conditions of military service impose much more than this. They differ, indeed, so obviously from those of citizenship as to be easily convertible, hypothetically, into efficient causes of lung disease all evidence apart. Consider, for a moment, the restrictions of parade, the marching pace, regulated, not by the sensations of those set in motion, but by orders in a book, the uniform clothing, distributing to all an equal amount to be worn after one pattern, irrespective of individual peculiarities. If one were set to seek by mere contemplation of the matter (joined with common knowledge, easily acquired in this country, of the ways of catching cold) for the causes of lung disease in the army, we should surely find it in that inability of the individual to accommodate himself to external circumstances which is only another expression for military discipline. Acquired phthisis should be found in the army most because there the personal surrender to an arbitrary standard in living and moving and having one's being is the most complete. Thus, for example, if the risk of exposure to weather be granted, this risk will be the greatest with the soldier, since he alone of the community, owing to the nature of military service, is often without the power of meeting the circumstances by change of pace or extra wrapping or appropriate muscular exertion.

It may be said, perhaps, that army service is not more deleterious than service of other kinds. In all combined labour the standard requirement disregards individuals and has almost necessarily its victims. In many trades, as with lead workers, silverers, needle-grinders, the mode of the mischief is obvious and measurable. But although these suffer most and by a poisoning so palpable that it may be traced from the material they handle into the very texture of their bodies, they do not suffer alone. The association of men in a common task must always involve a strain which will make

[1] Gallenga's 'Italy Revisited,' p. 10, i.

itself felt in this way or that according to individual susceptibilities. Whatever the form of coercion, whether handicraft or the requirements of military discipline, or the despotic ruling of fashion, some must suffer—there will be a survival of the fittest. According to the nature of the work, the least muscular or the least stable in nervous temperament or those with the most "delicate lung apices" are overtaxed. It is hardly an exaggeration to say, with our complex life and keen rivalry, that under the operation of this law hardly any escapes. Who of us is not assisting in the spoiling of his own life in the effort to accommodate himself to the rules of his trade or the habits of his class or of the society which either voluntarily or at the desire of his wife he frequents? Should any seek deliverance by the exercise of a rare caution and obeying the laws of health as formulated for his benefit in a rapidly growing literature, so singular a mode of life would hardly fail to operate to the prejudice of the mind, if, indeed, it did not involve the cruellest coercion of all.

There is a danger, no doubt, of being carried away by such considerations, of giving them undue weight or mistaking either the manner or the extent of their application. Yet admitting the principle, it seems probable that it would operate to the detriment of the soldier, so as to produce the results actually observed. The absence of individual choice in such matters as clothing and exposure and night duty, if injurious at all, would be injurious by producing pulmonary disease. But while common experience justifies this general conclusion, can we go further and determine upon observation either the exact mode in which the result is accomplished or the exact nature of the result itself?

The subject of night sentry duty is just now occupying the public mind in connection with assertions as to the growing unpopularity of the service and difficulty in obtaining recruits. Professor Welch, agreeing with the Commissioners of 1859, does not lay great stress upon this point. He admits, however, that a certain deleterious result is traceable to night employment *per se*; "a hot guard-room, sleeping on a bench in a great coat and tunic, and roused from probable perspiration to perform a 'monotonous' to-and-fro 'sentry go' in all weathers, can hardly fail," he observes, "to conduce to surface chilling and pectoral disorders." Yet, inasmuch as he finds similar duties imposed upon the police (not without leaving its mark, however), he declines to recognise this night duty as producing the excess of lung disease in the army as compared with the civil class.

While thus Professor Welch, not altogether free from the bias of a theory, reserves the force of his figures for a vitiated barrack atmosphere, it is impossible to overlook the strong evidence which his own statistics afford of the truth of the popular belief that night sentry duty is provocative of pulmonary disease. The comparison between the sentry and the policeman seems, indeed,

hardly relevant. The most conscientious policeman has variety of scene, cheerful companionship and conversation, with the pleasing sense of performing a useful and, it may be, perilous duty. The sentry, confined to his beat, solitary and vacant, is often employed upon a service which he knows to be useless, as when Her Majesty's Guards (conspicuous, even in the army, for pulmonary disease) keep watch over clown and pantaloon at the Drury Lane pantomime. It is in regiments doing the heaviest sentry duty that the mortality and (what is more to the point) the *invaliding* are the highest. "In the foot guards," says the last official report on the health of the army, "the highest ratios of admissions (to hospital) of deaths and of invaliding are combined in the 1st battalion of the Grenadiers, stationed in London and at Windsor."

It is easy, of course, to deride the maternal care and anxiety of the doctor, who sees peril in the act of standing at attention, and would issue clogs and umbrellas for army use. With a people as indifferent to danger as they are sensitive to ridicule, banter of this kind is singularly mischievous. The question is not one of avoiding necessary danger, either in peace or war, but of ascertaining its nature and the direction whence it comes. Every calling has its inevitable hazards, but in the army, as in the mine and the factory, these may be reduced to a minimum by suitable precaution.

Allowing, then, that the circumstances of the soldier's life are adequate to explain the exceptional frequency of pulmonary disease in the army, it remains to be considered whether the nature of such disease can be more definitely described—whether, in a word, it is always, or almost always, *phthisis* which is thus frequent.

Now, it must be remembered that the loss from sickness in the army is, not from death, but invaliding. It is so described in this treatise. By a slow process the soldier gradually becomes unfit for active service, which he accordingly leaves and is lost sight of. Of the later stages of his affection, therefore, little is seen. It is hardly a safe inference that phthisis (meaning thereby lung destruction) is the end in every case. Professor Welch, indeed (in a note on p. 22, which deserves particular study), admits that "the statistics do not express the actual numerical frequency of lung destruction. No small amount of early and unpronounced disease," he adds, "finds a refuge under the heading 'bronchitis.'" The author endeavours to show that this is in fact an error. That it is an incomplete statement we may well believe; yet, inasmuch as but a small proportion of the whole number of cases came under his own eye, his correction is founded on hypothesis. We must accept the fact, however. It is easier to convict the army of pulmonary disease than of phthisis.

Of what may be the effect of atmospheric exposure we are not

without evidence in civil life—evidence which has the additional value of being complete. The individual remains under observation throughout, from the time when the bronchial tubes first become susceptible, and he strikes work through years of suffering to the end of his life and the post-mortem inspection of his lungs. But he does not suffer in the way here set down. What is the teaching of the out-patient room? Out-door labourers form the bulk of that constituency. Tracing their first attack of bronchial catarrh to some definite exposure, they will return, winter after winter, with the regularity of migratory birds, until, little by little, each attack getting longer than the last, their whole year is disturbed, they become permanently bronchitic, emphysematous, and finally "asthmatic" old men.

So large is the number of these patients at every metropolitan hospital, so uniform is their history and conduct, so obviously is their affection started, not once, but every time, by the advent of cold weather, that it would be absurd to deny that they represent generally the consequences of atmospheric exposure. They illustrate, indeed, abundantly that susceptibility of the bronchial membrane and ever-lessening power of resistance to which Mr. Welch calls attention in the case of soldiers. Yet phthisis is rare with this class; rarely is their affection localised or any abiding preference shown on the part of the catarrh for one lung rather than the other. There is, indeed, one spot which notably escapes these periodical attacks—it is this very apex which in the case of the soldier, as with the snake and tortoise, is credited with a special delicacy. So notable a thing is it that the breathing apparatus subjected to severe weather vicissitudes is wont to respond after this manner that, except for the present purpose, it would be inexcusable to dwell upon it. It seems pertinent to ask whether this form of affection—chronic bronchitis, namely, with more or less emphysema—which counts for so much amongst the labouring classes, is sufficiently recorded for the army. Whether "the large amount of unpronounced disease which finds a refuge under the heading 'bronchitis'" is entitled to any other shelter.

Whether this be so or not, our acceptance of Professor Welch's conclusions in their entirety, is not rendered easier from learning that "the primary lung destruction is followed by a period of latency and abeyance of diseased action," and that this stealthy process, set going either by organic foulness or atmospheric exposure, selects a particular part of the lung, owing to a natural delicacy of organization, witnessed to especially by Dr. Allbutt. Of a process which thus works in secret and owes its destructive character to its latency it is obviously difficult to speak precisely. Who shall say that the very first step towards phthisis is represented by a catarrh which is itself unperceived, or venture to distinguish between the arrest of

the disease by "rational treatment" and that period of abeyance which is its most fatal characteristic?

But the opinions we have been following have a far wider range than that to which our special subject would confine them. From what is observed of the fate of the soldier it would follow upon this showing that the yearly sacrifice of life from phthisis amongst those who from their worldly circumstances are able to order their own goings is to a great extent a voluntary sacrifice. "People are very exceptionally, if ever, born to die of phthisis." Of the preventability of phthisis in this unrestricted sense, in obedience to the doctrine that it originates in a local "idiopathic" inflammation, we have not now space to speak. It was necessary when the old notion of new formations was abandoned and the crude tubercle of Laennec was recognised as an inflammatory product whose elements were destined to undergo degenerative changes, and by mere pressure and bulk mechanically to arrest nutrition, that in response to such views a therapeutical system should arise proposing to order and adjust undue catarrhal proliferation and quell the disorder before it made head in destructive riot. The therapeutics of the school of Niemeyer are in perfect harmony with its pathology.

The time will come when we may fairly ask what is the practical outcome of these opinions and demand proof that phthisis is prevented in addition to assertions that it is preventable. Meanwhile the question involved is of the widest application. So soon as we pass the limits of mere anatomical description it meets us at every turn. Is disease to be constructed, after a method which we can arrange and observe, from the operation of certain elements of the body or material supplied from without, or are these elements and this material but the obedient instruments of a directing agency which itself escapes analysis and takes refuge in such names as "predisposition" or idiosyncrasy" or "diathesis"? As regards the present subject it appears to us to be the radical defect of Professor Welch's view, as of the school of which he is so able an exponent, that it regards the individual too little and his histological elements too much. Observing in these a potentiality to undergo and induce certain changes, it neglects to consider that such changes are not determined by the mode of aggregation or method of imprisonment of the cell elements, but by a controlling vitality to which mere physical agency is subordinate. It is no true picture which thus represents the individual as a theatre for certain pathological events which, with the circumstances duly arranged, may be expected to take place after a manner to be announced beforehand. It would be truer to describe him as himself, the actor, provided with material of which the general nature and tendency is known, but which must take its shape from the character of his own genius.

VII.—Recent Works on Diseases of the Heart.[1]

When Marey published his great work, 'Physiologie Médicale de la Circulation du Sang,' he gave an impulse to the study of diseases of the heart greater than any since the discovery of auscultation. The application of the graphic method to the study of the movements of the circulatory apparatus lent a new precision to the observation of phenomena and indicated fresh directions for fruitful inquiry. Familiar facts presented under a novel aspect awakened fresh interest, and the relations and interdependence of the features of arterial and cardiac movements were rescued from obscurity and recorded with wondrous ingenuity. This renewal of physiological inquiry soon reacted on the younger school of physicians, and a new era began in the clinical study of diseases of the heart. The old learning had been ably expounded by its great master, Stokes, and also by Walshe and Fuller. The new dispensation of modern physiology, however, required new prophets. In Germany and France the new learning has found able exponents in Von Dusch, Jaccoud, Raynaud, and others, but in this country the books before us found the work as yet undone. In the following pages we shall see how much of it they have accomplished.

The three books, oddly enough coming from the capitals of the three kingdoms, are remarkable contrasts in every way. The London book is modelled on the old treatises, and is both in its style and matter an anachronism. The Dublin book is encyclopædic in its fulness of information, and yet practical and well abreast of modern thought. "The time of life is short," too short we think, for books of 1200 odd pages on a special organ. The Edinburgh volume is a clinical rather than a systematic treatise, dwelling at length on some topics, while others equally important find little or no space. A book not equally well developed all through, but in parts full of valuable observation and careful thought.

In Dr. Hayden's book we find a first chapter devoted to the physiology of the heart, which for comprehensiveness surpasses any similar chapter in the language. It is invaluable for reference, and

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[1] 1. *A Manual of the Diseases of the Heart, their Pathology, Diagnosis, Prognosis, and Treatment.* By Robert Hunter Semple, M.D., Physician to the Standard Life Assurance Company, Physician to the Bloomsbury Dispensary and to the Eastern Dispensary, London, &c. London, 1875.

2. *The Diseases of the Heart and Aorta.* By Thomas Hayden, Fellow of the King and Queen's College of Physicians, Physician to the Mater Misericordiæ Hospital, &c. Illustrated. Dublin and London, 1875.

3. *Clinical Lectures on Diseases of the Heart and Aorta.* By George William Balfour, M.D. St. And., F.R.C.P. Ed., Physician to, and Lecturer on Clinical Medicine in, the Royal Infirmary, Edinburgh. London, 1876.

contains a number of diagrams and tables which cannot be found elsewhere. To show the untiring care of the author, we may mention that he has given no less than sixteen woodcuts of the various modifications of Lancisi's cardiac cycle; the sixteenth, we need hardly add, is his own, and gives a very perfect diagram of the cycle according to modern physiological knowledge. Dr. Balfour has published in his book a seventeenth attempt of this kind, but we prefer Hayden's.

As might be expected, all our authors devote a considerable space to that form of valvular disease, "mitral stenosis," which has been so much discussed of late years. Even Dr. Semple has carefully worked up the subject and writes moderately well upon it. The single case he records will, however, be hardly accepted as a contribution to the knowledge of the physical signs of this affection, seeing that the existence of a presystolic murmur is inferred solely because in a case of double aortic disease there was a "double murmur over the apex of the heart, the first before the radial pulse, and the second synchronous with it." The history of the recognition of the murmur of mitral stenosis gives Dr. Hayden the materials for a very excellent chapter. He shows that as early as 1824 the existence of this form of valve disease was diagnosed from the presence of a presystolic murmur by Bertin, and that nineteen years later, in 1843, Fauvel wrote a very clear and able account of the murmur, calling it by its now generally received name, presystolic. In this country Dr. Markham seems to have been the first to describe it, and seven years later Dr. Gairdner's well-known paper appeared, in which the name auricular-systolic was applied to it, and its intimate dependence on the auricular systole advocated. Since then the murmur has been gradually winning recognition, though it is still frequently confounded with the murmur of mitral regurgitation. Many causes have contributed to this slow acceptance of the character of a murmur, which to the adept in cardiac auscultation seems one of the most easily recognised of morbid heart sounds. The variability in the length of the murmur, its vanishing power, the peculiar flapping character given to the first sound by the altered mitral valve curtains rendering it very like an accentuated second sound, and the rapid irregular heart action so often present, are all conditions which have retarded the recognition of the murmur. The habit of timing the murmur by the radial pulse was another fruitful source of error, as the interval which is known to exist between the radial pulse and the ventricular systole was regarded as sufficient explanation of the occurrence of the murmur before the pulse.

Dr. Hayden gives a synoptic sketch of the modifications of the murmur, which he classifies as follows:

"1. The typical presystolic murmur, which answers to the pre-

ceding description, and is characterised mainly by being *prefixed* to the first sound, and separated from the preceding second sound by a long interval of silence.

"2. Next in frequency of occurrence to the preceding, often succeeding to it, and representing a greater degree of contraction of the mitral orifice, is a murmur of double rhythm, or broken up into two fragments, one of which adheres as a prefix to the first sound, and represents the ordinary presystolic murmur, whilst the other succeeds the second sound, being appended to it as a *suffix;* these two fragments being separated by a brief period of silence.

"3. A protracted murmur, extending backwards from the first quite to the second sound, covering the whole period of the long pause, and representing a further development of the preceding disintegrated murmur by a bridging over of the interval of silence, and union of the two fragments, and indicating a still further degree of contraction of the orifice.

"4. A murmur extending not backwards but forwards, and so completely fused into the first sound that it seems to be but the emphasised initial portion of it. This form indicates a much contracted and very rough state of the orifice."

Dr. Balfour agrees in the main with this classification, making, however, a simpler division into three kinds, viz. (1) a short, rough murmur, running up to the first sound and ending abruptly with it; (2) a prolonged murmur, commencing immediately after the second sound and ending with the first sound; and (3) a double murmur, of which part follows the second sound and is separated by a short interval from the rougher presystolic portion.

This division is, we think, preferable to the other. The murmurs here described really consist of a true presystolic portion and a diastolic portion. In the first case the presystolic portion is alone heard, in the second there is a prolonged murmur ending roughly with the first sound, and in the third this murmur is separated into its two parts, the diastolic and presystolic portions, by a short interval. Dr. Balfour refers the short, rough, presystolic murmur to a diaphragmatic condition of the mitral valve, and says truly enough that it is a murmur that often disappears. We ourselves do not quite agree with the first statement, as we have met this short rough murmur in cases of funnel-shaped valve, and we must consequently differ *toto cœlo* from the author when he describes the funnel-shaped valve as the form likely to give rise to the long murmur. In these funnel-shaped valves the soft short or rough and short murmurs have, in our experience, been very common. We are glad to have Dr. Hayden's support in this view. The short, rough, grinding murmur preceding and running up to the first sound is, however, the murmur usually heard with the roughened diaphragmatic mitral. That regurgitation occurs in many of these cases we freely admit, but Dr. Balfour puts this too strongly, and leads the reader to conclude that in most cases of mitral stenosis

there is free regurgitation. We would rather say, that in many cases of mitral regurgitation there is some narrowing of the left auriculo-ventricular orifice, but we must regard any attempt to call these cases mitral stenosis as ill advised, and likely to perpetuate the confusion which has hitherto prevailed on this subject. In the present state of cardiac pathology it is better to educate observers to recognise presystolic murmurs and systolic murmurs as separate classes, especially as in very many cases of mitral narrowing there is no systolic murmur whatever to be heard.

We find much interesting matter devoted to the consideration of dynamic murmurs in these two volumes. Dr. Hayden, in discussing the murmurs heard in the mitral area in cases of chorea, disposes very conclusively of the hypothesis of structural changes in the mitral valve in these cases, and of that secondary hypothesis of embolism as the cause of the chorea. The connection between the disorder and rheumatism has, he thinks, been overstrained. The rarity of any changes in the mitral valve and the extreme improbability of the occurrence of any irregular or choreiform action of the papillary muscles lead him to refer these murmurs to a softened or atonic condition of the walls of the left ventricle. In this he is in close accord with Balfour, who, adopting the same views, attempts to extend very widely the domain of these dynamic mitral murmurs. In his chapter on curable mitral regurgitation this author endeavours to reduce all so-called hæmic murmurs to this class, and to show that the so-called hæmic murmur, usually regarded as developed in the aorta, is nothing but the murmur of mitral regurgitation conducted towards the base by the left auricular appendix. This view, which is that of Naunyn, is expounded at length and very ably supported. At page 165 we read—

"The defective nutrition of the cardiac muscle, depending on the depraved constitution of the blood, has resulted in relaxation of the muscular tissue and dilatation of the ventricular cavity, this producing imperfect closure of the mitral valve, and hence a mitral murmur which has its position of maximum intensity at one time in the mitral area, and at another in the auricular area, according to circumstances which have been referred to, but which cannot as yet be adequately explained, while these murmurs are propagated in greater or less intensity into the pulmonary and aortic areas. When the normal condition of the blood is restored, the cardiac muscle recovers its tone, the mitral valve again closes perfectly, and all signs of a cardiac affection disappear."

Again at page 169 we have:

"The arterial murmurs are, therefore, not of independent origin, but are propagated from the mitral valve, which permits slight regurgitation between its segments, which are unable to close perfectly because the ventricular lumen is somewhat dilated. The

reality of this regurgitation is further proved by the accentuation of the pulmonary second sound, which is always present in these cases."

This last sentence is in direct opposition to the opinion of Dr. Hayden and most authorities, who say that in these dynamic mitral murmurs the signs of pulmonary congestion are often absent and the accentuated pulmonary second sound usually wanting. We must say that in our experience mitral regurgitation of the curable sort, in other words, mitral regurgitation depending on a weakened ventricle from spanæmia, has seldom been associated with accentuation of the pulmonary second sound.

Before passing away from the murmurs heard in the pulmonary area we must call attention to Chapter VIII of Dr. Balfour's book, "On Murmurs Audible in the Pulmonary Artery." Here we have a very excellent account of Quincke's views on the production of a systolic pulmonary murmur when the heart is uncovered by the retraction of the left lung. It would seem that the heart in its systole compresses the pulmonary artery, and the murmur arises from the formation of fluid veins at the compressed part. In cases of this kind the pulmonary artery can be seen beating, and it would seem that the return of the lung to its normal position is sufficient to remove the murmur, the soft lung cushion, as it were, coming forward to prevent the compression of the artery.

Dr. Balfour supports Quincke's views by his own cases; Dr. Hayden, on the other hand, objects that this murmur should always be present when there is visible pulsation in the pulmonic area, a criticism which Quincke by anticipation disposed of by appealing to the well-known fact that murmurs are often inexplicably absent when all the known conditions for their production are present, *e.g.* mitral stenosis. A second answer is contained in the statement that a very slight prominence of the chest-wall may obviate the compression of the pulmonary artery. In this difference of opinion we feel that Balfour has the best of it, and we therefore regret the more the tone in which he refers to his opponent in his notes. However well an author may be satisfied with himself and his opinions, he should endeavour to conceal his sense of superiority.

In his two chapters on incompetence of the aortic valves Dr. Balfour makes some very interesting observations and speculations as to the relation between the character and propagation of the murmurs and the amount of mischief at the valves. If the diastolic murmur is "inaudible in the carotid arteries, it is," he says, "invariably accompanied by a systolic murmur, having its maximum intensity at the aortic valves or in the so-called aortic area, and this indicates comparatively trifling incompetence with considerable obstruction at the aortic orifice, most probably from calcified semilunar valves. If this diastolic murmur be distinctly audible in the carotid arteries, it is invariably preceded by a loud systolic murmur

in them, the systolic portion of the murmur being not always very audible in the aortic or in any part of the cardiac area, and this indicates very considerable incompetence with comparatively trifling obstruction." We cannot help thinking that if Dr. Balfour had made himself master of the sphygmographic records of cases of this kind that he would have found in the modifications of the upstroke of the pulse trace, and in the changes in the dicrotic wave much more trustworthy indications whereby to measure the amount of obstruction or incompetence at the aortic orifice. Moreover, the presence of the diastolic murmur in the carotids is much too freely assumed. It is really not a common occurrence, and when heard may be manufactured by the stethoscope. To quote Hayden—

"Independently of the teachiug of actual experience, it is scarcely conceivable, on theoretic grounds, that a murmur of low pitch, as that of aortic patency certainly is, should be transmitted to a point so distant, *against* the current by which it is produced."

The elucidation of the means by which valvular lesions are compensated is one of the most valuable pieces of modern work in cardiac pathology. The old notions which regarded the hypertrophy as the great evil have been completely dissipated by the patient labours of German observers and the clear exposition of modern French authors. In these volumes the mechanism of the compensation of aortic lesions is well treated, and readers who are not acquainted with the writings of Friedreich, Jaccoud, and Von Dusch will find much novel and interesting matter. Dr. Balfour, in discussing the changes in cardiac nutrition consecutive to aortic lesions, very fairly joins issue with Jaccoud and others on the ground that they have too strongly insisted on the vital aspect of the subject. As he points out, when a patient with leakage through the aortic valves is in the erect position the interior of the ventricle, according to Pascal's law, "is being constantly dilated by a force equivalent to the weight of a column of blood the height of the cranium above the heart, and of the diameter of the ventricular lumen. From the moment that leakage is once established till the cessation of life the hydrostatic pressure never ceases, though it may be modified by position." Applying these considerations to his therapeutics, Dr. Balfour strongly insists on rest in the recumbent posture in the treatment of aortic insufficiency. This application of the physical law, notwithstanding its ingenuity, does not command our assent, for practical experience does not confirm the value of recumbency in aortic cases. It is a matter of very common remark that these patients more than any others are injured by rest in bed. Many a case failing day by day while confined to this position has found health and strength and greater length of days by disregarding the cautions of his doctor and practically ignoring his heart disease. Dr. Stokes gives a remarkable case of this kind, and

every physician knows how these aortic cases rapidly decline once the compensation is upset by the necessity of recumbence during some acute attack. It is somewhat curious, too, in face of this law, that these patients experience difficulty in lying down sometimes as the first warning symptom of any cardiac trouble, long before any pulmonary congestion or œdema can account for the inconvenience. The absence of dyspnœa from a long portion of the life-history of aortic incompetency is a fact which shows how long the compensation is maintained and pulmonary congestion avoided. Dr. Balfour has hardly laid stress enough on this point, we think, and is too much inclined to antedate the occurrence of mitral regurgitation. Possibly he has been led to this by his views on the effect of dilatation of the ventricle as a frequent cause of mitral leakage. The great capacity of the left ventricle in these aortic cases and the healthy action of the mitral valve, notwithstanding the dilatation, even through many years, is a pregnant commentary on these views. There is considerable diversity of opinion in these books as to the use of digitalis in aortic cases, though in other forms of heart disease opinions are pretty well agreed. Some authorities would regard aortic insufficiency as a nearly absolute contra-indication to the use of the drug. Balfour, however, regards it as the most valuable remedy in this lesion. It is, indeed, hard to reconcile these views. Hayden says—

"Given as the tincture, and combined with iron and ether as previously suggested, I have rarely known digitalis to cause unpleasant symptoms; on the contrary, in numerous instances it has been followed by the most favorable and satisfactory results. I have, in a few instances, been under the necessity of suspending the use of digitalis owing to threatened syncope; but in the cases referred to there was fatty degeneration of the heart, in some instances with, and in others without, disease of the aortic valves."

Dr. Balfour, however, will not allow the fatty degeneration to be the explanation, as he quotes a very good case in which the drug was most useful, and says, speaking of fatty heart, "I have never seen any reason to withhold digitalis, and have always hitherto been rewarded by considerable and well-marked improvement." There is possibly one other explanation of the different results arrived at by different observers on this point, viz. that, judging from his prescriptions, Dr. Balfour gives digitalis very frequently in combination with ammonia. This, like the ether of the other author, may serve to lessen the slowing effects of the drug. Our own experience of digitalis, given alone in cases of failing compensation in aortic cases, is not satisfactory. We have rarely seen cases benefited, we have seen many made worse by each successive dose, and in a disease in which syncope is so grave an occurrence the least risk of this should check the prescriber's hand. One or two full

doses of the tincture, as recommended by the Scotch author, are, however, worthy of a trial in these cases of threatened asystole. As regards the general therapeutics of heart diseases, both authors might with advantage have given more space to that which is after all the most important part of their subject.

In the foregoing remarks we have given a sufficient outline of the style and value of these works. Many other topics, equally interesting with those we have touched on, are discussed in their pages. The chapters on aneurysm in both authors are well worthy of attentive study. Hayden's table of cases is a mine of valuable knowledge, and Balfour's observations on the use of iodide of potassium will induce many to test the value of the drug. Other chapters, such as those on blood-clots and neuroses of the heart, suggest much that we would willingly refer to, but time and space fail us except for a general opinion on the volumes.

Of Semple's book we will not speak, except to say that we cannot recommend it as a modern exposition of our knowledge of cardiac pathology and therapeutics. Scattered through its pages there are some sentences of value. These chiefly consist of shrewd observations on prognosis, but their number and value are insufficient to justify the publication of the volume. It is difficult to make any comparison between the other two books. Each is good in its way, each might be better. They are, however, books that no modern library should lack.

The work of Hayden is at once the most learned and most comprehensive on the subject, but it is cumbrous, and in parts the mass of information it contains is ill-digested. The bad habit of the Irish school of quoting the name of an authority in every instance has been followed by the author too closely, and the effect is to leave the reader too often with much information, but little perception of its practical bearing. There is a want of direct practical outcome from many of these pages of learned writing. These are not grave faults, if faults at all, in a great work of this kind. Now we have it, we should like the author, instead of publishing a second edition, to write a small work of 300 pages conveying to us the pith of the 1222 pages and 150 cases he has so industriously and ably placed before us. When he does this, as we hope he may, a more extensive study of German and French writers will add to the value of his labours.

In this foreign literature, as we have shown, Dr. Balfour is quite at home, and in his book he has placed before English readers some very valuable German work. Many of his chapters contain much interesting matter of this kind illustrated by well-recorded cases of his own, and enriched by pertinent and thoughtful criticism. Some subjects are, however, hardly touched on sufficiently, as, for example, aortic stenosis and malformations of the heart. Omissions of this nature render the work less complete than it should be, to

fill the same place in our literature as the works of Friedreich or Von Dusch in Germany. Generally speaking, the style is good and the information is clearly conveyed, but occasionally, when a sentence runs over twenty lines, lucidity is hardly the author's characteristic. In print this tendency to long-windedness is not so very serious, but to the students who heard these lectures it must have diminished their value. In a successful teacher brevity of expression and a certain amount of dogmatism are essentials. The latter quality we do find here, not in its happiest form, but as an air of schoolmasterly superiority which irritates the reader. These are slight flaws in the midst of so much valuable matter. A new edition will, no doubt, correct them, but at present they lessen the authority of a book which by its original matter and able construction is well worthy of the author's reputation.

VIII.—On Diabetes.[1]

THE history of the art of medicine is a study singularly full of interest and instruction, seeing that its course has not followed that of the fine arts on the one hand, nor of the pure sciences on the other, but has followed a somewhat peculiar course of its own. While medicine has gained much in periods like our own, when the spirit of positive accuracy is encouraged, at the same time it has lost some of that virtue which roots itself in the accumulations of unverified experience; for, like many another shy plant, it may thrive on a heap of soil by the way side, while it wanes and withers on scientific composts. On the other hand rule of thumb has no certainty in present time and few gifts for the future; it may serve a man well for a while, or it may in some measure animate the work of a school, but sooner or later the recurring error it contains destroys its life, and it is better perhaps buried and forgotten than surviving as a dead and rigid tradition. Medicine in some cen-

[1] 1. *Diseases of the Kidney and Urinary Derangements.* By W. HOWSHIP DICKENSON, M.D. Cantab., &c. In three parts. Part I. Diabetes. London, 1875.

2. *Beiträge zur Pathologie und Therapie des Diabetes Mellitus.* Von ED. KÜLZ. Th. I, 1874. Th. II, 1875. Marburg.

3. *On Urinary and Renal Diseases.* By W. ROBERTS, M.D. 2nd edition. London, 1872.

4. *De la glycousrie ou diabète sucré, du son traitment hygienique.* Par M. le Dr. BOUCHARDAT. Paris, 1875.

5. *On the Relations between Diabetes and Food.* By A. S. DONKIN, M.D. London, 1875.

[To the works cited herein I should add a reference to the admirable essay by Dr. Salomon entitled 'Geschichte der Glykosurie von Hippokrates bis zum Anfange des 19 Jahrhundert.' I came upon this essay when the present article was roughly written, and I am indebted to it for several corrections and for some farther help. My own article, however, was, as I say, in a sense complete before I met with that by Dr. Salomon.—T. C. A.]

turies has gained by the tact, observation and insight of individuals — of such men as Hippocrates, Aretæus, Galen, Sydenham, Cullen, Graves; in others by the permanent scientific advances won by such men as Vesalius, Harvey, Morgagni, Bichet, Hunter; not that the former were without science, or the latter without art, but that medicine benefited by them variously. There is not this same variety in the history of the fine arts which seem to flourish best in obscurantist times and sects; nor in the history of the pure sciences, which wholly suffer in times when premises are unquestioned. Nowadays the curious survival and temporary reanimation of mediæval theology and forced æsthetic sentiment, which seem a blessing to fine arts and which are a curse to pure science, may add a warmth and colour to the art of medicine which cannot live by the bread of science alone. On the whole, then, the art of medicine has grown more continuously, and has found some food in more centuries than perhaps any other subject of human knowledge, save always the art of law and government, with which it has some community of nature. Both subjects have always gained by scientific handling, though neither of them, as practical arts, can be satisfied with positive treatment alone. The history of diabetes is an admirable sketch of the history of medicine writ small, and few subjects present more vividly the strife of man with circumstance, the early ignorance of the place of the evil and the form of it, then the awakening consciousness of its presence, the baffled first efforts to learn its nature and to defeat it, the long times of disheartening and even of apathy, the times of advance and of partial victory, and above all that which never fails, the old, weary and unintelligible waste, waste of precious burdens of human life, which age after age fall like the leaves while the secret of safety is yet hidden. The old story of the cry of death with none to answer.

It is somewhat remarkable that diabetes is undescribed in the Hippocratic writings, and that it was probably unrecognised by the early Greeks. It is not only that diabetes is unnamed in the Hippocratic writings, but that no sketch of a disease suggestive of diabetes is to be found therein. If we may reason from the geographical distribution of the disease in later times, it seems clear that diabetes must have existed in ancient Greece, and one may wonder that a malady with symptoms so marked should have escaped the marvellously quick eye of Hippocrates himself. So it is, however, and the history of diabetes, in the western world at any rate, does not begin until the time of Celsus, four hundred years later; nor was it until the second half of the seventeenth century that Thomas

Willis discovered a sweet taste in the urine of diabetics, although Paracelsus had detected in it the excretion of an enormous excess of solidifiable matter, which he called salt. The next great step was that of Rollo, who, at the end of the eighteenth century, demonstrated that the sugar was derived from the vegetable elements of the food, and founded a true therapy upon a restriction of diet. In our own time the researches of Bernard have initiated a new era in the history of diabetes, and the character of diabetes lore in our own time is the tendency to attribute its origin to the nervous centres. For the early writers, then, diabetes was a urinary disease; for the eighteenth and earlier nineteenth century, it was a disease of assimilation; and for the later nineteenth century, it is a disorder of the nervous system. This outline may profitably be filled up with some more detail.

It has been seen that Celsus, who lived about the time of the Christian era, was the first writer, if not the first physician, by whom the state now called diabetes was recognised. But Celsus did not take the step of naming the disease which he was the first to describe. In the second section of the twentieth chapter of the fourth book (I quote from the Elzevir edition of 1657, p. 234), entitled 'De urinæ nimiâ profusione,' where he says, "At cum urina super potionum modum mingitur, etiam jam sine dolore profluens maciem et periculum facit," &c. &c., his description clearly refers to diabetes, though of course to diabetes mellitus and insipidus without distinction. The section is short—indeed a mere paragraph—but in it he recommends muscular exercise and friction as beneficial. In diet he says, "Cibis teneris opus est," which does not mean much; and in another sentence he orders "Cibus comprimens," an astringent food, which is equally indefinite. The wine given must, he says, be dry—austerum—as opposed to wine which is luscious (which would have been called dulce), and "meracum," unmixed with water, which he probably wished to avoid as diuretic. The discovery of the uses of exercise, friction and dry wine were very fair bits of observation so far as they went.

Aretæus probably lived some generations later, for the disease in his time was better known, and had received the name by which it has since been distinguished. In his two books on the causes and symptoms, and on the treatment of acute diseases, he enters with some fulness into the nature, causes and treatment of diabetes, and he regards it as an escaping dropsy, which may be set up by some poison comparable to the venom of a serpent. He speaks of diabetes as rare. The full and excellent summary of Aretæus stands in great contrast to the short and

slight allusions of Celsus, and is worthy of one of the ablest of the ancient physicians, second perhaps to Hippocrates alone. The extent and character of his knowledge in this and other respects supports the opinion of those who consider that he belongs to the age of the Flavian Cæsars, rather than to the age of Nero or Augustus.

Fortune has been less kind to Aretæus than to Galen, whose voluminous writings have come down to us almost untithed by time. This celebrated author, whose writings even yet are familiar to instructed physicians, regarded diabetes as a kind of slackness or atony of the kidneys, which are unable to retain (*κατέχειν*) the liquids which traverse them. He compares it with diarrhœa, and, in a sense, might seem so far to foreshadow the doctrine of vasal paresis as to satisfy even a modern student of insipid diabetes, did he not, after his vicious habit of mind, in which he differs so much from Aretæus, go on to explain matters by saying that the kidneys have a hunger for fluid. He does not think, however, that the transudation is merely aqueous: he holds that the liquids run through unchanged or undigested, and thus deprive the body of its nourishment. He will not have it that there is any faulty assimilation in the chylopoietic viscera, *οὐκ ὀρθῶς ἔνιοι τῆς γάστρος εἶναι νομίζουσι τὸ τοῦ διαβήτου πάθος* (Loc. aff. lib. vi, c. 3).

Now, the extent and variety of Galen's writings,[1] his elegant verbosity, and, above all, his vicious leaning to metaphysical reasonings, made his works the Scriptures of mediæval medicine. To say what Galen thought is to set out the creed of a millennium, and the labour of generations upon generations of learned and enthusiastic physicians was spent not in observing for themselves, but on the barren task of reconciling the facts of nature with the words of their master. Shortly then, for the Arabians were but Galenists in masquerade, this dogma reigned unchanged for centuries—that diabetes consists in a slackness of the kidneys (a "lubricitas renum"), which permits the outflow of unchanged fluid nutriment; and when, in the fifteenth century, the sweet taste of diabetic urine was noticed, an additional nail was driven into the Galenic structure. The only argument, indeed, which seemed to arise at all was whether, to this atony, more or less of "inflammation" was superadded; and as this question gave rise to a discussion both sterile and endless, it found many debaters. And so we enter upon the long, dark waste of the middle ages, and find no ingenious thought and no acute observation upon diabetes until we come to that "searching and ingenious soul," Paracelsus, to

[1] There are extant 137 treatises or fragments of treatises, of which 82 are probably genuine. Many more are said to have perished by fire or otherwise.

whom it is hard to deal out adequate justice. As to his contemporaries, he may have truly said—

> . . . With awakened eyes they took revenge
> For past credulity in casting shame
> On my real knowledge and I hated them.[1]

Fantastic as he was, Paracelsus had a saving hatred of intellectual thraldom, and some shrewd and curious hankering after facts, which might well amaze and alarm his generation. Whilst safe and cautious men of large experience were muttering divers verses of St. John backwards, or triturating and distilling filthy nostrums to cure diabetics, Paracelsus bethought him to look into facts and set himself to examine their urine to see of what nature it is. He took a quantity of this urine and evaporated it, and of course obtained an abundant crystalloid residue, a residue which was enormously greater than that of normal urine. This he assumed to be saline, and the thirst of the kidneys was to be explained by the presence of salt in such abnormal quantity. How near may a man come to a pregnant truth and yet fail to grasp it! Had it occurred to Paracelsus to taste his "salt," he would have anticipated Willis by two centuries and a half. As it was his method, his fruitful method, gave the first impulse to the first living school of modern medicine—the iatro-chemical school. The clinical observations of Paracelsus were faulty and badly recorded, and although Paracelsus gave the first impulse to the iatro-chemical school, his vanity, his mysticism and his recklessness forbade him to be its founder, a place which was taken by the well-known Sylvius de la Boe. In judging of Paracelsus perhaps, and of de la Boe assuredly, I think that Dr. Daremberg forgets how impossible it is for the greatest of minds to escape from the pressure and the infection of the times in which they live. Sylvius, while praising experience and seeking inspiration largely from direct experience and observation, did at the same time develop upon the narrow foundation which he reached many a fantastic superstructure. Not even Harvey was free from theorizings which seem vain enough to us, and Sylvius was a much smaller man than Harvey. Sylvius was, however, a reformer after his kind, and deserves our respect and our gratitude in that he broke the bonds of tradition, busied himself with such chemistry as he could command, and proposed a humoral doctrine which was a great advance on previous doctrines; but which, like other partial doctrines, became in its turn a bondage in after times. Still diabetes was not badly explained by Sylvius, who laid down that this disease is a dis-

[1] Browning's 'Paracelsus,' pt. v.

order of the blood and a general ailment, an ailment of nutrition, and not, as the Galenists taught, a renal disease.

Thus matters stood until Thomas Willis, the anatomist of the brain, in 1675, published his 'Tractatus de Urinâ.' Willis was a leading iatro-chemist and a follower of Sylvius. Amid much that was fanciful and erroneous in his physiological theories, he added some sound knowledge to his century. Among other things he practised a good method of examination of urine, and he first detected the sweet taste of diabetic urine. He found ('De Medicament. oper.,' sect. iv, ch. 3) the urine in diabetes "quasi melle aut saccharo imbutam mirè dulcescere." Willis did not separate the urinary sugar, but his observation gave a certainty to future diagnosis.

It is curious how common a disease becomes after it has once been accurately described. Rare in the opinion of the earlier writers, it is now, says Willis, "Creber, ne dicam quotidianus." Of course, with Sylvius, Willis held that diabetes was a blood disease; and as the blood thickens, he says, more water is drawn from the tissues. Willis describes the causes and symptoms of the disease with much truth of derivation, but in many points, of course, time has contradicted him. It may be noted that he refers it, among others, to mental causes, "sadness or long sorrow."

Diabetes lent itself perhaps better than any other disease to the iatro-chemical theories, and it lent itself less readily to those Italian schools known as the iatro-physical or iatro-mathematical. It cannot be said that Borelli and his disciples added anything to our knowledge of diabetes, or gave an intelligent account of it until this school of solidists allied itself with or lapsed more or less into the animistic doctrines of Stahl, and when Sauvages in his Nosology distinguished polyurias with as little precision.

It was not indeed until the appearance of Sydenham in the seventeenth century that much more was done for diabetes. Sydenham speaks but briefly of the malady, though what he does say is very precise. He regards it as an incomplete coction of the nutritive juices which being immature tend to be eliminated as foreign and injurious. Hence the strength lessens and the body weakens, its substance being as it were pumped off through the common sink of the bladder. He also points out that old men become diabetic after prolonged intermittents although the fever be gone. Morton who, like Sydenham, had a clear, direct, simple way of looking into things, made the valuable observation that diabetes is often hereditary, and Dr. Donkin will be glad to hear that he prescribed a milk diet with chalybeate waters as a means of cure. Mead made the happy

suggestion that diabetes owes its origin to a disorder of the liver, a suggestion which was baseless enough so far as he was concerned, and he found few to believe him. Boerhaave combined the galenical with the chemical theories, and taught that diabetes consists in a relaxation of the urinary vessels concident with a diluted state of the juices of the body. Omitting to name many other able physicians who formed more or less crude theories concerning diabetes, we may pass forward to the name of Matthew Dobson, of Liverpool, who probably is but a name to the general reader. Dobson, who wrote in 1774, seems to have been the first person who actually separated the sugar from diabetic urine, and he moreover detected the taste of sugar in the blood, and argued that by way of some fermentative process the proper nutrient materials in the blood pass into a permanent saccharine form; so that that which should nourish the body escapes by a bywash—the kidneys being themselves healthy. These admirable observations were published just one hundred years after Willis. Also in this latter half of the eighteenth century wrote Francis Home, a subtle thinker and close observer. His teaching was of much value in the gradual discovery of diabetes, which he defines as a disease marked by an increase of urine, which is sweet, by continous thirst and hunger, and a dry and generally scurfy skin. He separated sugar from diabetic urine in the proportion of one ounce to the pound in one of his cases, and of one and a half ounces in another. With yeast he showed that this urine fermented and became like small beer. He noticed that the teeth of diabetics were generally loose and bad. He was the first, too, who clearly explained that the urine in diabetes, though more abundant than the fluids drunk (super potionum modum, as all observers had seen to their perplexity from Celsus onwards) does not exceed in quantity the water taken in all food and drink together. He also notices in the evaporating dish that not sugar only, but sugar and salts are present together. Finally, he hints at rather than prescribes an animal dietary. Cowley, however, a contemporary physician, was the first to declare that the mark of diabetes is the presence of sugar in the urine, and not the presence of sugar in præternatural quantity. As a curiosity, the theory of Erasmus Darwin may be stated, which set forth that diabetes consists in a flux of lymph from the stomach which finds its way, not necessarily through the kidneys, but by some direct channel with the bladder.

