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CLINICAL LECTURES ON SURGERY,

DELIVERED AT THE

HOTEL DIEU, PARIS,

BY

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[R^evised, before translation, by the Baron himself.]

FRACTURE OF THE HUMERUS

AT THE LOWER EXTREMITY, SIMULATING
LUXATION OF THE ELBOW BACKWARDS.

THE diagnosis between fractures and luxations cannot be too carefully discriminated, meeting as we do, every instant, in our hospital practice, numerous cases which have eluded the sagacity and observation of the great masters of our art. It is thus that affections of the coxo-femoral articulation, scapulo-humeral luxations, fractures of the lower extremity of the humerus, those of the lower extremity of the radius, and, in general, all solutions of continuity in the vicinity of joints, become in practice the sources of numerous errors. Many of these subjects have been treated of by us in preceding lectures. To-day we proceed to discuss those fractures of the lower extremity of the humerus simulating dislocations of the fore-arm backwards.

Nothing is so common as to see a fracture of the lower extremity of the humerus, immediately above the cubito-humeral articulation, mistaken for a luxation backwards of this luxation. It is important, however, to distinguish these accidents, since defective treatment may induce a perfectly incurable deformity. Let us suppose the fracture to be transversal, and immediately above the condyles; the olecranon is drawn backwards and upwards by the triceps brachialis muscle: the upper fragment is borne forwards, and resembles the lower articular surface of the humerus. The projection formed by the olecranon is so marked, that, in comparing the two joints, the apophysis of the injured side exceeds that of the other,

by from twelve to eighteen lines. Lastly, the antero-posterior diameter of the arm, near the elbow, is sensibly augmented. Thus you have, here, apparently, all the symptoms of luxation. If this opinion be formed, efforts of extension and counter-extension are practised, and the reduction ordinarily presents little difficulty. A bandage is applied, and the surgeon plumes himself on the facility with which he has replaced the bones. But they soon become deranged again, and in five or six days something unusual is noticed, through the swelling of the parts. The accident is generally attributed to the patient, who, accordingly, is accused of being a very refractory person. The reduction is again accomplished and the deformity recurs as before, and still more considerable swelling now takes place. The surgeon, however, rests secure while this tumefaction lasts, but when it has disappeared, in a month, six weeks, or two months, why he then recognises his blunder; but the patient is maimed, the thing is incurable, and the movements of the limb are materially diminished, and singularly embarrassed. In general, indeed, if twelve or fifteen days elapse after the fracture, and if the nature of the accident be then mistaken, nothing more can be done for the patient. The swelling of the surrounding parts opposes an almost insurmountable obstacle to a complete reduction, and the deformity is consequently beyond remedy.

CASE 1.—*Fracture of the Extremity of the Humerus, mistaken for a Luxation.—Deformed Callus.—Impeded Movements of the Humero-Cubital Articulation.*

Late in December, 1832, a young child was brought to the consultation, who had fallen from an ass about a month before. Two medical men, called in successively, pronounced the accident *luxation*, and treated it accordingly. It seems that the second of these gentlemen was called in to a *relapse*. When examined at the Hôtel Dieu, there was a tumour anteriorly, which presented inequalities, and which was evidently the lower extremity of the humerus. The olecranon was eminent behind. Very probably, from the youth of the child, there

was only a disjunction of the epiphysis. However, the two fragments were defectively united by a mishapen callus. What was to be done? The rupture of the callus appeared to be dangerous, but as it was found that the principal inconvenience resulted from the impossibility of extending the fore-arm, a machine was applied, devised to accomplish the extension by degrees. The plan partly succeeded, but there will always remain considerable deformity and incapacity of motion.

The chief sign distinguishing fracture from luxation is crepitation. If the surgeon, then, is called in a short time after the accident, he ought (seizing the arm in one hand and the fore-arm in the other) to move the surfaces on each other, upwards and downwards, or backwards and forwards. He almost invariably then perceives the characteristic sound of fractures. Add to this, that moderate efforts of extension and counter-extension ordinarily bring back the parts to their natural position. Nevertheless, it is true, that the luxation of the elbow itself is one of those reduced with the least difficulty or exertion. But this valuable symptom in cases of fracture, I mean the crepitation, becomes either very obscure, or ceases to be perceived at all if swelling have supervened. Then, it is true, the reduction of the displacement is always more easy than in luxation, and the mobility of the parts is greater. But who would venture to pronounce on such indications? We have still, however, a capital resource left, a pathognomonic symptom, in fact, which can prove a substitute for crepitation. Seize a fragment in each hand, the thumb applied anteriorly, and directed towards the fracture, and thus try the reduction. This simple effort suffices in the majority of cases, especially in twenty-four or thirty-six hours after the fracture. When the reduction is thus completed, move the fore-arm backwards. If it be a luxation, the reduction remains unaltered. If it be fracture, the displacement instantly recurs.

M. Malgaigne, who has published, in the *Gazette Medicale* observations on this species of fracture, thinks that other modes of diagnosis may be also employed. In the luxation, he states that the articulation is destroyed, and the movements of flexion and extension are impossible. In fracture the luxation is intact, and these movements are probably retained to a certain extent. Still this circumstance can only be of use in the first period of the fracture. But at whatever epoch you please, there is an anatomical character, which seems to me infallible whenever it is recognised, namely, that however great be the posterior eminence of the olecranon, it is never further distant, in

fracture, from the humeral tuberosities, than in the natural state, whereas in luxation it is considerably more so. In the latter case also, the anterior projection is more rounded, and not so broad. In the former case the projection is of the size of the articulation itself. We see no chance of error here; the diagnosis is as certain as anatomy itself. The cases would then remain where the swelling of the soft parts would mask the natural prominences of the bones, a thing possible, it is true, but scarcely to be conceived. But then the problem would not be to determine the nature of the displacement; indeed, it is probable that the displacement could not then be recognised.

CASE 2.—*Fracture of the lower extremity of the Humerus, simulating a Dislocation backwards; Cure without Deformity.*

D—, æt. 27, of high stature and strong constitution, fell into a pit on his left elbow, and was immediately after received into the *salle Saint Come*, to be treated (as the surgeon he first consulted would have it) for a "luxation" of the elbow. On admission, the left cubito-humeral articulation was out of shape. An enormous swelling occupied the lower extremity of the arm, which was tense and painful. The forearm was in a state of demiflexion. Despite the tension of the parts, a hard, slightly unequal, rugose projection was perceptible to the touch in the bend of the elbow, and lifting up the brachialis anticus and biceps muscles. Behind, the olecranon projected beneath the skin, and was slightly elevated above the level of the condyles. Flexion and extension were impossible, and the attempt to produce them occasioned intense pain. Doubtless, so far, we had every symptom of luxation backwards, so easy is it, without great experience in fractures, to be misled by the symptoms thus described. However, the movement communicated, as already mentioned, to the inferior extremity of the arm and superior extremity of the forearm, made perceptible an unusual mobility of the parts, and distinct crepitation. No doubt could thenceforth exist as to the nature of the lesion, which consisted of a fracture of the lower extremity of the humerus, some fingers' breadth above the condyles.

All the symptoms which could thus simulate the luxation of the elbow, are readily explained by the seat of the fracture; the continuity of the humerus being interrupted, its lower articular extremity no longer presented a solid fulcrum. The olecranon process obeys the contractions of the triceps muscle, which tends necessarily to move it upwards. In consequence of this ascension (which at the same time explains the projection of the olecranon beneath the skin, and the constrained demiflexion of

the forearm), the inferior fragment of the humerus, intimately connected to the cubit, participates in the impulse communicated to this bone. By their reciprocal direction, it necessarily experiences a see-saw motion, by means of which its upper extremity is carried forward, and projects beneath the brachialis anticus and biceps muscles.

The day of the accident, the perfect reduction could not be obtained, on account of the pain produced by every attempt at its performance, and especially on account of the swelling of the parts. The treatment was confined, then, to placing the limb in a state of demiflexion on a horizontal plane formed by pillows, after having covered it with compresses steeped in solution of Goulard. A copious venesection was made from the other arm; his diet was restricted, and he was ordered calming and diluent drinks. Next day the reduction was completed thus:—The shoulder of the injured side was depressed, and while an assistant performed traction on the demiflexed forearm (so that the limb was converted into a lever of the third order, the fulcrum being the patient's hand, clasped in that of the assistant, the power in the bend of the arm, where the other hand was applied, and the resistance on the inferior fragment), I seized with both hands the lower extremity of the humerus, on a level with the fracture, pushing the olecranon strongly forwards, and the lower fragment backwards at the same time. The reduction being performed, the arm, maintained on the couch of pillows in a position intermediate between flexion and extension, was placed on the ordinary apparatus of Scultetus. Some graduated compresses were applied around the lower extremity of the arm, so as to correspond with both the bony prominences, and to replace the fingers of the surgeon, which had caused these prominences to disappear. They were also repressed by two long compresses, and, successively, by the other parts of the apparatus previously moistened with cold water, rendered more sedative by the addition of a certain portion of acetate of lead. Lastly, the two lateral cushions were folded on themselves at their lower extremity, so that in closing the apparatus, the straps could be especially directed on the graduated compresses, and urge them continually against the olecranon and upper extremity of the inferior fragment of the humerus.

On the second day the patient's state was very satisfactory: the apparatus, relaxed by the decrease of the swelling, was proportionately tightened. On the sixth he complained of pain, and the apparatus being undone, a slight displacement of the fragments was detected. Some efforts of extension sufficed to effect their complete reduction, and the bandages, &c. were immedi-

ately reapplied. On the following days no pain was felt. Nevertheless the apparatus was occasionally loosened to examine the parts. The thirty-third day it was definitely removed, the consolidation of the parts being complete, and devoid of the slightest deformity. The forty-fifth day he quitted the hospital, able to accomplish considerable movements of flexion and extension. In a few weeks after, one of the pupils again saw this man, and found that no visible trace of the fracture remained, and its only sequel was some little embarrassment in the movements of the articulation.

CASE 3.—*Oblique Fracture of the Left Humerus, near the Elbow, with external Injury: Reduced; and cured in 54 days.*

In another case, that of a female, æt. 25, the accident in question was determined by a fall backwards on the ground. There was a wound over the olecranon, and the symptoms were such as to simulate, closely, a luxation. The lower fragment ascended very high upwards and backwards, thus resembling the superior extremity of the bones of the forearm, while the upper fragment of the humerus descended very low, before the inferior one, forming there a projection which represented the lower extremity of the os humeri in the case of a luxation of the elbow backwards. The limb was, moreover, shortened, and every kind of movement impossible. But the mobility of the fragments, their crepitation, and especially the integrity of the articulation of the elbow, soon explained the nature of the lesion. It is unnecessary to pursue the details of the treatment, so completely do they resemble those of the case above described. In forty days the fracture was completely consolidated, and no apparent deformity remained. The fifty-fourth day the patient left the hospital perfectly cured in configuration, and commencing to resume the motions of the limb.

It is said that the celebrated Cooper has himself remarked, that this fracture was much more frequent in children than in persons of more advanced age. Still, the examples we have related, and many others I might mention, are sufficient to prove that maturer age is by no means exempt from the occurrence.

These principles being laid down, let us look for a moment to the duty of the surgeon. In fracture it is readily replaced, but the slightest movement of the patient reproduces the displacement. This circumstance at once denotes the nature of the accident. At any rate if we err in our diagnosis when we decide the case to be fracture, we err on the safe side. The patient it is true remains in apparatus for a considerably greater time, but this inconveni-

ence is nothing when contrasted with the results of a contrary mistake. The diagnosis established, what apparatus can we apply? I shall describe it now in sufficient detail. The extension, counter-extension, and coaptation, being suitably performed, and the reduction obtained, the limb is placed on a plane of pillows preliminarily covered with the ordinary bandage of Scultetus. The position to give to the arm is that intermediate between flexion and extension. Graduated compresses, about three fingers' breadth wide, and three or four inches long, a little thicker towards the fragment, are then placed on the anterior and posterior faces of the humerus. These compresses are brought to bear circularly on the fragments, and maintained by two longitudinal ones. The bandage, in separate pieces, is then applied; then a cushion is placed on, one of the extremities of which is folded up, so as to be doubled on the point which should press on the lower end of the humerus. The olecranon is to be similarly pressed on. In this manner the humerus is forced backwards, and the olecranon forwards. A short strap is then placed on each cushion, and the clasps are then tightened pretty closely, in order to aid the action of the cushions. Twelve or fifteen days after the application of this apparatus, the fragments are so placed that they cannot be deranged again. The tumefaction of the surrounding parts is another obstacle to this occurrence; thus, the swelling, which in the case of fracture is taken for a luxation, in the course of a few days prevents the reduction, and becomes, when we do not make any mistake, a very potent auxiliary in effecting a cure.