The eighteenth century was closed by the discovery of the true doctrine of diabetes by Rollo, who also founded a rational treatment upon his discovery. Rollo was the first to point out that the sugar in diabetic urine is derived from the vegetable diet, and

that the flow of urine is due to this formation of sugar. By restricting the dietary of diabetics to animal food he thus cured or relieved many sufferers, and was the means of procuring relief to many generations of diabetics, who are and probably will remain ignorant of his name. Certainly we now know that the doctrine of diabetes cannot be expressed in this simple way, and that in severer cases even animal food becomes a source of sugar; but the generalization was substantially true, and a great advance upon any previous knowledge. From the time of Rollo the progress in our knowledge of diabetes has been steady and rapid. Mr. M'Gregor, of Glasgow, demonstrated the presence of sugar in the blood of diabetes and separated it; he attributed its formation, as Prout did, to an incomplete or irregular conversion of the contents of the stomach. But all previous observations were displaced by the cardinal facts laid bare by Claude Bernard, one of the ablest and most brilliant physiologists of modern or of any other times. His discovery of the glycogenic function of the liver and of the direct relations between this process and the central nervous system threw a flood of light on this mysterious malady, and is too familiar to every reader to need description in this place.

The riddle, however, is yet unread. What we know is that the blood of the diabetic is charged with an excess of sugar, and that this excess is not disposed of in the blood circuit, seeing that the portal vein, which should be practically free from sugar, contains it more or less distinctly. This sugar acts as a diuretic upon the kidneys which blindly carry off the food of the body.

In the next place it is clear that this excess of sugar is to be credited to the liver,[1] and observation, both direct and incidental, tells us that in diabetes the liver is preternaturally active. Up to this point, then, we find no structural disease, the kidneys are not less but more busy; the liver, in like manner, turns out more than its tale. If we press the matter a step farther we find the kidneys remaining above suspicion; for their work, if too zealous, is rightly performed, unless in a later stage this zeal may have led to their own undoing. But is this true of the liver? Is it that the liver does more work than it should do, and, like the kidneys, does it well? or is it that it does more work than it should do and does it ill—makes more than its tale of bricks, and makes them without straw? That the liver makes a normal quantity of liver-starch only, but that this is rendered unstable by some excess of ferment in the blood can

[1] It seems scarcely necessary to discuss the hypothesis now generally discarded, that diabetes consists in an incomplete destruction of sugar formed in natural quantities.

scarcely be the case. For if it be that the liver makes good starch, but that a spendthrift blood dissipates it too rapidly, we must at any rate credit a willing liver with a vast and abnormal supply. But although observation, both direct and incidental, tells of a præternatural activity of the liver, this activity may be the activity of a purulent discharge; it may be that energy is spent in producing quantity rather than in raising material to its due molecular standard. It may be that the liver is spinning what the Yorkshire weavers call shoddy. This hypothesis, which is far from improbable, is however as yet unproven; and I think we have some grounds of complaint against the physiological chemists that they have not made a larger series of comparative observations, contrasting the amyloid contents of diabetic livers with the amyloid contents of the non-diabetic. The few observations we have seem to support the hypothesis.[1]

This hypothesis is, however, contested by a large number of able physiologists led by Professor Schiff. By these observers it is stated that the liver works both abundantly and thoroughly, but that the blood contains an excess of the ferment which reduces the starch, so that the well-woven material, like Penelope's web, is no sooner laid up than it is again unravelled. There can scarcely be any doubt, in spite of the admirable checking experiments of Pavy and McDonnell, that liver starch is made to be redissolved as sugar, and as some soluble crystalloid to be consumed in the muscles. But it seems scarcely well to speak, as many writers do, of the ferment by which this change is produced, as *the peculiar* ferment. The change is in itself a simple one, and we know as a matter of fact that many juices of the body do reduce starch with ease, so that it is difficult to suppose the blood ever to be devoid of ample reducing power. Saliva, we know, reduces liver starch promptly; but Pavy has not found that the injection of saliva, nor has Kussmaul found that the injection of diastase, into the blood of a healthy animal calls up sugar in its urine.[2] Again, transient diabetes may be caused by the ingestion of an excess of glycoid food; or indeed a permanent diabetes, if we are to rely on the stated prevalence of this disease among the Simhalese. This scarcely looks like more starch and more ferment, but rather like imperfectly stored glycoid materials. And if we approach the question from the other side, if we stimulate the functional activity of the liver without modi-

[1] Dock ('Pfluger's Archiv,' v, pp. 571—583, quoted by Dickenson from 'Br. Med. Journal,' 1872, p. 276) states that no starch is found in the liver of a rabbit diabetized by woorara.

[2] If it be true that disease of the pancreas is found in a very large proportion of diabetics when examined post mortem the fact is curious and no doubt important. But disease of the pancreas would be attended with a diminution, not with an increase of its proper secretion.

fying the diet, we again obtain a transient diabetes. This may be done very readily by such operations as the irritation of the liver substance with needles, by some sections of the abdomen, and by many other means, all of which concur to excite the arterio-capillary activity of the organ, and all of which point to no modification of ferment, but to more work worse done. That the diabetes thus artificially produced differs in kind from natural and permanent diabetes, it lies with those who assert it to prove. For my own part I think all diabetic states are essentially identical, that "hepatic glycosuria" is an idle term, corresponding to no difference in nature, and that there are no clinical differences among diabetics, save those which depend on the stage and intensity of the malady and the quality or age of the patient.

Granting all this then we have yet found no disease. We find kidneys working actively and thoroughly, and we find the liver working hurriedly no doubt, and so not thoroughly, but not on that account to be called diseased. Were diabetes a transient disorder, like a migraine, it might be enough to say that the liver ran irregularly, though even then we should ask the reason why. But surely, if there be a disease in this world, diabetes is a disease; a stealthy, abiding, fatal change, no mere gusty state of function. Diabetes most assuredly depends upon organic degeneration somewhere, and we must assume that the morbid activity of liver and kidneys is but the manifestation of some deep-seated mischief as yet undiscovered. And however physiologists may please themselves, it is idle to seek to put off hospital physicians with talk about juices.[1] Physicians who have to treat diabetes know well enough that so steady, pertinacious, unsleeping an evil is no mere dynamic condition, but has a local habitation, whether its place be discovered or still concealed. Diabetes unchecked by art has but little tide and no turning. Now the abdomen has been ransacked, and in it we find two organs goaded into præternatural energy, but no primary disease: nor is there any primary disease in the organs of circulation and aeration. The well-known course of pathological inquiry would lead us then to expect to find the secret place of static change in the nervous system, as more remote than the parts already ransacked, more complex than they and far more difficult of investigation. Nor are we without physiological grounds for such anticipations, for Bernard's researches, and the diabetic effects of such a neurotic poison as woorara, have provided us with such grounds. Nor are we

[1] The reader will distinguish of course between the absolute increase of fermentative action due to absolute increase of blood supply to the liver, and the hypothesis of a relative increase of a "peculiar" ferment in the system.

without pathological grounds for such anticipations, as from the time of Stephen Blancard[1] (1670) downwards we have an abundance of post-mortem evidence of coarse disease within the skull associated with diabetes. Nor are clinical suggestions wanting. Diabetes is a hereditary disease, and heredity is, to say the least, as strongly marked in nervous diseases as in diseases of other parts; it is, moreover, associated in families with nervous diseases of other characters, such as epilepsy. Not only so, but its best known causes are of a kind which especially tell upon the nervous system, namely blows upon the head and other physical nervous shocks, and strain or distress affecting the mind.[2] As Dr. Roberts says in this second edition of his well-known and admirable work, "When we do seem to put our finger upon a cause it seems to lie somewhere in the well-known nervous circuit" which is set out in the anatomical plans published by Brunton and Dickenson; otherwise we are left in a "sea of conjecture." Further researches, he says, "can alone reduce these conjectures to order and certainty" (p. 239). It would seem then that these further researches must be made in the nerves and nervous centres.

It is somewhat remarkable that if we set aside such isolated and exceptional observations as that well-known case recorded by Luys, in which a coarse lesion was found in the medulla oblongata, no pathologist has published any careful investigations into the state of the nervous centres of diabetics. Men have been busy enough about diabetes, and it was not until I consulted the most recent editions of our best English and foreign text-books, and looked through the year books of pathology for the last few years, that I assured myself that Dr. Dickenson was the first to produce any adequate evidence of the state of the nerve centres in diabetics. Many persons had talked about it; many persons like myself, no doubt, have had diabetic brains in pickle more than once, with good intentions likewise in pickle; and years ago, Dr. Lockhart Clarke actually showed me one section from the medulla of a diabetic patient presenting morbid changes: but of systematic investigations we seem to have none up to the time of those first published by Dr. Dickenson in the 'Transactions' of the Medico-Chirurgical Society for 1870. These observations are republished, and form a cardinal part of the work which is now under review; and, right or wrong, they form an answer to the enigma we have spoken of as yet unsolved. Let us first see what the proposed solution is; and secondly,

[1] Obs. lxxxi, p. 298. Quoted by Salomon (loc. cit.) "Virgo aliquot ante obitum annis diabete correpta fuit . . . . Mortuæ cranio aperto insignis vesica aquosa inventa est," &c.

[2] "Deprimirende Gemuthsaffecte" and "schwerer Kummer." Seegen.

let us see whether the answer is satisfactory. If it be a true answer, diabetes is explained—explained, that is, so far as any chronic degenerative disease is explained.

It is in his second chapter that Dr. Dickenson deals with the pathology of diabetes, and from this chapter the author's views, so far as the nervous centres are concerned, may be thus condensed.

The brain of diabetics is, to rough examination, natural, though a practised eye may detect in it a cribriform appearance, each puncture containing a vessel much smaller than itself. More rarely considerable cavities are to be seen. On the minute examination of eleven cases, the nervous matter is found to be destroyed and absorbed along the course of arteries, and in some instances there is evidence of the escape of the contents of the vessel into the surrounding tissues with consequent degeneration. This escape seems rather to be a migration of corpuscles than of blood in bulk. In extreme cases globular or funnel-like excavations, sometimes large enough to hold a pea, are widely scattered through the brain, being few and large about the central parts, numerous, small and closely set in the white matter of the convolutions.

The corpora striata, optic thalami, pons, medulla and cerebellum are the chosen seats for the largest and most striking holes. In the centre of the pons and in the medulla, on each side, in contact with the root of the facial nerve, cavities are often found. The cavities are usually confined to the white matter. The brain matter around them is absolutely natural. The changes in the cord are similar to those in the brain but less declared. In one case, a woman who died of diabetes at the age of eighteen, miliary sclerosis was found throughout the brain and cord. These changes point to the symptoms observed; glycosuria resulting from nervous irritation rather than from loss or interruption of nervous power.

Such are the revelations of the eleven cases examined by the author, and this much seems proved, that changes such as those described seem to be constant in diabetics—a very curious discovery to say the least of it.

Dr. Dickenson disarms the reviewer, whose main point must be to ask whether the changes described as the cause are not really the consequences of glycæmia, by carefully considering this objection and summing up that which can be said for it. He says (p. 44):

"Is it possible that the erosion and perivascular destruction should be due to the action of the saccharine blood upon the delicate nervous tissue? . . . . This hypothesis at first sight seems not improbable; but looked at more narrowly, we cannot but discern considerations which make against it. Diabetic blood is not gene-

rally irritating to the tissues. The absence of atheroma and vascular degeneration is as striking in diabetes as is their presence in albuminuria. . . . . And it is also contradicted by the limitation of changes to the course of the arteries. If the destruction were wrought by a circulating agent, it would probably show itself rather in the territory of the capillaries, where the blood is brought into its most intimate relation with the tissues, than about the arteries when it is divided from them by thick walls. . . . . Localized loss of arterial tension, repletion of the affected trunks, consequent serous leakage or corpuscular migration, and finally disintegration and removal of the permeated tissue, appear to constitute the alterations of diabetes as of other diseases of the nervous centres."

Dr. Dickenson then compares diabetes with other nervous diseases, naming especially tetanus and general paresis, "from which they (the lesions of diabetes) differ in disposition, date, and degree, and not in kind farther than that with general paresis cellular degenerations are superadded" (p. 46). Surely this saving clause has something suspicious in its simplicity, and the difference is one by ordinary minds less easily dismissed. It is as though one should say that nephritis and inflammation of the urinary passages differed only in a certain superaddition, and scarcely in kind. The gist of the whole question in either case is wherein did these changes take their rise; and if it be answered that they take their rise in subordinate structures we must make the best of such an answer; but we must not pretend that it satisfies us,—as we are satisfied with the discovery of changes in dominant tissues—with the discovery, for instance, of cellular degradations in general paresis. General paresis seems to consist primarily in a diffuse interstitial encephalitis; the first change in the vessels being a proliferation of the adventitia, and even a formation of new vessels. In tetanus we find acute changes similar to the chronic changes in diabetes, and this sudden or gradual vascular stress may be due to aggravated cell-function; but in tetanus surely this aggravation follows peripheral antecedents, and is secondary; while in general paresis central inflammation seems to be the primary event. To compare diabetes simultaneously then with two other affections essentially disparate is to darken counsel. To which shall we liken it first? To tetanus?—then the phenomena point to a peripheral initiation. To general paresis?—then we find no evidence of that ultimate ganglionic waste for which time enough has elapsed. Dr. Dickenson's comparisons, therefore, are a little too ingenious, for, put as he should put them, they point not to a central, but to a peripheral initiation which is yet to be discovered, and the riddle is yet unread.

On the other hand, it seems quite clear that Dr. Donkin and

others, who explain these morbid phenomena as the results of the distension of decaying arteries, or as mere corrosions, are bound to show that similar changes are more or less equally distributed all over the nervous system, and that they are to be found elsewhere in the body wherever arterioles run in delicate tissues. Now this is known not to be the case. On the other hand, the changes are certainly found most abundantly in and near the parts indicated by Bernard, and they are remarkably corroborated by Dr. Dickenson's very interesting observations upon the urine of the insane, by the well-known concurrence of central nervous disease and glycosuria, by the equally well-known effects of blows upon the head, of mental shock, and by other clinical facts. Dr. Donkin's essay, which shows some intelligence and some reading and observation, but which is rather a hasty polemic than a sober contribution to knowledge, contains a somewhat contemptuous estimate of Dr. Dickenson, as one who bases his views on data not obtained from a study of the clinical development of diabetes, but solely from anatomical changes detected after death (*vide* p. 90). These strictures are neither becoming nor discreet, nor are they true. If we turn from Dr. Dickenson's chapters on pathology, which, whatever may be their ultimate value, at any rate give us weighty statements controlled and acknowledged by other observers, we shall be much struck with the temperate, truthful and modest way in which the clinical facts are also estimated. Master of an admirable scientific style, a style which is concise and accurate, and at the same time neither dry nor pedantic, Dr. Dickenson's work is easily taken for a smaller thing than it is; and it is only on a second or third reading that the strength and culture of the author are seen beneath his quiet and unassuming manner.

Frequently in a sentence of his we find that which in another and more prolix writer would have occupied a page, and many of his points are lost upon readers who are accustomed to more display. In his acknowledgment of the work of others Dr. Dickenson is just without being gossiping or fulsome; and every chapter, including that on treatment, bears the mark of his own personal experience. The paragraphs on the remedial virtues of strychnine and iodide of potassium are good examples of the author's just and shrewd habit of mind. The clinical cases incorporated with the work have far more than an illustrative value, for they have been carefully analysed, and the results tabulated over sufficiently long periods. In this respect, however, the two volumes of Dr. Külz have an especial value. I do not know that any extant monograph on diabetes can be compared to this in respect of the tables it contains, assuming,

as I confess I do, their accuracy. Dr. Külz investigates the conditions of the insensible perspiration in diabetics, the presence or absence of sugar in the juices of the stomach in diabetics, the influence of Karlsbad water therein, of alcohol, of glycerine, of bodily activity, and so forth. Some cases again are recorded and tabulated on account of peculiarities of an instructive kind in their course and phenomena. Dr. Külz also states that diabetes can be produced at will, in frogs, by subcutaneous injection of sulphate of methyldelphinum in doses sufficient to produce palsy of motion. As to the Karlsbad cure, Külz repeats that at best it is valueless, and that the milk cure can only be occasionally useful. All these points are supported [1] by long and minute analyses. On the other or positive hand, he gives some most interesting observations concerning Levulose and Inulin. As regards Levulose, Dr. Külz shows that some fruits may, under certain restrictions, be included in the dietary of the diabetic; and as regards Inulin, that this substance is a permissible and nourishing hydro-carbon, which might be made into bread for diabetics were it not so high in price. As this high price is, however, an accidental and not a fundamental difficulty, Dr. Külz trusts that it will be reduced if the demand arise. He reminds us that not a few years ago chloral could not be had for its weight in gold.

Dr. Bouchardat's treatise need not detain us long. It is the record of the experience of a veteran physician whose clinical acumen, mental vigour and large field of observation give great weight to his words, and to his earnest commendation of hygienic as well as dietetic treatment. The work cannot, however, be compared for a moment in substantial acquirement with the works of Dickenson and Külz, and can have but little permanent value. Indeed, it is but a collection of essays reprinted from medical journals, many of which date too far back to be of much interest at the present day, except to some kindly and leisurely readers, who will take an interest in the development of the mind of Dr. Bouchardat for its own sake.[2]

[1] With respect to the milk treatment it seems certain that patients can die as suddenly under this as under any other diet. In my own experience they have shown an especial alacrity in this, and I believe my colleague Dr. Eddison has had the same misfortune. After a trial in twenty-one cases I have ceased to prescribe it as it only seemed to answer well in two cases, and in one of these there was probably some error of observation. In other cases the issues were most unfortunate. On the other hand, I think Dr. Dickenson goes too far in proscribing milk altogether.

[2] Many of Professor Bouchardat's aphorisms are put in that epigrammatic way which is the bane of French medical writing, and which so often arouses a suspicion in the reader that the facts have been trimmed as gardeners trim box trees. For instance, he says, "La glycosurie non traumatique abandonnée a elle même dure toute la vie." This surely is not absolutely true. A patient of mine

Our present article would be incomplete did we not make some reference to the curious chapter on diabetes insipidus, with which Dr. Dickenson's volume ends. The author by marshalling together and comparing the phenomena of the two chief kinds of polyuria, establishes a relationship between the two, which in a measure justifies their similarity in name, and which impresses forcibly upon us the importance of Bernard's discovery of their like causation by puncture of the medulla. For instance, Dr. Dickenson tells us "that the intracranial sources of diabetes insipidus, though various in kind, are constant in position. Where the position has been exposed after death, it has always involved the medulla or cerebellum" (p. 189). With respect to the presence of inosite in the urine of this disease, Dr. Dickenson regards it as accidental; and, indeed, it may be added that Strauss himself has given up the hypothesis that inosuria is characteristic of the disease. Cases, moreover, have been reported in which inosite was absent from the urine.[1] The author's experience coincides with that of other competent observers, who see in diabetes insipidus a congener of affections more obviously neurotic, and he might have noted that epilepsy is generally associated with transient polyuria, and that epilepsy and diabetes insipidus are not infrequently seen in the same, in similar or in consanguineous persons.

Dr. Dickenson, however, omits to point out the distinction between polydipsia and diabetes insipidus, a distinction which is certainly important, and readily to be known in individual cases by the state of the skin, which in the former malady is moist, and is dry in the latter. The curious preference of diabetes insipidus for young patients is borne out by the experience of all physicians and by the author; at present, however, I have two cases under my care, one of whom is a gentleman in easy circumstances, aged sixty; the other a labourer, aged forty-five, who is now in the Leeds Infirmary. The former patient had the complaint for some years as a young man, and it reappeared six months ago after great domestic anxiety. In this latter patient the affection was, and now is again, attended with prickling and tingling of the skin over the buttocks, phenomena which point rather to the cerebro-spinal than to the sympathetic system of nerves.

The chapter on the treatment of diabetes insipidus is meagre, and in my own opinion belladonna is more useful than the author is disposed to acknowledge, though I agree with him as to the inertness of ergot.

The section concludes with a curious comparison of the effects

became diabetic on the reading of his father's will which unexpectedly and unkindly disinherited him. Recovery was complete and is so far permanent.

[1] Cf. ex. gr. 'Virch. Arch.,' Bd. 43, p. 299.

of diet in diabetes insipidus contrasted with those in diabetes mellitus.

Two "typical subjects of insipid diabetes, who were together in the hospital, were fed as exclusively as might be upon starch, that diet being abruptly alternated with one as purely nitrogenous" (p. 230). "In the insipid cases, starch failed to develop any trace of sugar, or to increase in any marked manner the quantity of urine. . . . . But vegetables caused an immediate diminution in the discharge of urea; whereas with the saccharine, the first effect of a non-nitrogenous diet was to increase the excretion of this nitrogenous compound—a striking result which I (the author), have noticed in other instances.[1] It appears to depend on the increased discharge of water, and . . . is not long maintained, but after a few days is exchanged for a deficiency. . . . . The vegetable diet agreed much better with the insipid subjects than the meat diet; . . . . . under meat case B especially did badly; lost weight and became more thirsty."

Here, unwillingly, we must close our article; unwillingly, because the book with which it has been chiefly concerned is rich in matter, sound in premise, and acute in reasoning, while at the same time the material is handled with a literary skill which gives a pleasure of a widely different kind from the facile, effective, somewhat prolix and colourless style of the modern newsmarket. Neither sparkling nor emphatic for the sake of sparkle or emphasis, Dr. Dickenson's style is chastened and penetrated with culture, and betrays a characteristic and sometimes rather grim humour. As I had lately to say concerning Dr. Liveing's essay on migraine, it is a happiness to see that the physicians of the present day still in scholarship and literary tone sustain the honour of their learned profession, while they add to these a scientific acquirement and an insight into the logic of nature which no physicians of any previous age have been able to reach.

T. Clifford Allbutt.

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[1] To this the present writer can testify, and he has noticed it also in cases of heavy non-saccharine urine.

IX.—Watson on Diseases of the Nose.[1]

London is now such a large place that there are enough of people in it both rich and foolish enough to pay a guinea for an opinion as to the health and well-being of every organ of their body, one after the other, and it is the fashion of the day to believe that no one can be the highest authority on any given subject unless he is famous for it alone.

The story is already a classic one of the lady who had so and so for her heart, another for her lungs, Mr. —— for her generative organs, and Dr. —— for her kidneys. An oculist, aurist, dentist, and chiropodist are mere necessaries of life. Why should not the nose, long ago victorious over the eyes in the great question of the ownership of the spectacles—why should it not have a doctor to itself? Such a specialist will first need to poach a little, but it will be on so many manors that no one will feel the loss too severely. From the dermatologist he must take the sensitive prominent feature, so often the victim of acne; from the liver doctor he must beg the treatment of the one variety of red nose; from the specialist in dipsomania that of the other; rhinoscopy he must filch from the specialist in laryngeal diseases, for the laryngoscope does not necessarily include the rhinoscope; from the dentist he may surely take at least half of the lachrymal sac and lachrymal canal. And may not the dentist spare from his wealth of material the antrum?

Specialists may flourish—in London we suppose many do make money—but the rage for parcelling out the human body and the practice of our profession on that body into regions is being at present overdone. Some specialisms, such as mechanical dentistry, are justly made separate professions. Others, as ophthalmic surgery, embrace so vast a field of work, and require such special tastes, powers, and talents, as to be most suitably practised as a separate branch; but we fail to see any benefit either to the profession or the public in the present tendency to confine men to separate little corners of practice in either medicine or surgery. The American gentleman who "was not posted up in rashes," but was "death on fits," was not a greater or more useful physician on account of the limitation of his range of study.

One comfort is we must soon know the worst; there cannot be many more surgical specialisms remaining for the young gentlemen of the future.

We have seen the tongue and the prostate, the tonsil and the

[1] *Diseases of the Nose and its Accessory Cavities.* By W. Spencer Watson, F.R.C.S. Eng., B.M. Lond., Surgeon to the Great Northern Hospital, &c. &c. London. 1875.

clitoris, each serve its purpose. The epiglottis, perhaps, has as yet hardly had fair play, and the arytenoid cartilages still need a special coryphæus. After that, perhaps, there may be a pause.

The worst of it all is that the literature of surgery is becoming so overloaded, the wheat necessary for the nutrition of the student is hidden in the chaff which he must afterwards blow away. Trivial details hide great principles, information which loads the memory stifles original thought. We have to read so much, we have no time to think. Wisdom is exchanged for a certain fluency of speech.

Having so far relieved our feelings against undue division of labour, we must now turn to Mr. Watson's book of 472 handsome pages, and determine, if we can, how he has fulfilled his self-imposed task.

The very first clause of the first chapter is, in its mode of expression, apt to rouse the natural contradictiousness of every reader with a well-regulated mind. "The *nose*, the central, most prominent region of the face, is symmetrical." Is it, indeed? Of course, our own; but how very few of our neighbours' nasal organs are.

After this comes a careful, but altogether too elaborate and detailed account of the anatomy and physiology of the organ, well enough suited for the pages of an elaborate and exhaustive anatomical text-book. It includes an account of Max Schultze's discoveries, diagrams from Frey's 'Histology,' and copious quotations from well-known text-books.

A very good account of rhinoscopy and the various instruments used in nasal surgery forms an introduction to the second chapter on the non-ulcerative affections of the nasal fossæ. The first of these described is a common cold or nasal catarrh, of which a very careful and practical account is given, including a résumé of the varied means of treating this frequent, troublesome, and weakening malady. Epistaxis is included in the same chapter, and described under three varieties—traumatic, plethoric, and adynamic. The various modes of treatment, in the cases where treatment is necessary, are given in detail. The nasal douche with ice-cold water is an admirable method if the patient is sufficiently collected and free from nervousness. Mr. Watson points out that in cases in which the posterior nares have required plugging the plugs should not be left too long, as there is risk both of pyæmia and tetanus. He highly recommends an apparatus for the purpose made by Mr. Coxeter, under the direction of Dr. Cooper Rose, of Hampstead. "It consists of a gum-elastic tube about five inches long, with lateral perforations near the end, and covered with thin caoutchouc membrane in the form of a spirally twisted bag for the last three or four inches of its length. The cavity of the bag can be injected with air or water from the gum-elastic tube, the end of which has fitted to it a piece

of india-rubber tube for the purpose of connecting it with a syringe or india-rubber injector. To use it the membranous bag is smoothly folded over the contained tube, and the, whole being oiled, is passed along the floor of the nares till it reaches the pharynx. The bag is now inflated or injected with water, and the expansion of the bag both in front and behind suffices to completely close both apertures. By letting out the air or water it is as easily withdrawn." (Pp. 55, 56.)

Another plan, by M. Negrier, of Angers, France, is credited by the inventor with uniform success. It consists in causing the patient, in a standing posture, suddenly to raise one or both arms perpendicularly upwards, and to retain them for a short time in this position. If only one is raised, it should be that of the side from which the hæmorrhage proceeds, and then the patient may compress the bleeding nostril with the other hand. The elevated position of the arm should be sustained for a few minutes, in order to give the blood in the bleeding orifices time to coagulate. Dr. Negrier explains the result by the consideration that as the blood in the erect position of the arm requires a much greater force to sustain it than when the arm is pendant, the energy of the heart's contraction must be in the same proportion diverted from the carotid to the subclavian.

This method has been long known and taught in many schools, but the explanation we have always heard given is that the rush of blood, being encouraged by position down the subclavian vein, so far assists and relieves the circulation down the jugular vein also.

In cases where epistaxis depends on the presence of a bleeding surface in the upper part of the pharynx and soft palate the hæmorrhage may be most troublesome, and plugging of all kinds may fail to stop it. A good case is given which, after much trouble and danger, recovered under treatment by Mr. Banks and Dr. Creghern, of Liverpool, by means of styptic injections through one nostril, the other nostril and the mouth being closed. An autopsy of a somewhat similar case of extreme interest in a medico-legal point of view was lately reported by Dr. Littlejohn, of Edinburgh, in the 'Edin. Med. Journal,' 1875.

On the general treatment of persistent or recurrent epistaxis we think Mr. Watson is very deficient, for, besides making no mention of injections of ergotine, he entirely ignores the extreme value of the internal use of turpentine. This remedy, given in twenty or thirty drop doses in milk, or in capsule, rarely fails in checking the most severe and long-continued cases.

Nasal calculi are described, and some cases of this rare disease given. Gelatinous polypi receive, as from their great frequency they deserve, full attention, and numerous modes of treatment are described. After a pretty full experience we have little doubt that

in the great majority of cases avulsion with a pair of dressing forceps practised on the neck is the safest, quickest, and, in the end, least painful method. The use of tannin, in the form of snuff, after a day or two is valuable, both as causing any small ones which may have been overlooked to wither up and also as rendering the mucous membrane more healthy.

The third section begins with an account of ozæna, that most painful and disgusting disease, or rather symptom of various diseases; depending on fœtor of the nasal secretions for its name, and generally on ulceration of the nasal mucous membrane for its chronic nature. Mr. Watson gives several varieties. 1. Accidental, from retained mucus in uncleanly individuals. 2. Constitutional, generally in phthisical patients. 3. Syphilitic. The question of prognosis and treatment is very fully gone into, and any one desirous of full details as to the practice of various authorities, as Trousseau, Lee, Rouge of Lausanne, will find very full information here.

Glanders is fully described, the researches of Elliotson, Travers, Virchow, and the Gamgees, being carefully collected and arranged.

Lecture IV is on the ulceration of bones and cartilages. In describing rapid cases of such mischief in syphilis Mr. Watson recommends, and we quite agree with him, an activity in local treatment, which is too often postponed or neglected, from a very natural feeling on the surgeon's part that the case will very likely go wrong and leave deformity, and that his credit may suffer if he attempts interference and is unsuccessful. Mr. Watson recommends the early, free, and bold use of powerful caustics, specially in cases of ulceration of the septum, such as the acid pernitrate of mercury, chloride of zinc, or, perhaps best of all, strong nitric acid. These should be applied under chloroform and very freely. Along with large doses of the iodide of potassium (twenty, thirty, or even forty grains) are well borne.

Rouge of Lausanne removes decayed pieces of mucous membrane and necrosed bones from the nasal cavities, by lifting the upper lip and nostrils together; having first freed them by incisions through the mucous membrane of the mouth and division of the cartilages at their attachment to the upper jaw. The anterior bony nares are thus completely exposed, and a very good view obtained of the interior of the nasal fossæ, with a large space for the introduction of instruments. Of Dr. Rouge's eight cases one died of pyæmia and meningitis, while the rest were successful.

Diseases and injuries of the frontal sinuses are treated in Section V. Injuries, abscess, acute and chronic, cysts, polypus, tumours, and foreign bodies, each has a page or two; the foreign bodies are sometimes very curious—worms and maggots of various kinds seem not uncommon, especially in hot climates.

Lecture VI, on the antrum, its diseases, injuries, &c., is very full of excellent and well-arranged information, though some of it may certainly be said to trespass on alien ground, as the description of fractures on the upper jaw is very full. One error in surgical history we must correct. Mr. Watson seems to have copied Heath, who in his turn copied Mr. Butcher, of Dublin, in the mistaken idea as to the facts and dates of the first operation in Europe of removal of the upper jaw. These three authorities together give the credit to Gensoul, of Lyons, who, they say, was speedily followed by Lizars, of Edinburgh, and Mr. Watson says by Mr. Scott, of the London Hospital. The real dates are as follows :— Lizars *proposed* the operation in 1826. Gensoul performed it with success in May, 1827. Lizars attempted it, but failed from excessive hæmorrhage notwithstanding ligature of the carotid, in December, 1827; Syme removed it with success in May, 1829, the patient recovered; Lizars removed it entirely, the patient dying on the nineteenth day, in August, 1829. Lizars removed it successfully in 1830.

Lecture VII, on diseases of the lachrymal sac and nasal ducts, is a legitimate and successful invasion of the territory of the ophthalmologist. Containing nothing new or original, it gives a very good idea of the present opinions on the surgery of this interesting region.

Lecture VIII is on diseases of the skin and subcutaneous tissues, and discusses the æsthetic aspect of the nasal organ. We cannot help rising from the perusal of this chapter with the feeling that there is here and there a tendency to *over-treatment*. Thus, after a description of *comedones*, including a description of the *steatozoon* or *acarus folliculorum*, which fills a page, we are told that the "treatment consists first in remedying any constitutional defect that may be manifest. The secretions are always more or less unhealthy, and a combination of steel and aperients is generally useful. Amenorrhœa or leucorrhœa and dyspepsia will be met by remedies appropriate to each particular case. With regard to local treatment, the skin may be stimulated by gentle friction, kneading, and by the application of hot fomentations without soap. These comedones, which are evidently hard and dry, and cannot be removed without mechanical aid, may be gently pressed out, and subsequently friction with fine oatmeal and borax lotion will in most cases very soon improve the general condition of the cutaneous surface. Later on, bichloride of mercury with almond emulsion, alkaline washes, alum lotion, and lastly the weak hypochlorite of sulphur ointment, may be used with much benefit."—P. 232.

Fancy the work a "nasist" would make for himself in a school for boys between twelve and seventeen, and the lots of washes for comedones, and the elaboration of mechanical aid, friction, &c.

Also for acne the treatment is equally active and energetic. "Locally, the application of a lotion containing oxide of zinc and calamine powder suspended in water, with a small quantity of bichloride of mercury, will be very useful when applied warm to the face. The later stages will require more stimulant applications, &c. &c. (p. 233).

Lupus is well described and many modes of treatment given.

Boils in or near the tip of the nose are well described, and receive the attention they deserve from their frequency, painfulness, and disfiguring character.

Lecture IX, on sarcomata and recurrent fibroid polypi, leave the pathology and treatment of these not very well-known growths in much the same state of confusion in which they were before.

Lectures X, XI, XII, XIII, and XIV, on injuries of the nose, malformations, distortions and mutilations, rhinoplastic operations, functional derangements of smell, and its cranial relations and medico-legal bearings, contain much interesting matter which space forbids our analysing.

An appendix of eighty cases, some well known, others new, bearing on the questions treated in the book, greatly adds to its value.

When we add that the work contains some fair woodcuts, four not very good lithographic plates, a copious index, and a table of contents, it will be seen that Mr. Watson has done what he can to make his work a useful book of reference on the subject of which he treats.

X.—Carter on Eye Diseases.[1]

WITHIN the last ten or fifteen years the interest taken by English medical practitioners in the special departments of practice has been decidedly on the increase. In no speciality has this been more manifest and more real than in that of ophthalmology. General physicians have been even more emphatic than ophthalmic surgeons in their praises of the diagnostic value of the ophthalmoscope in certain states of the general system and in some forms of intra-cranial disease, while general surgeons not infrequently find themselves confronted with cases of severe inflammation of the various structures of the eye associated with some constitutional morbid state, such as syphilis, or are often called upon to treat cases of injury to the eyeball or its adjacent structures. Without some acquaintance with the healthy functions and the morbid changes of the eye the responsibility of un-

[1] *A Practical Treatise on Diseases of the Eye.* By ROBERT BRUDENELL CARTER, F.R.C.S., Ophthalmic Surgeon to St. George's Hospital, &c. With numerous Illustrations. London. 1875.

dertaking the management of injury or disease of that organ is immense. It was meet, therefore, that there should be commensurate with the increased interest taken in ophthalmic affections an extension and improvement of ophthalmological literature. But no one could fairly or reasonably complain of a paucity of works dealing with diseases of the eye. The philosophical manual of Professor Wharton Jones, the complete and scientific volumes of Mr. Soelberg Wells and Mr. Power, the small but encyclopædic text-book of Mr. Lawson, and its not unworthy rival by Mr. Macnamara, the clinical treatise of Mr. Haynes Walton, the medical ophthalmoscopy of Dr. Clifford Allbutt, and the essays of Dr. Hughlings Jackson, seemed to have left little to be desired. Mr. Brudenell Carter, however, made bold to think that there was an opening for yet one more work on affections of the eye. The book now before us, designated a practical treatise on diseases of the eye, is intended to fill that gap, and is said to embrace the substance of the author's lectures at St. George's Hospital, and to contain but slight reference to modes of practice of which he is unable to speak from experience. As to the propriety of relying almost exclusively on his unaided experience in writing a work in which the principles of ophthalmology are explained and discussed, Mr. Carter may be assumed, of course, to be the best judge, but his readers may in turn claim the right to appraise the merits of the performance. What these merits are we shall endeavour to show.

In his preface the author explains that his aim has been to place before the profession, in a concise and readable form, a general view of the present state of knowledge with regard to the nature and treatment of the more important diseases of the eye, and that he has not thought it necessary to dwell minutely upon maladies of rare occurrence or upon details which are interesting only to specialists; nor has he attempted to give completeness to his work by what he terms undigested compilation.

It is, however, doubtful whether the attainment of this object will be expedited by the long, irrelevant, and "undigested compilation" of the anatomy and physiology of the eye which occupies the first thirty-three pages of the work. Although the anatomical portion of the chapter bear very strong indications of its indebtedness to the article in 'Stricker's Histology,' it is marred by many deficiencies and inaccuracies. For instance, at page 11 we are told that "the cones are most abundant in the region of the macula lutea, where each one of them is surrounded by a single circle of rods." If Mr. Carter had taken the trouble to read a little more of Schultze's admirable article on the retina he would have learned that "In the percipient layer the rods already begin to fail, even at the outermost edge of the macula lutea, their place being taken by the cones, and they ultimately altogether disappear."

It is very characteristic of the style of this work that after this exhibition a satire should be found on the very next page on the ignorance respecting the arrangement of the layer of rods and cones recently displayed by certain Continental ophthalmologists, whose stupid error left "nothing behind but the wholesome moral that an acquaintance even with the anatomy of the retina may afford security against ludicrous blundering under the disguise of knowledge." Again, at page 20, Mr. Carter states that each eyelid "is formed by a thin plate of cartilage." The fact is that what is here called cartilage is not cartilage at all, but consists of a dense connective tissue. Again, as another instance of blundering, at page 23, the axis of the globe is defined to be an imaginary line drawn from the fovea centralis to the geometrical centre of the cornea, whereas the axis of vision, on the other hand, is said to be an imaginary line drawn from the *fovea centralis* through the object looked at. It would be satisfactory to know whether these definitions are Mr. Carter's own or not, inasmuch as the careful and exact researches of Senff, Helmholtz, and Knapp have proved that the optic axis and the visual axis very rarely, if ever, touch the same point on the retina.

Helmholtz proved by exact mathematical measurements that in the normal eye the visual line lies to the inner side of the optic axis at the cornea, but to the outer side, in the fovea, at the retina, so that the fovea is outside and a little below the optic axis.

The physiological portion is not any better than the anatomical, the mechanism of accommodation is practically undealt with, and the physiology of vision is not really considered, although several pages are wasted on a description of the optical defects of the eye, which had, however, been long before admirably portrayed by Helmholtz in some lectures which were more recently translated into English by Mr. Atkinson. As Mr. Carter nowhere refers to Helmholtz's exposition, the great similarity between the writings of the two authors may be regarded as a remarkable coincidence.

In the more practical chapter on the examination of the eye Mr. Carter states that "cataract is common in diabetic persons." If this has been the author's experience it is, to say the least, not usual. That cataract does occur in diabetic persons and that it is sometimes due to the constitutional dyscrasia few will deny. But it should not be forgotten that there is a very great difference between the experience of ophthalmic surgeons and general physicians. Dr. Garrod stated that he had not met with cataract in any of the large number of cases of diabetes which he had seen, and Dr. W. Roberts alleges that he has met with cataract only once in forty-five cases of diabetes.

For the purpose of testing the acuteness of vision for distance Mr. Carter employs, of course, Snellen's test-types, but, for reading,

he seems to have a preference for the type, prepared, at his suggestion, by a Gloucester house. But as far as we can judge, the chief merits of these is that "the letters nearly correspond with those of Jäger's scale."

Nor is the method of determining the existence of ametropia more precise.

"In order to ascertain the state of refraction," says Mr. Carter, "we first cover the eye which is not under examination, and then, taking a weak concave and a weak convex lens, say $+\frac{1}{30}$ and $-\frac{1}{30}$, hold them in front of the eye alternately, quickly placing and removing them, the patient still looking at the distant test-types."

If vision be improved by the convex lens Mr. Carter infers that there is hypermetropia, if by the concave lens that there is myopia, but if both lenses be prejudicial that there is emmetropia. As this method is, by Mr. Carter's own showing, both fallacious and untrustworthy the half page devoted to its recommendation might well have been saved. It is, perhaps, a too implicit reliance on this mode of examination that has led Mr. Carter into the error of imagining that "as in myopia, so also in hypermetropia, it is rare, even with the most complete optical correction which can be obtained, to arrive at $V=1$." If by this is meant that Mr. Carter has rarely with such optical correction arrived at $V=\frac{20}{20}$, he has been singularly unfortunate with his patients. To say that in uncomplicated cases it is rare with proper correction *not* to arrive at $V=\frac{20}{20}$ would, we are convinced, express more accurately the opinion of those who have had the largest opportunities of treating anomalies of refraction.

In discussing the imperfectly understood question of the field of vision Mr. Carter may certainly claim to speak *ex cathedrâ*. He denounces the early attempts to map out the field of vision as rude and primitive and in every way objectionable. Even Donders' method failed in several important particulars, and it was reserved to Professor Förster, of Breslau, to seize the true principle of perimetry. Unfortunately, Förster's instrument was in its details also "extremely faulty." But Mr. Carter, after enduring all the inconveniences of this instrument for a certain time, was provoked to attempt to overcome them, and this he happily believes he has effectually accomplished, at all events he says "my own instrument appears to me to be the best." That he should think so is, perhaps, only natural, but we fear that the majority of busy practitioners do and will continue to adhere to that rude and primitive method which Mr. Carter so ruthlessly condemns.

In the third chapter Mr. Carter has given a fairly good and intelligible account of the ophthalmoscope. A brief reference is made to the history of the discovery of this instrument, and a due

meed of praise is dealt to those two Englishmen, Mr. Cumming and Mr. Charles Babbage, who seem to have been the first to entertain the idea of the practicability of illuminating the interior of the human eye by means of reflected light; but the credit of recognising the true principles of ophthalmoscopy is rightly accorded to Professor Helmholtz, while to Von Graefe belonged the merit of utilising these principles and adapting them to ophthalmic practice.

A full and detailed description of the mode of using the ophthalmoscope, both for direct and indirect examination, has been given; but too much space has been devoted to an explanation of many minute details which are to be learned only by practice. Mr. Carter is careful to show that all ordinary ophthalmoscopes now in use, whatever name they may bear, are only modifications of Ruete's original instrument, made to suit the requirements of those persons who have suggested the particular alterations. Indeed, one cannot but admire the mild satirical vein in which some of these so-called inventions are referred to. This, coming as it does from one who has had the honour to suggest and introduce to the profession *two* ophthalmoscopes which bear his name is singularly honest and ingenuous. Of all the ophthalmoscopes which have been introduced Mr. Carter says "the best instrument, on the whole, is that which fulfils the greatest number of indications, and I am inclined to make this claim on behalf of my own model;" but, a few pages further on, he explains that that made after the pattern of Dr. Noyes is superior to his, "in so far that it is available for every purpose to which an ophthalmoscope can be applied, while the small one is useless for the erect image in cases of myopia, or for determining the exact degree of ametropia in any case." This unfortunate disqualification, and the fact that in Mr. Carter's ophthalmoscope the aperture in the mirror is so small that it excludes nearly all the light from the observer's eye, will, we fear, deter those who do not wish to carry two ophthalmoscopes from purchasing the miniature model.

The applications of the ophthalmoscope are fairly well worked out, but some of the deductions which Mr. Carter draws are not in accordance with the notions which prevail among the majority of ophthalmoscopists. For instance, in speaking of the estimation of the degree of ametropia by means of the ophthalmoscope Mr. Carter observes that "the concave lens which gives him (the observer) the best defined image of the fundus, or the strongest convex lens with which a clearly defined image is still visible, is that by which the myopia or the manifest hypermetropia of the patient will be corrected." As a matter of fact, however, if the examination have been conducted properly, the myopia of the patient would be over-estimated and the hypermetropia would be under-estimated, inasmuch as allowance must be made for the distance that intervenes between the optical centres of the patient's and the observer's eye.

Mr. Carter, indeed, confesses, at p. 552, that he really does underestimate hypermetropia with the ophthalmoscope and cannot trust his calculation.

In speaking of the clinical significance of visible retinal venous pulsation, Mr. Carter attaches a graver import than most ophthalmic surgeons do to that phenomenon. "In persons beyond middle age," says he, "in whom a venous pulse either exists, or can be produced by very slight pressure, the eye is generally to be regarded as standing near the brink of glaucomatous change. An arterial pulse is seldom of this character." This opinion, it need scarcely be said, is exactly the reverse of that entertained by the majority of practitioners.

The fourth chapter is, perhaps, that which is best known to the medical profession, from having been the cause of a passage of arms in the medical journals between Mr. Carter and Mr. Vose Solomon. It may be remembered that the latter gentleman protested against the objectionable and erroneous representations which Mr. Carter had given of his views respecting the cause of sympathetic ophthalmia, for which Mr. Carter subsequently apologised. But this chapter, which deals with the principles of ophthalmic therapeutics, has a wider interest, and is indeed one of the most useful in the whole work. A clear and succinct account is given of the influence exerted by the fifth nerve on the nutrition of the cornea. The experiments of Meissner and Schiff are referred to, but no mention is made of the still more important investigations of Meynert and Merkel on the functions of a small root of the fifth nerve that arises from the anterior ganglion of the corpora quadrigemina. Excellent and encouraging advice is given on the administration of mercury in syphilitic iritis; but the author should, we think, have laid more stress on the inestimable advantages which may be derived from the intelligent employment of Heurteloup's artificial leech in many cases of acute and chronic inflammation of the eyeball. An explanation of the best mode of using this valuable instrument would have proved more useful and gratifying than the insertion of so many frivolous jokes and useless engravings.

It is, however, in the fifth chapter that Mr. Carter evidently puts forth all his power. In dealing with the principles of ophthalmic surgery he labours to show that operations on the eye are founded on a wide and intelligible theoretical basis, and that the art of operating on the living eye should only be undertaken after long, careful, and persevering practice on inanimate models. But the obvious reflection arises that the complicated method of training the hands and fingers for operating, described by Mr. Carter, can only be necessary for persons originally very maladroit and clumsy.

The hyperbolical assertion of Beer that an ophthalmic operator will spoil a hatful of eyes before he cures one is, of course, dragged

in to illustrate the natural awkwardness of some operating surgeons. This assertion is, however, a gross exaggeration, and applicable to few, if any, of those who now devote themselves to the practice of ophthalmic surgery. Mr. Carter, like most men whose forte is operating, magnifies unduly the importance of operative procedures, and seems to forget that, after all, operations are the opprobria of surgery, and that an eye is only operated on when other remedial measures have been neglected, misapplied, or have failed.

Ten pages are devoted to a description of the kind of training the hands of a person aspiring to the position of an operating surgeon should, in Mr. Carter's opinion, undergo. There is much that is plain common sense, but there is not a little that is fanciful and chimerical. The allusion to the want of mechanical dexterity in the Hebrews and in women does not appear to have a better foundation than that Mr. Carter has been told "of a Jewish ophthalmologist, the subtlety of whose brain work is unsurpassed, that he 'operates from the shoulder!'" It might with justice be replied that the chances are that this gentleman is after all a better and safer practitioner than one who has spent all his time and used up most of his energies in acquiring the art of legerdemain. It is perhaps the great familiarity that Mr. Carter has with all kinds of instruments and the dexterity he has attained in their use that induces him to make the unnecessary and unbecoming remark that the shoulder in Mr. Bowman's stop needles is a contrivance that "would perhaps be useful if the needles were to be worked by machinery, but for surgically educated fingers it is at least superfluous." This is, we confess, the first occasion on which we have known the objection urged against an instrument that it is too safe. With all deference to Mr. Carter's opinion, however, we should prefer shoulders on the needles, if for any reason it were necessary to introduce a couple of needles into the anterior chamber of either of our eyes.

Passing over the incomplete chapter on diseases of the eyelids we come to those on diseases of the conjunctiva and cornea. In the seventh chapter Mr. Carter recommends, apparently without any qualification and without stating any precise indications, the inoculation of an eye with some purulent discharge, whenever the vascular network which forms on the cornea as the result of chronic granulations of the conjunctiva is very close, constituting the condition called "pannus." "The cornea," he explains, "is protected from sloughing by its vascular character;" but that it is not always so protected, Mr. Carter himself indirectly confesses, when he says a few pages further on that he has seen one case "in which the corneæ of both eyes sloughed, although they were both highly vascular when the inoculation was practised."

Respecting the preventive treatment of contagious ophthalmia in large institutions Mr. Carter can speak with considerable authority

and experience. The measures he recommends for the prevention and for the limitation or eradication of the disease are minute perhaps, but practical. Stress is rightly laid on the importance of personal cleanliness, and thorough ventilation, but it is somewhat hazardous of Mr. Carter to unreservedly commit himself to the strenuous recommendation of Tobin's system which, except for the inordinate laudation in the 'Times' newspaper, has not yet received the sanction of the best authorities as even an efficient method.

At page 248 Mr. Carter makes the extraordinary statement, "that it is impossible to draw any line of demarcation between the mildest form of conjunctivitis and the most severe purulent ophthalmia;" and a few lines lower down he remarks, "The more severe forms display an early period of acute irritation in which any stimulation would be mischievous, and would increase the severity of the attack. This period must be tided over by the local use of atropine and sedatives." As a matter of fact, the severe cases generally start with very rapid vascularisation, great swelling of the lids, rapid formation of pus and copious discharge. In all such cases time should not be lost in "coquetting," as Mr. Carter would call it, with atropine and sedatives, but early recourse should be had to sufficiently strong astringents.

In treating of diseases of the cornea, in the eighth chapter the author states that the treatment recommended by Professor Saemisch for ulcer of the cornea has not yielded in his hands such good results as have been claimed for it by others. He even goes so far as to say that most of the cases eventually did badly, and required enucleation of the injured eyeball. It is for this reason that he prefers an iridectomy, in which for cases of recurrent corneal ulcer, it is sufficient to snip out a minute portion of the pupillary margin. It may, however, be asked whether tapping of the anterior chamber would not prove as effectual as this slight operation.

In discussing that form of keratitis in which there is a tendency to interstitial cell-proliferation, which, by almost universal consent, is now regarded as the result of hereditary syphilis, Mr. Carter makes the astounding statement:

"That the physical peculiarities and proclivities which are usually due to syphilitic parentage are sometimes produced, even in an aggravated form, by vaccination with lymph yielded by a vaccinifer who has himself inherited the disease, and that this may happen without the production of any derangement of the course of the vaccine vesicle, or of any thing which could at the time be recognised as evidence of a specific inoculation."

This is not the place, nor have we the room, to discuss this remarkable statement as it deserves. We may, however, incidentally remark that this allegation may be taken as an example of the self-confident and unscientific tone in which much of the work under

consideration is written. Sir William Jenner, Sir James Paget, Mr. Marson, Dr. West, and many other eminent English and Continental authorities, out of an aggregate experience of hundreds of thousands of carefully observed cases unanimously aver that they have not seen any disease but the vaccine disease produced by inoculation with lymph from a true jennerian vesicle, while the investigations of Heim, Taupin, and Cullerier have shown experimentally that the transmission of syphilis by vaccination is at least improbable, if not impossible. They have even taken lymph from syphilitic children and have vaccinated healthy children, and watched the result. In no instance has syphilis been communicated. Vaccine lymph has even been taken from a vesicle raised near a chancre and inoculated into a healthy subject with negative results as far as syphilis was concerned; and Sigmund's experiments proved that when syphilitic virus was deliberately and purposely mixed with vaccine lymph and inoculated into a healthy person syphilis, and syphilis only, resulted. Indeed, all clinical and experimental evidence goes to prove, 1st. That it is impossible to inoculate any other but the vaccine disease with lymph taken properly from a true jennerian vesicle. 2nd. That if by any means syphilis be communicated the transmission is declared by unequivocal local signs. Both these propositions were, moreover, confirmed by Dr. Viennois. Yet in the face of the testimony of all these careful and exact observers, in spite of the strongest experimental proof, Mr. Carter by an ill-founded, illogical "conviction" deliberately gives the sanction of a legally qualified medical practitioner to the insane agitation of those fanatics and fools who ignorantly opposed the salutary legislative measures for the prevention of smallpox.

Considering that the ninth chapter embraces diseases of the iris, including irido-choroiditis and sympathetic ophthalmia, it is very brief compared with the diffuseness of some of the other chapters which deal with less important subjects. There are here errors both of omission and commission. The direction of the fissure in coloboma of the iris and its situation are not mentioned, nor is the important difference between posterior synechia and persistent pupillary membrane even referred to. No other treatment than tapping or iridectomy is recommended for iritis accompanied by great pain. On many of those remedial agents which have gained the confidence of the best clinicians, Mr. Carter places a very doubtful and wavering reliance. He seems, for instance, to discountenance the internal administration of mercury in iritis except in those cases which seem to be going hopelessly to the bad, or in which iridectomy is from some cause impracticable. This disposition is well shown at page 318, where, after recommending the performance of iridectomy for serous iritis, he states that that disease "never requires mercury (although this may be necessary

for the cure of some associated constitutional state) and seldom opium." This advice is somewhat conflicting when we bear in mind the strong faith that Mr. Carter places on the opinion of the late Dr. Anstie that mercury has a special affinity for the fifth nerve.

In the tenth chapter which deals with the large and important subject of cataract Mr. Carter has come nearest to fulfilling the task that he imposed upon himself in undertaking to write this book. But even here much that is really sound and useful is diluted and almost lost in the weary length of nearly fifty pages. Mr. Carter almost indiscriminately recommends the suction operation for all cataracts occurring in persons under thirty years of age. Of this treatment he has had considerable experience, which he alleges has been of a satisfactory nature. But although no one can deny that, in some instances, Mr. Teale's operation is a safe, simple, and expeditious mode of removing cataract, yet a considerable amount of testimony might be adduced that this operation is pre-eminently a selective one. Indeed, many experienced operators profess their inability to decide whether the suction operation is applicable or not until they have made the corneal incision and tested the consistence of the lens by rupturing the anterior capsule. We, by no means, desire to disparage the admirable device of Mr. Teale, in whose hands it has proved remarkably successful, but we do maintain that it is not so free from danger as some of its advocates declare.

For the removal of hard cataracts Mr. Carter, like most English ophthalmic surgeons, prefers a modification of Graefe's method, whereas, he expresses unqualified disapproval of all those operations in which the incision passes across the face of the cornea. But, here again, supposing Mr. Carter to be writing for the great body of the profession, the space which is devoted to the description of the various operations for the removal of cataract is as unnecessarily long as that previously squandered in the description of the many operative procedures that have been suggested for the relief or cure of conical cornea. The practice that Mr. Carter recommends of operating when only one eye is cataractous, although it has some plausibility to recommend it, is, we imagine, as little likely to prevail with most practitioners and patients as that in which he advises that when there are cataracts in both eyes, the immature one should be operated upon as soon as the safety of the first is assured and before the eye previously operated on has been brought into daily use by the necessary spectacles.

The eleventh chapter, on glaucoma, consists of sixteen pages, eight of which are devoted to the consideration of the various forms of glaucoma, and eight to a wearisome repetition of a de-

scription of the operation of iridectomy. The account of the symptoms of the different varieties of glaucoma is very meagre and quite inadequate for the purposes of those who have not had special opportunities for observing and studying the complicated phases of this serious but often insidious disease.

Mr. Carter relies chiefly on the increase of the tension of the globe as the sign whereby the presence of glaucoma may be recognised. Although this sign is valuable and even pathognomonic, perhaps, for the ophthalmic surgeon, it is a treacherous one for fingers not surgically educated.

It may, however, be affirmed without prejudice that the chapter on the diseases on the fundus oculi, is the most unsatisfactory in the whole book. Respecting that form of optic neuritis which is now known by the name of "choked disc" Mr. Carter adds nothing to the results of the labours of Dr. Hughlings Jackson, and Dr. Clifford Allbutt, except, perhaps, that he reminds his readers of the well-known fact that that condition may sometimes exist without coarse lesion of the encephalon. His experience of the value of strychnia in atrophy of the optic nerve has been singularly unfortunate. This may perhaps be partly accounted for by the fact that he has sometimes entrusted the administration of the subcutaneous injection of strychnia to some member of the patient's family, and partly by the fact that he does not seem to have been in the habit of giving the drug in the largest dose that the patient could bear. Nor has his success with the continuous current been more favourable. For stimulation of the nerve and retina, he recommends the somewhat doubtful practice of freely exposing the eyes to light and of encouraging the patient to attempt to read daily "the smallest type which he can puzzle out by the aid of a strong magnifier."

Respecting nerve atrophy Mr. Carter seems to think that the evidence that has been produced by many excellent observers of the causal relation of tobacco smoking and of excessive alcohol drinking to atrophy of the optic nerve is not more trustworthy than that which might be obtained "to connect the disease with black hair or with any other common personal peculiarity." Such is the flippant manner in which this author dismisses a question of the highest clinical and scientific importance. No mention is made in this chapter of the changes that take place in the fundus of the eye of diabetic persons. The effects of perverted nutrition in the posterior segment of the eyeball are scarcely referred to and the important question of the relation of posterior polar cataract to disease of the deeper parts of the eye is not even mentioned.

Two things in the thirteenth chapter need only to be mentioned to be condemned—one is, that Mr. Carter recommends the uncomfortable and unnecessary precaution of plugging the orbit with a sponge for three or four hours after enucleation; whereas, most

operators would find one minute sufficient, the lids being subsequently compressed with a smooth, well-made pad of cotton wool and lint supported by a bandage.

The other point is the employment of sutures for lacerated wounds of the cornea.

In the fourteenth chapter, which deals with squint and affections of the ocular muscles, Mr. Carter expresses many opinions, the originality of which no one can doubt. He advocates operation on both eyes in all cases of convergent strabismus. It is, perhaps, this practice that has given him so many opportunities of elaborating and perfecting his operation of re-adjustment. That both eyes should be operated on when there is a certain degree of internal squint no one will doubt, but it is equally certain that there are cases of unmistakeable convergent strabismus in which a double operation would certainly produce divergence.

The description of the symptoms of paralysis of the various ocular muscles is extremely unsatisfactory; there is no definite mention of the changes that take place in the vertical meridian of the eye in some of the forms of paralysis, and we very much doubt whether any person not previously acquainted with the disease could diagnose a case of paralysis of the superior oblique muscle from Mr. Carter's description alone. Indeed, it is impossible for any one to form a right conception of the symptoms produced by the paralysis of any of the ocular muscles unless he have previously carefully studied the somewhat complicated action of these muscles in their healthy state. If Mr. Carter had occupied some portion of the thirty-three pages which he has lavishly bestowed on the anatomy and physiology of the eye he would have rendered to his readers a real service.

The subject dealt with in the fifteenth and last chapter is one in which Mr. Carter, by his previous performances (viz. the translation of 'Zandar on the Ophthalmoscope,' and 'Scheffler on Ocular Defects'), may be presumed to have special knowledge. By the aid of Donders' classical work Mr. Carter has been able to present a fairly intelligible account of the anomalies of refraction, except in those instances in which he has been bold enough to deviate from the copy, for it is precisely in those instances that he exposes himself to criticism. It is doubtless, in appealing to his own experience, that Mr. Carter illustrates astigmatism by always speaking of the vertical meridian as the meridian of least refraction, and the horizontal as the meridian of greatest refraction, whereas the experience of others has been so uniformly the reverse of this, that such an arrangement of the principal meridians has, by common consent, been regarded as "*contraire à la règle.*" But even this misapprehension is not sufficient to account for the statement made at page 576, that if a patient sees a horizontal line best with a concave lens of twelve inches he has horizontal myopia $= \frac{1}{12}$, and that if he sees the vertical

lines best with a concave $\frac{1}{8}$ he has vertical myopia $= \frac{1}{8}$, the astigmatism being represented by the difference between these two lenses of a $= \frac{1}{24}$. The truth is that in the first instance the myopia would be vertical, and in the second horizontal, the distinct perception of vertical lines depending on an accurate adjustment of the horizontal meridian of the eye, and *vice versâ*. By taking another of Mr. Carter's illustrative and apparently typical cases, which is again *contraire à la règle*, we may see how Mr. Carter treats a case of astigmatism. He supposes that there is myopia for vertical lines $= \frac{1}{20}$, that is, of course, in the horizontal meridian, and hypermetropia for horizontal lines $\frac{1}{24}$, that is, in the vertical meridian. The astigmatism being the sum of these two factors, viz. about $= \frac{1}{11}$, would, Mr. Carter says, be corrected by a convex cylinder of $= \frac{1}{11}$, with its axis horizontal. In such a case, however, the anomaly would not be corrected any more than it would be if a concave cylinder $= \frac{1}{11}$ with its axis vertical were used. A spherical lens should be used in addition to the cylindrical.

It is refreshing, after reading this exposition of astigmatism, to meet, on the next page, with the following sentence in Mr. Carter's best style. In commenting on some of the errors committed by his professional confrères in correcting astigmatism, he says, "I have already had occasion to say that London consultants enjoy great opportunities of learning humility from the errors of their neighbours." As we have already referred to Mr. Carter's errors in estimating the degree of hypermetropia and myopia by means of the ophthalmoscope, we may pass over the six pages devoted to a consideration of Dr. Scheffler's "orthoscopic spectacles" and many of the fanciful allusions to myopia, with as little notice, as Mr. Carter gives of one of the most complicated questions in refraction, viz. the treatment of those cases in which there is a difference in the degree of the refraction in the two eyes, which Mr. Carter dignifies by the term anisometropia.

We have now completed the thankless task of reviewing this book, the style of which is not less open to criticism than is much of the matter. There is scarcely a page which is not marred by wilful personalities and vain conceits. The reputations of some of the most skilful, learned, and accomplished ophthalmic surgeons in this country have been handled with unbecoming levity and freedom, and we would fain believe that the kind of writing that Mr. Carter has thought fit to indulge in is as distasteful to the members of the medical profession as its introduction into a work dealing with many important scientific problems is novel in English medical literature. If ever the work should reach a second edition we trust that better counsels will prevail, and that Mr. Carter may see the frivolity and unseemliness of his work.

Holmes's Treatise on Surgery.[1]

No living surgeon holds a higher reputation for profound knowlege of the literature of his profession than Mr. Holmes. His able editorship of the system of surgery which goes by his name, his exhaustive lectures before the Royal College of Surgeons on the treatment of aneurism, his work on the Surgical Diseases of Children, and other writings, have deservedly earned for him the position he holds. Nor is it merely in theoretical knowledge that Mr. Holmes is to be ranked among the foremost of British surgeons. As a teacher of anatomy and surgery he has had abundant experience, and his connection with St. George's Hospital has given him opportunities of practice scarcely to be excelled. It was, therefore, with feelings akin almost to excitement that we looked forward to the appearance of Mr. Holmes's text-book of surgery. We hoped to see something original, something new; some arrangement of the vast mass of details necessarily included in a work on surgery in a way calculated to help the student in the difficult task before him. We hoped to see the work freely and usefully illustrated—for men learn in surgery almost as much from good illustrations as from reading; and we hoped also to find the old empirical way of teaching surgery abandoned and some attempt made to connect more clearly pathology with practice. It is with feelings, therefore, of considerable disappointment that we have read Mr. Holmes's book. There is nothing new in its arrangement or style of teaching; the illustrations are badly executed, and for the greater part of the pathology we are referred to other works. Mr. Holmes modestly states in the preface that his object has been to produce a "treatise which shall not be unworthy to rank with the other excellent text-books in use in our schools." This is no very ambitious task, and we may say with truth that he has succeeded in it. We should place this work above some well-known text-books and below others; it may fairly take its rank among them, and like all the rest it has its good and bad points.

The work commences with a description of inflammation containing an excellent and detailed account of the all-important process of emigration of the white corpuscles. All other questions concerning inflammation are dismissed with surprising briefness, there being no general discussion of its causes or of its treatment. The author first explains the four great signs

[1] *A Treatise on Surgery, its Principles and Practice.* By T. Holmes, M.A. Cantab., Surgeon to St. George's Hospital. 8vo, pp. 928. London, 1875.

of inflammation, and here we find some statements which are rather surprising. In the first place Mr. Holmes tells us that "the period of dilated arteries and increased stream is that of 'active congestion,' that of dilated veins and diminished movement 'passive congestion.'" In a note we are informed that passive congestion may also be caused by anything which obstructs the venous return, and has usually nothing to do with inflammation. Why, then, group together under one name two conditions so absolutely different, especially when the customary use of the words does not require it? On the next page we find it stated broadly that the cause of *inflammatory* fever is the heat generated at the inflamed part. This also is corrected in a foot-note which will, we fear, leave the student in a hopeless state of mental confusion. It runs thus:—"I do not mean that all the phenomena of inflammatory or traumatic fever depend on the mere rise of temperature of the blood, but as inflammatory fever rises so are these phenomena developed. Whether they depend on the rise of temperature or whether they have a common cause is another question." We were under the impression that this theory of the body being heated by an inflamed part as if by a fire was definitely abandoned. The development of heat at the inflamed part is so slight that the most delicate thermo-electrical experiments are required to demonstrate it. How is it possible, then, that this could raise the temperature of the body five or six degrees above its normal standard? To do this, unless the part inflamed were of great size, it would require to be raised to a temperature sufficient to cook it before it could heat the whole body to such an amount. Further on Mr. Holmes discusses the question more in the light of modern theories and observations, but he still adheres to development of heat locally as one of the chief causes of fever.

On p. 5 we have the following:—"Lister has observed changes going on in the pigment-cells lying in the intervascular spaces of the frog's web, which testify to an action entirely independent of that in the blood or the vessels and the action of irritants on the non-vascular tissues, such as the cartilages, will be found illustrated in subsequent pages." Here we have mentioned together in one paragraph observations which have led to exactly opposite views of inflammation; Lister's view that the essence of the early stages of inflammation is a diminished activity of the pre-existing tissues of the part, and Virchow's that it is an increased activity. It surely would have been better to omit all mention of the subject than to dismiss it thus shortly.

We cannot help thinking that Mr. Holmes scarcely appre-

ciated the importance of the task before him when he sat down to write an account of inflammation. The student who uses Mr. Holmes's text-book will probably read his first account of inflammation in its pages, as the study of surgery is always commenced before that of medicine or general pathology, and we fear he will rise from reading it with the idea that the study of the process of inflammation may be very interesting to those who can understand it, but of very little use to a practical man. Now this idea is, we think, one of the most dangerous that can possibly enter into the mind of a student. Sir Astley Cooper commenced his first lecture with the following words: "Irritation.—This being one of the most important topics in surgery requires to be attentively studied and its effects carefully watched before any one can practise his profession with credit to himself or advantage to others." Fifty years have added vastly to our knowledge of irritation and its effects, and have in no way weakened the force of the above statement, and we think a few pages on this subject pointing out the varieties of irritants, their effects on living tissues and the part they play as causes of inflammation would have added very greatly to the completeness and utility of this work.

After dismissing inflammation, Mr. Holmes proceeds at once to treat of wounds and bruises, and again we have the same complaint to make. Union by first intention is dismissed in a few lines and without a single illustration. In former times probably it was a useless task to attempt to instil any ideas of scientific pathology into the minds of ordinary students, but now, when each has been prepared by a class of practical physiology to understand histological diagrams and descriptions, it would have surely been better to give them here (even at the risk of increasing the book by a few pages) than to refer the student to "works which treat specially of pathology." Moreover, in the description of this mode of union no mention is made of the necessary serous discharge which flows from the surface of a fresh wound during the first few hours, a free exit for which must be provided if union by first intention is to be obtained. All through the book we find the same brief treatment of pathological subjects, and on looking to the preface for an explanation we read the following:—"For the minuter details of pathology I must refer the reader to some of the many admirable works on that subject." If it were only for the minuter details that we were referred to such works there would be no cause for complaint; but it is not so. Take, for instance, diseases of bone. There is not a single illustration showing the microscopical appearances of inflamed bone, and the whole account of the process is contained in two paragraphs occupying

about half a page. For the histological pathology of rickets and mollities ossium we are referred entirely to other works, and pulpy degeneration of synovial membranes is dismissed in a few lines. We can only conclude that under the term "minuter details" Mr. Holmes includes almost the whole of pathological histology, which he seems to regard as a subject to be more fitly studied after the practical part of surgery has been mastered. In this we cannot agree with him. Medical science has made great advances during the last twenty years. The microscope is now almost as common an instrument in the student's hands as the scalpel, and no work can be considered to represent the present state of medical knowledge and to be adapted to the present state of medical education that does not recognise this fact. The separation of pathology from practice seems to us a great mistake. If pathology is of any use at all it can only be by giving us rational ideas by which we can guide our treatment, and it should therefore precede, or, at any rate, go hand in hand with the practical study of disease.

It may be objected that space would not allow of such a treatment of the subject, but surely the addition of a few pages to a work containing nearly a thousand cannot be a matter of very great difficulty, and an increase of five per cent. on the size of the book would have provided ample space for all that would have been required. We regret all the more that Mr. Holmes should have taken the view he has in writing this work, as we feel that no man could be more competent than himself to furnish the ordinary student with such an abstract of histological pathology as should render it unnecessary for him to go beyond the pages of this book.

Let us turn now to the practical side, and take as an example the chapter on injuries of the head. It commences, of course, with the old rule, "never to look lightly on any injury of the head, however trifling it may appear." Then follow injuries of the scalp. For some reason or other Mr. Holmes refers us to works on obstetrics for a description of cephalæmatoma, although we believe this affection is not exclusively confined to new-born infants. In treating wounds of the scalp Mr. Holmes breaks free from the "old traditional horror of sutures,"—modern observations, especially during the American war, having shown that there is no ground whatever for the idea that they favour erysipelas. We do not find however, under this heading, a good description of burrowing of pus under the tendon of the occipito-frontalis and the diagnosis of this condition from erysipelas, upon which the nature of the treatment must of course depend. We next come to an excellent description of the symptoms of intra-cranial suppuration following

scalp wounds with exposure of bone, but here again we have no clear statement of the mode of attempting to solve the difficult problem, whether the fever and rigor in the early stages is due to pyæmia or to the formation of pus. Trephining for hæmorrhage from the middle meningeal artery is next fully described, but as space is so precious in this work, we cannot conceive what should have induced Mr. Holmes to fill the greater part of two pages with a lengthy account of blood cysts from the dura mater, accompanied by three large woodcuts, two of which are absolutely useless. The subject of fractures of the skull is treated with great clearness and conciseness, but we do not find laid down with quite as much definiteness as we should expect, the conditions which have given rise to the rule of trephining in all cases of true punctured fracture. In compound depressed fracture (under which Mr. Holmes apparently includes punctured fracture) the rule of practice laid down is not to elevate without symptoms of compression unless the depression is very small in extent, and so somewhat resembles a punctured fracture. Then follows an account of concussion and compression. Under concussion we find a short account of the changes in temperature (illustrated by a thermograph) which accompany such injuries. The elevation during re-action following a depression which occurs immediately after the accident is of great interest as furnishing about the best marked instance of traumatic fever, certainly not dependent on the absorption of septic matter and not connected with any known inflammation, as it subsides in favourable cases about the time cerebral inflammation usually makes its appearance in those which terminate unfavourably. To this fact, however, Mr. Holmes does not allude.

Then follows an account of traumatic inflammation and a paragraph on injuries of the cranial nerves, and the article concludes with the operation of trephining. Here we find no mention of the danger of applying the trephine directly over a sinus. In fact, in one of the illustrations of depressed fracture, the trephine seems to have been applied exactly over the longitudinal sinus. We think, too, that if under trephining a recapitulation of the conditions justifying the operation had been given it would have been of great value to the student. We have taken this chapter as a fair sample of the book, and it will be seen that it differs but little in all important points from similar chapters in other text-books, and this we should say is characteristic of the whole book. It is therefore needless to enter further into detail.

With regard to modern instruments and modes of treatment employed in surgery we find Mr. Holmes has made every effort

to give the student the latest possible information. The antiseptic treatment of wounds receives a sort of qualified support. Mr. Holmes does not enter into the theory upon which it is founded further than to state that "as far as he is competent to form an opinion" he considers Lister's opponents have the best of the argument. In spite of this Mr. Holmes adopts the system of dressing, as he finds that practically it is capable of preventing or limiting decomposition, and at the same time of reducing suppuration and surgical fever. We remark, however, that the aspirator and the galvanic écraseur have received scarcely any notice in this work, whether intentionally or not we cannot say. The article on the diseases of the eye has been written by Mr. Brudenell Carter. It is in every way a most complete and excellent chapter, and forms a valuable addition to the book. We cannot conclude without again noticing the characters of the illustrations. We believe, if we have counted correctly, that 277 of the 411 illustrations are original, excluding of course thermographs; of these 70 are mere drawings of splints, &c.; of the remaining 207 we should say at least 90, or nearly half, are absolutely useless, and the remainder are bad. They in no way do justice to the text, and are one of the most serious blemishes of the work.

We regret that we have not been able to speak of this book in terms of higher praise. It seems to us that Mr. Holmes has found the limitation to space (and perhaps, for all we know, to time also) extremely irksome, for the book does not seem to be written with the same ease as his other productions. Without question, however, most of its faults will be recognised and corrected in the second edition, which will doubtless soon be required, and we then hope to be able to give that commendation to the book which the well-known ability of its author seems almost to compel, and the withholding of which has been the unwilling duty of the critic.

Bibliographical Record.

Contra-indications for Turning in Shoulder Presentations.[1]—The present thesis is one embodying much research, in which the author enters fully into the various means that have from time to time been adopted to remedy the difficulty occurring in shoulder presentations.

Assuming that version or turning is the general law in these cases, and that unless resorted to the mother and the child are alike doomed to certain death, he proceeds to discuss, in the first part, the contra-indications, which may be briefly summed up as follows:

1st. The non-dilatation of the cervical orifice when this non-dilatation depends upon anatomical or spasmodic rigidity of the cervix, or where it results from some organic affection, as the existence of a cancer or fibroid tumour.

2nd. The deep engagement in the pelvis of the fœtal presentation.

3rd. Tetanic contraction of the uterus.

4th. Narrowing of the pelvis. In this case it is important to ascertain if the child be living or dead. Where it is dead it will be necessary to practise version, whenever it is possible to introduce the hand; if the infant be living he proposes to perform Cæsarian section.

Cases illustrating these several forms of difficulty are given, together with remarks by the author.

In the second part he proceeds to discuss the measures which may replace version, where the non-dilatation is due to cancer. If the morbid process has only invaded a portion of the cervix, dilatation, aided by a few small incisions and then turning, may possibly succeed. But if, on the contrary, the whole of the cervix be involved, the grave question of which we are most to consider, the maternal or the fœtal claims, becomes paramount; and the author thinks it

[1] *Des Contre-indications de la version dans la Présentation de l'épaule, et des moyens qui peuvent remplacer cette operation.* Par le Docteur ADOLPHE PINARD. Paris, 1875. Pp. 140.

is a wholesome precept that the accoucheur should regard as his chief aim the preservation of the fœtal life, and with this object in view Cæsarian section should be resorted to. Equally so in cases of fibroid tumour, where, having studied the size of the tumour, its point of attachment, its mobility, and consistence, it is deemed impossible to deliver in the ordinary way, Cæsarian section should be resorted to.

Where the presenting shoulder is so profoundly engaged in the pelvis as to render turning impracticable, M. Pinard regards the method of waiting patiently for spontaneous evolution as pernicious and objectionable. Unless in exceptional cases, it is both very dangerous and quite contrary to the rules of art.

Where the membranes have ruptured and the liquor amnii escaped, the uterus being in a state of tonic contraction, it is necessary to employ opium, anæsthetics, and above all prolonged warm baths, but never venesection, and wait until we can operate.

In contractions of the pelvis—

1. If the infant be at full term and living, but presenting by the trunk, in cases where the contraction of the pelvis is under two and a quarter to two and three quarter inches, version by external manipulation having been attempted cautiously with a view to facilitating the immediate application of instruments, and having proved impracticable, Cæsarian section is proposed.

2. The fœtus not being at full term, version having been found impracticable, amputation of the arm will certainly favour the movement of evolution of the fœtus, but, on the other hand, decapitation and extraction may be accomplished if the fœtus has not advanced much beyond the seventh month.

3. Finally, if the infant be dead, even at term, whatever difficulties and dangers may be incurred by the series of operations successively necessary to deliver the woman *per vias naturales*, the Cæsarian section is absolutely unjustifiable, the cephalotribe being employed to break up successively the different presenting parts of the fœtal trunk.

In the third part of his thesis M. Pinard enters into the historical account of embryotomy from Hippocrates downwards, tracing out its development until the present time, and giving instances recorded by various authors, which may be classified under three principal headings.

1st. Evisceration, preceded or not by amputation of the arm, having for its aim and definite object forced evolution.

2nd. Evisceration without amputation of the arm, sometimes spondylotomy, having for its aim and object forced evolution.

3rd. Section of the vertebral column at the junction of the trunk with the neck, having for its aim the successive extraction of the two parts of the fœtus; rachiotomy.

In the description of each process, in place of giving a *résumé* of the manual operations, the author has thought it preferable to give cases from various authors, commencing with Robert Lee, Veit, Paget, Baron Dubois, and various other authors, entering into the detail of the operations, and giving his reasons for preferring or rejecting them.

The third part is eminently practical, and occupies over seventy pages.

The thesis gives a fair *résumé* of the subject, and is worthy of perusal.

Penetrating Wounds of Joints.[1]—An introduction, peculiarly French both in manner and matter, prepares the reader to take a personal interest in this work. A neat little apophthegm from Voltaire heads the chapter, and then with a sweet compliment to the charity and humanity of the society which has offered the prize, we are put at once in personal *rapport* with the author by his telling us of the cases which, in his student days, turned his attention to joint injuries, to the teachers under whom he studied, to the success of his practice, and to the "respectable phalanx" of wounded ones, whom he has saved from amputation and watched for years.

Penetrating wounds of joints are either simple, or "essential," or complicated. The first of these are most important to study. "A joint once opened is like a Pandora's box, from which issue the most cruel evils for the economy; pain, inflammation, fever, suppuration, hectic, death either slow or rapid, not to mention the most painful infirmities."

The knee-joint is the most typical one, and it is not necessary that the wound should be large, bloody, or severe in its appearance, it is enough if the joint is opened. Indeed it is of smaller wounds that this brochure treats; for in great and severe wounds the question is not one of joint injury, but of the life or death of the limb.

The causes, diagnosis, and progress of such penetrating wounds are carefully described; and in such a description there can be nothing original and little to criticise.

The inflammation which results from such wounds is described under different heads, which however are only different in degree, not in quality, as *excessive* and *moderate*.

To explain the gravity and danger of joint wounds many theories

[1] *Des Plaies Pénétrantes des Articulations.* Par le Docteur DECHAUX, de Montluçon, Médecin de l'Hôpital de Montluçon, "*Mémoire Couronné,*" par la Société de Médecine et de Chirurgie de Toulouse. Paris, p. 124.

have been advanced: entrance of air, extreme tenderness of tendons and ligaments, the resistance and want of relaxation about joints, presence of foreign bodies—each have had their advocates.

The author s opinion regarding the cause of the gravity of such wounds must be given in his own words if we would do it full justice:

"Mais pour moi, la gravité des plaies pénétrantes des articulations provient de la difficulté réelle qu'a la nature à réparer une articulation endommagée—et de l'étonnement de l'organisme devant ces travaux de réparation, qu'il s'exagére et que quelquefois il ne peut accomplir en effet sans une transformation de fond en comble de l'articulation."

The innocent self-contentedness of this whole chapter will not be easy to beat even in medical literature. In explaining his theory, the author contrasts the vascularity, and consequent rapidity of repair in muscles and cellular tissue, with the absence of blood-vessels, and of activity in such tissues as tendons, ligaments, capsules of joints, &c. In passages of considerable pretensions to eloquence he describes the complicated and beautiful arrangements in the joints; and then tells us that on injury of such a part, nature stops the machine, and concentrates all her vital activity on the repair of its wound.

In comparing the relative dangers of wounds of different joints our author unhesitatingly and rightly gives the place of honour (of greatest danger) to the knee, and describes forcibly the reasons of such dangers and the results of such wounds.

This description of the methods of repair of joints has no pretension to original observation or even to scientific precision—it is a mere description in a popular manner of the naked-eye appearances.

On treatment: we have, first, a few general rules given as to what is to be done in the first stages immediately after the receipt of the wound. *Rest;* a simple cerate on linen; no great efforts to exclude air; no attempts forcibly to close the wound, for by this means you produce tension, and if suppuration once begins, you have shut up a "wolf in the fold." Such are the simple, common-sense rules of this ancient surgeon. Little recks he of cotton wadding dressings to exclude dust; of antiseptic spray and antiseptic injections to kill germs; he can trust nature to a much greater extent than some of the younger surgeons do now, and he waits for symptoms. What are these symptoms to be? *Arthritis excessive,* or acute inflammation, is the worst, and is to be treated locally and generally; locally by leeches, generally by a smart blood letting of from fifteen or twenty ounces at once, followed by soothing poultices and by opiates; irrigation, blisters, and purgation follow in due course.

Moderate inflammation is to be dealt with on the triple line of *rest*, "*résolutifs*," and *patience*.

The question of amputation or conservation is discussed, but in such general terms, and with such platitudes as to be practically useless.

The last fifty pages are occupied by clinical cases, observations, and quotations, with reflections by the author, some amusing enough, others forcibly put, for example:

"CASE IV.—For some years I have had in my practice a case of loose cartilage in the knee-joint, but I have taken good care not to extract it."

Again:

"M. Beaumers, who has written a very interesting thesis on the subject of extraction of loose cartilages from the knee-joint, and who has collected all the cases published, has found twenty deaths in fifty-two operations, and of the thirty-two who survived, a large number had had abscesses, had seen great risks, and their limbs remained anchylosed in more or less vicious positions.

"If a surgeon is tempted by curiosity to open a knee-joint and go a-hunting for a loose cartilage, he should first descend into this cavern (this Bluebeard's chamber) where the twenty bodies hang, or walk the hospital where lie the thirty-two damaged or escaped ones, and ask their opinion."

Some very fair, others very ordinary, cases are recorded, and the conclusion of the whole matter is—

From these facts it results, penetrating wounds of joints are accidents of the greatest danger, but in the midst of all the evils resulting from the opening of a joint, as at the bottom of Pandora's box, there lies at least Hope, hope of a cure in many cases, hope to stimulate the surgeon to treat the severe disease, and to dissuade him from the heroic remedy of amputation.

This little book is easy reading, and doubtless to its genial and chatty author it was easy and pleasant to write. Would we could say it has added a single fact to the sum of our knowledge, or a theory worth the name to aid us in systematizing our thoughts.

Resection of Joints for Gunshot Wounds.—The subject of resection of joints for gunshot wounds is one of the greatest interest, and our knowledge concerning it has been very greatly increased during the last few years by the publication of the surgical records of the Franco-German war of 1870. The work before us has no pretensions to novelty or originality. It consists merely of the notes of thirty-five cases of resection of the shoulder, elbow, and ankle, which came under the observation of Professor Bergmann, chiefly in the great reserve hospitals in Germany, and they are consequently almost all specimens of secondary excision. The object of

[1] *Die Resultate der Gelenkresectionen im Kriege.* Von E. BERGMANN, Professor of Surgery at Dorpat. Giessen, 1874.

the author has been to trace out, as far as possible, the after results of these cases, especially with regard to the utility of the limb. As, of course, every patient has to present himself periodically for some years after he is supposed to be cured, so that the pension inspectors may ascertain the exact amount of disability from which he is suffering, these reports of final results are not difficult to obtain. The thirty-five excisions here recorded are thus divided: nine of the elbow with two deaths, fifteen of the shoulder with three deaths, and eleven of the ankle with two deaths. These results may be regarded as very satisfactory when we consider the disadvantageous circumstances under which the operations were performed. The fatal cases almost all perished from causes which could not affect our judgment as to the comparative safety of amputation and excision. Both the fatal excisions of the elbow and two of the shoulder were attacked by ordinary pyæmia. One of the excisions of the ankle died from septicæmia and thrombosis of the veins of the leg, and another, some months after the operation, from an attack of erysipelas, commencing in an unhealed sinus. The remaining fatal case is more interesting as a specimen of the evils of departing from established rules in the treatment of secondary hæmorrhage than as a case of excision. Soon after the operation vomiting and diarrhœa set in, and about a week afterwards the patient had a rigor. On the twelfth day free bleeding occurred suddenly, but was arrested by plugging the wound. No mention is made of any attempt to find the bleeding-point. The hæmorrhage soon returned, and again there is no mention of any attempt to ligature the bleeding vessel, but the axillary artery was at once tied in the axilla, and in a few days hæmorrhage again took place, this time, as the post-mortem showed, from the seat of the ligature. Amputation was tried as a last resource, but the patient never rallied from the operation. Careful examination failed to show any distinct source for the first hæmorrhage. Very likely it was from some small vessel, or perhaps only from the walls of the cavity, and if so the death of the patient was due solely to the ligature of the axillary artery, in direct opposition to all the rules of surgery.