EXOSTOSIS OF THE GREAT-TOE,

At the upper surface of the last Phalanx.

In addressing you, some lectures since, on the incarnation of the nail of the great-toe, I designedly omitted to speak about the exostosis of the upper surface of the last phalanx, waiting for a favourable illustration of the subject. This has occurred within the last few days, and it has convinced me, at the same time, that our ideas on this subject are not generally known. A distinguished surgeon of the capital, who formerly served as a surgeon in the French armies, came to consult me for his child, who he believed to be affected with an incarnate nail. I examined the little patient carefully, and soon ascertained that the supposed incarnate nail was an exostosis of the upper surface of the phalanx, and that the matrix of the nail was not at all altered. You remember the young woman who came to the visit about three months since with a swelling on the upper surface of this

joint. At first sight we might have thought that the nail was altered. An incision of the nail at each side, however, soon discovered the nature of the evil. I removed the exostosis at once, and the patient in some time was completely cured. The three following cases will give us more precise notions on the subject.

CASE 1.—*Exostosis at the Extremity of the Great-Toe.*

Louise Emery, a mantua-maker, æt. 22, of good constitution, and regular courses, of healthy parents, and free from any venereal affection, came to the Hôtel Dieu on the 28th of December, 1831. During two years this young woman had, on the last phalanx of the great-toe, and near its outer edge, a very hard osseous tumour, indolent, except on hard pressure. Its broad base passed beyond the nail, which was consequently wasted and corroded. She assigned no cause for her disease. It commenced two years previously, by pain in the toe, increased by walking and pressure. The tumour gradually attained the volume described. The patient consented to have, and had, its extirpation performed.

CASE 2.—*Exostosis of the last Phalanx of the Great-Toe.*

Catherine Loury, ætat. 20, had, for eighteen months, been affected with a swelling of the external and lower part of the great-toe of the left foot, consisting of a hard, bony, tumour, of the size of a nut, and the progress of which had been very slow. The patient could assign no cause for the production of the tumour. It appeared to spring from the first phalanx of the toe, the nail of which it slightly elevated. It was not painful in itself, but it was a considerable impediment to walking. On the 8th of January this young woman came to the consultation, and consented to the extirpation of the tumour, which was effected in the following manner:—The patient was laid on a bed, and her foot held firmly by an assistant; the tumour was included within two demi-ovoid incisions, and almost removed by the first cut. Some portions were then consecutively separated. On examination, the substance was found to consist of bone, with a compact external layer, and a spongy interior. On the 12th of January the wound was in a fair way for cure.

CASE 3.—*Exostosis beneath the Nail of the Great-toe; Aggravated by Cauterisation.—Extirpation.*

A young woman, æt. 25, had a tumour for two years beneath the great-toe nail. Very small at first, it gradually increased, elevated, and deformed the nail, and rendered walking very painful. The patient

then consulted a very skilful smith, who believed the swelling to be a wart, and cauterized it accordingly. Contrary to what he expected, the disease only increased, the nail curved up, and its anterior extremity turned backwards, nearly touched its root. It was wrinkled, unequal, and of a deep-yellow colour. On the 3rd of June the extirpation was proceeded with. A semilunar incision was made with a bistoury at each side of the tumour—the osseous tumour beneath the nail was thus completely laid bare, and then separated with the same bistoury. The tumour was harder than was at first supposed, and its section attended with some difficulty. Nevertheless it was entirely removed, and the simple operation was performed with the utmost celerity.

This disease has not been described by authors as far as I am aware. It consists of a pyramidal exostosis, some of the effects of which are seen in the preceding cases, and often occasions mistakes which lead to painful and useless operations. In its commencement it is but little dangerous, though very inconvenient; it is not painful, but becomes so as the nail is lifted up by the subjacent tumour. But a shock of the foot, for example of the toe against the pavement, renders the pain intensely distressing. The causes of the affection are unknown in the majority of cases. It occurs in individuals who have received no injury or hurt of the toe, and who have not worn too tight shoes or boots. Nevertheless, it is sometimes seen in persons who have suffered some violence in the part affected. The venereal virus or scrofulous diathesis does not seem to determine its peculiarity. Generally the patients mistake the tumour for a wart, and this error has often been shared by the practitioner, who, influenced by this idea, resorts to cauterization, which in this case always produces bad effects. In other cases the affection is supposed to be one of the nail, and the nail has accordingly been often extirpated.

On dissecting the tumour, when fully developed, we find it formed by the skin, by fibrous tissue, and by a pyramidal bony excrescence springing from the upper surface of the terminal phalanx. This exostosis is composed of spongy tissue covered by a layer of compact tissue of variable thickness. Sometimes its texture is not very hard, and can be cut through with a bistoury; sometimes, however, it is very hard, and requires the gouge and mallet for its extirpation. If allowed to increase continually, ulcerations more or less malignant may occur, and render walking still more difficult and painful. I have once, I remember, seen a surgeon remove the last phalanx of

the great-toe, for an ulcer determined by a tumour of this kind. The only mode of ridding the patient of the inconveniences to which this affection gives origin, is by the complete extirpation of the excrescence. The removal of the nail is sometimes necessary. In the vast number of cases it is however quite useless. The mode of operation has already been sufficiently described in the cases brought forward. It is only necessary to add, that we must not be satisfied with the removal of the summit of the tumour. It must be completely extirpated; if not, it will grow again. I have had occasion, I believe, to remove thirty of these tumours, and I have, by the operation mentioned, invariably been successful in the cure of the patients.

LECTURES

ON

MEDICAL PATHOLOGY,

DELIVERED IN THE UNIVERSITY OF PARIS,

By M. ANDRAL, D.M.P., &c. &c.

LECTURE XVI.

PERVERSIONS OF SENSIBILITY. HYDROPHOBIA.

WE have now discussed the various lesions of sensibility, in which it was either exalted or diminished. Another species remains, namely, that in which the sensibility is neither exalted nor diminished, but simply altered or perverted. Under this head might be appropriately included the history of various hallucinations, but this subject has already been sufficiently discussed under the heads of mental alienation, ecstasy, &c. I merely allude to it now, in order to mark this as a suitable place for its introduction.

The examples of perverted sensibility are extremely numerous. Singular cases of a depraved sense are recorded, as, for instance, in persons who have experienced a strange desire to feed on human excrements. The case is related of a woman who felt a singular pleasure in smelling the same substances, as well as assafetida, and a variety of other perfumes, equally agreeable. All these things are important, inasmuch, at least, as they afford an additional proof that perversion of the actions of organs is to be taken into account in our nosological reasonings, as well as their exalted or enfeebled conditions. It would not be difficult to give much greater development to

this branch of our subject, did time permit it, or did any particular circumstance render it necessary. In this lecture, however, I will confine myself to the particular notice of one of the perversions of a natural instinct, which may, at the same time, illustrate the classification, and be in itself of practical importance. Let us take then the natural instinct that teaches us the necessity for drinks, and which, under the influence of certain circumstances, may be perverted to complete horror of fluids, reaching to such a degree, that their deglutition becomes almost impossible. This state has been denominated *hydrophobia*—it is essentially characterised by the perverseness of the natural instinct for fluids.

The Two Varieties of the Disease.

This hydrophobia may be *spontaneous*, and it may consist solely and exclusively of one symptom, the horror of swallowing fluids—a simple nervous perversion of no serious character. At other times it is *accidental*, originating in a perturbed state of the functions of the nervous system, thus constituting a symptom of some other disease, as is seen in hysteria, in many fevers, especially in those of the ataxic kind, in some of which diseases it may, even, be the predominant symptom. Inflammations of the œsophagus or pharynx may, again, be accompanied with a true hydrophobia. In all these cases, however, the symptom itself is but of little consequence, disappearing when its source of origin is removed. To be sure it may persist so long, and to such an intensity, as to exercise a prejudicial influence over the economy, but these rare cases do not require more particular notice here.

The second variety of hydrophobia is, however, in point of serious importance, of a very different character. We now meet it as the constant attribute of a dire contagious disease, the *canine rage*, which, generally considered, itself includes two kinds, the spontaneous and the symptomatic; the latter of which is produced only by the operation of a subtle contagion, whatever be the nature of the virus by which it is propagated. The existence of such a virus has, you know, been denied by some individuals; but this is quite certain, that under particular influences, a poison is generated in particular animals, which, inoculated on the bodies of others, produces the same disease. This is the rigorous expression of facts, and is ascertained as the phenomenon of the transmission of the variolous infection, or virus, or whatever other name you may please to give it.

The disease, then, which the rabid animal produces by the inoculation of this poison, is never spontaneous in man, or in

any other variety of animal but the *canine* and *feline* species. In all others it is the result of contagion. It is remarkable, too, that the intensity of the virus decreases as it passes from one body into another. Thus, the chances of infection by the bite of an animal, in whom the disease originated in inoculation, are less than in the case of a wound by an animal in whom the disease was of spontaneous origin. It was for a long time thought that hydrophobia was merely the effect of imagination, or of fear, but when we consider the number of animals who are too ignorant of the nature of what has happened to them, either to fear or exaggerate its consequences; and when we recollect the numbers of children who have been thus infected, this obsolete opinion becomes too absurd to require formal refutation. With respect to the poison itself, it seems to be sufficiently proved that it is formed by an altered secretion of the glands and follicles of the pharynx, salivary apparatus, and throat in general. This seems to be a special vehicle of the poison. At any rate inoculation with the blood of rabid animals, or its injection into the veins, has not produced the disease in others, and the same remark applies to the solids of the system; such as the cellular substance, muscles, &c. There are, to be sure, some ambiguous cases told, which seem to indicate transmission of the disease in other ways; as, for example, the case of an individual in whom the disease was developed, after pricking his hand while handling the hide of a rabid animal. The case is, however, too badly told to prove anything. At any rate I remember wounding myself once with a spicula of bone during an hydrophobia dissection at La Pitié, and I suffered no inconvenience from the accident.

A question has arisen, whether it be necessary or not that the epidermis should be penetrated, in order that the poison should be effectual. There are many facts which appear to prove the negative, but I am inclined to believe that it is necessary that the dermis itself should be in contact with the virus. It is still less proved, that the mucous membranes receive the poison without abrasion. It is readily intelligible how error may creep into observations of this kind. Perhaps we thus may account for the case related by Chaussier of a man who contracted hydrophobia after having applied to his nose a handkerchief on which a quantity of the foam of a rabid dog had been collected, and another case in which the disease followed, after a quantity of foam had fallen on the back of the hand. With respect to transmission of the disease by the dead body, there is no well-authenticated or conclusive case upon record. The next question is, When this poison is inocu-

lated, what becomes of it? Does it remain in the wound, or, on the contrary, is it received at once into the circulation, and diffused over the body? Formerly, the authors on this subject generally considered that the virus remained in the part until the disease was developed. More recently, it was asserted by Marochetti, that after three days it entered the circulation, and soon after concentrated itself in the form of pustules beneath the tongue; hence he derived the inference, that the tongue should be assiduously watched, and these pustules opened and cauterized, to prevent the reabsorption of the virus. All I can say about these statements is, that the pustules have been carefully sought for in France, in numerous cases, and have never yet been found.

The length of the interval between the occurrence of the bite and the development of the disease is very uncertain. From fifteen to twenty days is common; from thirty to forty still more so. But the period has in many cases been much longer; several months, two years, for example.* The latter case is very extraordinary, and *à priori*, would seem a thing to be rejected. But the fact is incontestable, and gives us another proof that we must not always reject at once whatever seems to be impossible in its nature. The fact, moreover, is not without its analogies in the history of other diseases.

The Symptoms of Hydrophobia.

The symptoms of the disease induced by this poison, next claim our attention. Generally some premonitory signs may be noticed. After the occurrence of some violent emotion, a fright or passion, after some unusual excess or exposure to cold, or insolation, or some similar agent apt to influence the nervous system, the individual commences to experience the special phenomena of this horrid malady. The wound, however long cicatrized, generally, but not invariably, opens afresh. I say not invariably, for many cases are on record in which this circumstance did not occur. Mellier, for instance, relates some in the *Archives Générales de Médecine*; but whether the cicatrix opens or not, it invariably becomes painful before the eruption of the disease in its decided form. Meanwhile, the patient feels a singular degree of uneasiness, of an indescribable kind; horripilations occur in various parts of the body; pain is felt in the back or limbs; he seeks in vain to sleep;

the sleeplessness is either complete or interrupted by frightful dreams; he complains often of a great and distressing weight in the head; the digestion is impeded, anorexia occurs. In all these symptoms, however, (excepting those relating to the cicatrix), there is nothing specific, nothing to distinguish the canine madness which is about to supervene, from a host of other disorders. These prodromes, such as they are, may last from one to four or five days.