Having thus disposed of the fatal cases, we can proceed to examine the results in those who survived. To commence with the excisions of the elbow-joint, we have seven recoveries to notice. It would naturally be supposed that, in such cases as these, in which the bones have been smashed by such terrible missiles as the modern rifle-bullets, the great difficulty would be to save enough to get any degree of firmness in the joint. It seems, however, that this is not the case. Of these seven operations five resulted in complete ankylosis, one in a movable elbow, and only one in a loose joint. The cause of the very unsuccessful results is, we think, not difficult to find. Every arm, immediately after the operation, was

firmly fixed on a splint of some kind by means of plaster of Paris. After about a month of this treatment it was in some cases put into an ordinary windowed plaster of Paris bandage. Under these circumstances, by the time any attempt was made to obtain a movable joint by means of passive motion, such firm union must have taken place between the surfaces of the bones, and such an amount of consolidation in the surrounding parts, that the necessary manipulations must have been unbearably painful. Passive movement seems, therefore, to have been more or less neglected, and as a natural result ankylosis soon rendered it impossible. One case was even allowed to ankylose at an obtuse angle. The single case which resulted in a loose flail-like joint seems to have been a primary excision, so that the author does not know how much bone was removed.

The excisions of the shoulder-joint were all secondary. The single longitudinal incision along the line of the anterior edge of the deltoid was adopted whenever possible. After the operation the favourite mode of fixing the limb seems to have been by a broad strip of plaster wound round the body, binding the arm to the side of the chest. This, no doubt, was most effectual in promoting rest, but it must have had a tendency to drag the upper end of the humerus inwards on to the ribs below the clavicle, and also to press somewhat on the vessels. Probably, as a consequence of this, we find mentioned in almost every case considerable swelling of the arm and forearm as one of the troubles during after-treatment, and the upper end of the bone is described in all cases as being in the above-mentioned position. The traction of the muscles would of course tend to pull the end of the bone into this position, but we think this was needlessly aggravated by the mode of fixing the arm. A frequent complication seems to have been burrowing of pus from the wound downwards along the humerus. This, we think, might have been prevented by better drainage of the wound. The wound left after excision by the single anterior incision is always difficult of drainage. There is necessarily a cavity of considerable size, which, unless kept empty in some way, will only serve as a pocket for the accumulation of putrid discharges. If this happens burrowing of pus, necrosis of the sawn surface of the bone, and other accidents are sure to occur. The periosteum was in these, as in other excisions, saved, as far as circumstances would allow. The final results of the successful cases were decidedly encouraging. Even where a very large amount of bone was removed a useful arm remained to the patient. Passive mobility seems to have been abundant in every case, but the power of abducting the arm was always very slight; the deltoid in most cases became completely atrophied. This seems somewhat difficult to account for, as the operation was always performed in such a way as not to injure either the deltoid muscle or the circumflex nerve.

In one case only was observed the so-called quasi-paralytic condition which has been described by some authors as occasionally following this operation. Langenbeck is of opinion that it is due merely to want of use; but in the case before us it seems hard to explain all the conditions on this theory. The wasting and paralysis of the muscles extended over the whole arm and shoulder. Passive movement was quite free in the shoulder, and to a limited extent in the elbow, yet the patient himself was unable to move even a finger. As no mention is made of any loss of sensation, we must conclude that the condition cannot be accounted for by any injury to the nerves. The state of this patient did not improve in any way during the two years following the war. In one case secondary amputation was necessary on account of osteo-myelitis of the humerus. The patient finally made a good recovery.

The excisions of the ankle were somewhat irregular in their character. Thus in one the whole astragalus was removed, in two more a slice was sawn from its upper surface and removed with portions of the bones of the leg, in one the malleolus externus only was taken away, and in the rest various irregular portions of the tibia and fibula. The results were not quite as satisfactory as those of the operations on the shoulder. One ended at last in amputation after the unhappy patient had borne the burden of a useless foot for two years. In all the duration of the treatment was very prolonged. Sinuses remained open often for months after the operation, and if the patient could hobble with a stick in a year he was fortunate. It is possible that with more judicious after-treatment the results might have been better. The position of the foot was in three cases that of slight valgus; in another the heel seems to have been allowed to drop considerably backwards. These accidents seem to have arisen entirely from fixing the limb for some weeks together in a plaster of Paris bandage, the position of the foot at the time not being perfect. An interesting fact to which Professor Bergmann draws attention is that the production of new bone round the joint is much greater after operations for injury than after those performed for disease. From this it arises that movement is difficult to obtain after excision of the ankle-joint, complete or almost complete ankylosis being the rule. The results, however, such as they are, justify the opinion of Langenbeck, that this operation may in many cases serve as a substitute for amputation.

The work is admirably illustrated, with one page of lithographic drawings of the fragments removed in the various operations, and twenty excellent photographs, showing the results of the more successful cases. We have no doubt it will help to encourage the rapidly increasing tendency to conservancy in our treatment of injured joints.

On Deformities of the Fingers.[1]—The author commences by stating that although the study of the pathological conditions of the fingers has long attracted the attention of surgeons, it is remarkable that, although they mention the constriction, notches, complete or partial amputations, deformities and shortening of these members, no one has sought to reconcile these singular conditions, and the indications which they may furnish both as to prognosis, treatment, and prothesis. This is the object of the author in this treatise, and he doubtless has succeeded in doing so. The first of the series he takes is the ainhûm, the cause and nature of which, however, he confesses is not understood. He subsequently discusses spontaneous amputations, &c., those of a traumatic character, those consequent on irregularities of vascular nutrition, on syphilis, or dependent on want of nervous nutrition. The work is useful as one of reference, since the author has collected cases affording the reader interested in this peculiar subject an easy means of reference, but the only originality we are able to discover in the book lies in its painstaking compilation.

Fracture of the Lower End of the Radius.[2]—That errors in teaching, and mistakes, are multiplied in text-books from the blind acceptation of whatever has been advanced by great names, has at last begun pretty generally to be acknowledged. In our own experience, the subject of Prof. Gordon's treatise is perhaps one of the very best examples of this state of things. In the first portion of his work on the mechanism of the production of and the subsequent treatment of Colles' fracture, he shows that its cause is really the result of a "cross breaking strain," from forced extension of the hand, displacing the lower fragment backwards and outwards, with an alteration of the aspect of its carpal surface, and that its treatment by the pistol or straight splint cannot correct the deformity. We have long been at loss to reconcile the explanation of the *cause* of fracture usually given, with the results. "Gordon's splint" is so well known that to transcribe here its author's description and laudation of it would be unnecessary; but we may mention that he has improved upon his original model by the addition of an ulnar portion, which prevents the rolling outwards of the splint, or its being displaced downwards upon the lower fragment—the weak point hitherto in the apparatus.

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[1] *Des difformités des doigts (Dactylolyses); Etude de séméilogie.* Par le Dr. G. BEAUREGARD (du Havre).

On the Deformities of the Fingers (Dactylolyses); a Study in Semeiology. By Dr. G. BEAUREGARD.

[2] *A Treatise on the Fractures of the lower end of the Radius, or fractures of the Clavicle (and their treatment by a new clavicular apparatus) and on the reduction of recent inward dislocations of the shoulder-joint by manipulation.* By ALEX. GORDON, M.D., Prof. of Surgery, Queen's College, Belfast, &c. 1875.

Prof. Gordon affirms that *impaction* after Colles' fracture is untenable, "a mere phantom of the imagination, from the erroneous interpretation of pathological facts," these facts being that there is a new deposition of bone behind, with shortening of the radius externally and posteriorly. He next discusses the second form of fracture of the lower end of the radius, in which that bone is broken an inch or more above its lower end. The fractured ends are displaced backwards and inwards, producing a well-marked prominence on the back of the lower part of the forearm, with an increase of the concavity of the radius.

In the third form, or articular fracture, the fragment broken off contains the entire carpal surface, the styloid process, the supinator ridge, and a small portion of the anterior surface of the radius; this fragment is displaced upwards, forwards, and outwards. For each of these his splint, by a different arrangement of the fixed portions and straps, appears to be very applicable. With regard to fractures of the clavicle, he considers that the doctrines taught on the pathology and treatment are in many respects erroneous, and his own conclusions are as follows: 1. That the shoulder, though apparently, is not in reality depressed by the weight of the extremity. 2. That there may be a slight depression, due to the scapula falling inwards and forwards upon the thorax. 3. That to push the shoulder upwards, backwards, and outwards must increase the deformity, as this movement deprives the scapula of the support of the thorax. 4. That the only requisite condition in the treatment is to rotate the shoulder outwards and backwards, and maintain it in that position until osseous union has taken place. 5. That fractures in the outer third of the clavicle, which have been described and figured as external to the coraco-clavicular ligaments, are really in that part of the bone into which the ligaments are inserted. 6. That the inner or sternal fragment of the clavicle admits of movement in a direction forwards, upwards, and backwards. It is very frequently displaced, and the main cause of its displacement is the action of the outer fragment pushing it forwards and upwards. The apparatus appears to be rather complicated, but is doubtless effective, at least it is certainly ingenious enough to be so, and would seem to be capable of treating most injuries of the upper portion of the shoulder girdle by changes in its method of application. Prof. Gordon's idea on reduction of the recent dislocation of the shoulder-joint by manipulation have already appeared in the 'Brit. and For. Med.-Chir. Review,' July to Oct., 1866, and in the volume before us he amplifies his statements.

Surgery of the Arteries.[1]—Aneurism has latterly attracted the

[1] *Surgery of the Arteries.* Lettsomian Lectures of the Medical Society of London, 1875. By C. F. MAUNDER, Surgeon to the London Hospital. With 18 illustrations. London, 1875.

attention and study of several of our own countrymen, with the result, if of nothing peculiarly original or scientific, at least of adding very considerably to the value of the literature of the subject: of these contributions the volume before us is a verbatim transcription of Mr. Maunder's, with some additions, Lettsomian discourses.

The first of these on aneurisms contains a thoroughly practical and well illustrated treatise on the nature, treatment, and surgical anatomy of the blood-vessels, and concludes with the following review of the author's experience: 1. That no case of aneurism is to be regarded as necessarily incurable. 2. That some cases of aneurism are apparently cured by absolute and prolonged rest, restricted diet, and other medical treatment. 3. That, when possible, compression either proximal or distal, is to be employed in addition. 4. That in all aneurisms in which treatment by ligature is known to be of a very fatal operation, the above rules are to be first applied. 5. That the treatment of progressive aneurism at the root of the neck, by the distal operation, is justifiable after medical treatment has failed. 6. That in rare instances only may an aneurism be treated by ligature before compression has been tried and has failed. 7. The digital is to be preferred to instrumental compression. 8. That anæsthetics and morphia are valuable aids to compression. 9. That chloroform will probably prove to be a more effectual agent than morphia in all cases, but more hazardous. 10. That the value of morphia should be more tested.

In the second lecture on wounds, hæmorrhages and the antiseptic ligature, Mr. Maunder gives details of nineteen cases of wound and hæmorrhage which have come under his care, in which ligature of an artery in its continuity has been resorted to—with details of the methods of ligature. His conclusions are as follows: 1. That no operation is to be performed when bleeding has ceased, unless a repetition of it would directly endanger life. 2. That the bleeding vessel is to be sought at the seat of injury, and to be secured, if divided, at both ends, either by ligature or torsion, if only wounded, by a ligature above and below the wound, or after section, by torsion. 3 .That the injured vessel is only to be tied on the cardiac side of, and at, a distance from a wound in it, when the attempt to secure it at the wound has either been made and failed, or when such an attempt would be either anatomically injurious or pathologically useless. 4. That it is desirable to ligature the brachial artery rather than both radial and ulnar, for secondary hæmorrhage from the hand. 5. That ligature of the brachial while it stopped bleeding, also arrested destructive inflammatory changes caused by useless local efforts to check hæmorrhage. That blood flowing from the distal side of a wound in an artery or ligature upon it, will in the lower extremity be often, in the upper extremity occasionally, *venous* in colour and that in malignant disease

when the growth cannot be removed and it is impossible to check bleeding by milder measures, the feeding artery may be ligatured in its continuity. 7. When a part is more or less disorganised and hæmorrhage renders repair very doubtful, amputation should be performed, both to arrest bleeding and to remove a hurtful member. 8. Indirect compression will occasionally arrest severe bleeding. 9. That both the axillary and femoral arteries may be wounded, and a pulse felt at the extremity of the limb. 10. That a wound in an artery may be recognised by the warm blood impinging on the inserted finger. 11. That direct compression upon the bleeding point will often succeed *after* the main artery has been tied, though it failed *before*, and this fact is a justification for tying a main vessel. With regard to the use of the antiseptic ligature, Mr. Maunder after mature consideration has arrived at the conclusion that the fate or behaviour of a given antiseptic catgut ligature, applied to the continuity of an artery cannot be foretold. From some remarks in his concluding lecture on ligature of the main artery to arrest acute traumatic inflammation, Mr. Maunder, it would appear, narrowly escapes being an originator, and as far as Europe is concerned perhaps he is, in suggesting and carrying out ligature of the main artery in these cases, as a step further, and a more effectual method of treatment than compression. He argues his point well, and adduces instructive cases. We must refer the reader to the book itself, which is well worthy of perusal.

Parkin on Climate and Phthisis.[1]—The author of this small treatise is known to the profession by several works on epidemiology of considerable merit, each and all indicating a writer of decided character with very positive opinions. The present treatise furnishes another example to the same effect. Dr. Parkin has decided that all climates south of England are not only a mistake for the residence of consumptive patients, but actually detrimental to them. He blows a strong counterblast against all the fashionable places of resort for phthisical patients outside England; and if he succeeded in persuading mankind at large of the excellencies of the English climate for such sick folk, our little island should become the sanitarium of the world.

To Dr. Parkin must therefore be assigned the merit of boldness; for it is a bold man who attacks single-handed the cherished opinions and prejudices of his age. The question then must be, how far his attack is quixotic, and how far deliberate and reasonable. Our examination of his treatise leads us to the conclusion that his onslaught, though bold, cannot be sustained, and will not bear logical and critical examination. At the same time we commend it

[1] *Climate and Phthisis; or, the Influence of Climate in the Production and Prevention of Phthisis.* By JOHN PARKIN, M.D., F.R.C.S. London, 1875.

to the attention of our readers; for doubtless it raises questions on points which have been too quietly acquiesced in, and brings forward arguments and examples inimical to many pet theories and to much practice, which may be fairly called empirical. It is something meritorious to make a stand against the expatriation of our countrymen and countrywomen, so fashionable with the public and too indiscriminately recommended by medical men. It is something commendable to direct attention to the evils and dangers to health consequent upon enforced residence in foreign lands, and inseparable from it by reason of sudden and great climate changes, of altered and less sanitary social conditions, and of the deprivation of numerous home comforts and enjoyments. And, lastly, it is making a good point to show the probability that cold is not so morbific an agency in consumption as popular opinion represents it.

But Dr. Parkin is not content with any middle course. His denunciation is absolute and general. He puts forth his own arguments as irrefragable, and produces a long list of quotations from authorities, of one sort and another, to bolster them up. But he has no vision for opposite opinions and facts. He is a special pleader, and sees only his own side of the case. He can only quote writers on health resorts when he has some special instance to bring forward in opposition to their statements. He is silent on the subject of meteorological observations and charts which might go against his notions; and he opposes his individual opinion, gathered from casual observation, or a brief knowledge of localities, to that of trustworthy practitioners in those localities; lastly, he handles statistics in a loose manner, and ingeniously makes them tell in favour of what he contends for.

He sets out with the illogical proposition, that if the climate of England be chargeable with the production of consumption, and if this disease were unknown in other and different regions, therefore removal to these latter localities "ought always to be attended with beneficial results." Equally illogical is the wide assertion made with regard to those sent away from home, viz. "We only know that a large proportion of them die, either before or after their return—a proof that removal to another climate will not cure phthisis in the majority of cases;" for common experience tells us that it is the lot of all to die; and medical experience convinces us that phthisis is a very fatal disease, and curable in a majority of cases neither by change of climate nor by any other agency. Again, it does not, to our apprehension, solve the problem as to the value of a transfer to other climates to show that healthy Englishmen absent from home may become phthisical, and that phthisis prevails in foreign parts. Very few are the diseases which are restricted to particular lands, and wanderers from their native shores may, at any part of the earth's surface, be attacked by maladies familiar to them

at home. And because diseases are diffused, it does not follow that change of climate will not be beneficial to their sufferers; and even so, at times, when the climate resorted to is, on general grounds, not so salutary.

The basis for his attack on foreign climes is chiefly founded on the statistical returns of the army and navy. These show that the mortality among our soldiers and sailors is greater when on foreign service than when in home stations; and the more so when they serve in warmer climates. From the statistics he quotes, this appears to be true, not only of chest disease, but also of phthisis. But we must not omit to notice that when Dr. Parkin has not exact figures in respect to phthisis, he manipulates those representing chest diseases generally so as to favour his conclusions. He assumes that a certain percentage of those cases were cases of consumption, and argues therefrom. Moreover, where, as in the instance of the white troops in the West Indies, the ratio of the mortality is less than in England, he finds at once an explanation in the fact of the greater prevalence of fever in those islands, and in the assumption that "by a wise law of the economy, two specific diseases never prevail at the same time in the same subject."

He joins issue with those who maintain that cold and humidity are productive of consumption; and here we may at once observe, that the author's entire course of argument is against the doctrine that those agents are the positive causes of consumption, and that he is consequently combating an opinion which would find very few advocates. He analyses Dr. Buchanan's tables, constructed to show the influence of humidity of locality, and ingeniously extorts the conclusion that "the ratio of deaths is greater on very dry than on very wet soils," and that "wetness of soil is not a cause, either remote or proximate, of consumption." By like ingenuity of reasoning, he would make us believe that intermittent fever is a good thing as an antagonist to phthisis; and, with his characteristic boldness of opinion and operation, he denounces land drainage as a mischievous proceeding to health. Moreover, in forgetfulness of his general conclusion that England is the best home for the consumptive, he tells us that why phthisis is so prevalent in this kingdom, is "because it is the best drained country in the world;" ergo, by his logic, the most productive of phthisis. With like contrariety to general opinion he holds that "fogs and moisture may prove beneficial rather than otherwise, under certain circumstances, and in cold latitudes;" and that a dry warm climate is unfavourable to phthisical people.

Respecting the producing cause of phthisis he argues that as it is a universally diffused disease it must have a general cause, and that such cause is "the poison termed malaria." This poison produces effects varying in degree and kind according to the amount diffused in the atmosphere and the range of temperature of localities.

If it exists in a higher degree it develops fever, remittent or intermittent; but if more diluted, it causes phthisis. But concurrent circumstances, foremost among which is cold following heat, by determining the poison to the internal organs rather than to the external surface, favour its absorption in small quantities and the production of phthisis, the immediate cause or foundation of which is congestion or inflammation of the lungs.

Thus far we have only examined the author's introductory chapter. In the one following he carries his reader on an excursion to the principal health stations in repute; and by one sort of evidence and other delivers a verdict against them all as fitting places for invalids. Dr. Bennet gets severely dealt with; all his facts and figures are set at nought; and the Riviera, or "Undercliff of Southern Europe," is declared to be one of the most *unfavourable* and dangerous climates for chronic diseases of the respiratory organs, and especially for phthisis. Naples, Pisa, the Mediterranean generally, Malta, Egypt, Algeria, and Peru, get no better character; nor do the West Indies and Madeira. Even the very generally approved plan of a sea-voyage is pronounced a mischievous delusion; and the general outcome of his diatribe is, that, "in fact, every degree of latitude southwards that a consumptive patient traverses is simply putting another nail in his coffin."

Immediately after making this figurative denunciation, he adds, as if it were a necessary inference, "If so, Egypt must be an improper abode for" consumptives!

His next position is, that "the warmer the climate the greater will be the rapidity of the disease, and consequently, its fatality;" and, as a corollary, he adds, "not only is a hot climate inimical to consumptive patients, but the summer season in temperate and cold climates, more especially when the temperature is above the average, is found to be more prejudicial to such persons than the winter season;" an assertion, it is enough to remark, in direct contrariety to the results of all statistics, as Dr. Parkin may see by reference to the returns of the Registrar-General.

Having before his eyes the conclusions of another bold generalizer, he vigorously attacks the doctrine of the production of phthisis by re-breathed air; and in his ardour becomes the champion of impure air and bad ventilation. When a man's judgment is thus warped by his own theories, it is vain to argue with him; we consequently leave Dr. Parkin to the enjoyment of his eccentric notions on the excellencies of foul air and of malaria as an antidote of consumption and of malaria as its cause.

In the essay presented, the author has started with a foregone conclusion, and has, probably unwittingly, selected and adapted facts and statements to suit it. He talks of phthisis or consumption as if it were a disease of one type or character, the product of only

one kind of pathological process, and a condition amenable to only one description of climatic treatment. He may be right, in a large number of instances, in recommending consumptive patients to avail themselves of the health resorts of their own country, but his wholesale condemnation of foreign warm climates cannot be sanctioned. It is fully justifiable to hold aud to seek to prove the hypothesis of the malarial origin of phthisis, but it will need a very different course of proof and argument than furnished in this volume to secure its acceptance.

Transactions of the Obstetrical Society of London.[1]—The present volume is a handsome record of the year's work of one of our most flourishing medical societies. It contains a number of valuable papers, and, what is often of equal importance, the discussions elicited by them and by the exhibition of specimens and new inventions. Moreover there is here recorded in compact and permanent form the instructive discussion which took place before the Society last summer, on the subject of Puerperal Fever. Among the more noteworthy of the papers are the following :—Report of two cases of Cephalotripsy, by Dr. Braxton Hicks; two papers on Epithelioma of the Cervix uteri complicated with pregnancy, by Dr. Savory, and by Dr. Edis; inversion of the uterus after delivery, by Mr. Gervis; and two papers on Rupture of the Vagina with Recovery, one by Dr. Heywood Smith, and the other by Dr. Wiltshire. Both these cases are remarkable; in the latter, the rupture had occurred spontaneously before assistance could be rendered.

Some of the specimens exhibited were of unusual interest *e. g.* Dr. Wynn Williams's of intra-mural calcareous tumour impeding delivery; Mr. Oliver Barker's, of an anencephalic fœtus (dere-encephalic variety of Geoffroy Saint Hilaire); and Dr. Godson's of curious pigmentation of the breasts of a girl, of which an admirable coloured plate is given.

The discussion on puerperal fever was of great value in enlightening many on a subject concerning which very vague notions were prevalent among the bulk of the profession.

Although it cannot be said that the difficulties were, by any means cleared up, yet their true character became plainly apparent; and it is not too much to say that newer and truer ideas were inculcated in the course of the prolonged debate on puerperal fever which took place. The very form in which the subject was so ably introduced by Mr. Spencer Wells, suggested new lines of thought to numbers who had been accustomed to look upon "puerperal fever" as an entity, and not as a term representing a variety of very different diseases having but one thing in common, viz. their occurrence in puerperal women.

[1] *Transactions of the Obstetrical Society of London.* Vol. xvii, for 1875. London, 1876.

This was no small gain, for it opened the eyes of many to the numerous channels by which "puerperal fever" might assail the lying-in woman, *e.g.* erysipelatous, diphtheric, scarlatinous, cadaveric, and such like poisons on the one hand, and the autogenetic conditions generating or accompanied by a pyrexial state on the other, such such as cellulitis, retention of placental or other tissues, &c. Although it was granted on all hands that all these varied causes, infecting or other, were attended by a very similar train of symptoms in the lying-in woman, being governed by the puerperal state, yet there were but few advocates of the view that puerperal fever is a separate and distinct disease.

Dr. Fordyce Barker, of New York, most ably and eloquently advocated this view in a brilliant address; but opinions leant to the other side. The discussion well deserves reading carefully in spite of the occasional interpolation of some crudities by ambitious speakers. It has been admirably summed up and reviewed by the President, Dr. Priestley, in his annual address delivered at the January meeting of the Society.

The volume is in every way creditable to the Society and to those responsible for its year's work.

Aveling's Memorials of Harvey.[1]—The smallest contributions towards helping us to rightly appreciate so illustrious a man as Harvey, and to understand his works and his claims as a great discoverer, must be welcomed, and to Dr. Aveling belongs the credit of hunting out some minor incidents in the life of Harvey which his biographers do not appear to have noticed. Dr. Aveling also presents some facsimiles of Harvey's writing and autograph which in themselves are interesting; but we fancy he is in error respecting the great rarity of specimens of Harvey's handwriting, and that greater research will soon bring to light many considerable examples of it. For instance, we are told in the life of Harvey, prefixed to the College of Physcians' edition of his works, that his treatise on anatomy is deposited in the British Museum library; a statement which we read as referring to the original manuscript. Moreover, it is presumable that, remembering how largely Harvey was engaged in controversy with opponents both at home and abroad, more or less of his extensive correspondence, in his own handwriting, still exists. And should a satisfactory biography be written, such as Dr. Aveling desires, it would be desirable to make diligent search among the MSS. preserved in foreign libraries, especially in those universities with which Harvey's friends and adversaries were connected.

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[1] *Memorials of Harvey; including a letter and autographs in facsimile.* By J. H. Aveling, M.D. 1875.

Epidemics and Epidemic Media.[1]—This is a very able contribution to the subjects of epidemics and the circumstances under which epidemics are developed or favoured; and if all the conclusions arrived at by the author are not wholly convincing, the result is due rather to the intrinsic difficulty of the subject than to the manner in which it is treated. But as the confession of ignorance is the first step towards the acquisition of knowledge, so a plain statement of the difficulties surrounding any branch of scientific investigation is the first effort towards overcoming them. In M. Colin's essay these difficulties are very plainly set forth, and where human interference has succeeded in preventing the outbreak or arresting the course of what are known as epidemic maladies, the means of success are clearly indicated, and suggestions for still further amendment are cautiously advanced.

In the first part of the work, the question is discussed of the meaning which should be attached to the term "epidemic," and although the word is so engrafted on our common forms of speech as apparently to need no explanation, yet it will be found that its exact definition is somewhat difficult. In its etymological sense, and as originally used by Hippocrates, the word simply meant any disease which was prevalent among the people (επι δημος) at a particular time, and it might be used for bronchitis or catarrh as much as for typhus fever or cholera. Then, again, as M. Colin observes, a division has been made into *great epidemics* and *small* ones, the difference depending not only on the relative degree of frequency or severity of a given invasion, but on the fatality of the symptoms and the mysterious nature of its origin in the former class as compared with the latter. But, as M. Colin remarks, the difference between great and small epidemics is very ill defined, and what is a small one at one time may become a great one at another; and common diseases, such as small-pox, measles, scarlatina, typhus, may become entitled, under certain circumstances, to the name of great epidemics, while rare diseases of a very severe and fatal character, by being limited to certain countries and certain social classes, may be called small ones. It is also a curious but well-known fact, which has not escaped M. Colin's notice, that certain maladies which assume a dangerous epidemic character in certain countries may present a much milder type in other lands at no great distance, as for instance in the case of scarlatina, which unhappily prevails as a destructive scourge in our own country, but, notwithstanding our constant intercourse with France, only occurs in the latter nation as a rare

[1] *Epidémies et Milieux Epidémiques.* Par Léon Colin, Professeur d'Epidémiologie au Val-de-Grâce, &c.
Epidemics and Epidemic Media. By Léon Colin, Professor of Epidemiology at Val-de-Grâce. Paris. Pp. 114, 1875.

visitant and is generally rapidly arrested in its course. On the other hand, M. Colin refers to typhoid fever which, as is equally well-known, is endemic in France, and is mainly due, as he might have remarked, to defects in hygienic arrangements in the French capital and in most French towns.

These and the like topics constitute the material of the first part of M. Colin's essay, and the second part is devoted to epidemic media (milieux épidémiques), which include a great number of elements or factors out of which epidemic maladies arise. These elements are not confined to external circumstances, such as soil, climate, weather, season, &c., but they include the human organism itself, which is differently adapted to the reception of morbid material according to race, to peculiar predisposition, and even to temporary causes, as poverty, misery, and want of food. Hence M. Colin divides epidemic media into two categories, namely, into such as are stable or permanent, and such as are variable or adventitious. In the former are included the influeuces of climate, of locality, and of soil, the last, which is now called the telluric influence, exercising a greater power over the generation of epidemics than was once supposed. Among the variable media are the influence of the seasons; the influence of what is called the medical constitution, as where cholera breaks out during or after a period marked by numerous cases of diarrhœa; and individual influences, or those which are peculiar to persons themselves. Among these last influences, some for good, some for evil, are the crowding of persons in large towns or in ships at sea, as predisposing to or even causing typhus; and acclimatisation as protecting the inhabitants of marshy places from intermittent fever, vaccination as protecting against small-pox, &c.

With regard to the question, so much debated in the present day, of the influence of drinking water in the production of certain diseases, especially dysentery, typhoid fever, and cholera, M. Colin writes with considerable reserve. From the nature of these maladies, which present a superabundance of intestinal evacuations and a deranged condition of the alimentary tube, he admits the facility of explaining their propagation by the supposed existence of morbid germs in drinking water and the consequent admission of these germs into the digestive system, but nevertheless he doubts whether this view is sufficiently supported by the facts, and he adduces researches carried out by himself in opposition to the received opinions on this point. But while denying the specific effect caused by drinking-water, he nevertheless believes that in the course of epidemics, the ingestion of waters of bad quality plays a rather considerable part in predisposing the body to receive the morbid influences, and he therefore recommends that the danger of drinking them should be magnified rather than otherwise in order that it may be combated more efficiently.

Original Communications.

I.—On Meningitis and allied Changes in the Meninges of the Insane.—By Robert Lawson, M.B., Pathologist and Assistant Medical Officer, late Clinical Clerk, West Riding Asylum.

The almost constant existence of striking modifications in the brain membranes of the insane contrasts strongly with the very limited amount of change which, as a rule, is apparent to the naked eye when the brain substance is exposed by section. And even though these changes in the membranes cannot, as a rule, be regarded as in themselves pathognomonic of any particular form of insanity or even of insanity itself, yet their extent, their peculiarities of character, the time of life at which they occur, and the manner in which they must of necessity modify or obstruct the functional activity and alter the organic structure of the peripheral surface of the brain, point to them as conditions which could not fail to produce or at least to be associated with symptoms of mental aberration. In almost every instance in which the brain of a lunatic is submitted to examination the mere removal of the dura mater discloses such divergences from the appearances of health as to excite the attention of even a superficial observer. Serous fluid containing pus, lymph, or blood escapes in considerable quantity; marked opacity perhaps characterises a large stretch of the most prominent and anterior portions of the subjacent membranes; pus runs in the line of the large vessels; patches of organised lymph suggest the existence of recent inflammation, or bony plates afford permanent evidence of a process which, beginning in local meningitis, had passed through successive stages till a pathological exudation attained to the histological maturity which in health is a stage beyond the proper texture of the membranes. Large extravasations of blood occupy the arachnoid sac, or complete cysts are developed from them and separate the layers of the serous membrane from each other. Or the pia mater stands forward as a thick edematous membrane covering water-logged convolutions; and coexistent with any or all of these conditions the inner membranes may be firmly united to each other and the pia mater so intimately incorporated with the outer layer of the grey matter as to be incapable of removal without laceration. When this state of the meninges does exist the forcible

removal of the membranes leaves a roughly granular surface which frequently characterises the lateral aspects as well as the tips of the denuded convolutions. There is no doubt that some of these conditions have been frequently demonstrated as compatible with mental health. A similar opacity of the membranes, for instance, is frequently if not almost invariably found when the dura mater is removed from the brains of drunkards. But when this instance of marked opacity occurring in the brains of the ostensibly sane, is adduced as evidence that the change is not pathognomonic of insanity, grave doubts must arise as to whether or not this condition of the brain membranes of drunkards has not been induced by an artificial production of repeated attacks of mental disorder or a chronic change of cerebral nutrition dependent on the constant employment of substances which have a powerful action on the function of the brain, and a certain capacity for modifying the structure of it and its membranes. The occurrence of marked opacity and of alterations in the thickness of the membranes during the earlier years of life are more characteristic of insanity than those which occur in the dementia of later years, inasmuch as they denote a pathological anticipation of change which, by the operation of the vital laws regulating the modifications which occur during the incessant destruction and replacement of tissues, would in after life have to some extent taken place as a physiological process.

The dura mater, the arachnoid and the pia mater, are each of them liable to be affected in insanity in such a way as to lead to the production of new or the intensification of developed symptoms. Perhaps of all the membranes the dura mater is least liable to undergo such changes as lead to active and prolonged interference with the brain function. Occasionally, from the irritation of blows, or of subcutaneous inflammation of the scalp; from the direct action of inflammation of the subjacent membranes or caries of the bones external to them; from the irritation of tumours in its own substance or pressing upon it from within or without the membrane, the dura mater takes on a form of acute or chronic inflammation, which, if the seat of the inflammation is exposed to atmospheric air, tends strongly to suppuration. Less rapid and acute forms of pachy-meningitis externa, however, are characterised by simple adhesion to the inner surface of the cranium and progressive thickening of the membrane by increased growth of connective tissue elements. This new substance causes the adhesions already referred to, and is most rapidly produced in the neighbourhood of the sutures, especially the sagittal one. This new development of connective tissue is not confined to the external surface of the membrane, but takes place also in its interstices, and the exudation which gives rise to it may stop short at the stage of loose cellular tissue or develop into bone (Rokitansky). It is probable that when bone is the product, the origin has been

in the parietal portion of the arachnoid, though latter on the spiculum or lamina may appear to have more connection with the dura mater proper. Still there can be no possible reason why these bony plates should not be developed in the dura mater as well as from the arachnoid. Several instances are recorded in the West Riding Asylum manuscripts in which they were developed in true brain substance without being at all visible on the surface or perceptibly connected with the membranes. The thickening of the dura mater in these cases of prolonged inflammation is associated with induration and the existence of callosity. Sometimes it happens that during the interstitial changes in the membrane a sinus becomes occluded. Rokitansky has made this observation, but no instance of the occlusion of a sinus by the proliferation of connective tissue in the dura mater is recorded in the books of this asylum. A case of scirrhus of the dura mater, however, illustrates so exactly how such a condition might be induced that I shall quote the description of it from the post-mortem record.

J. C. *Case of Mania.*—The skull in this case was of average thickness, except in the occipital region, where it was much thickened. Where this great increase of osseous substance existed, the dura mater was strongly adherent to the bone, especially in the middle line corresponding with the position of the falx cerebri, and to the right of that line. On both sides there were deeply excavated grooves for the meningeal vessels. There was a remarkable thickening of the dura mater—amounting to a tumour—corresponding with the posterior half of the attachment of the falx into it. This tumour was about three inches long and an inch thick. It completely occluded the posterior half of the superior longitudinal sinus, and extended downwards into the falx and laterally into the dura mater. It was densely fibrous in consistence, and on section showed a glistening white and yellowish white appearance. It creaked under the knife, and had quite the look of scirrhus. It did not extend below nor involve the tentorium. There was slight wasting of the gyri of the frontal and parietal lobes, and very marked wasting of the first annectent and first tier of occipital gyri on the right side where pressure had been exerted by the tumour, which on this side was much larger than on the left. The substance of the postero-parietal lobule on the left side was adherent to the dura mater, and its surface was abraded in removing the membranes. These adhesions were close to the median fissure. The superficial cerebral veins were almost empty. The pia mater was very thin and stripped freely. There was no thickness nor cloudiness of the arachnoid. The brain substance was soft. There was cancer of the liver.

History and Progress.—The patient had a well-marked history of

idiocy in his family. Previous to his admission, in 1872, he had been insane for several years. During that time he had bouts of drinking and attacks of marked excitement followed by great depression. He was pugnacious and of sullen temper. Between his outbursts of profligacy and dissipation he was ostentatious in the performance of religious ceremonies. His mental derangement was popularly dated from a blow on the head. During the twelve years of his residence in this asylum, the symptoms prominently manifested by him were those of mania with alternating periods of depression. He was very restless and discontented, and repeatedly made his escape. Several times he had severe attacks of abdominal pain and bilious vomiting. Towards the end of 1873 he began to sink, and died rather suddenly in January, 1874. There is no history of paralysis.

Our museum contains numerous specimens of bony laminæ and spicula, resulting from inflammatory and developmental changes in the dura mater. The following abstract will convey an idea of the post-mortem conditions and the symptoms which generally characterise such cases:

A. R.—This was a case of mania. On post-mortem examination the bones of the skull were found to be greatly and universally thickened: they were of a dark colour and deeply eroded for the meningeal arteries. The dura mater was not adherent to the skull, but was firmly attached to the brain in the region of the longitudinal fissure. This adhesion was most marked posteriorly where near the perpendicular occipital fissure there was a plate of bone about the size of a sixpenny piece, and irregular in shape. The arachnoid was not clouded, and the pia mater stripped freely. No embolism existed in any of the vessels at the base, and only a small spot of extravasated blood was found resting at the angle of the horizontal limb of the fissure of Sylvius and the ascending parietal convolution. The ganglia at the base were wasted.

Symptoms and Progress.—This patient was admitted in December, 1866. She was very incoherent in her conversation and irrational in her conduct. She was liable during her stay in the asylum to frequent outbursts of excitement, but in the intervals was quiet and peaceable. During her periods of excitement she was very quarrelsome and disposed to fight. On the 9th of August, 1871, she was found in a comatose condition. Her head was thrown back and she breathed slowly, laboriously, and with stertor. The skin was moist and the temperature 102°. The pupils were insensible to light, and the right one dilated. The mouth was drawn to the right side and the flaccid facial muscles were at intervals twitched convulsively. Similar twitchings were frequently observed to occur in the limbs of the right side. The left leg was occasionally moved in a similar

manner. On the evening of the same day the temperature was 106°, and the pulse 120. The patient appeared to be partially conscious, but the twitchings continued. Next morning vomiting set in, and was accompanied by hiccough and subsultus tendinum, and followed by coma and death.

It is observable in connection with these and other cases of inflammation of the dura mater, that the pathological tendency of the disease is to produce thickening not only of the membrane itself, but also of the cranial walls; that the conditions which lead to these changes lead also to the development of bony plates in the dura mater, and an analogous increase in the production of the proper osseous material of the neighbouring bones, which renders the calvarium not only thick, but dense and hard. In addition to this, the inflammation of the membrane has some tendency to spread to the subjacent textures, and in this manner local adhesions are found binding all the membranes together, at the same time that the pia mater becomes adherent to the brain, by products of such exudation as is thrown out between their opposed surfaces and around the vessels which pass from the one into the other. At the same time it is evident, from a review of numerous cases, that pachymeningitis externa is a form of inflammation which is relatively unimportant as an agent in the establishment or intensification of insanity. When it is observed in lunacy practice, it is generally found in cases where it is exceedingly difficult to determine the existence of any causal relationship between the meningitis and the cerebral disorder. The only observation on the subject for which there appears to be a reasonable amount of evidence is that chronic pachymeningitis with thickening of the dura mater is generally associated in the insane with outbreaks of excitement, in which pugnacity is a somewhat common feature.

With regard to the arachnoid and the pia mater, there appear to be two great classes into which inflammation of these membranes can be divided. The first would embrace that pathological condition in which there is marked opacity and thickening of the visceral arachnoid, associated with adhesion of the arachnoid to the pia mater, and of the pia mater to the convolutions. The second, though forming one group, would embrace numerous varieties, of which the common characteristic would be the distinct exudation of lymph and the presence of pus in relation with the gyri, the pia mater, or the arachnoid. In one respect this division is not strictly accurate, inasmuch as the opaque thickened and adherent condition of the membranes, which is present almost invariably in cases of general paralysis, though to a great extent the result of a slow proliferation of connective tissue, owes some of its features to the exudation of lymph occurring during transient attacks of cerebral hyperæmia. Still the division is sufficiently precise to afford a patho-

logical distinction of two forms of meningitis, of which the one is generally acute or intercurrent, and the other generally chronic. Nothing can be more striking than the thickening and opacity of the arachnoid and the adhesion of the pia mater, especially to the convolutions of the frontal and parietal regions, which are almost invariably found over the brains of general paralytics. The frequency with which this condition occurs in an asylum, where every death is followed by a post-mortem examination, and the observations of Rokitansky, Griesinger, Niemeyer, &c., emphatically confirming the opinion that chronic inflammation of the pia mater and arachnoid is a pathological condition which is always observed in cases of dementia and paralysis, resembling those of general paresis—should do away with every doubt that this form of meningitis is an important accompaniment of the latter disease. At the same time unaided and microscopic observation shows that the cortical substance undergoes modifications which, if to some extent they are dependent on the inflammatory changes in the membranes, are probably in a greater measure primary in their origin and independent in their progress. Rokitansky, Griesinger, Niemeyer, and others who have followed in their course, are shown by observations which are almost daily made in this asylum to have failed in differentiating sufficiently between the pathology of the brain membranes in prolonged simple dementia on the one hand, and general paralysis on the other. They speak as if the opacity of the arachnoid and the adhesions of the pia mater were pathological accompaniments of the dementia which is common to both. But a retrospect of numerous post-mortem examinations and an analysis of records extending over more than eight years, show that the alterations in the membranes are in the one set of cases very different from what they are in the other, both in the method of their production and in the actual pathological condition which characterises them after death. Thus in cases of senile or prolonged consecutive dementia, the common condition of the arachnoid is that of opacity and thickening. But this opacity is not as a rule so extensive as that which occurs in general paralysis, and the relation between the pia mater and the surface of the convolutions is very different in the two forms of disease. In general paralysis the pia mater in the parietal and frontal region is almost without exception firmly adherent to the grey matter. Sometimes, also, this adhesion occurs in the temporo-sphenoidal region, and less commonly over the occipital lobe. This adhesion is evidently of a most intimate character, as on removal of the pia mater a considerable depth of the grey matter is torn away, leaving a lacerated and completely disintegrated surface. In long-standing cases of simple dementia, the relation between the pia mater and the convolutions is completely different. The membrane lies so loosely over the convolutions, that if care is exercised the

whole pia mater of one side may be removed intact. In general paralysis the gyri which are most conspicuously wasted are those over which the adhesions of the pia mater and the opacity of the membranes are most decided, showing that the associated condition of the gyri and the membranes is due to some abnormal influence which in its operation involves the convolutions and the membranes in simultaneous pathological changes. In prolonged and uncomplicated dementia, however, the more general wasting of the gyri is not associated with any changes in the membranes that cannot clearly be traced to proliferation of connective tissue and immersion in fluid exuded around the hemispheres by the withdrawal of that vascular support which, in a well-filled cranium, is afforded by the nicely graduated pressure of the brain surface. That this is a correct statement of the difference which exists in the membranes and in their relation to the grey matter in cases of general paralysis on the one hand, and simple dementia on the other, will be rendered clear by the quotation of random instances of these disorders.

C. O. *General Paralytic.*—This patient was for the third time admitted into the West Riding Asylum on the 19th of October, 1874. More than a year before this date he had manifested great mental derangement. He had had attacks of excitement in which his conduct was violent and aggressive. He was destructive in his habits and incapable of self-control. His conversation was somewhat incoherent and his speech husky and thick. He also had delusions and hallucinations. After a short time the excitement, which for the time being was the leading feature of his case, was considerably subdued. His delusions and hallucinations disappeared, and the only noticeable feature in his case was the persistence of tremulousness of the facial muscles and general uncertainty of gait. Consequently he was discharged on May 19th, 1874. On his readmission in October of the same year, it was learned that up to a short time before that date he had been able to earn his livelihood, but that latterly he had become much excited and restless in his manner, and had expressed his intention of killing two men by stabbing them with a sharp knife which he carried for that purpose. During examination he manifested emotion, expressed delusions of suspicion, and was prolix and somewhat incoherent in detailing the grounds of his insane imputations. He said that he was God's child and under God's special protection: expressed anxiety that telegrams should be sent over the country to enable him to establish his good character; and stated that he was about to marry a woman who had £800, and in every sentence showed a decided impediment in speech, and a drunken thickness of articulation. Physically he was in fair condition. The pupils were unequal, the right being the larger. There was some tremor of the tongue, and also slight un-

steadiness of gait. At this time he was 38 years of years. The following is the diary of his case:

October 20th (the day after admission).—He has been rather flighty, excitable, and talkative. Yesterday he broke a picture with the heel of his boot. To-day he is quieter, but still talkative.

December 11th.—He is evidently failing in bodily condition and losing strength. The general tremor, the defect of articulation, and the awkwardness of gait increase. At present he is less excitable than he has been.

February 4th, 1875.—He is steadily going down hill. All last night he wandered about in a restless manner, and this morning is depressed. He entertains the impression that he has committed some great crime, and asks repeatedly, "What have I done?" During the past month he has lost 13 pounds in weight.

February 5th.—At bedtime he became furiously excited, and remained so during the night. He was in incessant movement, and during the excitement resulting from vague terror trembled from head to foot. His head was shaved for the purpose of applying croton oil, after the use of which he became somewhat quieter.

March 10th. He is again more excited, and is feebler in bodily condition.

June 1st. There is hæmatoma of the left ear, and he gets daily worse.

From this time he sank and died on June 22nd, 1875.

At the post-mortem examination of this case special attention was directed to the extent of the adhesions of the pia mater. The brain was placed in water and every convolution was carefully stripped, and the result registered on charts. The arachnoid was thickened and milky in its opacity principally over the frontal and parietal regions on both sides. The pia mater was firmly adherent to the convolutions of the whole of the frontal region of both hemispheres on the orbital as well as on the vertical surface. Over the temporo-sphenoidal lobe of both sides and over the postero-parietal region on the right side the adhesions were also firm and pronounced. The ascending parietal convolution on the left side was also adherent, but that on the right side was free. The convolutions of the frontal region on both sides were somewhat wasted. The grey matter was of average thickness and ordinary colour. The white matter was somewhat soft and its vessels were coarse. There was no recent and no trace of previous extravasation. The skull-cap was symmetrical, and there were no adhesions of the dura mater, which was of ordinary thickness.

In this case the adhesions were exceedingly well marked, and the significance of the relation between the extent of these adhesions and the continuousness of the excitement which the patient manifested will be subsequently referred to. In another recent case similar conditions existed.

S. H. *General Paralytic.*—This patient, whose age was thirty-four, was observed to have undergone some decided mental change shortly before her last confinement, in February, 1873. She was admitted into the asylum in July, 1874. It was said that previous to the birth of her youngest child she had manifested a peculiar heaviness of manner and indifference of disposition. She became unable to perform her ordinary duties, and squandered money in a reckless manner and to no purpose. About this time also she was said to have had some seizure which was regarded by her friends as a slight attack of paralysis. After her confinement these symptoms continued, and as her friends were unable to manage her, she was sent to the workhouse. While under control she was less troublesome, but on being again sent home re-exhibited all her former symptoms. She was irrational in her talk, unreliable in the performance of her domestic duties, and exceedingly restless. She does not appear to have at that time manifested any delusions or hallucinations, but the symptoms recorded became intensified. Latterly she inclined to wander out of doors without purpose or rational motive, refused to work, undressed herself at all hours of the the day, and talked frequently about building churches and infirmaries.

When examined in the asylum it was found that her comprehension was dull, her manner heavy and stupid, and her memory deficient, but that she was fully conscious of these defects. She manifested considerable dementia with regard to the use of figures and the appreciation of the passage of time. Her conversation was weak and childish, but not characterised by appreciable delusions. When completely left to herself she talked in a maudlin, sentimental manner on religious subjects. Her expression was very heavy and her enunciation slow, so that her appearance and speech at the time of examination strongly resembled those of an epileptic. Her tongue was tremulous, and protruded slightly to the right of the central line. The pupils were both dilated and somewhat unequal, the left being the larger. There was no impairment of motion in any part of the body. The following notes were taken regarding the progress of the case:

July 29th, 1874.—Since admission, the patient has on the whole been quiet, but mischievous. To-day it is noticed that the irregularity of the pupils is still present.

September 11th.—No ordinary fits have been reported, but it is stated that three weeks ago, while walking round the grounds, she had three separate seizures, in which she fell suddenly forwards, appeared to be unconscious, and during the few minutes over which the attack extended was somewhat convulsed. It was reported by those in attendance upon her that the convulsions were not at all like those of an ordinary epileptic fit.

October 2nd.—Yesterday she was found to have exalted ideas. She spoke in an unusually cheerful tone about her going home. She stated that she was about to have thousands of pounds left her, and that through her relatives she would obtain large sums of money. Physically there is hesitancy and tripping of speech, tremor of the tongue, and tremulous vibration of the lips under emotion. Both pupils are dilated, but unequally so, the left being much the larger. There is also uncertainty of gait, most marked in the turning movements.

November 19th.—She has had repeated slight convulsive attacks.

January 25th, 1875.—Dilation of both pupils still well marked, but the left is double the size of the right.

February 2nd.—Appears to have had a congestive attack during the night.

April 27th.—For some time back she has been getting much weaker, and has had repeated congestive attacks. Yesterday she was sweating very profusely, and her pulse was quick and weak. To-day the pulse is 162, and the respirations 42. The percussion is marked by slight general dulness; the breathing was puerile, with the expiration somewhat prolonged, and moist rales were universally heard over the chest. The condition was one of general pulmonary congestion, with profuse œdema, from the immediate effects of which she died in about forty-eight hours.

Post-mortem examination.—The skull was somewhat more capacious on the right than on the left side. The bones were rather thick, but of average density. The dura mater was adherent to the cranium in the frontal region, and the sinuses were almost empty. There was cloudiness of the arachnoid over the whole vaulted surface of the hemispheres, except in the occipital region. The pia mater was thick and tough, and firmly adherent to the crests of the convolutions in the frontal region on both sides, and to a less extent in the parietal region. There were also adhesions on the temporo-sphenoidal lobe and the orbital surface of the frontals. The substance of the brain was generally soft and was flattened by its own weight. The grey matter was in the regions of the adhesions wasted and shallow. The white matter was soft and of a dirty colour. The ventricles were considerably dilated, and their lining membrane slightly granular. About six ounces of fluid escaped on removal of the brain and from the ventricles. The ganglia were free from clots or other appearance of marked disease.

These cases sufficiently indicate the existence of marked changes in the relation between the pia mater and the brain surface in cases of general paralysis. Frequently, however, the depth of the adhesion is greater than was observed in these two instances. Sometimes not only the crests of the convolutions, but those aspects

of them which form the walls of the sulci are involved in the morbid process. That these adhesions and opacities are the pathological accompaniments, not of the dementia, which is common to the advanced stages of general paralysis and other cerebral disorders, involving prolonged dementia, but of features more restricted in their distribution, will be evident by the perusal of the following cases:

W. R. *A Case of Senile Dementia.*—This patient, when admitted in October, 1874, was 66 years of age. It was stated that five years before admission he had manifested loss of memory and incapacity for his work as a cordwainer. He appears to have at that time shown signs of rapidly advancing dementia. Though his tools were quite near him he could not find them, and he forgot the name of the most common implements and articles of furniture. Gradually, the power of speech became impaired, till on admission he could command only a limited number of words. He became restless at night, and would frequently rise and disarrange the bed clothes. He manifested violence on several occasions, but never attempted suicide. He never had any stroke or fit of any kind, and had been a teetotaller for thirty years. On admission he was completely fatuous. When asked his name he said he could not tell, and in response to other questions gave irrelevant answers. He showed a tendency while under examination to repeat frequently words once employed. He was at the same time restless, got out of bed and made vague, indefinite, and purposeless movements. Physically he was emaciated, and had an anxious expression of countenance. His muscular movements were shaky and tremulous. In his habits also he was dirty and degraded.

On October 13th (six days after admission), he was noted as being very feeble and fatuous, and on subsequent days he was recorded as becoming gradually weaker, till the time of his death, which occurred on the 7th of November.

Post-mortem examination.—The autopsy was held 44 hours after death. The skull was of average thickness and density, and quite symmetrical. The dura mater was strongly adherent to it in the frontal region, where strips and patches of the membrane were left attached to the bone. There was a small quantity of dark coloured blood in the sinuses. The arachnoid was slightly cloudy over the parietal lobes, and the pia mater was rather anæmic and stripped from the brain surface with great ease, being nowhere in the slightest degree adherent. The convolutions of the frontal and parietal regions were a good deal wasted and waterlogged. The grey matter was pale, and the white matter pale and watery. Seven ounces of fluid escaped on removal of the brain. The vessels in

the basic ganglia was coarse and thready, but there was no trace of clot. The other organs were comparatively healthy.

In this case, as in the subsequent one and in many others recorded in the books of the asylum, adhesion of the dura mater to the cranium is found as a well-marked condition. It is most probably an independent phenomenon, occurring as part of a systemic senile degeneration, and as such is frequently associated with analogous changes in the heart vessels and serous membranes resulting in the formation of bony plates, cretaceous deposits, and the thickening of fibrous and serous structures. In the following instance this degeneration was well marked:

C. W.—A senile dement was admitted in December, 1870, and it was then stated that she had been weak minded for four years. She had been restless, noisy, and destructive. She became very stupid, lost her memory, and believed that her relatives were constantly in her neighbourhood, and employed herself in looking for them. She was also mischievous, and on one occasion snatched coals from the fire to throw at those about her. She was very incoherent in her talk, and seemed to have no idea what she was speaking about. When admitted she was found to have mitral regurgitation. In January, 1871, she had become quieter, remained in bed during the night, and was more orderly during the day. She mistook identities. In July, she again became excited and talkative, and destroyed her bedding. The left pupil was slightly smaller than the right. In a few days after the administration of ergot in drachm doses, she again settled down, rested in bed, and ceased to be destructive. In August, after a period of freedom from active excitement, she had a severe attack of syncope. Between that date and October, 1875, she had several similar attacks, but her mental condition was more subdued than on admission. She died in March of the present year.

Post-mortem examination, 47 hours after death.—The skull was of average thickness and quite symmetrical. The dura mater was slightly adherent in the median line, and the sinuses contained dark fluid blood. There was greyish cloudiness of the arachnoid over the frontal and parietal lobes, and under it in these regions was a large amount of serosity filling up greatly widened sulci, which separated wasted convolutions. The grey matter was very thin and pale. The ventricles were of large size and contained with the sub-arachnoid effusion three ounces of fluid. There was no trace of clots. The aortic valve was competent, but had bony plates around the attached edges of the segments. The mitral flaps were thickened and puckered, and at the base of one of them there was an osseous spiculum. On the valves and the aorta were several patches of

atheroma, and though the muscular substance was healthy, there was a considerable amount of fat deposited around the heart.

It is important to observe that if adhesion of the pia mater to the convolutions is exceedingly rare in cases of senile dementia, opacity and thickening of the arachnoid are also in similar instances frequently absent. This observation at once shows that it is not merely the prolonged duration of insanity that leads to these conditions of opacity, thickening and adhesion inasmuch as the progress of general paralysis is comparatively short and definite, while senile dementia may relatively run a long and indefinite course. The consideration of the cases which have just been summarised support the opinion that there is a much closer and more exclusive connection between excitement and adhesion than between dementia and adhesion. It appears, in fact, that the existence of dementia and the presence of adhesions of the pia mater to the convolutions are little more than coincident conditions, the dementia being the result of prolonged peripheral cerebritis, while the adhesions have their origin in the transient attacks of hyperæmia, which characterise the progress of the disorder. Hyperæmia leads to stasis, and stasis to exudation, and when on the occurrence of atrophy vascular support is partially withdrawn, the vessels dilate, their walls become flaccid, and a varicose condition is established, which in its turn favours the passage of a less nutritive form of exudation. These changes produce the opacity which is common to both general paralysis and senile dementia, and increase the extent and intimacy of the adhesion, which is proper to the former. That these adhesions mainly result however from the exudation thrown out during attacks of hyperæmia is supported by the fact that recent adhesions and the conditions capable of giving rise to them—namely, congestion of the pia mater and softening of the cortical substance previous to contraction and hardening—are frequently found in cases where death has suddenly occurred during acute mania. Important evidence in favour of this view is also supplied by the fact that in general paralysis each congestive seizure, with its accompaniment of excitement or twitchings, or both, is followed by a marked indication of the progress of the malady, such as would be most likely to result from the spread of the adhesion and the vitiation of the function of portions of the convolutions which had been previously unaffected. But as senile dementia also may have its recurrent attacks of excitement, the question arises, why are such attacks not subsequently found to be followed by adhesions of the pia mater? The answer lies in the conditions inherent in the nature of the two diseases. The one occurs at a time of life when exudations are most prone to be of a plastic character; the other becomes developed during the period when in the progress of physiological retrogression serous effusions and purulent degenerations are more frequently met with. That this plastic tendency is a great element

in leading to the development of adhesions in general paralysis is well confirmed by the frequency of large and fully formed arachnoid cysts in that disease—cysts which in little more than twenty days have been known to have been transformed from an amorphous blood clot to a firm double-walled sac, with fully developed vessels, and all the characters of a physiological tissue. The character of the excitement also in senile dementia, the cry approximating to that of the organic dement and the gradual wasting of all the powers, without, as a rule, special paralytic affection, show that the disorder is one of the whole cerebrum, and not so much as general paralysis, associated with peripheral disturbance, such as would lead by alterations in the *vis à fronte* to irregularities in the circulation and consequent exudation. Dr. Major, in examining into the comparative histology of the brains of the Simiæ, has made an important observation, which may throw some light upon the direct mode of the production of the adhesions of the pia mater in general paralysis.[1] He discovered fibres passing down from the fibrous structure of the pia mater into the grey matter which appeared to form the ground structure of the neuralgia. When the pia mater is torn off, these broken fibres can be readily seen. It appears, therefore, that the immediate anatomical connection between the pia mater and the brain is exactly analogous to that which exists between the dura mater and the skull. In both instances the normal adhesion is by blood vessels and small fibrous processes which pass from the membrane into the adjacent textures, and inflammatory changes, acting on these delicate fibrous processes passing from the pia mater into the brain substance, could not fail to produce indurations and contractions of them, which occurring in the cortical substance would transform the normal adhesion into an abnormal one. With regard to the thickening and opacity of the arachnoid, there can be little doubt that the former condition is partly produced by œdema. Still both the one and the other are largely due to the increased deposit of nutritive exudation, aided perhaps by simple proliferation of connective tissue elements and alterations in the force with which the brain affects them in the performance of its pulsatile movements. In this case they would have a great analogy to the milk spots which are frequently developed on the pericardium.

Bony plates may be developed in the arachnoid as well as in the dura mater. In one case recorded in the asylum books, several rough laminæ were found lying in the arachnoid over the upper frontal convolutions on both sides. In this instance the brain as well as the membranes showed traces of long-standing inflammatory action. Both the grey and the white matter of the left ascending parietal convolution were of a purple colour and could not be distinguished

[1] Graduation Gold Medal Thesis, Edinburgh University, 1875.

from each other. In other parts of the brain the grey matter generally was reddish in colour, and the white matter had a pinkish hue. Everywhere the brain was soft and watery. The lining membranes of all the ventricles were gritty, and the skull, the dura mater, and the arachnoid were thickened. At the beginning of his mental disorder, the patient was exceedingly wild and excited and desperately suicidal, owing to the delusion that he was in hell. He had also hallucinations of sight and smell. After about two months' residence, he became less excited and for some time was perfectly quiescent. He manifested delusions of a somewhat exalted character, and believed that he had the power of curing many of the patients. Three months after his admission he again attempted to commit suicide. During the subsequent year he developed symptoms of general motor impairment, which went on increasing, even though his mind remained comparatively clear till about three years after admission. He was one morning found to be comatose and affected with twitchings, which were spasmodic and almost confined to the left side. In another instance a bony nodule was found immediately under the arachnoid in the frontal region, the arachnoid itself being thick and in places opaque.

There is one condition which, whether inflammatory in its origin or not, is intimately connected with modifications in the state of the cerebral membranes. I refer to arachnoid cysts. Rokitansky and other recent pathologists and specialists have believed that these cysts were the simple result of extravasations of blood around or upon the hemispheres. Rokitansky especially has stated that blood poured into the arachnoid sac, precipitates its fibrin on either side, so that in the progress of organisation the mass takes the form of a cyst with firm walls and dark or rusty brown or serous contents. This view, however, appears to have been supplanted by the theory of Virchow, who has advanced the opinion that these hæmorrhages are indirectly of an inflammatory nature, inasmuch as they are believed by him to occur between the meshes of pseudo-membranes, resulting from previous inflammations of the dura mater. He states that the hæmorrhage is poured out from the newly formed vessels in these inflammatory products, which are said to consist of a delicate yellow or brown connective tissue, firmly adherent to the inner surface of the dura mater. Instances may occur in which arachnoid cysts are formed in this manner, but the history of many cases in this asylum goes to show that such a method of production is not invariable. I do not find that in general paralytics, in whom these cysts very frequently occur, these vascular pseudo-membranes lying on the inner surface of the dura mater have ever been observed. In fact, in general paralytics inflammation of the dura mater, such as would give rise to so marked an inflammatory product as could develop vessels capable of exuding so much blood as would be required

to form one or two large arachnoid cysts, is not at all common. Amongst the sane, amongst drunkards, and in cases where injury has induced pachymeningitis externa, this production of arachnoid cysts by the rupture of new vessels formed in inflammatory products might readily occur. It could only be by such an assumption that cases referred to by Griesinger, where years intervene between cranial injury and hæmatoma of the dura mater could be explained; but it is evident that at least the large majority of cases of hæmatoma in the insane originate in direct rupture of vessels and extravasation into the arachnoid sac. This has been seen in many cases where in the class of patients amongst whom arachnoid cysts constantly occur, blood has been extravasated into the sac, has maintained the same relation to the hemispheres, and has partially advanced in organisation, without the recognition of any trace of previous pachy-meningitis externa. *A priori*, also there is in such cases of insanity as are associated with wasting, a condition of the circulation where rupture of a vessel is most liable to occur. The natural support of the vessels is removed. They dilate both in consequence of this mechanical change and on account of the general derangement of the vaso-motor system. Their structure participates in the universal deterioration of the tissues and the recurring periods of excitement which invariably characterise the course of diseases involving brain wasting, strain them to the utmost, till after a congestive seizure it is not astonishing to find that new symptoms of pressure on the hemispheres have appeared, and that subsequent pathological examination discloses evidences of past extravasation. The condition which admits of a large effusion of serous fluid in senile atrophy is the same as that which leads to a danger of rupture of the cerebral or meningeal vessels in general paralysis, but in the latter there is an additional danger resulting from local inflammation, which must always have a tendency to weaken the coats of the vessels in that disease.

A case of arachnoid cyst which forms one of the specimens in our museum will illustrate these remarks.

H. H. *General Paralytic.—Post-mortem examination.*—The skull was of average thickness and density, and quite symmetrical. The bones had a rough dry appearance on their inner aspect; but the dura mater was not adherent and not thickened. There was a rusty brown staining over its inner aspect. The sinuses contained dark clots and fluid blood. On removing the dura mater large arachnoid cysts, one over each hemisphere, were exposed. That upon the left side was much the larger and thicker of the two. It had a dirty pinkish brown colour—most pink towards the outer margin, and most brown over the parietal eminence. It covered the whole of the upper surface of the hemisphere, and thinning into a

delicate membrane extended round the base over the orbital lobule and temporo-sphenoidal lobe. It was everywhere closely adherent to the dura mater, and in some places loosely adherent to the visceral layer of the arachnoid. Minute vessels were very distinctly visible ramifying over both surfaces of the cyst, but they were best developed on the under surface. At the most vascular spots the vessels were arborescent and appeared to branch in a stellate manner from a more or less definite centre. On removing this cyst from the left hemisphere it was found to be markedly adherent in the parietal region, where certain gyri were denuded during the process of stripping. Patches of these convolutions were stained a rusty brown colour. This rusty staining affected the posterior extremities of the three tiers of frontals, the ascending frontal and parietal, the postero-parietal lobule, and the lobule of the supra-marginal gyrus. The staining dipped down into the sulci, and was, indeed, more distinct in their walls than on the summits of the corresponding gyri. It had a deep mahogany colour, penetrated into the outer layer of cineritious substance, and was deepest in hue and in extent at the bottom of the sulci. Over the parietal lobe the cyst on the left side was three-eighths of an inch thick, and was composed externally of firm pseudo-membrane, and internally of rusty brown friable clots. The colour of the external aspect was mainly ashy grey, with patches of brown and reddish spots of arborescent blood-vessels. The left hemisphere was much compressed and flattened. The right cyst also covered the whole hemisphere and extended round to the base. It was not so thick, but consisted of two distinct layers, with fluid contents. The whole brain with the cysts attached weighed 1244 grammes. The left cyst, after the escape of some serous fluid, weighed 39·5 grammes.

Symptoms and Progress.—When the patient was admitted his brother stated that he had been well till Christmas day, 1872. He was brought to the asylum in February, 1873. On Christmas day he fell in the street in a sort of fit. When brought home he had four more similar seizures and remained unconscious throughout the night. For a week he was dull, heavy, and stupid, and unable to rise. Subsequently he went to work, but three weeks before admission he travelled to Halifax without reasonable motive, and remained from home for two days. From that time till the middle of February he was said to have been gradually getting worse. His speech was impaired, and he became much emaciated. He manifested no delusions and his mental condition appears to have been that of dementia. His wife said that about six months before the date of his admission she had observed that his manner was peculiar, and that during the night he ground his teeth in a way that attracted attention. During the existence of dementia his appetite was enormous. He was said to have been an abstemious

man. An uncle on the mother's side committed suicide during an attack of acute melancholia. When examined on the day after admission it was found that his memory was much impaired. He did not know the day of the week, and thought that he had been a fortnight in the asylum. He had optimistic views regarding his own capabilities, and believed that he was quite fit to be at his work. His speech was impaired, and characterised by great thickness of utterance and difficulty of articulation. The eyebrows were elevated. The zygomatic muscles twitched, and the lips trembled during the attempt to speak. After having dressed himself improperly, he upheld that what he had done was right, and was firm in the belief that his mental condition was unimpaired. The pupils were unequal, the left being the larger. His gait was awkward, and his power of movement somewhat limited. The following is the diary of the case:

March 12th, 1873.—He is much worse; is getting weaker, and cannot dress himself.

July 23rd.—He was restless and noisy last night. Physically he is improved, but the dementia increases. He can utter only a few words, and these very indistinctly. He is occasionally dirty in his habits.

October 28th.—No change to record.

January 13, 1874.—Although quiet during the day, he is very restless at night and sleeps very little. He is rapidly getting weaker and more demented.

April 6.—He has hæmatoma of the left ear, and is utterly demented.

9th.—He had epileptiform convulsions last night. The twitchings were confined to the right side and chiefly to the arm. To-day he has regained his usual state, except that his right arm is somewhat paralysed.

22nd.—He had a fall to-day, and remained unconscious for some minutes.

In September, he was observed to have bronchorrhœa, and in November had an attack of diarrhœa. In May, 1875, he showed signs of bastard pneumonia, sank gradually and died on the 11th June.

In this case the evidence appears to be conclusive that the extravasation of blood was not from the new vessels of an inflammatory product thrown out from the dura mater. There was no post-mortem appearance whatever of that membrane ever having been inflamed, and no trace of such a deposit on the dura mater as would be likely to have given rise to previous, or in the event of life having been prolonged, to have led to subsequent hæmorrhages. There can be no doubt that the first hæmorrhage occurred during the Christmas day of 1872, when the patient fell in the street and was

convulsed, and that the extravasation went on at intervals during the following night. The nature and distribution of the twitching or convulsions experienced at this time are not recorded, so that from the history of that seizure it cannot be determined which side of the brain the hæmorrhage affected. There can be no doubt, however, from the appearance of the cysts and the extent of development which they had reached that they were the result of two separate extravasations. The other occasion on which extravasation is most likely to have taken place was on April 9th, 1874, when during the night the patient had an attack of epileptiform convulsions affecting the right side and principally the right arm. On the following morning, the *right* arm was partially paralysed. This shows that the blood from which the left and largest cyst originated was most probably extravasated at that date. Other evidence supports this view. For instance, the left was the larger cyst, inasmuch as owing to the progress of dementia, and presumably of atrophy, there was more room for extravasation at that time than there had been in 1872. Again, the section of the thickest part of the left cyst showed that a considerable quantity of modified blood clot still remained within its walls, while the right one consisted of two more fully and compactly formed membranes filled with serous fluid. The only statement that could be advanced against this view would be that one would have expected coma to result from the extravasation of the larger clot equal at least to that which followed on the smaller. But it must be remembered that after eighteen months' progress in rapid dementia, and the partial accommodation of the brain to the persistent pressure of a large body over the complete extent of one of its hemispheres, coma would be less likely to result than when the brain was comparatively little diminished in bulk or blunted in functional activity. Now assuming that the first decidedly notable feature in this man's case, namely, the prostration in the street, was the result of extravasation of blood on the right side of the brain, the probability is—judging by the previous noticeable alterations in his manner, the temporary nature of the direct symptoms of compression, and the rapid progress of subsequent dementia—that disease of the grey matter had already considerably advanced. Though, however, the man's history was fully given by two of his relatives, nothing in that history could be taken as indicating the previous existence of pachymeningitis externa. The conclusion is that the rupture was a direct one, probably due to diminished vascular support during the progress of changes in the grey matter occurring in the first stage of general paralysis, and involving a certain amount of atrophy of the brain; and as neither the subsequent clinical history nor the searching autopsy afforded signs of inflammation of the dura mater, the reasonable deduction appears to be that both hæmorrhages or series of hæmorrhages were simple extravasations

in vacuo. But other evidence may be found which seems to contraindicate the opinion that these hæmorrhages result from rupture of new vessels in inflammatory products. A large quantity of blood thrown out in proximity to a membrane already predisposed by previous inflammatory action to the exudation of plastic material would surely be liable to give rise to adhesions between the organised product of the clot and the surface of the dura mater, but such a condition is not at all well marked in such cases. In fact, the adhesions when they do exist are so slight as to be fully accounted for by the irritation which in the performance of pulsatile movements must affect the outer membrane of the cyst and the arachnoid surface of the dura mater. The changes in the fibrin of the extravasation are also, as Rokitansky points out, of such a character as to contraindicate the existence of inflammatory action.

Those forms of meningitis which are more or less acute or intercurrent, and associated with exudation of lymph or the presence of pus, are not in the insane characterised by any special features. In fact, the prior existence of mental disorder and organic cerebral changes generally modify or obliterate the symptoms and signs of intercurrent meningitis to such an extent as to render diagnosis problematical. Regarded from a pathological point of view, two great forms of this acute and subacute inflammation of the membranes may be said to embrace almost every individual instance of the kind. The first is inflammation of the arachnoid with effusion of lymph. The second is true meningitis or inflammation of the pia mater. In the first variety the arachnoid is thickened and covered with lymph, while numerous bands stretch from one lobe or portion of a lobe to another, matting together the whole of the structures within the reach of the inflamed arachnoid. These shreds of false membrane are sometimes substituted by more extended patches of a new fibroid formation. This condition is shown by the records of this asylum to occur with more than usual frequency at the base of the brain, and is not as a rule associated with the formation of pus. Still it occasionally happens that the exudative contents of an effusion in the arachnoid sac undergo purulent changes. The following instance will illustrate that condition:

R. C.—*Senile Dement.*—*Post-mortem* examination nineteen hours after death. The skull was dense and thickened on both sides posteriorly. It was fairly symmetrical and of normal colour. The dura mater was also thickened, and the parietal arachnoid presented a generally diffused orange-red colour. On it were small spots of extravasation and petechial patches. A large quantity of serous fluid containing lymphy flakes and pus occupied the sac of the arachnoid. Lymph could also be scraped off the inner aspect of the dura mater. The extravasations on the membrane and the

thickening and discoloration were most marked at the base. Ten and a half ounces of dull brick-coloured fluid escaped during the removal of the brain. The pacchionian bodies were converted into small abscesses filled with pus. A considerable quantity of greenish yellow pus was found on the upper aspect of the cerebellum and also about the lower part of the middle cerebral lobes. The ventricles were large but contained no fluid. The choroid plexuses were thickened, congested, and adherent to the surrounding parts, and in their meshes contained isolated spots of pus. The pineal gland was enlarged and contained pus. The pia mater stripped readily from wasted convolutions and widened sulci. The large vessels at the base were markedly atheromatous and at some parts calcareous.

When admitted, this patient manifested the domestic suspicions which are frequently associated with senile dementia, and his expression was one indicating the anticipation of personal injury. He had constant nervous movements of the head, and was exceedingly reticent and intolerant of interference, even when his own interests were being consulted. He was very emaciated and much demented. His vascular system was markedly atheromatous. He never manifested any symptoms except those of dementia, gradually declined in health, and died about three months after admission.

The second form—the affection of the pia mater—is decidedly the variety of purulent meningitis most common amongst the insane. Even in metastatic cases of meningitis this is the condition which is generally found, on post-mortem examination, to have existed during life. When the examination is carefully made, the visceral arachnoid is generally seen to be free from pus, but it is lifted up by the subjacent presence of purulent fluid. The pus pervades the meshes of the pia mater, and not unfrequently passes into the ventricles through the anatomical relation of the choroid plexus with the membrane. The whole condition corresponds with Rokitansky's second variety of true meningitis, which he says is most liable to occur amongst children, amongst people of the tubercular constitution, and as a secondary effect of various brain disorders affecting patients with impaired constitutions. The effusion is generally seropurulent, but may be albuminoid or even gelatinous, and is always abundant. In one case of dementia, in which a slight alteration of the head symptoms and a marked tendency to syncope, with a general increase of physical weakness, followed immediately upon the rapid healing of an ulcer in the leg, a very copious gelatinous exudation was found pressing up the arachnoid. Frequently, also, flocculent lymph adheres to the processes of the pia mater, and with these processes occupy the cerebral sulci. When the exudation is more fibrinous, very firm adhesions may result, and the spread of

the inflammation to contiguous parts. The pia mater adheres to the brain and the arachnoid, and the two layers of the latter become attached to each other by bands of lymph. Finally, the dura mater becomes firmly adherent, till as a result the brain substance is indirectly attached to the bone. This general state of adhesion is well illustrated by the following post-mortem record :

A. B. *Organic Dement. Post-mortem examination.*—The skull was dense and thick, and the dura mater also much thickened. Over the cerebellum and at the point of exit of the fifth pair of nerves the dura mater was adherent to the skull, and it was subsequently found that the arachnoid, pia mater, and brain substance were also in these regions mutually adherent, while the visceral and parietal layers of the arachnoid were firmly bound together, so that the brain substance was indirectly glued to the skull. The arachnoid was thickened and opaque wherever it was separable from the other membranes. At the external margin of the fissure of Sylvius the adhesion of the pia mater to the brain substance was very marked, so that when torn off it showed a whitish tuberculous appearance of the brain substance, which at that spot was fibrous to the touch. The membranes were adherent over the pons and medulla.

Symptoms and Progress.—The patient was admitted in May, 1868, and was thirty years of age. No information could be obtained regarding the commencement of the attack. When brought to the asylum he was exceedingly restless and demented. He talked incoherently and in a very husky tone of voice. He was quite deaf. Both the pupils were much dilated, but the right was the larger. There was ptosis of the left eyelid, and the left side of the face was flattened and somewhat numb. In walking, the feet were lifted and came down suddenly upon the ground, but progression could not be accomplished without support. Under the influence of hyoscyamus and good diet, the restlessness abated. In July he had an attack of dysentery, and died in August.

In the following instance the pathological condition is characteristic of the majority of cases of unhealthy inflammation of the pia mater :

J. B.—Post-mortem examination held thirty hours and a half after death. The skull was of average thickness, but symmetrical. It bulged laterally in the parietal and posteriorly in the occipital region of the left side. The dura mater stripped freely and dark clots occupied the sinuses. On reflecting the dura mater, the brain substance was seen to present a dirty yellow colour, owing to the presence of purulent effusion in the meshes of the pia mater. The pus which was under the arachnoid was widely diffused. It was most abundant over the left hemisphere, and completely covered the

frontal and parietal regions. On the right side it passed round so as to cover the orbital lobule, and on this side extended also to the temporo-sphenoidal lobe, the circle of Willis and the pons Varolii. Some purulent effusion existed on the under and upper surfaces of the cerebellum. The occipital lobes were exempt. The pia mater was thick, tough, and non-adherent. The left hemisphere weighed 568 grammes, and the right 536. The brain substance was soft and pulpy, and the white matter was peculiarly dingy in colour. Five ounces of fluid escaped on removal and section.

This patient was brought to the West Riding Asylum in November, 1873. At that time he was 42 years of age. About six weeks before his admission he had what was described as an epileptic fit, and between that time and the date of his admission fits had recurred on two occasions in bouts of two or three. A month before this date he had given up his work and had in the interval done little or nothing. He had been excited and peculiar during the few days immediately preceding his admission: he became much worse. His disorder was ascribed to religious excitement. When admitted his condition was one of mania. When addressed emphatically, he could answer questions, but immediately diverged into incoherent conversation. During both night and day he was agitated and restless, and under the influence of hallucinations, by which he saw ducks and other animals around him: he manifested excitement of a hilarious character.

Physically his muscular movements were unsteady. His limbs were tremulous. The tongue was unsteady in protrusion, and had the mark of a bad bite. The voice was husky. The eyes were prominent and the pupils unequal and sluggish: the left was the larger. The head, face, eyes, and neck were much flushed. In about ten days the excitement almost disappeared under the influence of Succus Conii. In eleven days he was quite rational, and described with fair accuracy the nature of his attacks. The medicine was stopped, and in a few days the restlessness again appeared, and the flushing of the face returned. Half-ounce doses of the Succus again subdued the mania, and for more than a month he was regarded as convalescent. Subsequently, however, he began to have regular epileptic fits, and developed disquieting hallucinations regarding hell-fire and similarly vague conditions and subjects. The subsequent history of his case is one of successive epileptic seizures. Before each seizure he was exceedingly violent and excited. He attempted to pitch himself through the windows, dashed himself against the wall, and was very dangerous. After the fits he was drowsy. By this time the pathological condition had undergone some great change. At first the left pupil was larger than the right, but now the right was almost double the size of the left. There were thickness and impediment of speech, tremor of the

lips during emotion, jerky protrusion of the tongue and unsteadiness of gait. For a month his condition was much improved by the use of scruple doses of iodide of potassium, but at the end of that time the fits again began to appear in rapid succession. The convulsions were mainly confined to the right side, and were chiefly manifested in the arm. The head, eyeballs, and mouth were drawn to the right. Chloral hydrate was administered with good effect when the doses were sufficiently large. Six months after his admission the diseased condition had evidently made rapid progress. The convulsions were more general.

In a fit witnessed at this time by a medical officer, the seizure was preceded by a faint cry, which during the tonic spasm was continued in an undertone. The mouth was opened widely and almost simultaneously the eyeballs rolled upwards and to the right. The muscles of the neck and then those of the limbs on both sides became involved. There was first a strong tonic spasm, lasting for about fifteen minutes, in which the limbs were rigidly fixed in extension. Then clonic movements commenced and lasted for about forty seconds: they were equally general. There was comparatively little twitching of the face. Some time after such attacks the pulse measured 120 and was weak; his breathing was hurried; he was bathed in perspiration and was semi-conscious. By this time he was much demented. When not having fits, he lay on his back, rolling his head from side to side, contorting his features, and making clutching movements with his hands. In another couple of months the fits had increased in intensity. They always commenced and were most severe on the right side. When the fit was over, there was violent inward rotation and simultaneous extension of both arms, and this movement along with lateral motion of the head was frequently repeated. During the subsequent month, dementia increased, and the fits and paroxysms of excitement were diminished. He ground his teeth, tore his clothes, and was exceedingly fidgety. His face, ears, scalp, and chest were suffused with a bright red flush. He could walk in a shuffling manner, and the right forearm flexors were constantly twitched. Finally, he lost his appetite, was exceedingly wakeful, was attacked with pulmonary congestion and œdema, and died in March, 1875.

This case brings out fully the progressive results of inflammation of the pia mater, combined in all probability with simultaneous encephalitis, and shows at the same time how much the existence of synchronous dementia affects the interpretations of the common symptoms of diseases of the brain and its membranes. The patient is frequently unable to express objective symptoms in any other way than by the manifestation of a delusion or an hallucination, and subsequently feeling becomes so blunted and intellect so impaired, that even that method of expression is rendered impossible,

and sensations and ideas which otherwise would have been made plain by the use of intelligible language, are only hinted at by horrid shrieks, irrational self-injury, the repetition of aimless muscular movements, and the throwing off of necessary, comfortable, and unoppressive attire.

It only remains to refer to another form of meningitis, which is not at all infrequent amongst the insane—the metastatic form. It occasionally happens that when a patient suffering from organic brain disease becomes in addition affected with erysipelas of a limb, the local affection yields readily to treatment, but perhaps in a few days after the healing up of the requisite incisions, renewed heat of skin with great restlessness and excitement supervenes, and after a fatal issue, pus is found surrounding the cerebral hemispheres. The case of a general paralytic, who shortly before his death had a severe attack of phlegmonous erysipelas of the thigh, illustrates this part of the subject.

W. M. *General Paralytic.*—Autopsy fifty-three hours after death. The skull was slightly thickened anteriorly, but symmetrical. The dura mater was normal. On removing it, however, pus and purulent lymph were found lying on the surface of both hemispheres, beneath the arachnoid and in the meshes of the pia mater. It was deposited in greatest quantity over the left hemisphere, and in the course of the vessels lying in the sulci between the convolutions. The same condition was found in the median fissure, over the corpus callosum, and in a still more marked degree at the base of the brain. It was most strongly marked over the hemispheres of the cerebellum and the line of junction between them, at a point immediately behind the medulla oblongata and upper portion of the cord. There the lymph deposited was of a pale dirty yellow colour, viscid and purulent in character, and a couple of lines in thickness. The same condition was very obvious in the circle of Willis and over the structures contiguous to it, while at the same time it was well marked over the lower aspect of the middle lobes of the brain and in the neighburhood of the fissure of Sylvius. The convolutions were wasted and the grey matter pale. On section, the brain substance was found to present abnormal appearances, showing the previous existence of encephalitis. Superficially the colour of it was an ashen grey, and as the ventricles were approached it became almost black. The lining membrane of the ventricles was thickened and smeared with purulent lymph.

There was nothing characteristic in the history of this case. The patient began by showing a motiveless tendency to wander about the country. He then began to take convulsive attacks, each of which commenced with simple tremulousness. He manifested paroxysms of excitement. Subsequently he had phlegmonous

erysipelas, and when this local malady was removed, he died somewhat suddenly in a comatose condition.

The membranes of the brain may be affected in a manner which can scarcely be called metastatic, inasmuch as it appears that the affection spreads to a certain extent by continuity. That condition was realised in the following case:

W. R. *Epileptic.*—The post-mortem examination showed that the bones of the skull were of a bluish tinge, of average thickness, and symmetrical. There was a small exfoliation of the outer table of the left side of the frontal bone, just above the orbit, corresponding to some outward traces of erysipelas, and exposed by an incision through the skin. Here the dura mater was of a dark colour. On removal of the brain two ounces of fœtid sanious pus escaped. The whole of the upper aspect of the left hemisphere and two thirds of that of the right were covered with a thick layer of pus and lymph which extended to the base, and was especially marked along the fissure of Sylvius on the left side. It lay above the visceral arachnoid, which was adherent to the pia mater and through it to the cineritious substance. The left hemisphere was much flattened and compressed. The base of the brain was deeply injected. Under the purulent deposit the brain substance was of a greenish-black tinge.

The patient had been an epileptic from his youth up. He was suffering from consecutive dementia when admitted. Nothing particular occurred during the period of his residence till he fell into the status epilepticus, and two days afterwards manifested symptoms of necrosis of the frontal bone, due probably to an injury during a fall. This caused puffiness and erysipelas with extension of the inflammation. The eyelids became exceedingly œdematous, especially on the left side. An incision over the left eyelid showed the presence of pus. On the tenth day after the appearance of erysipelatous action, it was observed that he was much better, but three days afterwards he was found to be perfectly unconscious and died in a few hours.

There can be little doubt that in these cases the brain membranes are in a condition which renders them liable to low inflammatory changes. Chronic affections of the membranes and the cortical substance, the constant presence of serous effusion in the arachnoid sac, the impaired nutrition of the vascular walls, and the disorder of general trophesis supply numerous conditions favourable for the action of any intercurrent inflammatory agency. The dementia which however generally accompanies these conditions frequently places it beyond the power of the physician to do anything more than guess at the diagnosis of the new intracranial state, and the amount of organic change almost precludes the possibility of making any successful effort at its rectification.

The cases and observations recorded in these pages may be recalled to memory by a very brief summary of conclusions.

(1.) The past or present existence of pachymeningitis externa in the insane is shown by thickening of the dura mater, sometimes also by thickening of the bones of the skull and frequently by adhesions between the two, and the development of bony plates.

(2.) The existence of simple inflammation of the dura mater does not to any great extent influence the progress of brain diseases in the insane.