These premonitory symptoms having run their course, a sudden and rude access of the true malady takes place. A paroxysm of suffocation is experienced, as frightful to the spectator as tremendous to the patient,—as if a noose were thrown round his neck and tightened violently, to strangulation. For some time the air ceases to be inspired, deglutition becomes impossible, intolerable constriction is experienced in the larynx and pharynx. All these phenomena are of simultaneous occurrence. Sometimes convulsions coincide with these terrible symptoms. When this happens they are rarely general, but are usually restricted to the face. Frequently there are singular shudderings of considerable violence, and affecting the entire body. If in this state we present a drink, all the symptoms are augmented, and it is repulsed with horror. If we force the fluid to the mouth, suffocation is imminent, nay, even death may immediately occur; the patient may perish as if mechanically strangled. An increased secretion of the salivary glands now commences; the foam issues in vast quantities from the mouth, trickling down from its corners, or puffed out to a considerable distance. The face is now coloured, the eyes are injected, the tongue is parched and dry, and, to lighten to the last degree the sufferings of the tormented patient, his thirst becomes intense, in proportion to the impossibility of allaying it.

Such are the constant characteristic phenomena of this disease. They are essential, specific, and invariably present. But in addition to them, are others generally present, but not invariably, and the absence of which still leaves the disease perfect. These relate to affections of the intellect and of the external senses, to some conditions of the muscular system and genital organs. First, let us see what are the phenomena which the mind presents during this fearful struggle of the body.

Three different conditions of the intellect may here be noted. In one set of cases its functions are completely preserved, from the commencement to the last moment. The patient may even know his state, be aware that death is impending; yet he acts as if

* A well-authenticated case, which occurred at Rotherhithe, in which eighteen months elapsed between the receipt of the bite and the out-break of the disease, is recorded in No. 330 of THE LANCET, page 438.

ordinary maladies, and exhibits the dispositions common to rational persons in their dying illness. In a second group of cases, the intellect is entire in the interval between the paroxysms, but in the paroxysm itself, it is for the time abolished, and delirium supervenes. In this state the patient not unfrequently foresees the access approaching, and cautions the by-standers to protect themselves from his delirious attempts. In the third set of cases, the intellect is troubled from the commencement, the character of the derangement assuming various forms; it may be restricted to mere delusions, or amount to complete fury. The manifestations of this fury vary in different animals, corresponding to their previous instincts or habits of destruction. The dog, for example, attempts to bite, while that impulse is but an accident in man. The herbivorous animals use their horns in their rabid attempts to do mischief. With respect to the state of the organs of the senses, there is nothing remarkable in a great number of cases. In some, however, they have been noticed to acquire a remarkable degree of exaltation, as in the case related by Magendie, of a patient who was habitually and completely deaf before he contracted the disease, and regained his hearing during the paroxysms. The genital organs sometimes present some remarkable phenomena. In females, nymphomania, to a violent degree, may be noticed; in males, seminal evacuations, and intense desire for coition, during the paroxysms.

When once developed, the disease is composed of an uncertain number of paroxysms, such as I have already described, but generally increasing in intensity to the last, and of corresponding intervals of greater or lesser duration. In the interval the patient is calm, fatigued beyond description, often in a state of great prostration, frequently able to drink, but, what is remarkable, can never sleep. In proportion as the accessions increase in frequency, the pulse becomes small, and the face singularly expressive of one of the dreadful states of the patient. Usually death takes place in one of the paroxysms, with all the phenomena of true mechanical suffocation; but what is particularly strange is, that patients often die suddenly in the interval, or during the access, without any prominent symptom of dyspnoea, or altered circulation, or cerebral commotion, which could explain the mode of death. The period within which death occurs after the commencement of the symptoms is usually from forty to sixty hours. In some rare cases the disease proves fatal in eighteen or twenty hours, and in still rarer instances, it persists for six or seven days.

Morbid Appearances.

On opening the bodies of those who die of this terrible affection, we find nothing constant or complete, nothing which elucidates in the least degree the phenomena which the malady exhibited. In the nervous system, in the generality of cases, there is no tangible organic alteration; and when alteration is observed, it seems to be rather accidental than of any specific importance, and is never sufficient to explain the cause of death. Dupuy, of Alfort, found cerebral ramollissement, hemorrhage, into the substance of the brain, injection of the meninges, effusion into the theca of the spinal chord, &c. in some dogs he examined. Similar appearances have also been met with in a few cases in the human subject, but in all these there is nothing special. The same appearances occur in a thousand other diseases, and even in hydrophobia they are only contingent on a small fraction of the cases. Some remarkable statements have indeed been made with respect to the nervous chords. In the *Journal Universel* for 1829, there is a memoir on this subject, the author of which states, that in the case respecting which he writes, the pneumogastric nerve, and the fourth, fifth, sixth, and seventh pairs, were of an intensely red colour, so much so that they resembled muscular fibre. This fact was too remarkable not to excite universal attention and inquiry. Many individuals have consequently looked for similar appearances. Amongst others I have myself examined with the utmost care the bodies of hydrophobic patients. Directing my attention specially to this point, I am bound to say I have never found anything of the kind.

The digestive apparatus may also exhibit changes, but like those of the nervous system, they are neither constant nor specific, and consequently cannot be taken into account when we attempt to explain the nature of the malady. The fortuitous appearances, then, are, occasional swelling of the salivary glands, thickening or abrasion of the mucous membrane of the pharynx and œsophagus, redness and injection of the stomach, remarkable development of the mucous follicles of the intestinal canal. It is almost needless to remark, however, that these appearances may exist in an infinity of other diseases, in small-pox, scarlatina, cholera, typhus fever, dothinentherite, &c. They are, consequently, not special to hydrophobia; they are merely the effects of a general cause, and are certainly not the cause itself.

The respiratory organs, larynx, lungs, &c., are in the majority of cases free from alteration, at least of any constant kind. The blood is equally normal in its condition,

The body has been said, I may add, to undergo putrefaction, with extreme facility and rapidity. The assertion, however, is not exact, and is contradicted by the observations of Mellier, and by the cases I have myself witnessed.

Treatment of Hydrophobia.

The treatment of hydrophobia is either premonitory, or during the presence of the disease. The former may be considered under the heads of local and general.

The local treatment refers to the wound itself. It consists of the application of various modes of cauterization; by fire, by various corrosive and escharotic fluids and solid substances, &c., varying according to the depth and character of the wound, which should be well washed with water before the application of the caustic. Some practitioners are in the habit of making the wound bleed a great deal before cauterization, and with this view, as well as for the purpose of preventing the absorption of the virus, they apply cupping-glasses to the bitten parts, a plan certainly calculated to be of much advantage. This mode of suction, washing, and cauterization, is not only proper immediately after the accident has occurred, but at all times before the disease is developed, and even after its first symptoms have appeared. The wound, though cicatrized, should be opened by caustics, the glasses applied, and free and copious suppuration established. The excision of the bitten part is, however, preferable to all this, whenever it can be conveniently practised.

A vast number of prophylactic remedies have, as you are aware, been successively vaunted in this affection. I shall only allude to a few of the most remarkable, none of them having been ever proved to have produced the happy effect of preventing the occurrence of the disease. In the vegetable kingdom two plants were especially invested with this virtue, namely, the *Alisma Plantago* and the *Guaco*. Their influence, to say the least, is, however, extremely doubtful. Still we should not thus be discouraged in our search for a specific for this singular and terrible disorder. Because a specific has not yet been found, we must not thence conclude that none can be discovered. There is more than one remedy in medicine, the mode of action of which sets *a-priori* reasoning at defiance. Who, for example, could have suspected that the vaccine matter would prove a neutralizer of the virus of small-pox? and who is there who can satisfactorily and unequivocally explain the mode of action of this counter-poison?

In the developed disease all sorts of specifics have been tried without success, and similar ill fortune has attended the practice of more ordinary methods of treatment.

Bleeding, carried to an enormous extent, has been tried in vain. Injection of tepid water into the veins was proposed and practised by M. Magendie without success, but with the singular effect of arresting the access, diminishing its violence when it recurred, and alleviating remarkably the sufferings of the patient. The disease was not the less fatal, notwithstanding this transitory calm. It is altogether useless for me to occupy your time with an enumeration of all the remedies that have been tried in vain by many distinguished practitioners. Opium, for example, has been given in immense doses by M. Dupuytren; tartar-emetical by others, &c. &c. The same melancholy event has, however, always taken place, no matter what was the treatment adopted. I shall therefore quit this fruitless topic, and proceed with my classification of the *perversions of sensibility*.

ON THE

ETYMOLOGY OF "CHOLERA."

It may appear a work of supererogation to direct attention to the derivation of the word "cholera;" but if its extraction as generally received, and as given by medical writers, is shown to be probably incorrect, and if the inquiry tends to throw any light on the question as to whether this disease is of modern origin, or has at former periods ravaged the nations of the earth, the research may be divested somewhat of its scholastic character, and the inquiry may not be entirely devoid of interest. The perusal of an able academical discourse on this subject, lately read before the Academy of Sciences at Caen,* and of an article to be found in the *Gazette de Normandie*, le 6 Juillet, 1832, has excited my attention to it.

Those writers whom we are accustomed to regard as authority in these matters, differ materially in their derivations of the word cholera. Celsus makes it a compound of *χολή βίω bile flux*, whilst Traillian traces it to *χολὰς βίω intestinal flux*; and a third[†] ingeniously suggests that as *χολέρα* also signifies a *water spout*, the word may have been applied figuratively to mark the force with which the evacuations are projected, whilst its adjunct "*morbus*," was added merely to distinguish between the two applications of the word. It does not appear that either of these derivations can be traced

* "Recherches Etymologiques sur le cholera morbus." Par M. E. H. Smith, B. Art. de l'Université de Cambridge, le 31 Juillet, 1832.

† "Cyclopædia of Practical Medicine." Article "Cholera."

to Hippocrates, who mentions neither bile nor a flux as necessary to constitute the disease, but admits two varieties, the *χολέρα ξηρά* dry cholera; and the *χολέρα υγρή* humid cholera. The translation runs thus: "Humida cholera dicitur in qua magna contentione virium jactura, sursum deorsum excretio fit puritrium incocturumque—ab arida cholera venter inflatur, strepitus fiunt, dolor laterum et lumborum, nihilque alvus dejicit sed astringitur."

The intelligent author before mentioned renders it highly probable, that cholera is derived from the compound Hebrew word *choli-rá*, literally a "malignant disease," and this word is clearly shown to have been applied 3300 years since in eastern countries to a pestilential visitation afflicting mankind. Indeed Moses mentions the existence of such a disease amongst the Egyptians, and probably refers even to a more remote period than the above. Our author says that he traces *choli-rá* more particularly to the description of a disease accompanied by trembling or spasm, and affecting the intestines; and as if to mark the extreme suffering which attends it, the sacred writers constantly apply the word, figuratively, to the writhings of the soul, "pour les souffrances de l'ame."

The substantive "*choli*," signifies suffering or disease, or figuratively, it is applied to mental distress. It comes from "*chala*," to suffer, to fall sick, and this is derived from "*choul*," to have spasmodic pains, to tremble.

The adjective "*Rá*" is, literally translated, very bad, destructive, and, used substantively, it is rendered an evil, a calamity, a punishment inflicted by God. It is a derivative of "*rad*," to break, to bruise; the root of which is, "*rouh*," to be wicked, to do evil. "*Choli*" and "*rá*," with their ramifications, are often used variously conjoined.

Examples.

Deuteronom. ch. xviii, ver. 59. "Then the Lord will make thy plagues wonderful, &c. &c., and sore sickness (*plagas magnas vulgate*) (*cholaim raip*, plural of '*choli-rá*'), and of long continuance."

....., ch. vii, ver. 15. "And the Lord will take away from thee all sickness (*languorum*) (*choli*), and will put none of the evil (*raim*) diseases of Egypt upon thee."

2 Chron. ch. xxi, ver. 15. "And thou shalt have great sickness (*tu agrotabis pessimo languore*) (*cholaim*) by disease (*choli*) of thy bowels, until thy bowels fall out, by reason of the sickness (*choli*)."

1 Kings, ch. xvii, ver. 17. "The son of the woman, &c. fell sick, and his sickness (*languor fortissimus*) (*choli*) was so sore, that there was no breath left in him."

The plague which smote all the first born of the Egyptians is attributed by some sacred writers to evil angels, and is rendered by "*raim*;" but in *Psalm lxxviii*, ver. 51, the same is called a "pestilence."