(3.) When, however, any character is given to mental diseases by the existence of pachymeningitis externa it leads to paroxysmal attacks of aggressive excitement.

(4.) In the insane, opacity, thickening, and adhesion of the visceral arachnoid are exceedingly common. They are not the result of mere prolongation of mental disease, but generally of inflammatory changes following upon excitement. This excitement which occurs in paroxysms is probably dependent on hyperæmia induced by alterations in the *vis à fronte*, which in its turn follows upon changes in the structure and function of the grey matter of the convolutions.

(5.) Thickening opacity and adhesion of the arachnoid and pia mater are most frequently met with in cases of general paralysis of the insane.

(6.) Arachnoid cysts in the insane are usually the product of direct hæmorrhages into the arachnoid sac.

(7.) Inflammations of the arachnoid characterised by deposits of lymph and the presence of pus are comparatively rare amongst the insane.

(8.) Inflammation of the pia mater with abundant diffusion of pus under the arachnoid is the most common form of a plastic inflammation of the membranes.

(9.) In the insane, all forms of meningitis, especially when they occur or are intensified towards the close of organic diseases of the brain, are obscure in their symptomatology, and generally fatal in their issue, on account of the presence of extreme dementia and the existence of extensive and irreparable changes in the structure of the brain and its membranes.

II.—On Reflex Paralysis and Urinary Paraplegia.—By D. J. Hamilton, Pathological Assistant, Edinburgh University.

Under the term Reflex Paralysis have generally been included both those diseases which result from a true physiological contraction of the blood-vessels of the spinal cord, and those which follow suppurative affections of the genito-urinary tract. I shall endeavour in the following to add a few facts to the testimony of those who hold that the etiology of the two affections is entirely different.

True reflex paralysis consists in a more or less complete paralysis of motion, generally confined to one or both of the lower extremities, and caused by some peripheral irritation of the skin, or of some internal organ acting on the spinal cord, or perhaps more especially on its blood-vessels. Brown-Séquard, in opposition to the views of Sir W. Gull, Sir Thomas Watson, and others, has been led from his experiments on animals to believe that there is no pathological lesion of the spinal nervous centre in those cases, but that the peculiar paralysis is brought about by an impression being conveyed from the periphery acting on the cord so as to produce a tonic contraction of its vessels, or those of its motor nerves.

Sir W. Gull, in criticising (Guy's Hospital Reports for 1861-62) Dr. B. Séquard's statement, that he has seen the vessels on one side of the pia mater contract in an animal when a tightened ligature was applied to the kidney, when the kidney itself was irritated, or when the blood-vessels and nerves of the supra-renal capsules were similarly encircled with a ligature, says that on repeating these experiments he did not get any of the results described. The blood-vessels of the pia mater are very small, and are distributed indiscriminately on both sides. How they could be seen to contract on one side more than another he cannot understand.

Mitchell ('Diseases and Injuries of Nerves') again refutes Brown-Séquard's theory of the paralysis being due to a prolonged spasm of the blood-vessels, and believes that any exhaustion, it may be from constant or excessive exercise of normal functions, is capable of inducing it. He considers it possible that, in some instances, over-excitation might result in dilatation of the blood-vessels, which, were it to continue for any length of time, would be sure to produce some lesion. The instances of paralysis of the lower extremities due to the presence of parasites are sufficiently common and familiar to everyone. Death however in such cases is rare, and naturally what we know of the post-mortem appearances is little or nothing. The following case, for the notes of the history of which I am indebted to Dr. Davidson of Liverpool, seems to me a pure instance of reflex

paralysis ending in death. The post-mortem examination was performed with great care, and a thorough examination of the nerve centres made.

J. W —, a cotton porter, was admitted into the Northern Hospital, Liverpool, on the forenoon of September 10th, 1874.

He had been suffering from a dull pain in the lumbar region for the last month, and had also suffered from pains of a similar character in the legs, especially when he walked about. Since these pains came on he has felt weak in his legs, but has had no paralysis, and has been able to walk short distances. Since he had this pain in the back he has noticed that his urine was diminished in quantity and that it was very thick, but whether this was so immediately after micturition or after the urine had stood for some time he cannot remember. He has never suffered from gonorrhœa. This morning, on going up stairs, he was seized with severe pain in the back of the same nature as on former occasions, but of a much more violent character, and, feeling faint, he lay down for a few minutes on the stairs. On attempting to rise he found that he had lost, in great part, the use of his legs, and his friends finding him in this helpless condition, at once brought him to hospital. Previous to this illness he was in robust health, and has been temperate in his habits.

On admission he was found to be a strongly built man, of average height, and dark complexion, moderately well nourished, and muscles firm to the touch. He suffered from slight headache and intense dull pain in the lumbar region. There was no pain on percussion over the spine, but he complained of pain on pressure over the lumbar muscles on both sides, and there was also tenderness in the hypogastrium and in the iliac regions. The loss of power in the lower extremities was so considerable that he could not stand or walk, but when in bed he could with some difficulty pull the legs up and push them down again. There also seemed to be slight loss of tactile sensibility of both legs, but more especially of the right. Reflex action was perfect, but he had a feeling of formication in the feet and legs. He had made no water since shortly before the attack this morning. A catheter was passed and almost three ounces of urine obtained. It was acid, specific gravity 1024, albumen $\frac{1}{12}$, dark straw colour, and deposited on standing a slight mucous cloud.

At nine p.m. he had not made any more water, and a catheter was again passed, but the bladder was found to be empty and evidently much contracted. He complained at this time of intense pain in the epigastrium, and the abdomen was swollen and tense. He had total loss of motor power, sensibility, and reflex action in the lower extremities and trunk, as far as midway between the umbilicus

and pubes, and apparently extending a little higher on the right than on the left side. There was also slight hyperæsthesia over the chest and upper extremities. Temperature 100·6° Fahr. in right axilla; 92·4° Fahr. in the right popliteal space. Respiration rapid. Pulse thin and depressed. Bowels have been freely opened.

Two a.m. Breathing became stertorous, and he began to be slightly drowsy. His arms were tossed about, sphincter ani relaxed, and no urine found in the bladder, which still seemed to be firmly contracted. The paralysis of the trunk and lower extremities still continued. He became quite comatose, and died at 4·15 a.m., about eighteen hours after the paralytic symptoms commenced.

At the post-mortem examination the aorta was found to be slightly atheromatous. Opposite the origin of the large branches of the abdominal aorta, and involving the superior mesenteric artery, a sacculated aneurism, about the size of a large lemon, projected backwards on each side of the vertebral column for a considerable distance. It was filled with partly organized and decolorized blood-clot, especially at its lower part. It had caused erosion of the bodies of the three corresponding vertebræ, but there was no communication with the spinal canal. The pelvic and lumbar veins were extremely congested.

The kidneys were both unnaturally large, but the capsule was not adherent: they were deeply congested, and the cortical substance was almost one third larger than in the normal condition. The left one was larger than the right, and on microscopic examination the tubercles were found to contain some amount of exudation. Both renal arteries were involved in the aneurismal sac, and the left renal vein passed over its anterior aspect and was much compressed by it. *The ureters* were of normal size and appearance, and the *prostate* and *bladder* were also normal, the latter being firmly contracted.

The spinal canal was perfectly healthy in every respect, and there was no deformity of the bodies of the vertebræ consequent on the erosion caused by the aneurism. There was a slightly increased quantity of subarachnoid fluid, and the cord throughout its entire extent was somewhat soft, but not more than would have been accounted for by post-mortem change.

The vessels of the cord itself were exceedingly tortuous, and the white matter was unusually pale and pearly. The grey substance consequently, stood out more clearly than usual. On microscopic examination in the fresh state, nothing abnormal could be seen. The blood-vessels were anæmic. The brain further than being a little congested seemed to be normal. The cord and brain were carefully hardened and systematically examined, but, notwithstanding that the greatest care was taken in their examination, *nothing abnormal could be found further than the vessels being unusually contracted and anæmic.* It was, at least, expected that some

abnormal appearance would be met with in the medulla oblongata or grey nuclei. Nothing of the kind, however, was found. They were all unusually healthy and no lesion of the nervous centres was revealed which could at all account for the symptoms during life.

The idea entertained from the first was that the paralysis was of a reflex nature, and the kidneys were naturally looked upon as the most probable exciting cause. The amount of disturbance in them, however, was small, and no purulent discharge of any kind had been noticed during life, nor was there any suppurative affection in connection with the genito-urinary tract to lead one to suppose that the paralysis was of the nature above mentioned. In the great majority of instances of urinary paraplegia there has generally existed some *suppurative affection* of the genito-urinary tract for a considerable time previous to the advent of the paralysis.

The existence of an abdominal aneurism was not suspected during life, but the amount of tympanitic distension of the abdomen was sufficient to obscure it. Its situation was opposite the origin of the large blood-vessels arising from the abdominal aorta, implicating both renal arteries, and, what was important, pressing on the left renal vein which crossed over the front of the tumour. This probably accounted for the albumen in the urine and the congested condition of the kidneys. It would appear also from the eroded state of the bodies of the vertebræ that the aneurism had existed for some time, and it is not improbable that the enlargement in size of the kidneys was due to the continued obstruction to the circulation caused by its pressure on the renal vessels.

That the paraplegia was reflex in its nature seems to be extremely probable, both from the symptoms, and from the subsequent post-mortem examination. What the exciting cause of this reflex action was, is not, however, at first sight, so apparent. The kidneys and genito-urinary tract generally, were naturally looked upon as the point of origin. I am inclined, however, to put a different interpretation upon the matter, and to place the point of origin of the reflex spasm not in the kidneys, but in the irritation caused by the abdominal aneurism. We have seen that it was situated in a region where not only would the nerves and blood-vessels of the kidneys be implicated and pressed upon, but what is more important, the large trunk of the sympathetic would also be involved. It would be pressed upon and irritated by the increasing dilatation of the aneurismal sac, so that we would, by this means, have a source of reflex irritation which would act most powerfully on the nervous centres, and especially, on their blood-vessels, causing them to contract and to produce a consequent anæmia. This is quite sufficient to account for the symptoms, and the post-mortem appearances support the theory in every respect. The vessels of

the cord were tortuous, this condition being very likely produced by the increased strain of the blood pressure from behind in forcing the blood through the contracted blood-vessels. That the case was one of ordinary "Urinary Paraplegia," I cannot admit. We have seen that there was no suppurative affection of the genito-urinary tract. Further, the rapid occurrence of the symptoms, when the patient was exerting himself, and when, in all probability, the pressure of the aneurismal sac on the sympathetic would be greater than usual, opposes the idea of its being of the nature of ordinary urinary paraplegia. Lastly, there is every reason to believe that, in urinary paraplegia, the symptoms are produced by a morbid lesion of the spinal nervous centre itself. In this instance, however, the cord was unusually healthy, further than its being anæmic. We must, therefore, admit that there exists a paralysis generally affecting the lower extremities, not due to any organic affection of the cerebro-spinal axis, but dependent, in all probability, on spasmodic contraction of the spinal blood-vessels through reflex irritation.

That the disease known as "Urinary Paraplegia" is also due to reflex causes is by no means so clear, and, indeed, when careful post-mortem examination of the nervous centres in such cases has been made extensive disorganization of the cord has been revealed. The history of the cases to which I refer is somewhat as following: The patient, who is generally a male, is usually in middle life, and has probably been suffering for some time from a chronic gonorrhoea or some other purulent affection of the urinary apparatus. The affection of the urinary organs is almost invariably accompanied by some *purulent formation*. He has, probably, some difficulty in micturating and a catheter is passed. Some time after this, he begins to get weak in the lower extremities, so that, when he walks, his legs shake under him, and there is, at the same time, more or less general constitutional disturbance. He suffers from headache, and his expression is languid. The weakness in the lower extremities increases and amounts to paralysis, but this increase is generally gradual in its progress. One peculiarity is that the loss of power is confined, in the majority of instances, to the lower extremities, and it is very often incomplete. As the urinary symptoms improve, the paralytic symptoms may disappear and the patient recover, or, as in the instance which I describe further on, the patient may partially recover, a relapse take place, and death ensue as a consequence. This last course however, is rare, a fatal termination being unusual.

Sir W. Gull ('Guy's Hospital Reports,' 1861-62) in his valuable observations on this affection, makes out its etiology to be quite different from that held by Brown-Séquard, who maintains that it is essentially reflex in its nature. The great point in favour of Gull's views as to its being due to direct morbid lesion of the spinal

cord, is the fact that they are supported by pathological evidence founded on post-mortem examination. He makes the observation that it rarely follows *primary pyælitis* resulting from renal calculus, but generally those suppurative affections of the bladder or urethra which have required the introduction of a catheter. The symptoms usually run thus: "Stricture for many years, frequent catheterism, at length, numbness and tingling in the legs and imperfect paraplegia; or stricture, catheterism, intermittent febrile symptoms, and, subsequently (after an indefinite period), paraplegia." Brown Séquard states that in his experiments irritations of the kidney caused paraplegia of the limb on the same side as that to which the irritation was applied. Sir W. Gull accounts for this by the injury to which the psoas or iliacus were subjected. He regards the fact of the female being less subject to urinary paraplegia than the male as dependent on the circumstance that the urinary apparatus in the former is less complicated than in the latter. It is especially in those cases where there is most inflammation, but little irritation that urinary paraplegia is met with. Uric acid and oxalate of lime calculi may cause hæmaturia and a great amount of irritation, but unless suppurative inflammation is set up paraplegia is not produced. A review of all the cases shows that it is the inflammatory condition and not merely simple irritation which leads to the paraplegia. Gull states that his examinations warrant him in concluding that the disease arises from altered nutrition of the cord, due to extension of the disease through continuous structures from the pelvis to the cord itself. In all the cases which he describes this could be distinctly made out, and distinct lesions, of the nature of disorganisation, could be traced in the dorsal region of the cord, and more especially in the anterior columns. Instead of looking to the nerves as the means of propagation of the inflammatory influence from the pelvis, we ought rather to regard the *veins* or the blood itself, as the channels of infection. He has found some of the vertebral veins in such cases filled with pus, while the cord was extensively disorganised.

The following case goes far to support the idea that the paralysis, in urinary paraplegia, is not due to reflex action, but to direct morbid lession of the cord itself. The history has been kindly furnished to me by Dr. Alexander, of Liverpool.

J. B., aged 47, single, a shipwright, was admitted into hospital suffering from somewhat obscure urinary symptoms.

History.—The patient in following his avocation as a shipwright, has lived in different climates, and has been much exposed to the vicissitudes of the weather. He had what he calls "ague fever" twenty-two years ago, accompanied by diarrhœa, and since then, has been subject to rheumatism, but has never had acute rheumatic fever, and has enjoyed tolerably good health for many years. About

a fortnight ago he noticed a peculiar numbness and stiffness come on somewhat suddenly in his legs. Previous to this, for a few days, he had not been so well as usual, and had slight shiverings, so that, although his symptoms were not sufficient to give him any alarm, yet, he felt that he was not in so good health as he generally enjoyed. These indefinite symptoms continued for about a week, when he found that the numbness in the legs had increased, and that he was unable to pass his urine. He was admitted into hospital, when, on introducing a catheter, three pints of purulent and alkaline urine were drawn off. He has suffered from gonorrhœa previously, but not lately, and has been a heavy drinker. On admission, his condition was the following: He lay in bed on his back, and had a somewhat anxious and pinched expression of face. He was considerably emaciated. There was almost complete loss of motor power in the lower extremities as far up as the hips. His tactile sensibility had almost gone, but his sensibility to pain remained, although decidedly deficient. He felt pain when a pin was inserted deeply into the leg, but had not the slightest sensation when it merely scratched the skin. The legs were cold and felt dead, and there was no formication or tingling. He had phymosis, and, as before mentioned, his urine was alkaline and *contained a large quantity of pus*. The patient not having been seen up till this time, it was impossible to form an accurate idea of his previous condition, but from the fact of his feeling out of sorts for a considerable period and from the urine containing a large quantity of pus, it is pretty certain that some morbid condition of the genito-urinary tract had existed previous to the occurrence of the paraplegia. The treatment was directed to the urinary symptoms, and his bladder was emptied night and morning. In about a week he recovered the power over his bladder, and the paraplegia greatly diminished but never entirely left. He was able to walk about with slight assistance. His urine, however, never altogether lost its purulent character. In about six weeks after this, he began to suffer from considerable pain in his right leg, and on examining it carefully, there was found to be marked hyperæsthesia of the skin. His paraplegic symptoms became more marked, the feet swollen, and the left foot persistently turned inwards. He was very feverish and sweated much at night; and the pain in the right lower limb was so severe that it was necessary to give him a full opiate every evening. During the next week he remained in much the same condition, the pain in the legs being very severe and requiring the repetition of the narcotic two or three times daily. At the end of this time, his breathing became greatly embarrassed, and he complained of much pain in the arms which were wildly thrown about. He, gradually became comatose and sank.

The post-mortem examination was made twenty-four hours after

death. The body was found to be much emaciated, and the face was pinched, the "risus sardonicus" being well marked. The muscles of the left leg were markedly wasted, and the left foot was turned inwards. *The pleuræ* were found to have some old adhesions, but the lungs were healthy. *The heart* was smaller than usual; its walls were thin, but the valves normal. *The liver* was slightly enlarged, but not otherwise abnormal. The kidneys were both much enlarged and pale coloured, and, on opening into them, the cortical substance was observed to be of a pale grey colour, the medullary substance was much congested in some parts, and in others, its tissue was broken into purulent material which, on section, filled the calices. Numerous small cysts were seen, some containing clusters of crystals of uric acid. The medullary portions towards the middle of the kidney were quite broken up and disintegrated, the tubes, in some places, resembling grey threads bathed in pus. *The bladder* was much thickened and contracted, and its mucous membrane was *sloughy and gangrenous. The cultus,* and especially the right, were much distended; both these and the bladder contained large quantities of pus mixed with urine. *The brain and spinal cord* seemed much congested, and the latter somewhat disintegrated at several parts. On carefully preparing the parts, the following was found to be their condition. At a level nearly opposite the first cervical vertebra, the cord seemed to be of normal size. The nerve fibres in all the columns were distinctly marked, and in no part of the white substance did there appear to be any softening. Throughout the section there was an abnormal amount of vascularity, and, in the anterior fissure, a branch of one of the anterior spinal vessels was seen to contain a thrombus evidently of somewhat recent formation. In the middle of the commissure there was seen another thrombus of firmer consistence and more regular outline, blocking up the entire lumen of a small vessel. A greatly dilated perivascular space was seen in the centre of the left anterior cornu, containing an artery and two veins also greatly dilated and entirely blocked up with thrombi. Under 300 diams. the walls of the space were seen to be lined by fine fibrous tissue, with greatly compressed nerve tubes. The vascular walls were thickened and deeply stained with carmine. The thrombus was composed of blood-corpuscles alone. No such dilated vessels were seen on the other side in the same situation. The blood-vessels throughout the whole of the grey substance were blocked up with extremely firm thrombi, distending the vessels, and completely obliterating them. Between the anterior and posterior cornua, in the situation of, and posterior to, the "tractus intermedio-lateralis," there was considerable destruction of the grey substance. The nerve tisue was broken down into a granular mass, and the nerve cells had disappeared. The nerve cells throughout the entire grey

matter were few in number, they had granular contents, and seemed thickened and somewhat horny in consistence. Throughout the white substance, the blood-vessels were in a similar condition to that which has been described above as met with in the grey. In a great many there was marked dilatation, and each vessel contained a well defined thrombus. The collateral circulation, spreading from the grey matter to the pia mater, seemed unusually distinct.

Opposite the second cervical vertebra, similar appearances were found to those which have been described above. The softening of the grey matter was more defined, and in a transverse section of the posterior roots lying outside the cord, three or four dilated vessels, containing organised thrombi of a dark brown colour, were seen. These were surrounded by nerve tubes, which, in the region of the obliterated vessels, were softened and broken down; others at a greater distance retained their integrity. A transverse section opposite the third cervical vertebra showed a curious distortion of the commissure, evidently caused by a patch of softening on the left side at the junction of the commissure with the anterior horn of grey matter. A transverse section of the posterior nerve root, outside the cord, again showed a dilated vessel more marked than in the previous section, and containing a well defined dark brown thrombus. The vessels in the grey substance were unusually dilated, and were surrounded by a well defined perivascular space which they did not fill. Throughout all these sections the central canal was almost entirely obliterated by cellular structures of large size. From this level down to the fifth cervical vertebra, the thrombi and softened grey substance were exceedingly well marked, but, below this, the softening was not so defined. From the fifth cervieal down to the first dorsal vertebra, the thrombi, and softened grey substance, were not so much noticed as above, and the amount of softening seemed to correspond with, and to be dependent on, the number of thrombotic vessels. Opposite the first dorsal vertebra the obliterated vessels were more numerous, and a softened patch was noticed on the left side of the commissure similar to that which was seen opposite the third cervical nerve. There was great thickening of the media of the arteries, and they were somewhat dilated. The inner coat of the pia mater was much thickened, and the softened portion of grey substance, between the anterior and posterior cornua was again well marked. At the same level, in another section, a little lower down, there was a fussiform dilatation of a vessel in the anterior fissure on the right side, producing complete solution of continuity for two thirds of its thickness, evidently from extreme blood-vascular pressure. Opposite the third dorsal nerve there was a small sanguineous apoplexy into the left cervix cornu and the portion of the grey substance between this and the anterior horn.

It had caused entire disintegration of the surrounding nervous tissue, the spaces being filled up with compound granular corpuscles and granular matter. Opposite the fourth dorsal nerve the thrombotic state of the vessels was very pronounced, and here a remarkable difference in their structure began to be presented. They became darker and more pigmented, of firmer consistence, and more organised. In a section between this and the fifth dorsal vertebra numerous *spaces* were seen all over the section. In the deep portion of the posterior columns, at this level, the capillary vessels were immensely dilated, and had compressed the neighbouring nerve tubes. In some places, at this level, the nerve tubes were filled with what appeared to be finely granular material which distended the medullary sheath. In some places the medullary sheath had broken down, leaving a small cavity filled with detritus. In another section from this same region was a minute apoplexy into the central canal. At the level of the fifth dorsal vertebra the perivascular spaces were dilated in the posterior columns. In the anterior fissure the contained vessel was filled up by a thrombus as far down as the anterior horn of grey matter on the left side, and, in the posterior fissure, the vessel had undergone fusiform dilatation. At the ninth dorsal vertebra there was a well marked apoplexy into the middle of the right half of grey substance, and at the tenth dorsal nerve a similar apoplexy was seen. These appearances continued as far down as the first lumbar vertebra, where the vessels became altered in appearance. The thrombi were not so numerous, of a dark brown colour, and contained much granular pigment. The vessels which contained them were shrivelled and contracted and had evidently lost their functional activity. At the same time, there was increased vascularity of the surrounding capillaries, and the softened portion of grey substance, between the anterior and posterior horns, was distinctly seen. The vessels of the posterior nerve roots were in similar condition to the above-mentioned, and the surrounding nerve tubes were softened and broken down in their neighbourhood. These appearances continued down to the filum terminale and in the medullaris another small apoplexy was noticed.

In the lower two-thirds of the medulla oblongata the vessels contained well marked thrombi, and a few similarly occluded vessels were found in the pons, otherwise the cranial nerve centres seemed normal.

The condition of thrombosis met with so abundantly throughout the spinal cord in this case was extremely remarkable. The vessels were not merely congested but filled with a firmly organised thrombus which, in the lower part of the cord, had become pigmented, and had obliterated the lumen of the vessel. The explanation of their formation is, in all probability, the following. We have seen

that the kidneys were in an advaneed state of pyælitis, and that the mucous membrane of the bladder was in a sloughy gangrenous condition. We know from the pathology of this affection that it is due to, or, at least, accompanied by the presence of large numbers of organisms (micrococcus) similar to those so characteristic of diphtheritic ulcerations in other places. The history of the case shows that in all probability the affection commenced as a cystitis, the catheter was used for the relief of the patient's symptoms, and, after this, the affection became aggravated. It is quite possible that the organismal inoculation was effected through the passage of the catheter, that the bladder became diphtheritic, and that this subsequently spread along the meters to the kidneys, inducing the pyælitis. The affection continued to pass from here along the lumbar and sacral veins to the vessels of the spinal cord. Here it excited a certain amount of alteration, probably of an inflammatory character, in the coats of the vessel and a stasis of the blood took place as a consequence. The areas of disintegration of the nervous tissue in the neighbourhood of the blood vessels were probably due to malnutrition, but certain of the localised areas, with somewhat sharply defined borders, seen in the dorsal region, probably had a different origin. We have seen that they were filled with what appeared to be finely granular matter, and at first sight, one might say that this was merely the result of the surrounding disintegration. I am inclined, however, to believe that a great part of this apparently granular matter was not merely dead material, but was largely made up of minute organisms (micrococcus) similar to those seen in the lymphatics of mucous membranes affected with diphtheria, and in pyælitis. I regret that at the time of examination of the cord further measures (action of reagents, &c.), were not taken to make this an absolute certainty. It was not, however, until after having my attention drawn, in Professor Recklinghausen's laboratory at Strasburg, to the organismal infiltration of diphtheritic parts that the similarity of the two cases struck me. The explanation, however, is tenable enough, and is supported by the cases reported by Gull where the lumbar veins were found filled with a yellow purulent fluid.

The evidence, therefore, of recorded cases and of the above shows that there is every reason to believe in the paraplegia resulting from chronic suppurative affections of the genito-urinary tract not being due to reflex causes. The paralysis is dependent on organic lesions of the lower part of the cord itself. These lesions are mainly found in the vessels in which an extensive thrombosis is induced. In cases where recovery takes place, the lesion probably proceeds no further than this, but, in fatal cases, the cord becomes extensively disorganised.

Chronicle of Medical Science.

REPORT ON MIDWIFERY, DISEASES OF WOMEN, AND DISEASES OF CHILDREN.

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Midwifery.

1. *On the Alleged Occasional Epidemic Prevalence of Puerperal Fever and of Erysipelas.* By Dr. Matthews Duncan ('Edin. Med. J.,' March, 1876).
2. *The Genesis of an Epidemic of Puerperal Fever.*—Professor Lusk, 'Amer. Journ. of Obstetrics,' November, 1875.
3. *The Physiologico-Pathological Phenomena of the Circulation in Pregnant Women.* By Dr. Barnes ('Obst. Journ.,' March, 1876).
4. *Remarks on the Pathology and Physiology of the Circulatory Apparatus in Pregnant and Puerperal Women.* By Dr. Fritsch ('Arch. f. Gynœk,' bd. viii, 1875).
5. *On the Diagnosis and Treatment of Peritoneal Extra-Uterine Pregnancy.* By Prof. Depaul ('Archives de Tocologie,' 1874-5).
6. *On the Application of the Forceps in Contracted Pelvis.* By Dr. Cohnstein ('Arch. für Path. Anat. und Phys.,' bd. lxiv.
7. *On Rupture of the Uterus.* By Dr. Bandl ('Centralblatt.,' No. 23, 1875).
8. *On Compression of the Aorta in Grave Hæmorrhages after Delivery.* By Dr. Leon Gros ('Bull. Gen. de Therap.,' t. lxxxviii.
9. *On Lateral Decubitus in Delivery Rendered Difficult by Antero-posterior narrowing of the Pelvis.* By Dr. A. Martin ('Zeitsch. f. Geburtsh. und Frauenk.,' bd. i, p. 30, 1875).
10. *Sudden Death after Injection of Perchloride of Iron.* ('Hygeia,' Aug., 1875).
11. *When should the Umbilical Cord be Ligatured?* By Dr. Budin (Bull. Gen. de Therap.,' Feb., 1876).
12. *Against the Pendulum Movement in Working the Midwifery Forceps.* By Dr. Matthews Duncan ('Edin. Med. Journ.,' Feb., 1876).
13. *Experiments on Ergot of Rye.* By Dr. G. Levi ('Lo Sperimentale,' Nos. 8 and 9, 1875).

14. *Observations on Gangrene of the Vulva in Newly Delivered Women.* By Dr. HUMBERT ('Union Medicale,' Oct. 1875).
15. *Remarks on Inevitable and other Lacerations of the Orifice of the Vagina and near it.* By Dr. MATTHEWS DUNCAN ('Edin. Med. Journ.,' March, 1876.
16. *On Milk Fever.* Dr. HOWE ('Amer. Journ. of Obst.,' Feb., 1876).

1. Dr. Matthews Duncan begins by stating that he has repeatedly alleged that puerperal pyæmia does not occur as an epidemic, but has never adduced evidence to that effect. This he now proposes to do. He used exclusively the statistics of London because of their quality of unity, the largeness of their numbers, the consequent obliteration of petty disturbing influences, the comparatively very great elaboration of the data as presented in the published reports and because of their convenient accessibility. Dr. Duncan gives the evidence graphically in diagrams, but he supplements it by a descriptive text. He shows, first, "that puerperal pyæmia or fever, and erysipelas, do not behave like diseases whose epidemic character is established; and, secondly, that they do behave like diseases which are recognised as having no epidemic character, or can be shown to have none."

After giving the diagrams (which we cannot reproduce here) with descriptive remarks, Dr. Duncan says, "I now assert that I have shown that puerperal fever and erysipelas do not occur as epidemics —that they are not the subjects of epidemic prevalence; and I cannot see how any ingenuous mind can evade the force of the demonstration.

Dr. Duncan also directs attention, incidentally, to the very strong evidence which Diagram III. gives against the often alleged alliance between scarlatina and puerperal fever. There is an utter absence of any relation as can be seen by comparing the two diseases at the time of the great scarlatina epidemics, such as in 1869 and 1870.

On the other hand, Dr. Duncan shows that puerperal fever behaves like diseases recognised as having no epidemic character, *e.g.* rheumatism.

2. An epidemic of puerperal fever of singular intensity and violence raged in the lying-in department of the Bellevue Hospital in New York in 1874.

Professor Lusk endeavoured to trace out its origin, progress, and growth, and has given a very interesting account of it in the present paper. A careful survey of all the facts recited seems (he says) to justify us in drawing two conclusions:—

1st, That puerperal diseases may be engendered by the atmosphere alone. . . . . Febrile disturbances were developed among the patients of a ward, which at once subsided on removing them to another locality—doctors, nurses, and utensils remaining the same. The poisoned atmosphere invariably was generated in the ward set apart for the newly confined, and, as a rule, only manifested itself with any special virulence during the winter months when the ventila-

tion became imperfect. At such times all lesions about the genital organs were covered by diphtheritic membranes characterised by colonies of micrococci, a fact which seems to point to a parasitic origin.

2nd, That, in distinction from the above, there is a form of puerperal fever possessing eminently contagious properties, not primarily derived from a miasm, but capable in time of generating a poisoned atmosphere.

3. At a recent meeting of the Metropolitan Branch of the British Medical Association, this distinguished physician laid down the following propositions:

(1.) There exists in pregnancy a peculiar state of the blood; an increased arterial tension, and a disposition to phlebectasis, general, or affecting particular regions or systems, which may, under certain circumstances, lead to hæmorrhagic effusions.

(2.) These hæmorrhagic effusions may be salutary or conservative. Hæmorrhages from the mucous membranes, by relieving excess of tension, may avert internal hæmorrhages or abortions, or that oppression of the kidneys which results in albuminuria and eclampsia.

(3.) Hæmorrhages from the uterus inducing abortion may, in the same way, avert more serious mischief, especially albuminuria and eclampsia.

(4.) But these hæmorrhages, although we may recognise in them a conservative purpose, easily exceed physiological or useful limits, and may themselves induce danger by anæmia.

(5.) Hæmorrhages, from whatever part, occurring in women within the sexual period of life should suggest the probability of pregnancy or of menstrual disorder.

(6.) The hæmorrhages described should be taken as indications to reduce vascular tension. This may be done in some cases by bleeding, general or topical; by purgatives; by salines; by abstinence from stimulants; by regulation of diet; by digitalis; by hydrocyanic acid and other means.

(7.) Bleeding from the arm may avert abortion, not only by reducing arterial tension, but also by a derivative action determining the direction of the blood from the pelvis to the opening in the vein. This derivative action of bleeding was much relied upon in a variety of cases by Lisfranc.

(8.) In some cases, where the hæmorrhages are frequent and great, and induce serious symptoms, the induction of labour is indicated.

4. The author discusses the dangers of mitral lesions, slackening of the puerperal pulse and physiological *bruit de souffle* with the first sound of the heart in the lying-in, and makes remarks on the origin of the first sound of the heart and accidental *bruits de souffle*. The author attributes the *bruit de souffle* which accompanies the first sound of the heart in lying-in women to the lessened quantity of blood which flows towards the heart after delivery in consequence of the diminished intra-abdominal pressure.

5. A Professor Depaul has written some very able papers on this subject which have appeared from time to time in the *Archives de Tocologie.*

Having first set forth the principal divisions of extra-uterine pregnancy adopted by different authors, Dr. Depaul agrees with Stoltz that in the actual state of science they may be reduced to two; the tubal and the peritoneal forms. Each admits of a sub-division: tubal pregnancy takes the name of interstitial when it developes in that part of the tube which obliquely traverses the uterine wall. Peritoneal pregnancy may be *primitive* when the ovum remains immediately after impregnation in contact with the serous membrane, or *consecutive*, when having been first enclosed in the tube it falls into the peritoneal cavity after rupture of its envelope. Dr. Depaul only retains the name of peritoneal pregnancy for the latter when the fœtus continues to live some time after the rupture of the tube.

The author relates many unpublished cases, and passes in review the principal elements on which the diagnosis may be based. These are important enough to permit the recognition pretty surely of most extra-uterine pregnancies which have passed the sixth month. M. Depaul supposes at the outset that the fœtus is living, and insists on the following characters as indicating that the ovum is not contained in the uterine cavity; deviation and consistence of the cervix which is not in correspondence with the time at which the pregnancy has arrived; the form of the tumour which is more superficial and easily outlined in proportion as it enlarges towards the abdomen, where it seems to spring from one or other iliac fossæ rather than from the pelvis, the rarity of the *bruit de souffle.*

The sound should be used with caution but will help in diagnosis. After the death of the fœtus the diagnosis is more difficult. Expulsion of shreds of uterine mucous membrane seems to be thought significant.

As regards treatment the author distinguishes two periods in which different measures should be resorted to.

In the first four or five months attempts may be made to arrest the evolution of the fœtus, such as by Ritgen's method (which seems objectionable) of starving, purging and giving ergot of rye; by puncture of the fœtal cyst with a trocar; by poisoning of the fœtus by injecting a toxic solution, as of morphia or atropia, into the interior of the ovum; by electricity; by compression of the tumour by means of sand bags so as to kill the fœtus. After referring to the advantages and inconveniences of these methods, Dr. Depaul inquires whether gastrotomy should be practised so as to avoid the dangers of rupture of the extra-uterine cyst. He does not advocate the operation in the earlier months owing to the uncertainty of the diagnosis and impossibility of extracting a living child.

After the fifth month gastrotomy is the only means capable of saving at once both the life of the mother and that of the child, as statistical researches of Keller have shown.

6. Dr. Conhstein's researches are critical, statistical, and experi-

mental. The last are original and were made in Virchow's laboratory by compressing the cranium in a number of newly born dead children.

In twenty experiments, in which the occipito-frontal diameter was very strongly compressed, the bi-parietal did not undergo any change in 55 per cent. of the cases; it diminished in 25 per cent. and increased in 20 per cent. of the cases. This shows that diminution of one diameter of the head does not necessarily involve increase in the opposite diameter, and that compression of the maternal parts situate in the right diameter need not be feared. When the forceps were applied over the temples the results were nearly the same: the right diameter remained the same in 50 per cent. of the cases, diminshed in 21·3 per cent. and increased in 28·5 per cent. In another series of cases Cohnstein simultaneously compressed the two diameters and observed a sensible increase in the vertical diameter.

7. The author gives his views on this subject founded upon thirty-two cases, thirteen of these observed by himself and nineteen taken from the records of the Vienna lying-in-hospital. He has not found in one single case that pathological change in the substance of the uterus, which has so generally been assigned as a predisposing cause, more especially in multiparæ. The uterus was always thick, well contracted, high up, and the cervix very thin. The fissure was nearly always found in the cervix, and even when the body of the uterus was torn it began there. The peritoneum was never separated from the fundus, only in the lower parts. Bandl believes the rupture is always due to disproportion; in the thirty-two cases there were nineteen of narrow pelvis, three of hydrocephalus, eight of shoulder presentation, one prolapse of the foot with the head and one case unexplainable. Pressure does not produce rupture, as frequently sloughing occurred without rupture, in one case both were present though at different points. An unyielding os uteri, rudimentary or double development, fibroid tumours, are often stated as causes, but this is not proved. Bandl agrees with Chiari, Braun and Spaeth in considering that the abnormality is due to an excessive thinning of the cervix occurring during labour; he has found by measurement that the walls of the uterus in multiparæ, especially when there has been disproportion in previous labours, were much developed. In normal circumstances, the cervix is drawn back over the head of the child by the muscular uterus: the orificium internum remaining as investigation on the living and dissection on the dead subject show, about the level of the brim of the pelvis. If there is a disproportion which does not allow the presenting part to descend into the pelvis, the cervix is abnormally stretched, the internal orifice is raised a hand's breadth above the brim and so rupture becomes possible. If this abnormal condition has once existed, it takes place more easily in future labours, which is the reason why ruptures take place more easily in multiparæ. Bandl believes that rupture can be recognised as threatening when

the internal orifice gradually ascends, whilst the cervix stretches and the fundus acquires a lateral position.—Centralblatt. No. 33, 1875.

8. The author desires to extend the use of this means of arresting post-partum hæmorrhages to which ill-founded objections have been made.

Ulsamer, of Würzburg, proposed it in 1825, and it was used by the younger Baudelocque in 1827, but it was not much resorted to until 1840 when Chailly-Honoré popularized it. Cazeaux in his treatise on accouchments advocated the plan also.

Dr. Gros regards it as the simplest, most inoffensive and efficacious remedy that can be employed against grave uterine hæmorrhage, and gives cases in support of his view. The aorta should be compressed by pressure upon the abdominal wall at the level of the sacro-vertebral angle. It may be necessary to prolong the compression until the uterus has recovered its contractility, whether spontaneously or under the use of ergot, &c.

9. In 1854 Professor Edward Martin (whose death we have lately had to deplore) remarked that in cases of medium flattening of the pelvis the head of the fœtus may be made to traverse the upper strait by placing the woman on her left side. He gave it as a rule that the side on which the occiput presented should be chosen. In certain cases, however, the opposite side must be selected, as is shown by two cases related by the author. The mechanism of delivery in these cases is thus explained by the author; the head undergoes at first a lateral deviation at the superior strait until the bi-temporal diameter is adapted to the antero-posterior diameter of the pelvis; then only is produced the rotation around that part of the head which is nearest to the symphysis.[1]

10. Dr. Cederskiöld read before the Swedish Medical Society ('Hygiea,' Aug., 1875) the following case of sudden death after the injection of perchloride of iron into the uterus. The woman was pregnant the second time; the uterus did not contract fully; the fundus could be felt over the pubes; there was considerable bleeding after the birth of the child; ergot was given, but was of little use; the hæmorrhage recurred from time to time. On the eighteenth day a strong solution of the perchloride of iron (1 to 7) was injected into the uterus; every precaution was taken, in doing this, not to inject any air or to exert too much pressure. When the injection was half completed the woman suddenly complained of pain in the breast, stretched herself backwards, drew a few short breaths, and was dead. At the autopsy made the next day, the small intestines were actively congested; a few spoonfuls of thin, blackish fluid were found in the fossa of Douglas, and on the peritoneum near there were numerous black spots. The uterus was pretty firmly contracted. The interior of the uterus and vagina was stained dark brown. The

[1] It must be remembered that the dorsal decubitus prevails on the continent as the usual position for delivery. Rep.

inner surface of the uterus was uneven and covered with a reddish granulation tissue, except on the sides and the fundus where three superficial, oval, ulcerated surfaces were found, each one and a half to two inches long; here the uterine substance was exposed, and had a ragged surface, in the centre of which there were leaf-like, somewhat firm structures 0·4 inches high, and 1·2 inches long. These were closely connected with the underlying tissue, and consisted of organic muscular fibres; at the sides of these formations there were open mouthed vessels, some of which admitted a fine probe which passed into the larger veins of the uterus. These were split up and followed into the hypogastric and iliac veins of the vena cava inferior. The blood in these veins was markedly coagulated and stained brown; bubbles of air were also found in them. The same condition was found in the right side of the heart. Other organs presented nothing abnormal.

[Such a case is regrettable, but should not deter us from resorting to this valuable method of treatment in suitable cases and with the precautions enjoined by its able author Dr. Barnes.—Rep.].

11. To this simple inquiry a very prompt reply would probably be given by the majority of practitioners, and that to the effect that it should be done as soon as possible, and yet, as Dr. Budin's excellent memoir shows, the question should be well weighed before so ready and positive a reply is given. Dr. Budin, who is a rising young physician of much promise, read his memoir before the Société de Biologie of Paris, where it was well received by the members present, including Claude Bernard and other celebrities.

Dr. Budin made two series of experiments, each comprising a number of observations. In one he did not cut the cord until pulsation had ceased, and in the other the section was made immediately after birth. In both the blood escaping from the placental end of the cord was collected, and it was found that whilst in the first series it amounted to twelve cubic centimètres, in the second it was no less than one hundred cubic centimètres. Dr. Budin concludes, therefore, that it is best to wait until pulsation has ceased in the cord before it is ligatured and cut, because if the section be made sooner the fœtus is deprived of eighty-eight cubic centimètres of blood. Dr. Budin moreover states that the fœto-placental circulation is a completely closed one, and therefore in the normal condition of things no blood escapes from the placental tissues externally.

12. Dr. Matthews Duncan objects to the pendulum movement of the forceps, and has long taught that it is useless and injurious. Litzmann, of Kiel, has also objected to the manœuvre. Dr. Duncan maintains with his usual incisive ability that "the pendulum movement necessarily involves an injurious amount of pressure and consequent friction, in all cases, between the parts of the head to which the blades of the forceps are applied, and the adjacent maternal structures;" and further that, "the pendulum movement, in

working forceps does not advantageously increase the power of the instrument to produce desired results." The use of the forceps is to contribute by artificial pulling to the strength of the natural expulsive efforts which push. To this traction, judiciously applied, the practitioner should apply himself. The oscillatory movement will contribute nothing to the forward traction, and it is the forward traction which is alone desirable."

13. Dr. Levi concludes from experiments undertaken by him that it is to the phosphoric acid it contains that ergot of rye owes its properties.

After having shown the effect obtained in three bitches by the employment of medicinal phosphoric acid he relates two observations made at the Maternity of Pisa by Dr. Garzella, on two women aged respectively twenty-five and twenty-eight years.

The following are the conclusions from the researches of Dr. Levi:—

(1) The therapeutic effects derived from ergot of rye are due to the phosphoric acid it contains.

(2) In the diseases in which the employment of ergot of rye is useful, the employment of phosphoric acid renders identical service.

(3) Phosphoric acid acts with equal intensity and rapidity with ergot of rye.

(4) The quantity of soluble phosphoric acid found in recently powdered ergot of rye is in proportion to the activity of the drug."

14. Dr. Humbert gives an account of twelve cases which he observed during an epidemic of vulvar gangrene. Six of the patients recovered without other accident; and six had metro-peritonitis—two dying.

15. At the outset Dr. Duncan declares that anatomically the orifice of the vagina and the obstetrical perineum have no direct connexion, and that the *orifice* of the vagina is invariably injured in the natural labour of a primipara. The perineum does not always escape laceration but it frequently does so. It is not only anatomically that the orifice of the vagina is distinct from the perineum; it ought to be distinguished practically.

Undilatability of the vaginal orifice is often mistaken for rigidity of the perineum; and it is a condition seen almost exclusively in primiparæ.

The lacerations that may take place are various. Besides the posterior laceration of the vaginal orifice, which is inevitable, there may be tearing of the lateral and anterior parts, and when the latter suffer, the hæmorrhage may be considerable.

Two important elements in the causation of laceration are susceptible of modification with therapeutical objects by the practitioner, viz., time and direction. The accoucheur can prevent the precipitate expulsion of the child, and its attendant evils. He can by supporting the perineum modify the direction of its advance,

and resist any undue pressure inferiorly or posteriorly arising from the curvilinear motion of the fœtus.

16. Dr. Howe has made a number of observations on the temperature of lying-in-women with especial reference to this subject, and his conclusions he gives as follows:

That there is such an affection as milk fever I fully believe, and I think it is due to two causes, which are as follows:—It has been shown that at the coming of the milk there is always a slight rise in temperature, which may become considerable, and which, I think, is due to the sudden development of this new function, for there is a congestion of the glands, which are very richly supplied with nerves and vessels, causing a certain amount of nervous action which will give rise to an increase of temperature; this increases as the congestion increases, but if the child draws the milk immediately as soon as it is formed the pulse and temperature fall. If this state of things is not relieved, however, a new force comes into action, the milk accumulates in the gland vessels, stretching the skin until it becomes tense, and the fever increases instead of diminishing, not on account of the congestion of the breasts, but owing to the stretching and irritation which this produces when the glands are fullest and the breasts hardest; then the temperature is at its maximum.

I would wish to say that I do not consider every rise in temperature during the lying-in state due to this, but I consider that the milk fever in a pure form is rare; . . . . the fever, however, which occurs often during the first week of childbed is not a simple one at all, but is due to a combination of circumstances, namely, wounds of the genitals, congestion of the breasts, indiscretions in diet, mental disturbance, &c,

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Diseases of Women.

1. *On the Pathology of Membranous Dysmenorrhœa.* Dr. Beigel ('Arch. für Gynœk,' bd. ix, heft i, 1876).
2. *Double Ovariotomy Performed for the Removal of Solid Ovarian Tumours; Transfusion of Milk four days after Operation; Recovery.* By Dr. Gaillard Thomas ('Amer. Journ. of Med. Sciences,' January, 1876).
3. *Researches on the Milk of Women Treated by Mercurial Inunction.* By Dr. O. Kahler ('Prag. Viertelj.,' vol. iii, p. 39, 1875).
4. *On the Influence of Diseases of the Heart on Menstruation, Pregnancy, and its Product, &c.* By Dr. Duroziez ('Arch. de Toc.,' Nos. 6, 10, 11, 1875).
5. *Case of Fibro-Cystic Tumour of Uterus. Removal of Uterus and Ovaries.* By Dr. Thomas Keith ('Edin. Med. Journ.,' March, 1876).
6. *On the Causes, Prevention and Cure of Lacerations of the Female Perineum.* By Professor Goodell ('Philad. Med. Times,' Nov., 1875.)

7. *On Excision of the Uterus by External Hysterotomy in Irreducible Inversion.* By Dr. Donné ('Annales de Gynec.,' January, 1876).
8. *The Vesico-Vaginal and Vesico-Rectal Touch.* By Dr. Noeggerath (Reprint from 'Amer. Journ. of Obst.,' May, 1875).
9. *On the Use of Nitrate of Alumina in Pruritus Vulvæ.* By Dr. Gill ('St. Louis Med. and Surg. Journ.,' December, 1875.

1. The author sums up with the following conclusions:

(1) The so-called membranous dysmenorrhœa arises as a sequence either of primary or secondary disease of the uterine mucous membrane. Endometritis.

(2) The disease is characterized by a pathological change in the mucous membrane, so that this, in consequence of an increased cell proliferation in its lower layers, is lifted up, and is separated in large shreds or in one piece.

(3) The expulsion as a rule is affected by uterine contraction after some hæmorrhage, which may be at the menstrual period or not, so that frequently the internal os is blocked up by the membrane, and there is a retention of the blood and more severe pains.

(4) Since menstruation takes quite a secondary place in the development of this affection, and the membranous structure is in no way whatever connected with conception or an abortion, it appears judicious to discard the designations "membranous dysmenorrhœa" and "menstrual decidua," and to adopt the name "endometritis exfoliativa."

(5) Microscopical examination of the membrane gives no uniform results. In one series of cases the normal elements of the uterine mucous membrane are found only pathologically increased; in another series these individual elements have disappeared or are degenerated; again, in a third series, embryonal connective tissue is formed; and in a fourth, flattened epithelium or cells, which are very like this, alone or in conjunction with the embryonal tissue of the principal constituent of the membrane.

(6) In every case there is a production of round, free cells, which brings about the detachment of the mucous membrane from the layers underneath it.

(7) The pathological changes in the mucous membrane in "endometritis exfoliativa" appear to be of such a nature that they do not permit of the development of an impregnated ovum, therefore the patient is sterile as long as she continues under the influence of the disease.

2. The patient suffered from disease of both ovaries, the tumours being adenoid in character. Both were removed, but the patient did not progress well, and at the end of four days was in a collapsed condition. Dr. Thomas then decided to perform transfusion, and having been dissatisfied with the results of three experiments with the transfusion of blood, he determined to inject milk.

A young and healthy cow having been procured, a sufficient quantity of milk was obtained warm, and, after straining, eight and a

half ounces were injected into the patient's arm by means of M. Colin's very safe apparatus. At first the patient said her head would burst, but at the end of the operation she was perfectly quiet. In an hour she had a decided rigor, the pulse rose to 150—160 a minute, and the temperature to 104°. This high temperature however now fell, and the patient slept for some hours. Twenty-one days after the operation the patient walked downstairs and completely recovered.

3. Kahler has published three observations, made on syphilitic women who were suckling, and who had been submitted to mercurial inunction. He could find no trace of mercury in the milk of either of them. He thinks that mercury does not escape with the milk until it is given in quantities sufficient to produce symptoms of poisoning; and he thus explains the positive results obtained by some writers who have experimented on animals. He therefore rejects the views of those who would give mercury to the nurse to cure syphilis in the child.

4. The author produces numerous clinical facts to show that the existence of heart disease retards the establishment of menstruation and that the catamenia are often irregular or too profuse. Sterility is observed in some cases, and in others abortions are frequent. The prognosis in young women who marry, being the subjects of heart disease, is serious. There is, as has been shown by the researches of Larcher, Blot, and others, hypertrophy of the left ventricle during pregnancy.

5. Dr. Keith in relating this case makes the following important remarks, which are all the more valuable as coming from one who had previously renounced the operation, and whose judgment and caution are alike admirable:

"Till recently I had looked upon the removal of fibrous tumours of the uterus by abdominal section as an unjustifiable operation under any circumstances. Had this patient come six months before, I would simply have sent her back again, lamenting my inability to give her any relief, leaving her to take the chance that, after eight or ten years of chronic invalidism—should she live so long—the tumour, on the cessation of the menses, would quiet down and give no further trouble, unless what arose from its bulk. Of such a favourable termination there was here little hope; for the tumour was one of those soft cystic growths that often destroy life as quickly as ovarian tumours. Within the last few months ('Lancet,' 15th May, 1875) I had operated in two cases of fibro-cystic disease of the uterus, in one case supposing the tumour to be ovarian—the only error in diagnosis in more than two-hundred operations—in the other, almost driven into operation by the dogged determination of the patient to have her tumour taken away. Both of these were miserably constitutioned women, worn out and emaciated, bad subjects for the simplest operation: yet both recovered and are now well. Dr. Millar's patient could not thus now be summarily

disposed of in the old way and sent home. After anxious thought, the removal of the uterus with the ovaries was advised and agreed to.

"The neck of the tumour immediate above the vagina was secured by Koeberle's instrument; the uterus, tubes, ovaries, and masses of cysts in the broad ligaments were then cut away. As it was doubtful whether the cut cervix could be secured external to the peritoneum without undue strain, a piece of thin india-rubber cloth was wrapped round the cervix and fastened by another serre-nœud, so as to form an investing tube. There was hæmorrhage subsequently, but the patient recovered perfectly."

6. Dr. Goodell contributes two excellent clinical lectures on this subject. We do not find anything notably new, but the teaching given is good. Dr. Goodell prefers the use of shot-clamps to any other method of suture. Of late in this country operators have preferred simple twisted wire sutures, discarding the use of clamps or quills. In some cases probably the quilled suture will afford better results and Dr. Goodell's method may then be very serviceable.

7. The author communicated a paper on the subject to the Academy of Médecine, Paris, in November last. He arrives at the following conclusions:

(1) External hysterotomy is an extreme but valuable surgical resource for those cases of irreducible inversion which threaten the lives of the sufferers.

(2) The operation is not followed by a greater mortality than that attending the majority of severe operations.

(3) In the present state of science and considering the improvements that have been made in this method, the ligature should be employed.

(4) During the first months or even during the first year, repeated attempts should be made to reduce the inversion, by lactation which as a general rule suppresses the hæmorrhages, and by palliatives of all kinds.

(5) The operation should be reserved for those cases which are known to be irreducible, and should be undertaken at a time sufficiently removed from the beginning of the affection, and where the inverted uterus is completely turned back upon itself, and has taken on a new and definite form, and this renders much less likely peritoneal inflammation, which should be avoided at all costs.

8. Dr. Noeggerath advocates a new method of exploration which he says he discovered by a lucky accident. It consists in the palpation of the uterus and its lateral appendages by the finger introduced through the urethra into the bladder. This practice as Mr. Teale, of Leeds, has lately shown in the 'Lancet' is of great value in the treatment of certain bladder affections in females and incidentally Dr. Noeggerath confirms Mr. Teale's statements. He

thinks it useful if its application be confined to certain morbid conditions of the womb; especially in those in which ordinary examination by a skilled observer fails to give satisfactory results: *e. g.* inversion of the womb and in suspected malformation or absence of the internal organs of generation.

9. Dr. Gill recommends the use of nitrate of alumina for this troublesome affection: it has in his hands given more satisfaction than any other remedy. He orders four to six grains to the ounce of soft water, to be used as a vaginal injection or external wash, once or twice a day if necessary.

Diseases of Children.

1. *On the Stenoses of the Digestive Canal in Children.* By A. von Hüttenbrenner ('Jahrb. f. Kinderkeilk.,' bd. ix, heft i, 1875).
2. *A Case of Tubercular Meningitis followed by Cure.* By Dr. Rinteln ('Berliner Klin. Wochenschrift.,' No. 21, p. 287).
3. *Primary Cancer of the Kidney in Children.* By Dr. Kühn ('Deutsch. Archiv. f. Klin. Med.,' vol. xvi, p. 306, 1875).
4. *Suppurating Peri-cystitis in a Child aged Twelve Years.* By Dr. Gallasch ('Jahrb. f. Kinderheilk.,' bd. ix, heft 2, p. 175, October 20, 1875).
5. *Practical Studies on the Normal Urine of the Newly Born with Clinical and Physiological Applications.* By Drs. Parrot and Robin ('Arch. Gen. de Méd.,' February and March, 1876).
6. *Clinical Studies on Pertussis.* By Dr. Noël Gueneau de Mussy ('L'Union Méd.,' Nos. 81, 82, 83 and 85, 1875).
7. *On the Diagnosis of Enlarged Bronchial Glands in Children.* By Dr. Eustace Smith ('Lancet,' August 11, 1875).
8. *On Melæna Neonatorum.* By Dr. Kling ('Dissert. Mang.,' Munich, 1875).
9. *Congenital Interstitial Hepatitis with absence of the Gall Bladder.* By Dr. Freund ('Jahrb. f. Kinderheilk.,' bd. ix, heft 2, p. 178, October, 1875).

1. The author has studied the contractions of the alimentary canal from the mouth to the anus. These stenoses naturally divide themselves into *congenital* and *acquired.*

Among the congenital atresiæ of the natural orifices of the digestive tract are found:—

(1) Microstomia.

(2) Imperforate anus, which may comprehend several varieties according as the skin alone is imperforate, or the rectum terminates as a cul de sac at a certain distance from the skin, or, as in another modification, the rectum terminates high up in a cul de sac.

(3) Stenosis ani—or narrowing of the anus.

(4) Congenital stenosis of the intestine in its length.

The acquired atresiæ comprise:—

(1) Narrowing of the pharynx and upper part of the œsophagus by cicatricial contraction (syphilis, diphtheria?).

(2) Narrowing after swallowing caustic substances (acid or alkaline).

(3) Stenosis of the intestine by strangulation of herniæ through the natural orifices or canals of the abdominal wall. Although rare enough in children this may nevertheless occur in inguinal herniæ, very exceptionally in crural herniæ, and lastly and most frequently in umbilical herniæ.

(4) Stenosis of the intestinal canal by intussusception, by twisting of the mesentery, causing strangulation of a part of the bowel, and lastly by a bridle of peritoneum (internal strangulation).

(*a*) *Invagination* is very frequent in children on account of the great mobility of the mesentery, of the greater intensity of the peristaltic movements and the frequency of lesions of the intestinal mucous membrane at this period of life. This invagination is, in fact, most frequently consecutive to diseases of the intestine which cause a swelling or a loss of substance of its mucous membrane (typhoid fever, scrofula, tubercle, dysentery, syphilis). It is rare after neoplasms, contrary to what occurs in the adult. The length of the invaginated portion of intestine varies considerably. The invaginations which are most frequently met with in children are first, ilio-cæcal, then of colon, and lastly of the small intestine. Rare before the third month, they are very common from the fourth to the fifth month. The occlusion of the intestine in these cases does not take place suddenly, but occurs as it were at two different times; the first corresponding to invagination pure and simple; the second to the intestinal obstruction consecutive to the swelling and inflammation of the invaginated part. The first period lasts from one to eight days.

(*b*) Intestinal stenosis by *torsion* and twisting of the mesentery on its axis is very rare in children; Hüttenbrenner has only met with it twice.

(*c*) *Internal strangulation* by bridles or peritoneal pseudo-membranes, by cords arising from the umbilicus, or by rents of the mesentery, is almost exceptional.

(5) Narrowing of the calibre of the intestine by degeneration of its walls or by foreign bodies.

On this the author remarks that carcinomatous growths are very rare in children, and that mucous polypi which are so often found in them are never of sufficient volume to obstruct the lumen of the intestine.

On the other hand a pretty frequent contraction in childhood is that which follows at the expiration of a greater or less time intestinal cicatrices consecutive to dysenteric ulcerations. Syphilitic narrowings are much less frequent than in the adult. Occlusions by foreign bodies are especially met with in the œsophagus and in the rectum. Worms and especially ascarides lumbricoides rarely cause complete obstruction.

We cannot follow the author into his account of the symptoms and diagnosis, but we may mention that he points out that in atresia ani we may be sure that that the intestine is not far distant

if we can see or feel a distinct projection in the anal region when the child cries or makes effort.

2. A boy, two years old, was seized with vomiting and general convulsions. This condition was followed by heat of head, flushed face, retracted belly, obstinate constipation, vomiting on movement and elevated temperature. At the end of a week the vomiting ceased but convulsions continued and were followed by coma. The pulse became slow, breathing irregular, and sighing with long intervals; the pupils dilated, and the head retracted. This state continued for four weeks, the patient taking only milk. All the physicians who saw him agreed in the diagnosis of tubercular meningitis with effusion, and gave an unfavourable diagnosis. Leeches, ice, mercurial inunction, ointment of tartarized antimony, calomel, iodide of potassium, &c., were used without success. The antimonial ointment caused an eruption which seemed to have no other result than to annoy the child. Some time after the cessation of the irritating inunctions and the dessication of the pustular eruption a largish boil apprared at the level of the seat of eruption, but there was no alteration in the symptoms until the boil burst when the coma disappeared; consciousness returned little by little, and the child gradually recovered after a long convalescence. The child has now grown up, and went through the campaigns of 1866 and 1870.

[Unfortunately, there is no record of an ophthalmoscopic examination having been made; and doubt may, therefore, be cast upon the diagnosis. Dr. Clifford Allbutt has, however, informed me of two cases at least of recovery from tubercular meningitis in which the diagnosis made by the aid of the ophthalmoscope was verified some years later at the autopsies of the patients. A case of my own, which presented all the classical symptoms except those revealed by the ophthalmoscope, proved to be one of congenital syphilis, and the patient recovered under the influence of mercury.—Reporter.]

3. Dr. Kühn relates three cases which he had the opportunity of observing from beginning to end. In one case there was a remarkable development of hair on the pubes and in the axillæ, and also there was a marked increase in the pigmentation of the tegumentary structures. In one case there was much hæmorrhage from the kidney, and in one there was none at all. In the second case there was hypertrophy of the heart.

4. The patient had pain in the supra-pubic region aggravated by attempts to micturate which were frequent and painful. There were tenderness and dulness in the supra-pubic region. After fifteen days of acute suffering a quantity of pus was discharged by the rectum, and the patient then rapidly recovered.

5. A newly born child urinates four times more than an adult in proportion to its weight.

Under exceptional circumstances the urine may give a slight deposit consisting of crystals of uric acid, oxalate of lime or urate

of soda. Vegetable ferments appear to develop more rapidly than in the urine of adults. It gives an acid reaction with litmus paper. Acidity of the urine generally indicates too long an interval between suckling. There is a constant relation between the quantity of urea and the colour and reaction of the urine. Under no circumstances did the urine of the newly born fœtus contain albumen.

6. Dr. de Mussy here demonstrates a new theory of his own on the mechanism of the convulsive cough which characterizes this disease. According to him every whooping cough is accompanied by bronchial adenopathy characterized by particular signs, which present nothing special as regards the species of enlargement. It is this lesion which is the determining cause of the spasmodic phenomena, in provoking the excitation of the pneumo-gastric nerves, and compressing the bronchi. To this anatomical condition the author refers chronic whooping cough, and all bronchitic affections characterized by fits of painful, fatiguing, and obstinate cough, but without respiratory whistling, affections which he designates under the name of "coqueluchoïdes"=pertussoid. Bronchial adenopathy develops from the first period of the disease; bnt it is especially in the second that it becomes well marked and readily appreciable by the physical means of chest exploration. Dr. de Mussy also attributes to the ganglionic tumefaction the moanings emitted by the patients during sleep, a phenomenon to which he was the first to draw attention, and which he has observed in the absence of whooping cough when the same anatomical conditions have been present. The way having been opened, Dr. de Mussy develops his ideas on the nature of pertussis: a malady essentially contagious, comparable to the eruptive fevers, always accompanied by a specific exanthem situated in the isthmus of the throat and pharynx: this eruption disappears towards the end of the third week.

7. Dr. Eustace Smith in his excellent communications has ably treated on this subject. He also shows that spasmodic cough is one of the symptoms of enlarged bronchial glands.

8. The author observed seventeen cases of melæna in newly born children at the Munich Maternity and he especially studied them in respect of their pathogeny. He first examines the opinion lately expressed by Landau, according to which melæna depends on embolism of an artery of the stomach or intestine and the corroding action exercised by the gastric juice on the mucous membrane of the part whose nutrition is damaged. The embolus comes from the thrombosed umbilical vein or the ductus Botallii; but the emboligenic thrombosis of these vessels is only produced when the establishment of the lesser circulation is hindered by an obstacle to the first inspiration. Kling has not in these cases met with any appreciable influence of the circumstances which habitually determine an incomplete development of the lesser circulation or asphyxia, such as long duration of labour in primiparæ or women with narrow pelves, irregular position of the fœtus, compression of

the funis, arrest of development of the child, &c. In six cases ending in death the source of the hæmorrhage was found twice in ulcers in the stomach or duodenum; in the other cases capillary or venous hæmorrhages were admitted. The explanation given by Landau then does not hold good for all cases.

9. This occurred in a newly born child whose stools, from the first day of its birth, presented a peculiar discoloration, being as white as milk. The icteric tint of its skin was marked. The child died three days after its birth, of a deep phlegmon of the scalp. At the autopsy the liver was found increased in volume. In place of the gall bladder a small cul de sac was found, from which a clear, viscous liquid escaped on incision being made into it.

REPORT ON MATERIA MEDICA AND THERAPEUTICS.

By Robert Hunter Semple, M.D.,
Fellow of the Royal College of Physicians, London, Physician to the Bloomsbury Dispensary, London.

On the Therapeutical Use of Alcohol. Discussion at the Congress of the Medical Sciences at Brussels, 1875.—The proceedings of the Congress at Brussels were opened by a report by Dr. Desquin, of Antwerp, who distinguished the physiological action of alcohol into two phases, the first being characterised by excitement of all parts of the nervous system, and the second by depression of all the functions of organic and animal life. The excitant action of alcohol, he maintained, is the only one applied in therapeutics, and is required in cases where there is great depression of the nervous system, as for instance in some low fevers and in malignant pneumonia; but alcohol is contra-indicated in simple feverish diseases, for it places the system in an abnormal state which renders convalescence long and difficult. The discussion which ensued after the report showed that the speakers were ranged in two groups, one advocating the views of Dr. Desquin, and others opposing them. M. Crocq, clinical professor in the Hospital St. Jean, at Brussels, entirely denounced the use of alcohol in typhoid fever, and stated that he employed it only in those cases of pneumonia where there was a commencing paresis of the heart due to fatty degeneration. M. Dujardin-Beaumetz, and Dr. Semola, of Naples, advocated the use of alcohol in acute inflammations, and the former quoted the experiments made in this country by Anstie and Dupré, and in Germany by Franz Riegel, which have shown that alcohol lowers the temperature and diminishes the amount of urea excreted by the urine and that of the carbonic acid exhaled. From the results of the whole discussion it would appear to have been the general

opinion that within certain limits and in certain conditions the administration of alcohol renders immense service, and that the introduction of the alcoholic treatment into therapeutics is an incontestable evidence of progress.—*Bulletin Général de Thérapeutique*, Oct. 13, 1875.

On the Preparation of the Phosphide of Zinc and its employment in Medicine. By M. Vigier, of Paris.—From experiments made on rabbits poisoned by phosphorised oil, it appears that phosphuretted hydrogen is produced in the intestines after the administration of this substance, and this gas is evidently formed in the stomach when phosphide of zinc is used in medicine. M. Vigier observes that the action of phosphide of zinc is more rapid than that of phosphorus, and he has proved the fact by giving a large dose of the phosphide to one rabbit, and a proportional dose of phosphorus dissolved in oil to another; and the animal which had taken the phosphide died at the end of three quarters of an hour, while the other resisted for a quarter of an hour longer. According to M. Vigier, four milligrammes of the phosphide correspond to half a milligramme of phosphorus. The phosphide crystallised and reduced to fine powder contains chemically the fourth of its weight of phosphorus, and what is extraordinary is that it acts toxically only as if it contained one eighth, namely, that as a medicine and as a poison eight milligrammes of the phosphide correspond exactly to a milligram of phosphorus. The reason, according to M. Vigier, is that by the decomposition of the phosphide by the hydrochloric acid of the stomach, a great part of the product passes into the state of hypophosphite of zinc, which is inert in the dose used, and another part into the state of chloride of zinc and phosphuretted hydrogen. M. Vigier gives some cautions as to the preparation of the phosphide, recommending that it should be prepared without contact with the air, which converts the zinc into an oxide; and pure crystallised zinc should be used, and not the commercial article which always contains lead.—*Bulletin Général de Thérapeutique*, Jan. 15, 1876.

On the Physiological Action of Lobelina. By Dr. Isaac Ott, of the University of Pennsylvania.—Dr. Ott has previously made some experiments with lobelina, which is a fluid substance and is the active principle of the *lobelia inflata*, and he has arrived at the conclusion that it is an excitant of the peripheral vaso-motor system. He has since continued his researches with the acetate of lobelina, and his late experiments have been made with a solution containing in each cubic centimètre one drop of the alkaloid. The animals selected for the experiments were frogs and rabbits. The results which Dr. Ott has drawn from a large number of researches very carefully conducted are summarised as follows. Lobelina, like nicotina and conia, paralyses the motor nerves, but it does not destroy the functions of the sensory nerves or the striated muscles. Like nicotina and conia, it depresses the excitability of the spinal cord, and destroys voluntary movement and co-ordinating power. Like nicotina, lobelina temporarily decreases the pulse, but this decrease is followed by a subsequent increase often beyond

what is normal. This action on the pulse Dr. Ott considers to be due to an action on the cardio-motor ganglia, provided that atropia paralyses the cardio-inhibitory ganglia. Lobelina, like nicotina and conia temporarily decreases the blood pressure, which, however, subsequently rises much beyond the normal, and this increase is attributable either to a peripheral vaso-motor action or an excitation of the spinal vaso-motor centres. Lobelina, nicotina, and conia all paralyse the pneumogastric nerves, but in large doses lobelina paralyses the vaso-motor centre, both to direct and indirect irritation. All three alkaloids accelerate the respiratory movements. After section of the vagi, lobelina and conia cause no increase of respiratory movements; lobelina increases and then decreases the temperature; but nicotina and conia decrease the temperature.—*Philadelphia Medical Times*, Dec. 11, 1875.

On the Action of the Salts of Silver on the Nervous and Muscular Systems, and on their Therapeutical Action. By Dr. Antonio Curci, of the Clinical School of Naples.—In a very elaborate paper Dr. Curci gives the results of a number of experiments made upon some of the lower animals, with a view of determining the physiological action of silver. In these researches he employed the chloride of silver dissolved in hyposulphite of soda, a double decomposition thereby resulting, and the mixture containing hyposulphite of silver and chloride of sodium. The preparation of silver actually employed was, therefore, the hyposulphite, the sodium salt having no action on the nervous system. The hyposulphite of silver thus employed is stated by Dr. Curci to have a very sweet taste, and a very small quantity placed on the tongue is sufficient to communicate a sense of sweetness to the whole mouth, and if it were not for an unpleasant metallic flavour in the fauces this substance might be used for criminal purposes. Employed in the above form silver produces no irritation either of the skin or the mucous membranes, and therefore a large quantity may be introduced into the system without any caustic action as in the case of the nitrate. When subcutaneously injected, however, it produces a slight inflammation and œdema. Dr. Curci distinguishes two periods in poisoning by the silver salt, namely, that of excitement and that of depression, the first being inconstant in its intensity and sometimes inappreciable, but the second being constant in its appearance even when the first is wanting. The period of excitement is observed in a typical form in frogs, but is scarcely or not at all seen in mammalia; in the former animals it may appear conspicuously as tetanus, but in mammalia there are only some slight convulsions. The effect on frogs is so well marked that silver has sometimes been called *the mineral strychnia.*

In poisoning by silver, it is shown by experiments on the lower animals that, whether a period of excitement has existed or not, insensibility and paralysis and apparent death are the results. Real death, however, only supervenes after several functions have been successively abolished, the order in which they disappear being as follows:—First, that of respiration, then sensation and reflex

movement, then voluntary motion, afterwards the circulation of the blood, and lastly the excitability of the motor nerves and muscular irritability.

In reference to the therapeutical application of the silver salts, Dr. Curci concludes from his researches that if they are employed in medicine they should be given only in certain nervous affections. In myelitis they are of no use, because they have no power to prevent softening and other destructive processes in the nervous elements; in locomotor ataxy they are contra-indicated, because the morbid effect produced by them may be called a species of locomotor ataxy; in paralysis agitans their employment is of doubtful efficacy. In epilepsy, however, they may be of great service, and in certain cases they should not be omitted. This disease, generally speaking, depends upon an augmentation of sensibility in the medulla oblongata, and as the silver salts act as sedatives on the nervous centres, they may be expected to act beneficially, and the good results reported by several authors may be thus explained. But it is necessary to consider that benefit can result only when the disease depends on nervous excitability, and that the silver treatment is useless when there is any anatomical lesion. Dr. Curci suggests that silver might be advantageously employed in asthma of a purely nervous kind, but the same remark applies in this disease as in epilepsy, namely, that the malady must be purely nervous and without any cardiac or other anatomical lesion. It is well known that an asthmatic attack usually originates in a peripheric cause, and thence ensues an excitement by means of the vagus, trigeminal, or some other nerve, or the medulla oblongata, and from the centre which regulates the respiratory movements a centrifugal excitement is reflected to the group of inspiratory muscles. Asthma, therefore, in its pure uncomplicated form, may probably be benefited by the use of the silver salts.—*Lo Sperimentale*, December, 1875.

On the Treatment of Chorea by Hyoscyamine. By Dr. Oulmont, of the Hôtel Dieu. — Dr. Oulmont has already published some remarks on the value of hyoscyamine in several spasmodic and convulsive affections, but he now specially directs his attention to the use of this alkaloid in the treatment of chorea. He states, however, that in his clinical researches he does not employ a well-defined chemical substance, but a very concentrated essence of henbane, in which the proportions of the active principle must be more or less variable. With this drug he treated five patients affected with chorea, all of them being adults aged from twenty to thirty-six, and in three of whom the malady had appeared at the beginning or during the course of pregnancy. In each case there was a notable improvement in the symptoms. Dr. Oulmont records two cases in detail in which the effects were very remarkable, the malady having previously resisted other kinds of treatment. In the first case the patient was hysterical, she had suffered from insanity when a girl, and the attack of chorea had occurred in the second month of pregnancy. In the second case the results were still more striking, as the disease had lasted a longer time, and there was a decided relapse of the sym-

ptoms when the drug was discontinued, and a fresh improvement when it was resumed. The doses employed at first were small, namely, two to four milligrammes a day (a milligrame is the one thousandth of a gramme, about fifteen grains), and they were gradually increased to eight or ten milligrammes. The improvement generally appeared, in all Dr. Oulmont's cases, about the eighth or ninth day of the treatment, and the dose of the drug was increased as soon as the amelioration began. When the system was saturated with the hyoscyamine, which was not always the case, there was a disagreeable dryness of the throat and dilatation of the pupils. Of the five patients who were treated by Dr. Oulmont two left the hospital completely cured, and the other three were only imperfectly cured, that is to say, there remained, either in the head or the limbs, some slight movements, which Dr. Oulmont thinks would have disappeared if the patients had consented to continue the treatment.—*Bulletin Général de Thérapeutique*, August 30, 1875.

Researches on the Action of the Jaborandi Plant. (From the Clinical Department of Professor Ziemssen's School of Medicine in Munich.) By Dr. M. Stumpf.—In this very able and elaborate paper Dr. Stumpf communicates the results of a long series of researches on the action of jaborandi on the human system. After briefly alluding to the circumstances under which this substance was first employed as a medicinal agent, and to the examination of its properties by French pharmacologists and subsequently by Sidney Ringer and Gould in this country, the author refers to the early notices of the action of the plant in Germany, where it would appear that the first opinions as to its application were unfavorable. Riegel, however, wrote enthusiastically in its favour in an extensive treatise, and he was followed in most of his views by Merkel, who published twenty cases from the Nuremberg Hospital; Lohrisch, who accompanied his remarks with a large number of observations from the Clinical School of Frerichs; Penzoldt, with seventeen cases from the Clinical School of Erlangen; and Rosenbach with twenty-three cases from the School of Jena. All these observers were agreed as to the energetic diaphoretic action of jaborandi, although Lohrisch considered this action to be changeable and uncertain, and salivation the only constant phenomenon.

Professor Ziemssen commissioned Dr. Stumpf to institute a large number of researches in the Clinical School of Munich, and the latter physician gave the drug in 54 cases, endeavouring in every instance to estimate all the effects, as far as possible, by numbers. He describes the methods he took to ensure accuracy in the results, such as the careful examination of the quantity of urine passed, of the rate of the respiration, the degree of temperature of the body, the careful weighing of the patient before and after each experiment, &c.; and he was present himself in each case in order to watch the patients carefully, and every half hour the temperature was taken in the rectum and the rate of the pulse and the respiration was recorded.

The jaborandi in all cases was used in the form of infusion.

The following is a general description of the phenomena observed by Dr. Stumpf as resulting from the administration of the drug. A few minutes after it was taken there was a subjective feeling of warmth, and the face presented a more or less red appearance, and the skin felt warmer than usual. Soon afterwards the salivary glands began to pour their secretion more copiously into the mouth, and then a feeling of nausea supervened, which, however, usually disappeared in a few minutes, although in fever patients vomiting sometimes occurred. A few minutes after the commencement of the secretion of saliva the skin became moist, at first on the chest and then successively on the other parts of the body, the upper extremities pouring out enormous quantities of perspiration, but the lower ones being less affected. About an hour after the drug was given there was generally a disturbance of vision, distant objects being misty and indistinct, and only near ones being distinctly visible. About an hour and a half afterwards the patients complained, for the most part suddenly, of great nausea and retching, often with hiccough, and these symptoms sometimes terminated with vomiting. Three or four hours after the medicine was taken the secretion of sweat diminished and gradually disappeared, that of the tears and that from the nose had already ceased, and only the secretion of saliva continued and usually only for a few minutes. The disturbance of vision and nausea also soon ceased, but the appetite was impaired and the patients complained of headache. At the same time a desire for sleep often supervened and the patients sank into a deep slumber lasting for several hours, and when they woke all the symptoms generally disappeared, with a few exceptions.

Dr. Stumpf gives in detail the history of his fifty-four cases, forty of which were without fever, and fourteen were cases of fever. It is impossible to do more than to describe briefly the results of these careful and laborious investigations which, however, may be summarised as follows. Dr. Stumpf says that the most striking and constant symptom in the action of jaborandi is the hypersecretion of certain glands of the skin, and of particular mucous membranes, and the sweat-glands are chiefly called into heightened activity. The loss of fluid by the skin and the lungs is estimated at about five times the natural amount. The salivary secretion is also notably increased, and likewise the tears are augmented and the secretion from the nose. The effect of jaborandi on the secretion of the urine is not constant, but it is clear that the amount of this fluid will be diminished under the enormous loss of water by the skin and the saliva. In patients not suffering from fever the drug caused in only a very few cases some lowering of the temperature of the body, but in the fever cases, ten in number, the temperature fell in seven. The diminution therefore is greater in fever cases, but unfortunately is not so constant.

In reference to the indications for the therapeutical use of jaborandi Dr. Stumpf offers some guarded but useful remarks. He thinks that a drug which withdraws on the average one sixth of

the water of the blood might be supposed to be efficacious in causing the absorption of effused fluid in the system, and he says that he has observed some good effects of the kind in a case suffering from general dropsy resulting from chronic pericarditis. Jaborandi is, he thinks, preferable, especially in private practice, to vapour baths, and hot baths, with the subsequent packing; for these often produce unpleasant consequences, the packing especially involving an unpleasant manipulation in patients suffering from dyspnœa and oppression at the chest. Jaborandi, like other diaphoretics, may be recommended in rheumatic affections, especially in the sequelæ of acute and chronic rheumatism of the joints. Dr. Stumpf concludes by observing that although jaborandi is a valuable addition to the materia medica, it can never be generally used until its price is reduced and it can be obtained in a pure and unadulterated state; and he adds that many of its preparations as now sold are inert and worthless, and to this cause some of the objections made to its employment may be attributed.—*Deutsches Archiv für Klinische Medicin. Leipzig*, November, 1875.

On the use of Butyl-chloral as an Anœsthetic. By Dr. Oscar Liebreich.—In an article in the *Deutsche Medicinische Wochenschrift*, Dr. Oscar Liebreich begins by stating that, from certain chemical considerations, the substance commonly called croton-chloral is, in fact, butyl-chloral. He then describes the physiological action of this body, which, from the results of a great number of experiments, he ascertained to be very constant. In rabbits anæsthesia was found to commence at the head, a state of hypnotism having been produced, and with fatal doses the respiration ceased before the pulse. The post-mortem appearances differ from those of poisoning by chloral, inasmuch as in the latter paralysis of the heart is found, both ventricles being full of blood, while in death from butyl-chloral the left ventricle is contracted and merely empty, the right cavities and the lungs being charged with blood. Dr. Liebreich believes that butyl-chloral may prove very useful as an anæsthetic in operations on the head, especially when it is impossible to give such agents by inhalation, and its practical value seems to be in its property of diminishing sensibility before producing narcosis. He found it very useful in two cases of tic in which the tenderness of the face was very great, but in which a gramme (about 15 grains) of the butyl-chloral produced complete relief, lasting generally for two hours each time that the dose was repeated. With regard to the mode of administration he first tried an alcoholic solution, but found that in this form the butyl-chloral underwent some change which impaired its action; he now, therefore, orders it in combination with glycerine and distilled water, butyl-chloral-hydrate, 5 to 10 parts; glycerine, 20 parts; distilled water 130 parts; the mixture to be shaken before it is used. The dose of this preparation is half an ounce, followed in five minutes by a second, and in ten minutes by a third. It is well to begin with a small dose so as to avoid hypnotism when the anæsthetic effect alone is desired, but to produce sleep, one,

two, or three grammes, according to the patient's constitution, are to be given at bed time.—*British Medical Journal*, Feb. 12, 1876.

On the Therapeutical Uses of the Iodide of Lead. By Dr. Schönfeldt.—Dr. Schönfeldt remarks that the iodide of lead is superior in its operation to other metallic iodides, and to other drugs having a similar action, in the fact that in small doses its action is not irritant. It does not disturb the organism as the preparations of mercury do which are applied for the same purposes, inasmuch as it does not produce salivation, and therefore may be used to any extent over large surfaces of the skin and in the immediate vicinity of the salivary and inguinal glands, and requires no great precautions in regimen or in reference to the influence of weather. Iodide of lead has been given internally as well as applied externally, the dose being from two to three grains. In the form of ointment the proportions are from half a drachm to a drachm of the iodide to an ounce of lard, and the addition of glycerine renders it more fit for absorption. Dr. Schönfeldt states that being a country practitioner he prepares a liniment of the iodide himself on the spot where he uses it. He has found the iodide very useful in many cases where the mercurial and other preparations of iodine had produced no effect. He gives the details of several cases in which he employed the iodide of lead with success. Three of them were instances of new formations, viz. lupus, syphilitic growths, and nasal polypus, and others were cases of indurations, exudations, and tumours. He considers that his cases confirm the reports as to the beneficial effects of iodide of lead in removing indurated tissues, in decomposing neoplasms and, after necrobiotic cell-life, in inducing a more healthy form of granulations, in limiting or removing exudations, and in dissolving the remains of plastic growths. He also thinks it would be worth while to re-introduce the internal administration of the drug and to institute more exact researches, especially in reference to new formations, either of a homœoplastic or a heteroplastic nature, especially as this iodide is proved to have but little poisonous action, in contradistinction to the mercurial preparations with which it nearly corresponds in its physiological action though not in its injurious consequences.—*Archiv für pathologische Anatomie und Physiologie, und für Klinische Medicin*, Berlin, December, 1875.

On the Therapeutical Action of Gelseminum Sempervirens. By Dr. Jurasz, of Heidelberg, and Dr. Berger, of Breslau.—Dr. Jurasz has employed with strikingly favorable results the tincture of gelseminum sempervirens in five cases of neuralgia, the dose given being five to twenty drops three times a day. In one of the cases quinia had been administered, and veratria ointment had been rubbed in without any permanent benefit, and in another iodine had been used internally and externally, also without relief. Dr. Berger, who employed both the watery extract obtained from the root and also the tincture, gave the drug to forty patients, most of whom were suffering from different kinds of neuralgia, partly of centric, partly of eccentric origin, and others were affected with chronic com-

plaints attended with pain and sleeplessness. From his researches he came to the conclusion that the extract in doses of from ten to fifty centigrammes almost always produces no sedative effect, but rather causes unpleasant symptoms, such as giddiness, double vision, nausea, vomiting, &c. The tincture was found by Dr. Berger to be inert in the dose of thirty drops, three or four times a day, and the symptoms of poisoning appeared only after doses amounting to three or four teaspoonfuls had been given.—*Schmidt's Jahrbücher der Gesammten Medicin*, October 20, 1875, and January 12, 1876.

In addition to the above abstract, it is only right to state that the properties of Gelseminum Sempervirens are now being investigated in this country by two most competent observers, namely, Dr. Sidney Ringer and Mr. William Murrell. Dr. Burdon Sanderson is also about to examine the action of the drug on the lower animals. The results published by the first two gentlemen up to the present time show that the gelseminum exercises a very important action on the movements of the heart and respiration, and it also appears to be poisonous in large doses. The observations already made by Dr. Ringer and Mr. Murrell, prove that in the lower animals the drug reduces the frequency of respiration; that the affection of respiration appears before paralysis appears, and passes off earlier; that gelseminum in fatal cases causes death by asphyxia, but that if artificial respiration can be maintained until the drug can be eliminated, recovery will take place.—*Lancet*, March 18, 1876.

On the Efficacy of a Milk Diet in the Albuminuria of Pregnant Women and its indication in preventing Eclampsia (puerperal convulsions). By Dr. Tarnier, of the Maternité, Paris.—The efficacy of a milk diet in Bright's disease has led Dr. Tarnier to believe that this regimen might be useful in the albuminuria of pregnancy, and he has therefore adopted it in the cases of the pregnant women at the Maternité who presented albumen in their urine. He has obtained very successful results, and has found that under the treatment in question there was always a rapid improvement, the albuminuria disappearing before delivery. Only once he observed the treatment to be useless, but in this case there was advanced Bright's disease, and the patient died soon after delivery, the kidneys being found to present the usual lesions on the post-mortem examination. He records the particulars of two cases in which the amount of albumen was measured daily, after precipitation by heat and nitric acid. Dr. Tarnier is led to believe that since the albuminuria of pregnancy may be cured by the milk diet, puerperal convulsions may be prevented by the same mode of treatment. To the objection that there are cases of puerperal convulsions without albuminuria, Dr. Tarnier replies that such cases are exceedingly rare, and that almost always the cause of these convulsions is to be found in the changes induced in the system by a pre-existing albuminuria, which must besides have lasted for some time. Without insisting upon any general conclusions at present, he states that in the cases subjected to the milk diet the albuminuria has disappeared or diminished, and there have never been any convulsions. He attributes the success he has

obtained to the employment of the treatment *at the proper time*, and he insists on the necessity of examining the urine of pregnant women, even although they may appear to be in perfect health. In all his own patients at the Maternité albuminuria was watched for, and as soon as it appeared in any instance the milk diet was adopted and continued until the disappearance of the albumen. If convulsions have actually occurred, the treatment is then too late.—*Bulletin Général de Thérapeutique*, Dec. 30, 1875.