Psalm xxix, ver. 3. The following passages are all translated from different derivatives of "*choul*," the root of *choli*. "The voice of the Lord *shaketh* the wilderness; yea, the Lord *shaketh* the wilderness of Cades." "The voice of the Lord maketh the birds to bring forth young;" (*Ou les fait trembler comme si elles allaient avoiter*).

Our author concludes by saying, "L'épithète '*Rá*' est appliquée à tout ce qui est mauvais au superlatif."

As the first syllable *cho* ought always to be pronounced as if spelt *ko*, it has been proposed that the spasmodic cholera should be named *koli-rah*, whilst the bilious epidemic, which is so distinct a disease, might retain its present name of "cholera."

I am, Sir, your obedient servant,

WESTON GOSS, Surg.

Dawlish, Devon, Feb. 25th, 1833.

ANATOMICAL AND PHYSIOLOGICAL CONSIDERATIONS ON THE CONNEXION OF THE PLACENTA WITH THE UTERUS.

On the Vascular Communications between these Two Organs, and on the mode of Circulation of their Fluids.

By M. E.-A. LAUTH (*Fils*), of Strasburgh.*

THE mode of union of the uterus and the placenta, and the reciprocal change of blood between the mother and fetus, have for a long time occupied the attention of anatomists and physiologists. Nevertheless, neither the one nor the other is capable of communicating clear ideas relative to the structure of these parts, or the manner in which they execute the functions that depend on them. At the time when the discovery of the circulation of the blood was made, the existence of an intermediate parenchyma between the uterus and the placenta (as everywhere else) at the ex-

* In compliance with the expressed wishes of a large number of correspondents, we here present our readers with a translation, from the *Repertoire d'Anatomic*, Vol. I, of the paper of Professor Lauth, lately quoted by Dr. Granville at one of the metropolitan medical societies. A sufficiently full abstract of the paper of Dr. Lee on the "Structure of the Human Placenta and its connexion with the Uterus," was published in No. 461 of THE LANCET.

terminities of the arteries and the commencement of the veins, was contended for. However, microscopic observation and injection having since then demonstrated the non-existence of a parenchyma in the rest of the body, physiologists judged by analogy that there was an immediate continuation of the uterine arteries in the veins of the placenta, and of the arteries of the placenta in the veins of the uterus. They were not long, however, without perceiving that it was impossible to inject the vessels of the fetus through those of the mother, and *vice versa*. The injection always was arrested in the arteries, between the placenta and the uterus, and formed effusions more or less considerable in the interstices of those two organs. If they employed much force in the injection, why certainly then it was returned by the veins, provided the matter of the injection was very limpid, and the operation was directed with care. These experiments, often repeated, and always with the same results, did but relieve physiologists from one error to cause them to fall into another. They do not concede more than they did in consequence of a direct communication between the vessels of the uterus and those of the placenta, but say that the arteries terminate by open mouths in the cells situated between the two organs, or in the uterine portion of the placenta, and that then the veins absorb the effused blood,—those of the placenta for the purpose of conducting it to the fetus, and those of the uterus to carry it back again to the mother.

In the experiments I have made on this subject, my first duty was to examine what we ought to understand by the fetal placenta and uterine placenta,—portions which, according to modern anatomists, are composed of entirely different vessels, one being the prolongations of those of the uterus, and the others being but the mouths of the vessels composing the umbilical chord. In vain did I make the most minute examinations; I could not discover these two portions, unless we wish to denominate the uterine placenta that portion of weak (*caduque*) membrane to which the placenta has become adherent, and which after it is more or less completely separated from the uterus, is expelled with the placenta, of which it forms the external covering. The formation of this weak membrane in the uterus, before the descent of the ovum into the viscus, and even independently of this descent, in the case of extra-uterine fetation, proves that it is a production of the womb, and that although the placenta comes to contract in connexion with its adhesions, it ought never to be considered as forming a body with it,—as constituting its uterine portion. There does not exist, then, a uterine placenta, such as has been described

and taught even to the present day. The weak membrane, like most false membranes about to be organized, receives a number of vessels which are the continuation of the uterine vessels, or which are, at least, in direct communication with them. As in all other parts of the body the final extremities of the arteries turn upon themselves to form the commencement of the veins.

The placenta is composed of the division, successively, of the umbilical arteries on the chorion,—arteries which having reached to the extremities of the villositities which cover it, fold upon themselves to give origin to the veins. The trunk that results from their reunion is the umbilical vein. I have often seen, without the aid of the microscope, the termination of arteries in veins, in human placentas, or in different species of animals,—placentas in which the blood-vessels had been previously injected. The artery turns upon itself abruptly to transform itself into a vein, in such a manner as to form a very small arch. I could, however, be easily assured of the termination of the arteries in the veins of the placenta by the injection of a thin liquid,—coloured water, for example; but I cannot make it easily pass from one order of vessels to the other. The bloodvessels of the placenta have no direct communication with those of the weak membrane. I could never inject the one by the other, however penetrating the matter of the injection I used. It is easy to repeat these experiments on the placentas, provided they have been preserved uninjured. Not an atom of the injected matter penetrates into the vessels of the weak membrane which covers it, and none escapes to the outside, unless the violence employed produced a rupture. This experiment proves, at the same time, that the passage of the blood from the mother to the fetus, cannot be made by means of that which is called the primitive absorbents of the veins, because those will be necessarily torn in a placenta detached from the uterus, and will give issue to the injected matter. This, we are about to see, is not the means that should be employed, but this passage exists by means of particular vessels furnished in their interior with valves which do not permit blood to enter excepting under certain circumstances, and under the influence of vital laws.

On examining with care a placenta still covered by this weak membrane, we see that these two parts are united by numerous small transparent vessels, which are directed from the former to the latter. These vessels cannot be injected, either through those of the placenta, or through those of the weak membrane, but a very fine tube introduced into one or the other of them,

fills sometimes the vessels of this membrane, and sometimes those of the placenta. From this it follows, first, that these vessels are of two orders, the one belonging to the weak membrane, and, consequently, to the uterus, and the other to the placenta; secondly, that they are not bloodvessels; thirdly, that they terminate,—the one in the bloodvessels of the weak membrane, and the other in those of the placenta, by orifices furnished with valves, which prevent them from being injected in a retrograde way. The vessels of which I speak must evidently be primitive lymphatics, of which they present all the characters, excepting that they do not appear to be allied to the general lymphatic system, because they are grafted upon temporary organs, with which they are expelled at delivery.

As to the cells that have been described as existing between the uterus and the placenta, or in the uterine portion of the latter where the arteries are about to terminate, and the veins to commence to absorb the blood which is effused, I have never been able to discover them, notwithstanding the care with which I directed my examinations for that purpose. To admit that they can give origin, is to allow of their existence. These are the effusions which the injected matter sometimes forms between the two organs. But these effusions depend on the separating of the placenta from the weak membrane, and on the rupture of the vessels which unite it to the uterus, and they are often extended to the point where the placenta does not adhere, excepting by its extremities. Perhaps they are also taken from cells of bloodvessels very much dilated, as we observe in all organs in which the vital properties are over-excited.

As it is now proved, that there does not exist a direct communication between the uterine vessels and those of the placenta, and as we see that the cells where the blood was to be effused do not at all exist, the only communication that we can admit of between the mother and the fetus, is that of the *lymphatic vessels*, as we have above described, the former of which terminate in the vessels of the placenta, and the others in those of the weak membrane, which themselves are but prolongations of the uterine vessels. These lymphatics, which terminate in the bloodvessels of one of these organs, appear to be grafted by their origins on those of the other, and in this manner those which arise from the uterine vessels, and which terminate in the vessels of the placenta, extract from the blood of the mother the susceptible materials which are to enter into the composition of the blood of the fetus—blood which is elaborated and accommodated to the wants of the latter, when it traverses the liver, where it seems

to be restored to its arterial placenta; and, on the other side, the primitive lymphatics grafted on the vessels of the placenta, terminate in the uterine vessels, and serve to secrete from the blood of the fetus the materials which could not be useful to it, or which might even become hurtful to it, for the purpose of returning them into the venous system of the mother. Some arguments still speak in favour of this opinion. The difference which exists between the movement of the blood in the mother and the fetus, appears to prove that their sanguineous systems are united by an order of bloodvessels very different from each other. The same conclusion ought to be drawn from the reciprocal independence which exists in the mother and the fetus with respect to their health. We see that an unhealthy mother will give birth to a healthy and strong infant; that the mother may be diseased with syphilis and small-pox, while the fetus is not affected by it; and that, on the other hand, the infant may be born with various diseases, the mother not being in the least affected by them. Without risking the reproach of having my opinions considered humoral, I think that we must admit, in all these cases, that the blood which passes from one individual to the other, undergoes essential modifications, and ought to be changed* by the absorbent vessels by a mechanism analogous to that in conformity with which a portion of the chymous mass, for example, is found changed into chyle, in passing through the chyloferous vessels. The analogy furnished by the examination of the incubated egg, proves that the novelty ought not to be its development with the successive addition of portions of blood already formed, but that this fluid is elaborated by the embryo itself, or by its dependencies, and that it secretes from the surrounding nutritive substance the materials necessary to the composition of its blood. Another argument still that I shall produce in favour of this opinion is, the experiment of the transfusion of the blood, which shows that the blood of one individual cannot be conveyed whole to another, as the serious accidents prove which have followed this operation. The losses which the body sustains cannot, then, be repaired by the simple addition of a certain quantity of blood, but it is necessary that this liquid should be increased by a fluid elaborated by vessels of the order of lymphatics—vessels which, before birth, are found in the placenta, and after birth are replaced by those of the intestines,—the chyloferous vessels.

* Some recent microscopic researches have shown that the globules of the blood in the mother, do not at all resemble those in the blood of the fetus.—G. B. in R. A.

We see then, in recapitulation, that the union of the placenta with the uterus is by means of vessels which are not bloodvessels, but which present all the characters of lymphatics;—that the function which now occupies our attention, is the true action of absorption;—that this absorption cannot be executed by means of veins, but that venous absorption, if it do exist, is only accomplished by transudation, and, in consequence, this function ought to be performed by the vessels which we have described, because it is only vessels of the order of lymphatics which are capable of modifying the blood of the mother, in a manner so as to accommodate it to the wants of the fetus;—lastly, that the placenta appears to fulfil in the fetus the functions which are performed at a later period by the intestinal canal, rather than those which have, even to the present day, been attributed to the lungs.

DESTRUCTION OF

STONE IN THE BLADDER,

BY BARON HEURTELOUP, WITH THE LITHOTRIPTIC INSTRUMENTS.

ONE of the last operations for calculus in the bladder completed by Baron Heurteloup, was that of Mr. Page, of Deal, in whom the stone was of a very large size.* When Mr. Page arrived in town for the operation, he placed himself under the care of James Powell, Esq., surgeon, of Great Coram Street, Russell Square, who, in a letter before us, addressed to Baron Heurteloup, expresses himself in the highest terms of the skill of the operator, and the advantages which resulted to the patient. Mr. Page stated that he had been suffering from symptoms of stone for nearly six years, and which, within the last twelve months especially, had been very distressing. His journey to London was attended with such severe pain, that he was obliged to stand upright on the coach during a considerable portion of the route. He was sounded by Baron Heurteloup on the 13th of November 1832, when a calculus of considerable size was found. "The first operation," says Mr. Powell, "was performed in my presence on the 16th of November, when you succeeded, with much facility, in grasping the calculus and breaking it into fragments. The patient, immediately after, passed, through a large catheter, some portions of stone, which proved to be composed of lithic acid. By the next day he had eva-

* We condense the previous narrative for the sake of space.—ED. L.

uated several considerable-sized fragments, with minuter portions. The steps of the operation were attended with no inconvenience to the patient, and upon this day he expressed himself easier than he had been for some time before, I presume from the division of the calculus having taken off a portion of its ponderosity. He continued to pass the fragments for several days, with much ease, and was again operated upon, with the same result, on the 21st of November, walking to your house before, and back again after, the operation. The subsequent operations were equally successful, the whole of the calculus having been broken down by the sixth time he came to you. As I had the opportunity of carefully sounding the bladder with yourself prior to his quitting town, I feel fully assured that the calculus has been entirely removed. I should think the quantity of fragments he was able to collect, amounted to very nearly one ounce and a half; and he says much passed at periods when he could not collect it."

Reflections by Baron Heurteloup.

The only remarkable feature of this case is the large size of the stone, which did not, however, interfere with the prompt recovery of the patient. The stone was entirely removed in six applications of the instruments, each lasting from three to four minutes, which makes a total of only twenty-four minutes. It is well known, that to extract a large stone by lithotomy, is often much more tedious, and especially when a stone is broken into pieces. Before I had introduced into the operation of *lithotripsy* the curved *percuteur*, and the *system of percussion*, I was far from being able to obtain so favourable a result, although I think I may say that I rendered much more rapid the comminution of a stone in the bladder by the invention of the "*evideur à forceps*," in cases of large spherical calculi, and of the "*brise-coque*" for the fragments.