On the Therapeutical Use of Nitrite of Amyl. By Dr. Robert Pick.—After describing the physiological properties of the nitrite of amyl, Dr. Pick adduces the experience of several German physicians as to its therapeutical uses in various affections, chiefly of a nervous character. In cases of epilepsy, for instance, the inhalation of a few drops of the nitrite, taken at the moment of the sensation before the paroxysm, acted as a palliative, and sometimes warded off the attack altogether. Dr. Mäurer, of Laubbach, near Coblenz, has given the drug in several complaints in which a spastic contraction of the vessels appeared likewise to play a part. In hemicrania, for instance, he found it very useful, and also in the colic of menstruation, and in anæmia of the brain; and as he thinks that many cases of epilepsy depend on this anæmia, caused by spastic vascular contraction, his success with the nitrite in some epileptic cases is explained. Dr. Pick relates a few cases from his own experience where the nitrite was more or less useful, the complaints being hemicrania, neuralgic pains in the face and head, and supra-orbital neuralgia. He also found it useful in a case of convulsions in a child, and he has often employed it with beneficial and rapid effects in cases of fainting fits.—*Deutsches Archiv für Klinische Medicin*, February, 1876.

On Salicylic Acid as an Antipyretic. By Dr. C. A. Ewald, of Berlin.—Dr. Ewald has made use of salicylic acid and its salts in Professor Frerich's wards, and his experience extends over upwards of one hundred cases. The majority of them were cases of typhoid fever, and the remainder included nearly all other acute diseases accompanied by high fever, and also cases of phthisis. The general action of the drug was the same in every case, the only difference being one of intensity. With regard to its preparation and dose, it is of little importance whether the pure acid or one of its salts (that of sodium is the best) be employed. The minimum dose required to reduce the temperature effectually is 77 grains (5 grammes). Dr. Ewald has seen cases in which 15 or 20 grammes of the salicylate have been given during twenty-four hours without any ill effect. It is quite soluble in water, and most patients make no complaint of its taste. Although its action was not the same in all cases, it proved to be, in Dr. Ewald's hands, more trustworthy and efficacious than all other anti-pyretics. In order to test the value of the drug more accurately, it was always given at midday before the usual afternoon rise of temperature. This began to fall almost immediately after the administration of the drug, the maximum result being reached in most cases in from four to five hours after the drug

was given, but sometimes not less than eighteen to twenty hours were required. As to its general action it was found that within fifteen minutes, or even less, of the administration, a copious perspiration broke out, and the temperature generally began to decline simultaneously with the outbreak of the sweating. The pulse and respiration were generally not at all affected, and Dr. Ewald thinks that the recorded accounts of irritant effects on the alimentary tract are due to the admixture of irritant matters, such as carbolic acid, with the salicylic acid. As to the action of the drug in different diseases, Dr. Ewald considers that in most affections accompanied by acute or chronic fever it has no effect upon the local process; thus, in pneumonia, erysipelas, phthisis, pleurisy, &c., though the temperature is reduced, there is no change in the local processes. With regard to the action of salicylic acid in acute rheumatism and typhoid fever, Dr. Ewald, and also Dr. Goltdammer think that it is efficacious, and more so in acute rheumatism than in typhoid fever. Dr. Ewald thinks he can say with certainty that in many cases after three or four doses, or even after five or ten grammes are given in the former disease, the fever is reduced and the articular pains are dispersed, so that in a few days acute cases may be looked upon as cured.—*The Practitioner*, March, 1876.

REPORT ON PATHOLOGY AND THE PRACTICE OF MEDICINE.

By John T. Arlidge, M.D., A.B. Lond., F.R.C.P. Lond.,
Physician to the North Staffordshire Infirmary, &c.

Diphtheritic encephalitis.—Dr. L. Letzerich bases the recognition of this special form of encephalitis upon an observation made in the case of his own child, who died, when six weeks old, after an attack of diphtheria which commenced on the twelfth day after birth, and appeared to have been cured fourteen days anterior to death. The pronounced state of somnolency, with half-opened eyes during sleep, and the peculiar sort of cry uttered by the child suggested the existence of some cerebral lesion. Accordingly, a careful examination was made five hours after death by a friend, Dr. Stephan. The parietal bones were considerably congested posteriorly, as were also the cerebral meninges; but the surface of the brain itself was very pale. When cut into there were many bloody points, the oozing blood being dark and fluid. The pallor of the grey matter was particularly remarked at the anterior and middle portion of the right hemisphere, and no boundary line separating it from the subjacent white substance could be detected. The right lateral ventricle was considerably smaller than the left, which presented nothing abnormal.

The diminution of the former appeared due to a swollen, spongy state of the surrounding brain matter; and on a section across the cerebrum, the right side was seen to be larger than the left. The grey ganglions of the cerebrum retained their normal colour and consistence. The cerebellum looked smaller than natural, and its greyish-red, rusty lamina had lost its colour. In the leaf-like branches of the arbor vitæ were found roundish, granular structures which on section protruded, and were as large as millet-seeds. The corpora dentata and medulla oblongata exhibited nothing abnormal.

Microscopical examination.—Very fine sections of the grey substance of the right hemisphere were remarkable for a softness and sponginess Letzerich had never before observed in an infant's brain. Under the microscope the neuroglia appeared like a finely granular detritus, in which numerous neuroglia cells were embedded. In company with these cells were also a huge number of bacteria in lively motion, with smaller and larger colonies of micrococci and plasma corpuscles. Never before had the observer seen such quantities of diphtheritic fungi in their different forms within so small a compass. Groups or masses of bacteria with plasma-corpuscles occurred within the ganglion-cells, surrounding the nucleus. In many cells the presence of the organisms had transformed their protoplasm into a clear transparent matter destitute of granules. It is generally easy in the healthy brain to detach ganglion-cells and to exhibit their processes, but such a proceeding was impossible in this brain. The wasting and disintegrating process was exhibited not only in the neuroglia, but also in the other elementary portions of cerebral matter, in the nerve-cells and their processes, and as a consequence the sections of brain would not bear the slightest force without breaking up.

Similar appearances were met with in a portion of brain taken from the anterior portion of right hemisphere. In one respect they, however, differed; the neuroglia-cells were less numerous, and the larger portion of the nerve (ganglion) cells were broken up and absent from their peri-cellular spaces which were occupied with plasma-corpuscles.

The bacteria had made their way into the cerebrum by medium of the lymphatics and blood-vessels, as evidenced by examination; capillary vessels being encountered containing micrococci. Such vessels also often presented blood-corpuscles which had lost their red colour, although retaining their normal structure; and here and there groups of these altered blood-discs had escaped the vessels, constituting a capillary hæmorrhage without red colouring.

In the cerebellum bacteria and the plasma-corpuscles of the diphtheritic fungus were likewise found. The ganglion-cells of the grey lamina were occupied with them, and in some few peri-cellular spaces the plasma-corpuscles were discoverable. On section of the coloured lamina the rusty-coloured substance was found absent; whilst in the white substance there was an extraordinary development of neuroglia cells, constituting nodules of the size of millet-seeds, and which when

cut across resembled glioma of the brain. But the ganglion-cells were not so changed as in the grey substance, and not nearly so much as those in the right half of the cerebrum.

The ganglions, both of the cerebrum and cerebellum, also contained bacteria, but in small numbers. The much greater prevalence of the diphtheritic fungi in the cortical than in the white substance of the encephalon Dr. Letzerich attributes to the differences in the distribution and course of the blood-vessels. In the white substance the vessels are straighter and less branched and anastomotic than in the grey, and consequently offer fewer opportunities for the lodgment of the bacteria corpuscles.

The decrease of size of the cerebellum, and the enlargement noticed in the case of the cerebrum are circumstances due to the same cause. For the greater portion of the grey and the whole of the rusty colouring matter of the cerebellum, where the bacteria existed, was destroyed, a condition necessarily attended by shrinking; so, again, the white substance pretty generally retained its consistence. But in the case of the right hemisphere of the cerebrum there was considerable granular exudation as well in the grey matter, as likewise between the fibres of the white substance, and, as a result of this deposit of new matter, an increase in bulk.

The preceding pathological changes of the right hemisphere necessarily suspended the physiological activity of the brain. Foremost in inducing symptoms of disease stand the disorder of circulation following upon over-filling of the vessels by stasis of blood from emboli of the diphtheritic fungus, with exudation and watery permeation of tissue, as well as the outpouring of fluid under the meninges, and particularly in the fossa cerebelli, as noticed in this case. Lastly, the cause of the child's death may be set down as diphtheritic paralysis of the brain.

[Virchow's 'Archiv für pathologische Anatomie,' &c. (Band lvi, 1875, p. 410).]

Chronic Spinal Amyotrophy.—M. Charcot has sought to distinguish and characterise the several forms of progressive muscular atrophy which have hitherto been confounded together under that one appellation. In the first group he places all those cases in which the lesion of the nerve-cells of the anterior cornua of the cord is absolutely primary, and does not extend beyond them. It is a true progressive muscular atrophy, characterised by atrophy of each individual muscle involved, by the retention of normal electrical contractility, by its slow course and wide diffusion. The primary lesion is in the nerve-cells; that of the roots of the nerves and of the peripheral nerves is secondary. The peculiarity of the muscular change is the concurrence of simple atrophy of the fasciculi with retention to the last of the muscular striæ, and the proliferation of the cellular elements of the sarcolemma. Granular fatty degeneration is only a secondary phenomenon. In a second group are included all the deutero-pathic or symptomatic spinal amyotrophias; such as arise in the course of some other malady of the spinal cord. The lesion is confined to the anterior cornua, the cells of which have

atrophied, with the result of atrophy of the roots of the nerves, of the nerves themselves, and of the muscles. This extension of the lesion is observed in many different conditions; the principal of these are, spinal hypertrophic pachymeningitis, sclerosis of the posterior radical fasciculi—the substratum of progressive locomotor ataxy, some forms of centric myelitis; intra-spinal tumours, sclerosis 'en plaques,' and especially symmetrical sclerosis of the lateral columns of the cord. It is worthy of note that whilst these various scleroses have a tendency to invade the anterior cornua, the descending scleroses, consecutive to different lesions of cerebral or spinal origin, very rarely extend to that portion of the cord.

There is a disease not yet clinically determined, described by Duchenne under the name of general spinal paralysis, which is accompanied by advancing amyotrophy. It may be regarded as curable. Its characteristics are, a descending or ascending paralysis, with flaccidity of muscles, muscular atrophy affecting the limbs wholly or only in part, with loss of faradic contractility. Neither the sensibility, nor the nourishment of the affected parts is interfered with. Its course is often irregular; it is usually curable, but labio-glosso-pharyngeal paralysis may be the cause of death.

M. Charcot concludes with a notice of the amyotrophias the anatomical basis of which is not to be discovered in the spinal cord. Such are pseudo-hypertrophic paralysis and lead paralysis. These lesions appear to depend on peripheral nerve injury; and the accompanying atrophy is marked by the abolition of faradic contractility. The lead amyotrophy may be so general as to stimulate progressive muscular atrophy. Local rheumatic and traumatic amyotrophy seems also due to peripheral nerve lesion. ('Revue des Sciences Médicales,' 1875, p. 559.)

The Temperature of Drunkards.—Dr. J. J. Reincke, who is physician to the police in Hamburgh, instituted thermometrical observations with respect to eighteen men picked up from the streets in a completely drunken state. They leave no doubt as to the great reduction of temperature in such persons, and Dr. Reincke considers his observations serve to solve the question whether the reduced temperature is owing to the action of the alcohol in arresting the production of heat, or in promoting the more rapid loss of heat. The primary action of alcohol shows itself by dilatation of the peripheral vessels, whereby more blood enters the skin and contributes to raise its temperature and to produce a redness and turgidity, with a subjective feeling of heat, which, indeed, is more or less objective, as Rebow has shown, being indicated by the thermometer placed in the axilla. Now if the body in this state be well clothed and protected from external influences likely to abstract heat, the reduction of its warmth is very inconsiderable: on the contrary, if exposed to cold, and placed under circumstances favorable to the abstraction of heat, there is a rapid loss of warmth from the blood circulating in the skin. In illustration, he quotes one case of a man removed immediately from the street, where he was lying helplessly drunk to the warm police station and put at once to bed in the Infirmary connected with

it, where he remained for several hours in a state of profound stupor without reaction, and yet his temperature never fell lower than two degrees (Réaumur) below the normal standard. On the other hand, he has witnessed in many examples of slighter intoxication, where the individuals who have been immersed for a short time in cold water, have lost from four to five degrees. In these it may be admitted that the surrounding cold has not been the sole factor in so considerable reduction, but that the action of the alcohol in staying the oxydizing processes in the blood and tissues may be made accountable for part of it.

The general conclusion is, that the depressed temperature of the victims of alcohol depends considerably on the external conditions favouring the withdrawal of heat from the body.

It should be noted that in taking the temperature in his several cases, Reincke introduced the thermometer into the rectum. The lowest temperature (lower than in any recorded instance in which the individual survived) met with was in the case of a man, æt. 34, picked up in the streets about midnight in February, when the temperature of the air was 30° Fahr. He was in a state of complete alcoholic coma, responding to no excitant. At 8 a.m., his temperature, taken in the rectum, was only 75°; at 10 o'clock it had risen two degrees, and at 12 reached nearly 82°. At this period reaction began to show itself, and he could mutter a few words. From this time the heat of the body gradually increased and had reached the normal point the following morning. ('Deutsches Archiv,' Band xvi, 1875, p. 122.)

Pigmentary Deposits in the Brain from Malarial Poisoning.—Dr. W. A. Hammond, of New York, read a very interesting paper before the American Neurological Society, which has since been reprinted in the form of a pamphlet. He takes a general survey of the facts and opinions set forth by various authors respecting pigmentary deposits in the various internal organs, but particularly in the brain, and he claims for himself the merit of directly associating such pigmentation with the existence of malarial fevers.

He supposes the pigment to be deposited in the brain, either in the form of emboli obstructing the smaller vessels and the capillaries suddenly, or of thrombi, the pigment being slowly deposited along the inner wall of the vessels and thus gradually leading to their occlusion; or, as a transudation into the perivascular tissue. It especially accumulates in the cortical substance, by reason of the peculiar vascular condition of that layer, and produces a chocolate or black lead colour. When in great quantity it is also manifested in the white substance by a greyish tinge and by the appearance of the blood-vessels as brownish streaks. It derives its origin from the spleen, from whence it is carried into the general circulation, and gets deposited within or around the cerebral vessels, often in the ophthalmic vessels, and very frequently in the liver.

The consequence of the deposit of the pigmentary matter in the brain is the production of various disturbances of function, which

have not escaped the observation of writers on malarial fevers as common phenomena attributable to those fevers.

Dr. Hammond has detected specks of colouring matter, by the aid of the ophthalmoscope, on the retinal disc, and likewise in the blood drawn from the enlarged spleens of individuals suffering with intermittent fever. In illustration of his views, Dr. Hammond narrates five interesting cases. In his first case there was deafness, and frequent epileptic convulsions, and principally at the outer periphery of the retina were detected some pigmentary deposits. He pierced the spleen of this patient with the point of a large hypodermic syringe, and drew off a few drops of blood, which he submitted to microscopic examination, and thereupon found numerous masses of free pigment, irregular in form, and varying in size from the one-thousandth to the one-three hundredth of an inch. In the second case a gentleman had suffered on two previous occasions with tingling in the right foot, gradually extending to the head, and accompanied by giddiness; during the last five years the eyesight also had been greatly impaired, and indeed, for a time was lost after protracted reading. When examined he still had frequent vertigo, pain in the head, great impairment of sight, and decided loss of mobility on the right side. Upon examining the fundus of the eye, optic neuritis, and large deposits of pigment were found; worse in the left eye. The masses of pigment were stellate in form, and followed, mainly, the course of the arterial branches. He also repeated the experiment of taking blood from the spleen and with like results as in the former case. The third case described was that of a youth, æt. 18, who had chorea, which supervened upon repeated vertiginous seizures of an epileptoid character, but without known loss of consciousness. Previously to these seizures he had had many attacks of intermittent fever. The ophthalmoscope revealed the like stellate pigmentary retinal patches as in the other cases, but the patient refused consent to taking blood from the spleen. In the fourth case the patient suffered with hypochondria, and once in the interval between the febrile exacerbations had an epileptiform paroxysm. The spleen blood gave both free pigment and pigment-holding cells, but no abnormal ophthalmoscopic appearances existed beyond choked disc on both sides. The last case was one of intermittent aphonia in a young lady, æt. 19. She had for three years, every spring and autumn, experienced attacks of intermittent fever, which were cut short by the use of quinine in large doses. The spleen had in the earlier part of her illness been very much enlarged. The splenic blood was not examined, owing to her objections, and there were no pigmentary deposits in the eyes, nor other abnormal conditions.

In all these cases Dr. Hammond used arsenic with most decided advantage; and in cases 2 and 4, he employed the fluid extract of ergot in the treatment of the enlarged spleens, injecting it in drachm doses directly into the organ by means of a hypodermic syringe.

The writer refers to some experiments made on animals by inject-

ing finely powdered indigo into the circulation. Although not concluded, these experiments show the possibility of the passage of pigment into the retinal vessels. A consideration of the whole subject warrants him in concluding:

1st. That as a consequence of malarial poisoning, the pigment of the blood undergoes a change in appearance and form, and that the alteration is effected in the spleen, leading to hypertrophy of this organ. 2nd. That this pigment may enter the general circulation from the spleen, either in a free condition, or in pigment-holding cells, and that it may be deposited in the cerebral blood vessels, or pass through their coats. 3rd. That these deposits may give rise to various symptoms, indicating derangement of the nervous system. 4th. That arsenic appears to have the power of, in a way at present unknown, so altering the character of the pigmentary deposits as to facilitate their removal, and to cause the disappearance of the symptoms to which they give rise. 5th. That we may have during the life of the individual, ocular demonstration of these facts by the presence of pigment in the fundus of the eye, as revealed by the ophthalmoscope. (Reprint from the 'Transactions of the American Neurological Society,' 1875.)

[It would add to the pathological value of Dr. Hammond's researches, to ascertain, when opportunity serves, the relations of the pigmentary matter to the medulla oblongata and spinal cord. The second, the third (chorea, so called), and the fifth (intermittent aphonia) cases intimate nerve disorder in parts other than the cerebrum.—J. T. A.]

It will be pleasing to Dr. Hammond, as confirmatory of his views, to read the account of melanæmia given by Dr. H. Meissner in his summary of facts and opinions on pigmentary diseases, published in 'Schmidt's Jahrbücher,' in the numbers for December, 1875 and January, 1876. In the account of melanæmia Meissner quotes two cases. The first was that of a physician who practised several years in a malarial district, and had repeated attacks of intermittent fever. A renewed attack, four years after the first, was attended with crural neuralgia and pain in the muscles of the neck and back. Large doses of quinine removed these symptoms, but a month later they were replaced by urinary troubles; scalding in the urethra, dragging pains at the symphysis, and in fourteen days more, fever supervened with general pains in the joints and periodical pale deposits in the urine, which was itself white and turbid. Quinine, residence at Marienbad and the drinking of the mineral waters, gave him relief. Dr. S. von Busch saw him in Vienna, and found him in a cachectic state with enlarged liver and spleen. Examination of the urine exhibited pale particles, some of them of the size and figure of lymph-cells, but the majority larger and irregular in shape. These enclosed more or fewer dark brown and finely granular portions of pigment-substance, and the like pigment-holding cells were subsequently found in the blood. The urine contained besides crystals of bibasic phosphate of lime, but no albumen. The source of the colouring particles found in it must therefore be traced to

the blood. The second case is quoted from Leon Colin. It was that of a man who had died from dysentery and marsh fever. The enlarged spleen was throughout of a black colour, being stuffed with a carbonaceous looking pigmentary matter. The pigment which occurred in the blood-vessels and about their walls and in the blood, had no resemblance to hæmatosin or the red colouring matter of the blood, and clearly was derived from the spleen, whence it found its way into the blood. It would seem that the white blood corpuscles are concerned in this process, for when coming into contact with the particles of colouring matter they seemed to infold them, and by means of their changing form and amœboid movements, to carry them forward into the smallest vessels. Meissner, in his comments on this case, submits that it is probable the circulation of this pigmentary substance must have some pathological effect, but he does not hint at the necessary association between it and the existence of malarial fever.

Stomach Dilatation and its Diagnosis.—Prof. Leube, of Erlangen, considers that dilatation of the stomach is often overlooked, and that the usual means of diagnosing it are insufficient:—neither dyspeptic symptoms nor the quantity vomited can show it. In rare cases the large and small curvature can be made out by palpation, but the connection with the transverse colon is a common cause of misconception. Auscultation supplies an important aid to diagnosis. Brisk pressure with the hand on the epigastrium causes a splashing sound from the admixture of air and fluid in the stomach; and this sign is remarkable in some cases of dilatation, but may at times also occur in healthy stomachs when some pints of liquid have been quickly swallowed. The like sound is elicited in some cases of dilatation when the patient is quickly turned from one side to the other, or when the body is rudely shaken. Another sign is, that when the ear is applied to the abdominal wall, the rush of fluid is heard over the whole space of the stomach when the patient is made to drink. Again, percussion will, under many circumstances, suffice to prove stomach dilatation (Gastrektasia). All Leube's endeavours to distinguish the percussion note of dilated stomach from that of distended intestine around, have failed when the attempt has been made on the empty viscus. He now, therefore, makes it a rule to use percussion in the partially filled organ, and finds that, in relation to the amount of dilatation and the quantity of ingesta, there is, at a greater or less distance below the ribs, a line of horizontal dulness, having above it the sonorous sound of the distended stomach. Moreover the area of dulness will vary according to the position of the patient, occupying always the lower level of the stomach area. This sign acquires greater certainty, and serves better to distinguish a dilated stomach from a distended colon, if, on the patient drinking a quantity of fluid, the area of dulness is found to enlarge. Movements originating iu the stomach walls themselves, or occurring in the adjoining transverse colon, may still interfere with these indications of the dimensions of the dilated stomach; but errors due to these disturbing causes may be overcome by filling and the emptying

the stomach with fluid, by means of the stomach-pump, and noting at the same time the varying line of dulness. By such expedients, says Leube, can the outline of the stomach be made out in most cases where its lower border, as indicated by the horizontal area or dulness when partially filled, does not descend below the umbilicus. Only in extreme cases is this lower border found below that region, where a distended colon thrusts itself in front of the stomach, a further diagnostic expedient is necessary, and Leube recommends the introduction of the stomach sound, and its manipulation, so that its point may be felt at the lowest border of the stomach through the abdominal parietes. In one extreme case in which he resorted to this proceeding the end of the sound could be felt, when pressed forward, just above the symphysis pubis, and when directed backwards could be likewise distinguished by the finger introduced per rectum. ('Deutsches Archiv,' Band xv, p. 394.)

Dr. Penzoldt, of Erlangen, has produced a treatise on this subject, and has noticed Leube's previously published paper. To make his work complete he has given historical notes of the malady from numerous authors and journals, and has brought together all the information he could collect respecting its pathology. He states that dilatation may be congenital, or may follow a congenital malformation of the pylorus, or otherwise may be produced by an accidental lesion of that part of the stomach or of the intestines. Again, it may be the consequence of habitual overgorging, or of debilitating diseases, as typhoid fever, or of catarrh of stomach and the dyspepsia dependent upon it. The walls of the stomach may be reduced to the thickness of paper; or, on the other hand, be hypertrophied; and, according to Kussmaul, the muscular fibres may undergo fatty degeneration or a colloid change. The gastro-intestinal mucous membrane likewise differs greatly in thickness. As a concomitant or secondary affection the symptoms presented will vary much according to the nature of the primary lesion. Dr. Penzoldt enumerates numerous symptoms, but they are by no means characteristic, being no other than accompaniments of dyspepsia in one or other form, or signs of the presence of sarcinæ, which possibly have some nearer relation with the existence of dilatation than the rest. To make out the extent of the stomach, the writer suggests the drinking by the patient just before examination of copious draughts of gaseous liquids for the purpose of distending it. With reference to the employment of the stomach-pump or sound, he wisely advises that we should first satisfy ourselves of the absence of cancerous disease of the stomach, or of ulceration; but, with this precaution, he concurs with Leube in recommending the filling and afterwards the emptying of the stomach, and the measurement of the quantity ingested and extracted; and, further, in suggesting the use of the sound and the measurement of the extent of its penetration.

From a series of observations Penzoldt has ascertained that in the case of women, taken at an average height of five feet, the medium distance between the incisor teeth and the bottom of the stomach is about twenty-two inches, of which number little more

than three inches intervene between those teeth and the back of the pharynx. In men, taken as about five feet three inches in height, the distance was about twenty-three inches from the incisors to the bottom of the stomach. Curiously enough, in the dead subject these distances are very appreciably shortened. The medium length found in three cases of dilatation was about four inches greater than that found in healthy men; viz., twenty-seven inches. And, as the author observes, it was a remarkable fact that the depth to which the sound penetrated was nearly equal to the length of the vertebral column.

As a rule, dilatation of the stomach is a very chronic condition. It has been stated to end in rupture by softening or thinning of the walls, but such a termination is very exceptional. Usually it destroys life by the attendant disorder of digestion and nutrition. Kussmaul was the first to point out the good results of washing out the stomach by means of the stomach pump or the siphon. The latter is a less violent and preferable proceeding. Plain water is usually the best fluid for the purpose, but any saline ingredients thought useful may be dissolved in it. Regimen is the first element in treatment, but it may be seconded by the use of tonics, of electricity, and of baths. ('Révue des Sciences Médicales,' tome vii, p. 137, 1876.)

[In the examination of this condition, dilatation of the stomach, the two writers whose papers we have analysed, appear to us to have lost sight of that peculiar displacement of the stomach, comparatively common among women, in which the viscus, instead of being nearly horizontally placed across the abdomen, assumes a very oblique, and even at times an almost vertical position. It is clear that when displacement of this sort exists, the introduction of the sound gives no certain criterion of dilatation, regarded as an expansion of the distance between the great and the lesser curvature; in fact, it would mislead the observer, who would rather get the measure of the distance between the cardiac end of the stomach and the pylorus. Further, this vertical displacement, although prone to develop dilatation by favouring dyspepsia, is not necessarily attended by dilatation. Consequently, the vertical length of the sound or tube when introduced cannot be taken as evidence of dilatation, unless by examination otherwise carried out, this vertical position of the stomach can be excluded from consideration.—J. T. A.]

Infrequency of Pulse.—Dr. Austin Flint has brought together the histories of five cases of infrequent pulse, which have fallen under his own immediate observation or that of friends; and he quotes likewise the remarkable case related by Mr. Pugin Thornton to the Clinical Society of London (,Transactions,' vol. viii.). A review of the whole number shows that the ages of those affected varied between twenty and forty-three: that four were males and two females, and that the lowest degrees of infrequency reached in the several cases were 16, 40, 35, 26, 38, and 26 per minute. Of Dr. Flint's five cases, the pulse in health was know to be not far

from the normal average in three; it was presumably so in one case, and there is room for doubt on this point with reference to the remaining one. In Mr. Thornton's patient cessation of the heart's action for eighteen and sixteen seconds was noted. In one of Dr. Flint's there was marked intermittency, and in another, at one time, irregularity. In the remaining number the rhythm of the heart was regular.

In each of the cases the patient may be said to have recovered from the cardiac disorder. The only qualification to this statement is, that in two of them the pulse remained at 40 per minute after evidence in other respects of recovery. With regard to these two individuals it is a question whether 40 was not the normal frequency of pulse. At least the patients were unaware of the peculiarity when in health, although one likely to have become known to them.

As a rare occurrence normal slowness of pulse may be an acquired condition; but usually is a concomitant of cerebral disturbance. Of the six cases collected this was true of all excepting one. Thus in two there were several epileptic seizures with frequent epileptoid attacks; in two other cases there was mental excitability, amounting to delirium, and in one there was great mental and physical prostration and gastric irritability, due apparently to cerebral disturbance.

On the causation of this condition of pulse the histories of the cases recorded throw no real light. It is a condition which, in practice, has to be detached from more or less similar pulse irregularities, such as those produced by organic affections of the heart, valvular disease and fatty degeneration. It may be also confounded with a functional disorder of the heart, characterised by a regular alternation of a ventricular systole, giving rise to a radial pulse, with one so feeble as to be not appreciable at the wrist. This is a curious form of disorder which may also be mistaken for a reduplication of both heart sounds. A comparison, however, by means of a stethoscopic examination, of the heart sounds with the radial and carotid pulse will enable us always to exclude not only this disorder, but also a veritable reduplication of the heart sounds. Such a comparison will show that infrequency of the pulse represents a corresponding infrequency of the heart's action.

Infrequency, farther, is common in cases of injury of the skull and intra-cranial affections, of which cerebral hæmorrhage, embolism, and thrombosis, and sub-acute and chronic meningitis, are examples. But in meningitis there are special cerebral symptoms, fever, excitability of special senses; all which are absent in the simple condition of infrequent pulse under notice. So again in the other cerebral lesions, the existence of paralysis is sufficient to make the distinction. Other examples of infrequent pulse are met with in jaundice and uræmia, and as a passing condition, in consequence of the use of certain drugs; viz., aconite, digitalis, and especially the veratrum viride.

Respecting the pathology of slow pulse, Dr. Flint expounds it by

the hypothesis of an abnormal increase of the restraining or inhibitory influence of the pneumogastrics over the sympathetic nerves concerned in the heart's activity.

The prognosis of infrequent pulse is favorable. As to special remedial measures, they do not seem of much avail. Alcoholics had little effect on the heart, bromides were of no benefit, but a combination of strychnia, atropia and iron, and subsequently phosphorus, appeared, in the one case in which they were employed, to have been of service. Dr. Flint himself suggests the employment of opium in small doses, so as to maintain the vigour of the heart's action. ('The American Practitioner,' January, 1876.)

On Irritation and Paralysis of the Vagus Nerve.—The following excerpta may be read in connexion with Dr. Flint's hypothesis of the pathology of infrequent pulse. Quincke has recorded two cases of cerebral disease, in the course of which by compression of the carotid, the pulse was greatly reduced in frequency. In one case there was diffuse sclerosis of the brain; in the other, atheromatous induration of the walls of the arteries at the base of the brain. In his comments on these cases Quincke remarks that a slowing of the pulse is a common consequence of pressure upon the carotid, both among the sick and the healthy. It may be induced experimentally by direct compression of the carotid on the level of the thyroid cartilage, whereby the diastole of the heart, as exhibited by the pulse, is prolonged several seconds, owing to the diminution of arterial pressure. Moreover, the retarded condition is followed by three or four pulsations of greatly increased force. Between the application of compression on the artery and the onset of the longest diastole an interval of from one to two pulsations occurs. Experiments were made on eighty individuals, some in health and others sick. In thirty-three the results were negative, and in forty-seven positive. The pressure when exercised on the right side was more energetic in its action than when made on the left. Excitement of the heart's action, whether from psychical causes or physical exertion, made no difference to the results. Regard being had to the augmented blood pressure in the aorta, the anæmia of the brain and the hyperæmia of the vagus centre, Quincke inclines to the belief that the phenomenon is due to direct mechanical irritation of the nerve.

Dr. Thannhofer, of Buda-Pesth, experimented on a pupil, compressing the vagi, whilst a sphygmometer was affixed to the right wrist. Immediately the pressure was exerted the lever of the instrument became depressed, and after 67 seconds made a slight movement upwards, and so remained on account of the heart's action being brought to a stand, and the necessity to stop the experiment by reason of the induced unconsciousness of the subject of it. Consciousness gradually returned, the pulse beat powerfully, for a considerable time vertigo was felt, and rigors came on with fever and nausea. Two hours after the hazardous experiment, the pulse which had been 80 in the minute prior to its performance, beat 74 in the

minute and unrhythmically. The student slept well the following night, but woke with headache, which, however, soon passed away and left him in his usual health.

To the preceding experiments may be subjoined a case detailed by Dr. F. Riegel, in which the left vagus was paralysed by disease, and the pulse, instead of being retarded, was greatly accelerated. A sawyer, æt. 53, who, in the previous autumn was treated for bronchitis, came under Dr. Riegel's notice in the spring of 1875, complaining of some dyspnœa with palpitation, a slight cough and on one occasion some hæmoptysis. Only some bronchial sounds could be heard. He was greatly emaciated. When perfectly quiet, respiration was normal in character and frequency; cardiac impulse indistinct. Radial pulse scarcely perceptible, beating 164 times in the minute. Heart sounds feeble but normal. The transverse dulness over heart not much augmented. Temperature normal, and at times below the standard. Death rapidly supervened from pulmonary apoplexy. The autopsy revealed infarctus of the lung, hypertrophied heart, without valvular disease, but containing dark clots in the ventricles, slight effusion in the right pleura; and nutmeg disease of the liver. But besides these lesions the bronchial glands were diseased and much enlarged, of a black colour and dense on section. The left vagus, just below the origin of the recurrent nerve, was found so imbedded in one of these diseased glands that it could not be detached. The nerve itself appeared less than normal, atrophied and somewhat softened; microscopically, its fibres looked sound in the portion above the gland, but within that organ this outline was defaced, the fibres were atrophied and enclosed fine fat-granules. Both recurrent nerves, and the pneumogastric on the other side were healthy. The enlarged heart may be regarded as a consequence of the paralysis of the nerve, and that respiration was not more disturbed is due to the circumstance that only one pneumogastric was affected.

Hayem published a case ('Archives de Physiologie,' 1869, p. 651) in which both vagi were involved in a mediastinal tumour, and where during life the pulsations ranged between 120 and 130 per minute. ('Schmidt's Jahrbücher,' January, 1876, p. 246, and 'Revue des Sciences Médicales,' tome vii, 1876, p. 168.)

Rheumatic localized lesions of muscular nerves.—Dr. Fr. Richter remarks on the loose manner in which the term rheumatism is used; whereby it is made to include the acute articular and febrile disease and various local articular and muscular pains having no pathological identity with it. He quotes authorities on the pathology of muscular rheumatism, and shows that their opinions differ on the point, whether it is in its nature due rather to existing hyperæmia and inflammation, or to a neurosis, and as to which of these two groups of morbid action is to be considered primary. As to its cause, the most active appears to be gradual reduction of temperature, rather than intense and sudden cold. Cold with moisture is likewise a more active morbific agent. These agents imply retarded reaction and more or less hyperæmia of the affected parts, conse-

quent on relaxation of the walls of the blood vessels, and the question arises, to what extent and in what mode the nerves stand related to the disordered function and are contributory to its production. In Richter's opinion, it is an alteration of the muscular, and probably also of the vasomotor nerves, determined by the varying action of cold, which produces the phenomena of neuralgia, spasm and paralysis of the muscles. Further, in association with the nervous derangement, are to be reckoned hyperæmia, exudation and inflammation. Experiment has shown that on the application of cold to nerves, the order of succession of disordered function is, neuralgia (pain and irritation), spasm and paralysis. The last morbid result is attended with inflammatory changes in the tissues, leading to atrophy; and again paralysis of pure motor nerves is associated with that of the sympathetic system, inducing dilatation of blood-vessels and hyperæmia with elevation of temperature; by the continuous operation of which the nerves themselves become structurally chauged; the muscular fibres affected by fatty degeneration and wasted, and the connective tissue unduly increased.

Richter contends against the assertion of Charcot, that irritation of vasomotor nerves never induces trophical changes, as erroneous; and, by way of résumé, writes: "In rheumatic lesions of the muscular nerves, hyperæmia and inflammation are not the primary morbid processes, but a neurosis of the nerves, which takes its rise from irritation and disturbance of their conducting power, and may exhibit itself in the shape of neuralgia, spasm, and paralysis. The irritation of muscular nerves with the accompanying irritative, and the consecutive paralytic conditions present two series of resultant phenomena, viz. on the one hand, hyperæmia with dilated blood-vessels, from paralysis of the sympathetic system; and, on the other, inflammatory affection of the muscular mass from hyperplasia of connective tissue. Again, the neuro-paralytic hyperæmia with its results, *e.g.* exudation products, exercises a further destructive action on the damaged muscular nerves, so that they, in connection with the inflammatory exuberant growth of connective tissue eventually suffer deterioration of motor power and atrophy." Lastly, irritation of the sympathetic may induce trophic disorder. Richter, in the next place, examines the pathology of rheumatism of the deltoid muscle, in connection with the views laid down. In the earlier and neuralgic state electrical irritability is augmented; but when the affection is of older date and the conducting power of the nerves considerably damaged, the electrical response is weaker, and at length is abolished. In recent rheumatic paralysis electric sensibility is heightened and electro-muscular contractility maintained; but in old cases these electrical conditions are decreased. Trophical disorders of the muscles exhibit a reduction of farado-muscular contractility, and an exaltation at first, but subsequently depression of galvano-muscular contractility. In the treatment of the malady, Richter considers no therapeutical agent at all comparable with the continued current to relieve the neuralgia; and either this or the

interrupted current does good service in the paralytic stage. ('Deutsches Archiv,' Band xv, p. 368.)

Pathology of Cardiac Murmurs.—The mode of production of cardiac murmurs has been generally regarded as a settled matter, at least with regard to most of them. But Dr. Leaming, physician to St. Luke's Hospital, New York, holds the interpretation commonly given of mitral regurgitation to be erroneous. He agrees with the late Dr. Cammann, that the signs of mitral regurgitation as generally taught—murmurs at the apex beat, blowing, sawing, rasping, &c., —are unreliable, and asserts that the true and invariable sign is a murmur of an actively different character—a soft murmur, a friction murmur, such as would naturally be formed by forcing fluids through an aperture, and which is heard behind, between the seventh and eighth vertebræ of the left side, close to their spines; and that with this sign alone mitral regurgitation is with certainty diagnosed.

In order to make good his opinion, Dr. Leaming finds it necessary to discuss the causes of the first sound of the heart. Of those advanced by physiologists he recognises only friction of blood within the heart; the sound of muscular contraction, and the vibration of the mitral valve caused by its closure and tension, and the forcing and rushing blood. But he advances as his special proposition that the first sound is caused by vibrations of the chordæ tendineæ connected with the mitral valve in the left heart, and with the tricuspid in the right, set in motion by the current of forced blood; and he urges that if pathology shows that plastic lymph may be exuded upon the surface of the valve, or upon its edges, glueing them together, and if, at the same time, the chordæ tendineæ are shortened and thickened by exuded plastic lymph, or glued down upon the valve so as to prevent vibration, then, if the first sound is altered, and all murmurs are abolished, it must be admitted that the proof of his hypothesis is sufficient. In elucidation of his views Dr. Leaming adduces three cases, particulars of which are detailed in the 'Transactions of the New York Academy of Medicine' (1875). In his first case the intermissions of heart action were remarkable (lasting sixteen seconds), and he assumes that the contraction of the auricles continued regularly, although the ventricles were in a state of rest, as indicated by the absence of the first sound and of the impulse. The heart after death was found with a completely adherent pericardium, and was greatly hypertrophied. The curtains of the aortic valve were thickened and shortened to incompetency, not holding water. The edges of the mitral valve were glued together, extending into the ventricle like a funnel: complete stenosis. The opening very small, the valve and chordæ thickened and covered with plastic lymph, white and glistening. During life the only murmur diagnosed was a slight systolic cardiac murmur, aortic, and obstructive.

In the second case there was a systolic murmur at the base, more distinct over the aortic valves; but no diastolic murmur; no mitral murmur of any kind. But after death, not only were the aortic valves thickened, shortened, and incompetent, but the mitral valve

also was thickened by lymph deposit, and white in colour and opaque, whilst its edges were adherent and its orifice narrowed so much that it would barely admit the top of the index finger. The whole valve extended downwards in the form of a funnel. The chordæ tendineæ were shortened and thickened by lymph deposits, and the musculi papillares thickened and lengthened. But everything was symmetrical: viz., the funnel-like condition of the valve, the hypertrophy of the cardiac walls, of the musculi papillares, and of the columnæ carneæ. With perfect conditions for producing a mitral direct murmur, it was absent. The heart disease was complicated in this case with Bright's disease (contracted kidney).

The third case was that of a man of consumptive predisposition, who suffered with hæmoptysis. The heart's action was quite feeble, though regular; the apex beat on a level with nipple in fifth interspace. After repeated examinations no murmurs could be detected. His death was sudden, with symptoms of paralysis. The autopsy showed embolism of middle cerebral artery of left side, with softening of brain tissue. Heart weighed fourteen ounces. Both right and left cavities contained large clots, and the substance of the heart was relaxed. There was stenosis of the mitral orifice, and shortening, with thickening and adhesion of the chordæ tendineæ. The anterior portion of the valve formed a bony mass, occluding that portion of the orifice. On the auricular aspect, the surface of the valve was ulcerated, the bony matter laid bare, whilst soft, reddish vegetations existed on the free border of the valve and upon the ulcerated surface. Pulmonary and tricuspid valves were normal, and there was a little thickening at base of aorta.

The first case, in Dr. Leaming's opinion, goes to prove that the auricular systole may take place regularly, even when the auriculo-ventricular opening is very much contracted by stenosis of the mitral valve, without producing sound. The second case is a demonstration of the cause and mechanism of the first sound. There was no mitral murmur. With stenosis of the mitral valve, if the chordæ tendineæ had not been rendered incapable of sound vibrations, by being plastered over with fibrinous deposit, there would have been a murmur, such as is usually heard in stenosis where the chordæ are free and uncovered. The first sound, and all murmurs connected with it, disappearing when the mitral valve and chordæ tendineæ are rendered incapable of sound vibrations, is as convincing proof of their cause, as is the experiment of hooking up a curtain of the aortic valve proof as to the cause of the second sound.

The second and third cases are confirmatory proof that the cause and mechanism of the first sound and the murmurs connected with it, depend upon the state and condition of the mitral valve and its chordæ tendineæ.

Dr. Leaming refers, in support of his opinions, to the experiments of Prof. Halford, published in this Review, in 1860. He rejects muscular contraction as a cause of sound, and attributes the first sound and the murmurs associated with it to the result

of chord and valve vibrations set in motion by the rushing blood. He further contends that the presystolic or auriculo-systolic murmur and the mitral direct murmur, are caused by vibrations of the chordæ tendineæ subjected to irregular tension, and not by the forcing of blood through the contracted opening of the valve. For he holds that the auricle is too feeble to drive blood through the narrowed aperture so as to cause sound capable of being heard through the chest wall, even in the empty state, and much less so when the ventricle is filled with blood. Lastly, he maintains that the murmur does not agree in length with the time of contraction of the auricle.

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