I consider this case as a convincing proof of the progressive impulse given to lithotripsy, by the safe application of the *system of percussion* to the comminution of calculi.

The judicious remark of Mr. Powell on the relief experienced by the patient after the first operation, is of sufficient interest to deserve a moment's attention. Mr. Powell considers that the relief results from the ponderosity of the stone being partly removed by its division into pieces. It is quite evident, that the less weight there is on the neck of the bladder, the less suffering there will be arising from that cause. I think there are, however, one or two other causes which contributed to this relief,—a cessation of nearly the same sort of contact between the stone and the bladder, a contact which becomes painful by its mere

continuance. As this continuance and sameness of contact arise from the shape of the stone remaining the same, and its position in the bladder being but slightly varied, it follows, that when this shape is altered by the rupture of the stone, the painful sensations experienced by the patient from this cause, no longer continue.

Some persons who could not have acquired much experience in lithotripsy, have said that the fragments of stone irritated the bladder. This certainly does sometimes occur, but far more frequently less irritation and pain result from the presence of fragments in the bladder, than from that of the stone when entire. This fact will be ascertained by any persons who observe carefully, and I am very glad that Mr. Powell by his remark should have led me to consider this part of the subject.

I operated upon Mr. Page in the presence of Mr. Powell, Mr. Travers, Dr. Arnott, Mr. Lawrence, Dr. Sigmund, Mr. Pettigrew, Dr. Copland, Dr. Fergusson, Mr. Burnett, Mr. A. White, Mr. Bransby Cooper, Mr. Simpson, Dr. Chowne, Mr. Jewel, Mr. Walker, Mr. Hodges, Dr. Negri, my pupil Mr. Biggs, &c. &c.

This patient, when cured of his affection, was presented to the Westminster Medical Society, and displayed the detritus of his stone.

USE AND PREPARATION OF THE

"POMMADE DE GONDRET."

To the Editor of THE LANCET.

Sir,—Having frequently witnessed the good effects of the "pommade de gondret," otherwise called "pommade ammoniacale," and as it appears to be little known in this country, although its ingredients are, otherwise, in daily use, perhaps a few observations on the subject may not be unacceptable to some of your numerous readers.

Dr. Gondret, a respectable member of the Parisian faculty, is understood to be the first who, some years ago, prepared this ointment in the manner upon which its virtues greatly depend. It is so quick in its action, so easily guided, and the surface required for an abundant discharge is so small, that it often becomes an excellent substitute for blisters. And for forming an issue on the occiput, &c., nothing, in my opinion, is superior to it. In many cases of diseased joints and indolent tumours, where the ordinary means to produce absorption fail, it may be applied with much success. The two following cases tend to confirm this statement.

Case 1.—Miss Cook, daughter of an upholsterer, residing in Charlotte Street, Blackfriars Road, aged twelve years, of slender form; complexion rather dark, and generally enjoying good health, was brought to me for advice in the autumn of 1830. About a year prior to this date, her right index finger began to enlarge without any known cause, and had now increased to an enormous size, measuring five inches in circumference, continuing with little diminution to the last phalanx, where it suddenly tapered off towards the nail. The upper part of the finger was equally enlarged, hard, and benumbed, affecting the hand sympathetically, along the corresponding metacarpal bone, up to the wrist.

This child had, for several months, been under the care of an hospital surgeon, by whom the finger had been carefully bandaged, and by whom other scientific means had been employed; but these not succeeding, the parents were directed to send her to the sea-side as a last resource. This being found inconvenient, and the enlargement still increasing, they applied to me. It had all the appearance of one of those hopeless joint-cases which we sometimes meet with, and I very much doubted whether any advice could render the patient essential service. However, I recommended a small issue to be made on the inside of the middle joint, with the *Pommade de Gondret*, which was soon followed by a slight serous discharge. In a short time this evidently produced improvement, and then another small issue was applied to the under side of the first joint, which, in the course of a few months, reduced the enlargement still more. At this period the case was shown to Mr. Kingdon, the surgeon, Bank Buildings, who also pronounced the bone diseased.

The same means being less or more persevered in for upwards of two years, the finger is now reduced to its natural size, with the exception of a very little remaining fulness scarcely perceptible in the ends of the bones of the middle joint. The favourable termination of this case might probably have been more rapid, had not the person to whom the dressings were intrusted been rather too indulgent. Very little medicine of any kind was administered.

Case 2.—Sarah Kelsey, a servant, æt. 23 years, rather of strumous habit, residing with a respectable family near Newington Church, was admitted under my care at the City Dispensary, on the 8th of Nov. 1831.

There existed under the anterior edge, and about the middle of the sterno-cleido-mastoideus muscle, a hard deep-seated indolent tumour, fully as large as a middle-sized hen's egg, accompanied with painful throbbing in the neck, produced, appar-

ently, by the confined action of the carotid artery, and which so greatly impeded her breathing, that she was frequently obliged to sit up in the night to prevent suffocation. She complained also of occasional dysmenorrhœa and general debility.

The patient said, that she had not otherwise been subject to glandular swellings; that about six years ago, after taking severe cold, this tumour first made its appearance; that it was then much smaller, and could easily be moved about in every direction, except towards the back of the neck. Medical advice was then procured in the country, and lotions, &c. were employed, but without beneficial result. Coming afterwards to town, she obtained admission to the South-London Dispensary, where she continued under treatment for some months, but the tumour still remaining undiminished, extraction was spoken of as the only available remedy; and not possessing courage to submit to an operation, all other means were then given up. Admitted, at a subsequent period, as an out-door patient at Guy's Hospital, very active aperients were given her, and ointments to rub in upon the part affected. These remedies were continued for several months, but not proving successful, extraction was again proposed, and this, for a year or two, put an end to all treatment, until the time I saw her in the state above described.

The names of the gentlemen who had attended to the case, were a sufficient proof that no essential remedy usually employed had been neglected; and in order not to hazard a similar treatment, I recommended an issue to be made on the diseased part, with the "Pommade Ammoniacale." A pale serous discharge soon commenced, and the patient was directed to renew the dressings night and morning, adding more or less of the ointment, according to the irritation it produced, so as not to allow the surface of the wound to exceed the size of a small pea.

By this means the enlargement gradually diminished, as also the throbbing and difficulty of respiration, and thus, in the course of nine months, the tumour was completely reduced. Tonics, consisting principally of the carbonate of iron and occasional gentle aperients, constituted the rest of the treatment. A few weeks ago the same individual returned to the dispensary to be relieved from a cold, but has not experienced the least return of the tumour. I am, Sir, your obedient servant,

H. S. CALDWELL, M.D.

Camberwell, Feb. 26th, 1833.

P.S. The ointment not being procurable in London, the annexed "Formule" was sent me by a gentleman who had been in the habit of preparing it for Dr. Gondret,

and who first directed my attention to its use when in Paris. The French language being so generally known, it is here given as I received it.

"Pour faire la *Pommade de Gondret*.—1°. Je fais fondre dans un paëlon de l'axongé auquel j'ajoute plusieurs ou seulement deux cuillerées d'huile d'olive, suivant que je veux avoir la pommade plus ou moins consistante.—2°. Lorsque la grasse, est fondue, je la verse dans un flacon ordinairement à 4 onces, bouché à l'emeril jusqu'à moitié de ce flacon, puis je le rempli d'ammoniaque liquide très concentré jusqu'à ce qu'il n'existe plus que quatre à cinq lignes entre le bouchon et le niveau du liquide.—3°. Je remue fortement le flacon après avoir serré le bouchon à l'aide d'une ficelle, et l'avoir entortillé dans une serviette je le plonge ensuite dans de l'eau froide, ou glacée."

* * * This is what we call, common ammoniac liniment, "tres concentré." It certainly puts us strongly in mind of the fact, that the French make a great deal of a very little thing.

LONDON MEDICAL SOCIETY.

March 11th, 1833.

Mr. KINGDON, President.

SINCE the last meeting the anniversary dinner and annual elections of this society have been held, and Mr. KINGDON, amongst other changes, was chosen President. To-night he was installed, and returned thanks; after which, votes of thanks were passed to the late President, Dr. Burne—the Vice Presidents, Mr. Callaway, Mr. Kingdon, and Drs. Whiting and Uwins—to the Treasurer, Dr. Shearman,—to the Orator at the late meeting, Mr. Salmon—to the Librarian, Mr. Dendy—and to the Secretaries, Messrs. Headland and Jones, who, severally (where present), acknowledged the honour in appropriate terms.

METASTASIS OF RHEUMATISM. TREATMENT OF RHEUMATISM BY MODERATE BLEEDINGS AND QUININE. COLCHICUM.

MR. CLIFTON then related a case of sudden metastasis of rheumatism to (as he thought) the diaphragm, which drew forth a discussion that occupied the remainder of the evening. He had never before seen a metastasis of that kind. The occurrence was one of great interest at the moment, and excited the highest alarm in the sick-room. He had been attending a delicate female, who formerly laboured under a pulmonary affection, but was now recovered from that,

and suffered an attack of acute rheumatism in the legs, feet, and knees. He gave her during several hours after it had set in, moderate doses of the *vinum colchici*, which subdued the pain; but two hours after he left her he was summoned again in great haste, in consequence of a violent accession of the disease in another part. On returning to the house, he found her breathing with the utmost difficulty, the countenance excessively anxious, and the symptoms altogether so startling, that both himself and all around her believed her to be in the agonies of death. The state of the pulse was, in fact, the only consolatory indication. Though the dyspnoea was great beyond measure, the pulse was but 88, and not small nor fluttering. This induced Mr. Clifton to believe, that the disease was not one of the heart. The attack did not involve the intercostal muscles, but appeared to be purely one in which the diaphragm was affected, preventing its descent into the abdomen. The labour of respiration resembled the extremest degree of that sort of difficult breathing which marks the advanced stage of peritonitis, when the tumefaction is greatly augmented. This continued for some hours, during which hot water to the feet, and doses of ether and opium, were resorted to, with the effect, in eight hours, of producing relief, subsequent to which the rheumatic pains in the extremities were wholly removed.

The case was regarded as a singular one, and led to a debate on bleeding in rheumatism. Mr. Clifton having first been asked why he did not bleed for the acute rheumatism, in addition to giving colchicum, Mr. Clifton's reply was, because the patient was too delicate to bear it. But first of all, Mr. PROCTOR rose to express an opinion, that metastasis of rheumatism to the heart, &c. was much more common now than it used to be, from whatever source the frequency might arise.

Dr. BURNÉ thought so too, and the cause he ascribed to the large bleedings employed by the moderns to subdue rheumatism, which appearing to them to be a disease that would allow bleeding with advantage, was consequently thus treated to an excessive extent, and ended in the frequent metastasis complained of. Rheumatism, however, was a constitutional disease, which bleeding would not cure. Continued depletions reduced the extremities to a state which rendered them unable to support rheumatic attacks, and the disease, consequently, not being really expelled the system, flew to the heart, as a central part which was better able to suffer and keep up the rheumatic diathesis. Entertaining these views, he bled sparingly in the disease.

Dr. WHITING thought this theory very plausible, and that acute rheumatism was certainly far too freely treated by bleeding. He used it moderately in most cases, just to subdue the inflammatory action, and then employed quinine to prevent the return of the attack. He begged particularly to call attention to this practice, and wished that practitioners would give it a trial. The success of the plan in his hands was such, that he would advise every practitioner in the kingdom to give it a trial. He positively found the quinine to be as remedial in acute rheumatism as he did in ague. As soon as he saw a case, he had the patient bled, and directly after the lancet was used, he gave two grains of sulphate of quinine every three or four hours, continuing it until the pain was gone. So effectual was this, that if a cure was not immediately performed, a subsequent attack, which was very rare, was sure to be a very mild one. (Much surprise was expressed by some members at these statements. The Doctor, therefore, subsequently enlarged on the subject, expressing the following views, partly made public some years since, he said, in one of the medical periodicals, and since fully confirmed by additional experience.) He regarded quinine not as a stimulant, but as a medicine which had the marked influence over the nervous system, of preventing the return of morbid action in cases where disease had once been subdued. Thus, in acute rheumatism (*arthritis*; not common muscular rheumatism, which was quite another disease), he first produced a reduction of the pain by a bleeding,—and then the morbid action of the parts being subdued—he threw in the quinine to prevent its re-appearance; and most effectual, surprisingly effectual, was this mode of treatment. He besought practitioners, generally, to try the quinine on their own persons, in this light, and in acute rheumatism he was persuaded that they would never adopt any other plan. He used to give colchicum, either bleeding or not in addition, as circumstances seemed to indicate, and occasionally it would cure a patient in a few days, but the quinine, after bleeding, was invariably successful in his hands. He had seen patients who had been lying very ill with rheumatism for a long period, defying all other treatment, whom the quinine had restored to full walking condition in two or three days. Many other medical men were also now employing it, and his former pupils often wrote to him to say how successful the plan was in their practice.

Dr. SHEARMAN thought that metastasis of rheumatism was now much more frequent than formerly, and he often was at a loss to account for it; but he now thought that the difference was to be traced to the increased

mania for evacuations, and to some change in the constitutions of men. To profuse bleeding he objected, on the ground that inflammation was an accidental concomitant, not an actual feature, of genuine rheumatism.

Mr. CLIFTON relied almost entirely on the wine of colchicum in his cases. Dr. Fordyce used to say, that bleeding was certainly very fashionable in rheumatism, but that he never in his life derived advantage from it.

Mr. PROCTOR, referring to the varying efficacy of the two forms of colchicum, observed that at Gravesend, where rheumatism is so common, they state that they cannot depend at all on the wine, but only on the powder of the root, half a drachm of which they usually give every four hours. They consider the powder there as a specific. In London, however, it was almost a useless medicine compared with the wine.

Mr. SALMON. Mr. Wigan, of Brighton, an experienced practitioner, finds the powdered colchicum in doses of ten grains three times a day, the most effectual of all remedies, and recommends it strongly.

Mr. HOOPER. Then it cannot be good, I fancy; for half a drachm, if pure, will, I have found, poison a patient.

Mr. DENDY. Yet I have given in one case, a drachm and a half of the *vinum colchici* in the course of the day, after taking twenty ounces of blood. The neighbours of the patient sent to me to stop the cries which his rheumatic pains produced. Three or four doses produced but little relief, and in my absence the patient was so determined on a cure, that he took four teaspoonsful at a dose of the wine, procured straight from Apothecaries Hall, four times a day, and the effect was to restore him at once to health.

Mr. COLÉ said he had known a scruple of the powder, given in three doses, produce, in a gentleman aged 70, ulceration of the intestines, which took three years in curing.

Mr. BLENCAIRNE said he had tried Dr. Whiting's plan, and been disappointed in it.

A glance at the clock stopped a further prolongation of the discussion.

** The three cases with which Dr. Blundell's name was connected at the end of Dr. Waller's observations last week, page 754, should have been stated to be cases of spontaneous evolution.

Monday, March 18th, 1833.

Mr. KINGDON, President.

RHEUMATISM.—ELATERIUM. ACUPUNCTURE.

The spirit of discussion was slow of descent this evening. The CHAIRMAN tried to invoke it by asking whether the mischievous effects of colchicum were produced on the sensorium or the mucous membranes, but the inquiry fell still-born. So Mr. DENDY started the novel proposition that the metastasis of disease was not the transference of disease from one spot to another, but a new attack, produced so violently in a fresh part, as to hide from the patient's feelings the effects of its continued existence in the original seat. The position thus stated was rather untenable, as Mr. Dendy afterwards saw, but it served as a stimulant, however, to conversation, inducing, by and by,

Dr. UWINS to rise, and speak of the treatment of rheumatism, and to recommend *elaterium*, in considerable doses, in the first stages of rheumatic affection, declaring, on the strength of his own experience, that "it would not only tend to shorten the duration of the disorder, but would, in fact, destroy the disease in the bud, and prevent a long course of inconvenience and suffering." It should be given in grain or half-grain doses, every morning, according to the degree of pain experienced, and the malignancy of the disorder. Its precise action he could not explain, but he was led to give it at first, from, he believed, its tendency to produce nausea, and set the secretions at work. His patients said that it was a very unpleasant medicine, but the relief produced fully counterbalanced its disagreeable mode of operation.

Mr. SALMON asked.—What of acupuncture in rheumatism? In my limited experience it has failed.

Mr. DENDY. And in mine too. When first it was proposed, it certainly effected some singular cures, but, of late, success does not seem to have attended it. Amongst the cures it formerly produced, was one that came within my knowledge, where it benefited both patient and practitioner in a very agreeable manner. The Earl of Egremont was a martyr to rheumatism, and some years since, after having been treated by every medical man of note in London, without obtaining relief, he retired to his seat at Petworth, in despair. A friend of mine, who resided in Sussex at that time, happened to get an early copy of Mr. Churchill's little work on acupuncture, and tried the remedy therein advocated with perfect success on an old woman who was a protégé

of Lady Burrell, the daughter-in-law of the Earl. Her ladyship heard of the cure, and told the Earl what had been done; the result was, that the surgeon was sent for forthwith to try the new process on the peer, into whose tortured person he accordingly introduced two needles, keeping them in for twenty minutes. The effect was, that the Earl, who had obtained no sleep for the past fortnight, that night slept for seven or eight hours. Filled with joy, he gave the fortunate practitioner a check for a large sum, sent him home with post horses, and that day bestowed on one of his favourite racers the name of "Acupuncture." The event made my friend's fortune. As regards my own experience, however, I may state, that I have lately had three cases in which I have tried this remedy without advantage.

Dr. UWINS spoke of the supposed production of rheumatic attacks by unsuitable food, which led

Mr. SALMON to mention a case that he had attended for retention of urine, where the patient was subject to periodical rheumatic pains in the intercostal muscles, and on one occasion brought on a violent attack of the complaint by eating two eggs boiled to great hardness. The pain began on the 4th day, lasted two days, and was relieved on the 7th by the patient's actually throwing up, as he (Mr. Salmon) himself witnessed, the eggs that had been swallowed a week before.

The practical interest of the debate diminished after this. Left sitting.

ANNIVERSARY MEETING.

On Friday week, the sixtieth anniversary of this Society was held at the London Coffee-House, Ludgate-hill, Dr. BURNE, President, who announced the names of the new committee and acting officers elected on the Monday previous, informed the meeting that the subject for the next Fothergillian gold medal would be "Diseases of the Uterus as connected with Pregnancy;" and for the silver medal, "The Characters, Physiology, and Treatment of Puerperal Fever." He then called on Mr. F. SALMON to deliver the customary "oration," which that gentleman did, by reading a paper, on "The Abuses of the constitution and management of the Royal College of Surgeons in London." Cheers followed the announcement of the title. The orator showed those abuses to be almost as numerous as they are injurious in effect, and to be of the grossest and most flagrant description. As he is about to publish the paper, we abstain from giving a detail of its contents. Applause followed the conclusion of the oration. The assembly subsequently dined together at the tavern.

WESTMINSTER MEDICAL SOCIETY.

Saturday, March 16th, 1833.

Mr. PETTIGREW in the Chair.

DRS. LEE AND GRANVILLE.

Dr. GEORGE GREGORY, thinking that the controversy between the above gentlemen ought not to remain as it was left last Saturday, moved, after some opposition from the chairman, who considered that it should not further be discussed, "That the charges of literary piracy brought by Dr. Granville against Dr. Lee had not been substantiated, and that the Society do express their opinion thereon, by ballot, at the next meeting." He deprecated the absence of Dr. Granville on the present occasion, after his having offered to bring forward certain parallel passages from the papers of Lauth and Lee.

Dr. SOMERVILLE considered the law which allowed such inconvenient proceedings as the present, to be a very bad one; yet he would second the motion. He regarded Dr. Lee as one of the most honest men in existence.—Mr. NORTH opposed the motion. He did not think such a demonstration in favour of Dr. Lee necessary, and expected that the Society would not support Dr. Gregory's proposition.—Dr. JAMES JOHNSON was present at the last meeting, and then distinctly understood, that Dr. Granville had withdrawn his charges, as the sense of the meeting was that they had been answered, and ought to be dropped.—Dr. COPLAND entertained the same impression.—Mr. HUNT considered that if nothing had been said out of the room (alluding to Dr. Granville's letter to the Editor of THE LANCET) on the subject since the last meeting, the Society would have considered the matter settled, and that, as the probable cause of the present motion did not originate in the Society, Dr. Gregory's proposition should not be entertained.—Dr. GILKREST thought the question was finally settled last evening, and that it should be reargued only in the medical journals. Dr. Granville had candidly admitted, that when Lauth preceded Lee, he preceded him (Dr. Granville) also.—Mr. CHINNOCK would have regarded the point as set at rest, but having that morning seen Dr. Granville's letter in an influential journal, he was induced to support the present motion.—Dr. SIGMOND considered, that as Dr. Granville had distinctly said he did not charge Dr. Lee with plagiarism, Dr. Lee's friends ought to be satisfied, and he therefore moved as an amendment (seconded by Dr. Johnson) that the subject be postponed *sine die*. (Some of the speakers subsequently disclaimed having taken up

the affair merely as Dr. Lee's friends.)—Dr. WEBSTER supported this latter proposition; and Mr. CHINNOCK said, that if the Society would tacitly consider that the charges against Dr. Lee had fallen to the ground, it would be better to withdraw the motion, to which suggestion Dr. Gregory ultimately acceded.—The CHAIRMAN then called on Dr. COPLAND to proceed with some promised remarks on delirium tremens.

DELIRIUM TREMENS.

Dr. COPLAND accordingly rose to bring certain points, either in dispute or unnoticed by authors, on this subject, before the Society, for their opinions relative thereto. He considered that the disease had never as yet been well described,—certain types only of the affection, instead of its varying phenomena, having occupied the pens of writers who had treated of it. Having briefly attempted to supply this deficiency and some others (with details which we cannot advantageously transfer to a report—for the whole topic, as Dr. Copland admitted was far better fitted to be treated in a manuscript essay than a short speech at a debating society), he proceeded to the subject of treatment, cautioning practitioners against the indiscriminate use of bleeding and opium, as in some cases the slightest depletion would destroy life, and in others the doses of opium often recommended, would prove equally fatal, producing narcotism and convulsions, effects which at the Baltimore Infirmary, under the care of Dr. Wright (see *Amer. Jour. Med. Sci.*, Nov. 1832), resulted from doses of from twenty to thirty grains.

Dr. SOMERVILLE, alluding to the slight and varying causes which might produce delirium tremens, referred to the case of Mr. Baker, the late secretary of the Society, a most abstemious man, in whom the drinking a single glass of hot brandy-and-water after exposure to cold, produced fatal delirium tremens. He also mentioned one case where that disease was produced by a blow on the head, and another by the habit of drinking ether.

Dr. A. T. THOMSON considered opium to be as nearly a specific in delirium tremens as any medicine was in any disease that he knew, provided it had been preceded by cathartics.

Mr. HUNT expressed a nearly similar opinion, and enforced, particularly, the importance of its combination with other medicines. He said that he himself usually began with ten grains of calomel, finding on all occasions that the symptoms, afterwards, thus precluded, gave way to opium far more readily than without it. Indeed he would

never again treat delirium tremens without employing calomel and purgatives before opium, which then became the saviour of the patient.

Dr. WEBSTER and Mr. CHINNOCK also regarded opium as the sheet-anchor of the practitioner in treating this disease.

Dr. GILKREST dissented from the opinion that previous cathartics were always necessary.

Dr. JAMES JOHNSON, referring to its causes, stated that he had lately seen four or five cases of most exquisitely-marked delirium tremens in young persons, all of them females under 20 years of age, who had not been guilty of the slightest degree of intemperance. Sleeplessness, (one of them had not slept for eight nights,) spectral illusions, ferretty eyes, cold clammy skin, and constant jactitation, were amongst the symptoms. The patients were ladies residing at a country boarding-school, and became the subjects of the disease, which he had never seen more complete in any drunkard, from moral causes. In three of the cases the ladies had remained at school during the holiday recess, a long way from home and from their parents, for the purpose of devoting that portion of time to study, in order to remedy the defects of a neglected education. They had laboured most assiduously, and the delirium burst out immediately after the return of the other scholars at the commencement of the new session. One of the young ladies he learned had been "irregular" three days before the disease set in. On visiting the cases, he first attempted to produce relief by opium, but that only aggravated the disease. Cold to the head, soothing treatment, and moderate nourishment, were then adopted, and with success. Drunkenness, however, was the common cause of the disease, and in most cases opium did harm, if used before the secretions were put in order. Then it was all in all. He had examined many bodies without once discovering unequivocal proof in the brain that inflammation was the cause of the disease.

Dr. COPLAND, in reply to one remark, (amongst many which subsequent explanations rendered it unnecessary for us to record), said that he should, in preparation for administering opium, give from five to fifteen grains of calomel, with, afterwards, a proportionate quantity of stimulants in combination with the opium, which otherwise would certainly do harm in the disease.

A great deal more was said during the evening, but such continual misapprehensions occurred amongst the speakers, that five-sixths of the remainder could be turned to no good account if ever so carefully amplified in print.

THE LANCET.

London, Saturday, March 23, 1833.

CONCOURS FOR THE CHAIR OF CLINICAL
MEDICINE, PARIS.

THE first public act of the concours for the vacant Professorship in Paris, took place in the amphitheatre of the Faculty of Medicine on Thursday the 14th instant, at four p.m. Although the meeting of that day was only a preliminary one, the audience benches were crowded to excess for more than an hour before the commencement of business. Considerable interest was excited by the appearance of the candidates, who formed a group within the precinct reserved beneath the tribune for the judges and the competitors. M. CAYOL was the first who entered the room. He is a portly, sleek, priestly-looking personage, about forty years of age, the *ensemble*, in fact, of a man who would be considered in England as *personally* qualified to ornament a chair in one of our Dean-and-Chapter institutions. MM. TROUSSEAU, ROSTAN, and GENDRIN, next presented themselves. TROUSSEAU is a tall, thin, inexperienced-looking man, with something of the Gascon in his attitudes and address. ROSTAN has a polished exterior, both in manners, person, and attire; he is the favourite of the students, and the dreaded rival of the champion of clinical Carlism, GENDRIN, who appeared next, with hair now "sable silvered," a brilliant, intellectual eye, but with countenance abashed at the presence of the spectators and judges. The last candidates who attracted especial notice were, M. CHAUFFARD, a provincial physician, from Avignon, whose lank form and sepulchral aspect seemed to hint that he was better qualified for teaching morbid anatomy than clinical medicine, himself being a portable museum of illustration; and M. BROUSSAIS junior, who certainly does

not inherit the physical decorations which nature has bestowed on his celebrated sire.

This preliminary meeting was rendered particularly interesting by the circumstance, that considerable difficulties had fortuitously occurred in the composition of the jury. M. BROUSSAIS senior being excluded by the fact of his son being a candidate, the vacancy was balloted for, and M. MOREAU chosen. M. MOREAU declined the office on the plea of unavoidable absence, and the perplexity then arose, that there remained but three or four professors from among whom to choose, and of those it was known that two could not attend. These circumstances almost deprived the balloting of the character of chance, and it was still further known, that the right of protest would be exercised, by more than one candidate, against any appointment that might be made in this manner.

Accordingly, after a long discussion, the Faculty found themselves driven to the expedient of constituting one of the *suppleans* a titular judge, thus commencing the concours in direct infringement of a regulation which requires the presence of two "suppleans*," and leaving their subsequent proceedings open to an appeal to the "Royal Council of Instruction." Similar difficulties arose in the *Académie de Médecine*. M. RECAMIER, three days after his election, was taken ill, and the question arose, first, whether he should be replaced by the *Académie* "suppleant," or by a new ballot; and, secondly, whether the name of the "suppleant" should be placed in the urn with the rest. Both these points were decided affirmatively. The ballot took place, and forth issued, on the eve of the concours too, the name of M. ABRAHAM, an octogenarian invalid, totally incapable of attending. What then was to be done? The

* Individuals who attend the concours from the commencement, in order to supply the place of any one of the judges who may be taken ill, or otherwise disabled from persevering in his attendance.

concours was announced for the next day; the provincial candidates were impatient, and their interests injured by the delay. In this conjuncture M. ORFILA came boldly forward, advised the self-constitution of the jury, *subject to the recognition of the candidates*, and a subsequent appeal in case of their rejection of the proceeding.

The jury accordingly met on the 14th, having previously, in a private conference, elected, by ballot, M. CHOMEL to the office of president, and M. ADELON to that of secretary. M. ADELON then read over the laws and regulations of the concours at full length, and explained the deviations therefrom which the above-described circumstances had occasioned. The roll of candidates was next called over, each candidate recording his name as it was called. The presentation of M. GENDRIN occasioned a considerable tumult. Loud hisses, groans, and cries of "Turn him out," issued from all parts of the theatre, and order was only restored when he returned among the other candidates. When this was concluded, the candidates retired to discuss the irregularities, and returned with a written document, agreed to by all except MM. CAYOL and CHAUFFARD, in which the irregularities were admitted and protested against, but by which the jurors were accepted by the candidates themselves. The refusal of M. CAYOL to join in this adhesion to the jury is very remarkable. He is generally considered the candidate most likely to succeed, but here again he opens for himself another avenue to success. If elected, he of course accepts his good fortune. If vanquished, he retains, and certainly will use, his right of appeal to the Council of Instruction, who, from their Carlist propensities, will, in all probability, annul the concours.

We have thought it right to enter into these minutiae, in order to stand the many difficulties with which the practice of the concours, as carried on in the French University, is still surrounded, and the ma-

nœuvres which can yet be detected in its details. But there is no principle, however good, which may not be contravened by imperfect machinery; and for the construction of machinery of any description, mechanical or metaphysical, our gallic neighbours have not, as some think, any very extraordinary genius. Let us not then blindly accept from them the faults as well as the virtues of their systems.

The subsequent sittings of the jury, for about ten days, will be occupied by the private discussion of the merits of the works and other anterior titles of the several candidates. The first trial-lecture will not take place, it is presumed, until the 2nd of April.

RETURN OF THE OFFICERS, OR OTHER PERSONS, BELONGING TO THE
NATIONAL VACCINE BOARD;
THEIR APPOINTMENTS, DUTIES, AND SALARIES.

Moved for by H. WARBURTON, Esq., M.P., in the House of Commons, and printed by order of the House. August 8th, 1832.

Letter from Sir HENRY HALFORD to Lord MELBOURNE.

MY LORD,—I have the honour to send your Lordship the enclosed Report of the Officers of the National Vaccine Establishment, in answer to your Lordship's wish expressed to the President of that Board, that he would transmit a detailed account of the same; and am, my Lord, your Lordship's faithful humble servant,

HENRY HALFORD.

Curzon-street, 27th July, 1832.

The National Vaccine Establishment is at present composed of a Managing Board, consisting of the President and senior Censor of the Royal College of Physicians, and the President of the Royal College of Surgeons, a Treasurer, Registrar and Inspector of Vaccinators, and twelve Stationary Vaccinators, a Secretary and Messenger. The following are the names, residences, and salaries of the present members of the Board:—

	Per annum.
Sir H. Halford, Bart, Pres. Roy.	
Coll. Phy.	£100
J. P. Vincent, Esq., Pres. R. Coll.	
Surg. Lincoln's-Inn-Fields	100

Dr. Thomas Hume, Censor, South-st., Grosvenor-sq., (by virtue of his office as senior Censor for the year)	100
Dr. Robert Williams, Censor, Bedford-place, Russell-square, (this appointment is ordered to be discontinued after September, 1832)	100
Dr. Clement Hue, Registrar, Guildford-st., Russell-sq.	200
Making salaries, per annum, of ..	600

The Board so constituted meet for the purposes of business once per week or fortnight, according to the urgency of business; and it forms the especial duty of the Registrar to attend to the correspondence, foreign and domestic, to submit such to the Board, and to keep a faithful register of the transactions of the establishment.

The executive part of the establishment consists of an inspector and twelve stationary vaccinators.

J. F. Simpson, Esq., Inspector, resident at the central station, No. 8, Russell-place, Fitzroy-square. His duty consists in visiting the several stations, receiving and distributing lymph to all applicants at home and abroad, and investigating all anomalous cases, as pointed out to him by the Board, or reported by vaccinators .. £200

J. C. Carpué, Esq., Dean-st.; vaccinates from ten to eleven o'clock daily at the central station
 £150 |

F. Agar, Esq., High-st., White-chapel
 50 |

C. R. Aikin, Esq., Baker's-buildings, Liverpool-st.
 100 |

J. Barnett, Esq., Charter-house-sq.
 50 |

J. A. Gillham, Esq., Surrey Chapel Blackfriars-road
 150 |

J. Harkness, Esq., Broad-street, Ratcliffe
 50 |

Edw. Leese, Esq., Baker-street ..
 100 |

W. J. Lewis, Esq., Spital-square ..
 50 |

R. Robertson, Esq., Jernyn-street ..
 50 |

R. Semple, Esq., Rufford's-row ..
 50 |

H. Sterry, Esq., Allscot-place, Bermondsey
 50 |

B. Ward, Esq., Wellclose-square ..
 50 |

Dr. Thomas Turner, Curzon-street, Treasurer
 25 |

Charles Murray, Esq., Chancery-lane, Secretary
 50 |

John Hutton, Messenger
 52 |

Making salaries, per annum, of ..£1227

The Board, as already stated, consists of the Pres. of the R. C. of Phys., the Pres. of the R. C. of Surgs., and the senior Censor of the R. C. of Phys. for the time being.

Their appointments may be considered as annual, as depending upon the offices which they hold respectively at their several Colleges. The more permanent and subordinate officers of the establishment were regulated and chosen by the Board.

H. H.

SUFFOLK GENERAL HOSPITAL,

Bury St. Edmunds.

ELECTION OF A HOUSE APOTHECARY.

(From a Correspondent.)

Candidates.—Mr. WILLIAM WARD, 140 votes; Mr. FAIRCLOTH, 114; Mr. HOWARD, 38; Mr. HUTCHISON, 11. Mr. Ward was accordingly declared to be elected. The vacancy took place on the resignation of Mr. Pyman, who was about proceeding to India, and the election was held at the hospital on the 13th of February. Six candidates had announced their intention of contending for the office, but only four came to the poll. Testimonials from many "eminent" men in the profession connected with the London Hospitals, were displayed in circulars and newspaper advertisements; and so dazzling was the effect upon many of the governors, that between the two first-named candidates no superiority could be detected, and the governors were consequently puzzled how to dispose of their votes. Happily some officious persons thought that the election ought not to pass without political references, which ultimately were the means of deciding the contest as it terminated. *Undue* influence was used; political prejudices were allowed to bias the votes; and party spirit seemed to predominate throughout the election. The ultimate declaration by the chairman, as to whom the choice had fallen on, struck with astonishment all the hospital oligarchy, who, with one or two exceptions, exerted themselves for the second candidate. Mr. Ward, however, is in every respect capable of performing the duties of the institution. Merit, and merit alone, should be the passport to medical honours. Mere "testimonials," though produced by the barrowful, are deceptive vouchers for knowledge and ability. They may be obtained by the most ignorant persons from our hospital surgeons, if asked for in a polite manner, after the metropolitan-hospital-practice-and-lecture-money has been paid. Open and full inquiry into qualifications ought alone to decide the question of choice at medical elections.

DR. GRANVILLE'S PARALLEL BETWEEN THE PAPERS OF PROFESSOR LAUTH AND DR. LEE, ON THE STRUCTURE OF THE PLACENTA AND ITS CONNEXION WITH THE UTERUS.

To the Editor of THE LANCET.

SIR,—I proceed to redeem the pledge I voluntarily tendered at the meeting of the *Westminster Medical Society* held on the 9th instant, and repeated through your journal of the 16th, of proving the striking similarity that exists between Professor Lauth's views of the structure of the human placenta and its connexion with the uterus, published in the 1st volume of the *Repertoire d'Anatomie* 1826, and those of Dr. Robert Lee on the same subject, read before the *Royal Society* in November 1831.

As Dr. Robert Lee has publicly declared in his written address on this subject, read at the *Westminster Medical Society*, that he was wholly unacquainted with the existence of Professor Lauth's paper, and did not know in what work to look for it until after he had called upon me on the 5th instant; and as

Dr. Lee.

What was Dr. Robert Lee's object in writing his paper "On the Structure of the Placenta and its Connexion with the Uterus?" We gather it from the two following general propositions contained in that paper; propositions which embrace the whole of the alleged discovery of Dr. Lee, and the originality of which that gentleman, with two or three of his friends, stoutly maintain.

A

1st. "A cellular structure does not exist in the placenta."

2nd. "There is no connexion between this organ and the uterus by great arteries and veins." (*Phil. Trans.* 1832, Part I, page 59.)

Now let us see how these two gentlemen work out, or develop, as the French would say, these two propositions—the only possible meaning of which (constituting in itself the discovery) is so identically the same in both the French and English writers, that it is impossible to make any tangible distinction between them.

To obtain this result, so as to present it in an analytical form to my readers, it is necessary to read both papers over and over again; for it must be confessed, that no two

he repeated this solemn declaration once again, on being questioned by some of the members present on the 9th instant, no charge of plagiarism, even had one been intended, can be brought against that gentleman, although the singularity of the coincidence of his views with those of Professor Lauth, as I shall presently exhibit, will remain not a jot abated.

In the addition of the unprejudiced testimony of your reporter to that of Dr. Brown, Dr. Jewell, and many others, who would have spoken, had it been necessary, at the meeting of the 9th, respecting the manner, apparent object, and extent of my statement on this question on a former evening, and his belief derived from the events of the evening, that those statements were made from a sincere conviction of their truth in my mind, without premeditation or inimical feeling, only an act of justice has been done me, which I had a right to expect, and have never failed to receive, from impartial men. For the opinion of individuals of a different character, or who are actuated by different principles, I cannot entertain the smallest consideration. And now to business.

Professor Lauth.

What is one of Professor Lauth's principal objects in writing his paper "On the Connexion of the Placenta with the Uterus," anatomically and physiologically considered? We collect this from the two following general assertions which Professor Lauth puts forward as placing the question of that connexion in a new point of view, the truth and originality of which he maintains.

A

1st. "Quant aux cellules qu'on décrit entre l'utérus et le placenta, ou dans la portion utérine 'du placenta,' je n'ai jamais pu les découvrir, malgré l'attention avec laquelle j'ai dirigé mes recherches."

2nd. "Il est prouvé maintenant qu'il n'existe pas de communication directe entre les vaisseaux utérins et ceux du placenta." (*Rep. d'Anat.*, vol. i, page 77, 1826.)

writers have ever done themselves so little justice in expounding their own discoveries, as the two individuals under consideration, for want of that *lucidus ordo* which is so essential in compositions partaking largely of the controversial character.

Both authors begin by stating how mistaken their predecessors have been in the views entertained on the subject in question, as well as in the experiments they made respecting it, and the conclusions drawn from them.

Professor Lauth.

B

Professor Lauth affirms generally, that although, for a great number of years, anatomists and physiologists have considered and studied the mode of union between the uterus and placenta, and the reciprocal exchange of blood between them, none of them succeeded in giving us clear and distinct ideas of the structure of those parts and their functions. (*Repert.* page 75.)

Having thus cleared the way for their individual opinions on this important question, by showing that all those who had preceded them were in error, both our authors proceed to point out those sources of fallacy by which their predecessors were misled in making their experiments concerning the structure of the placenta and its mode of union with the uterus. The principal of those sources of error relates to the inject-

C

"If we inject the placental vessels," says Professor Lauth—

"Pas un atôme de matière à injection ne pénètre dans les vaisseaux de la membrane caduque (qui recouvre le placenta) et il ne s'en échappe rien non plus au dehors, à moins que la violence qu'on a employée n'ait produit une déchirure."—(page 77.)

Again:—

"L'injection poussée dans les artères (de la mère) s'arrête entre le placenta et l'utérus, et forme des épanchemens plus ou moins considérables dans l'interstice de ces deux organes, si on a employé trop de force la poussant."—(page 75.)

Again:—

"Ces épanchemens (que forme quelque fois la matière à injection entre les deux organes) dépendent du décollement du placenta et de la membrane caduque et de la rupture des vaisseaux qui l'unissent à l'utérus."—(page 77.) (The tortuous and numerous small bloodvessels from the inner membrane of the uterus to the decidua of Dr. Lee, page 60.)

Having therefore, equally denied that any direct communication exists between the uterine and fetal vessels, it follows that both authors must repudiate this common notion of a maternal and fetal portion in the human placenta. Accordingly we again find Professor Lauth anticipating Dr. Robert Lee, or rather Dr. Robert Lee singularly coinciding with Professor Lauth.

Dr. Lee.

B

Dr. Robert Lee more specifically affirms, that although every anatomist in this country acquiesced in the opinions of the two great Hunters, those authors were mistaken in their notions of what "they supposed to be the discovery of the true structure of the human placenta and its connexion with the uterus;" and, furthermore, Dr. R. Lee asserts, that Noortwich, Røderer, and Haller, had not determined by their researches, in a satisfactory manner, "the connexion between the uterus and cells of the placenta." (*Ph. Trans.* page 59.)

ing matter which Professor Lauth's and Dr. Lee's predecessors pretend to have pushed into the substance of the placenta from the uterine vessels, and *vice versa*, by fair and direct communication; whereas in the Professor's and the Doctor's opinion, that result had been obtained by laceration of the decidua, which intervenes between the uterus and the placenta.

C

Dr. Robert Lee examines the injected uterus of Mr. Hunter instead of referring to any experiments of his own, and alludes to the injections made by the two great English physiologists, as well as by Noortwich, Røderer, &c., and says:—

"The laceration of the deciduous membrane followed the artificial process, (that of forcibly injecting fluids) as well as the formation of deposits of injection in the vascular structure of the placenta."—"The Hunters were also misled, by the effects of artificial distention of the placenta from the extravasation of the fluid forced into the uterine vessels."—(page 63.) "Elsewhere the injection had lacerated the deciduous membrane, and formed deposits in the vascular part of the placenta."—(page 63.)

Again:—

"Flattened portions of injection were observed in this situation (between the inner surface of the uterus and the placenta) having in many parts the form of thin layers (épanchemens), which had obviously escaped from the orifices of the uterine veins."—(page 63.)

D

Professor Lauth says,—

"J'ai du examiner ce que l'on doit entendre par placenta fœtal et par placenta utérin."—"Malgré les recherches les plus minutieuses, je n'ai pu découvrir ces deux portions." And farther on, "Il n'existe donc pas de placenta utérin, tel qu'on l'a décrit jusqu'à présent."—(page 76.)

Well, then, as we are told by two different writers, the one in 1826, the other in 1831, and, in words almost identical, or as nearly similar as the two languages will admit, what the human placenta is not, we will endeavour to make out, from different

E

"Le placenta n'est composé que de la division successive des artères ombilicales, sur le chorion, artères qui, parvenues à l'extrémité des villosités qui le recouvrent, se replient sur elles-mêmes pour donner naissance aux veines. Le tronc qui résulte de leur réunion est la veine ombilicale."—(Page 76.)

Now here is a bold, clear, and distinct opinion, advanced by Lauth on the result of "recherches minutieuses," constituting a discovery (assuming that opinion to be correct) of the "real structure of the placenta made in 1826."

Having disposed of the main discovery, we next proceed to compare several secondary points, although equally important in the study of the structure of the human placenta, in which we shall find again the Scotch physiologist anticipated by the French professor.

F

"La membrane caduque reçoit de nombreux vaisseaux, qui sont les continuations des vaisseaux utérins, ou qui sont, au moins, en communication directe avec eux."—(page 76.)

The professor had, moreover, insinuated that there is no communication by direct vessels through the decidua, when he stated that no injected fluid can be made to pass through it without laceration.

Thus far the two physiologists, being placed on parallel lines, appear to have advanced the same anatomical notions as to the supposed real structure of the placenta, or to its connexion with the uterus, and distribution of bloodvessels, and to have noticed, alike, the errors of their predecessors, the fallacy of previous experiments, and the incompatibility of their conclusions with real facts. Lastly, they have advanced, in support of all their several assertions, similar experiments. With regard to the latter, we infer from Lauth's statement, that he has made and repeated the experiments himself,—not so with Dr.

D

Dr. Robert Lee states,—

"The facts which have now been stated warrant, I think, the conclusion, that the human placenta does not consist of two parts, maternal and foetal."—(page 63.)

parts of their memoirs, their opinions as to what it is, and see whether in that also they coincide. Look we, therefore, into the Strasburgh professor's lucubrations first, and next into those of Dr. Robert Lee.

E

"Between the chorion and amnion and decidua, lie the ramifications of the umbilical vein and arteries, subdivided to an almost indefinite extent." Thus the placenta consists solely of a congeries of umbilical vessels."—(page 60.)

And here also we have a straightforward manly averment, constituting Dr. Lee's discovery. Pray let the reader point out the difference between the two discoveries, that of Dr. Lee being, moreover, like that of Professor Lauth youched as the result of the "minutest examination?"

F

"With the fibres uniting this placental decidua to the uterus, are mingled numerous bloodvessels proceeding from the inner membrane of the uterus to the decidua."—(page 60.)

The Doctor observes, moreover, that "there is no vestige of the passage of any great bloodvessel, through the intervening decidua from the uterus to the placenta."—(page 60.) Because no injection has ever passed through it without laceration.

Robert Lee, who leaves us to guess whether by "the examination of six gravid uteri, and many placenta expelled in natural labour" (page 59), he wishes us to understand that he himself made actual experiments upon the former, and has now in his possession the result of those experiments to show to his brethren. I take it for granted he has; in which case the discovery of the real structure of the placenta now alleged* would stand a fair chance

* I shall show in some future communication, that the discovery requires many important modifications, and the real structure of the placenta is not to be found in the "Philosophical Transactions."

of being adopted, backed as it is by the opinions and experiments of Professor Lauth and Dr. Lee. But to whom the palm of priority of such a discovery belongs, my present communication will afford ample means of deciding. At all events, I have shown enough, I trust, to prove, that I was neither presumptuous nor inaccurate (as stated by one or two individuals) in maintaining that there exists a singularly-striking similarity between Lauth and Dr. Lee,—that Mr. North, in his ardent defence of the latter gentleman, was too hasty in asserting that he could not trace the shadow of resemblance beyond what must exist between the papers of any two men writing on the same subject,—that Dr. Copland must have superficially looked over both papers, holding “one in each hand,” when he states, that he found *nothing in common between them!*—and, lastly, that the commissioners to whom was intrusted the duty of deciding whether Dr. Robert Lee’s paper contained views sufficiently novel on the structure of the placenta to deserve a place in the *Philosophical Transactions*, and who decided in the affirmative, could not have seen Professor Lauth’s paper.

Before I conclude, I must be allowed to add, that there is one part of Dr. Lee’s anatomical statement, which Professor Lauth does not specifically touch upon, and that is, the distribution of the large openings of the uterine vessels in the inner membrane of the uterus (page 60), over which the placenta covered by the decidua is directly applied. But as this observation is not claimed by Dr. Lee as a discovery of his own, inasmuch as he has represented it by a drawing taken from one of those predecessors (Ræderer) with whose accuracy he has found fault; it cannot, of course, form a part of the present analytical parallel.

After all, the reader of THE LANCET, and the editor in particular (whose decision, grounded upon the parallel herewith transmitted, it would afford me satisfaction to receive), must naturally feel desirous of knowing how the two writers, after having cut off all sort of direct communication between the uterus and placenta, proceed to account for the phenomena of growth in the fœtus, and change in the blood that circulates through it (if any take place), dependent as those phenomena must be, on some sort of connexion between the mother and her offspring. And here it is where the only and real difference between Professor Lauth and Dr. R. Lee’s papers exists; but sorry am I to add, that that difference redounds highly to the credit of the former, who, like an eminent and minute anatomist, as he has always proved himself to be, pursues the subject of the structure of the placenta further than Dr. Lee has done, to

account for the phenomena in question. Thus, while the latter is satisfied with a mere conjecture, thus expressed:—“Whatever changes take place in the foetal blood, must result from the indirect exposure of this fluid, as it circulates through the placenta, to the maternal blood flowing in the great uterine sinuses,” to account for physiological results, which can only be proved anatomically; the former proceeds to describe the existence of certain lymphatic vessels between the foetal vessels, and those of the decidua placed over them, and which he states to have observed, which are capable of modifying the blood of the mother, in an indirect manner, so as to fit it for the peculiar objects of the growth and nutrition of the fœtus. Mr. North, at the meeting of the 9th, chose to be merry on the subject of lymphatics, at the expense of the Professor of Strasburg, and pretended that, in good truth, the whole matter of that professor’s paper was contained in the last few lines, where he announced the existence of such lymphatic vessels. But he laughs best who laughs last; and inasmuch as my present communication will show that Mr. North was not quite correct in the second part of his averment, so will a future communication prove to him also, that the notion of lymphatics in the placenta is not a subject of so much jocularity as he imagines. I am, Sir, your obedient servant.

A. B. GRANVILLE, M.D.

Grafton Street, Berkeley Square.
20th March, 5 a.m.

GLASGOW FACULTY OF PHYSICIANS AND SURGEONS.

To the Editor of THE LANCET.

SIR,—As the ministry are about to bring in a bill to amend or abolish corporate bodies, I trust the medical corporations will not be forgotten.

The “Royal commission,” of which we used to hear so much, and of which we now hear so little, was to look after the affairs of the Scottish university.

It were well if they looked after the manner in which diplomas are granted by the College of Glasgow. Hitherto, degrees in medicine and surgery from that place have been considered respectable, but this feeling is fast diminishing. The trifling nature of the examination is destroying it. The examination is no test whatever as to the qualifications of the individuals examined. I could undertake to pass my youngest apprentice there with a few weeks preparation. I was educated at Glasgow myself, and would ill like to see the College go to ruin. It is on the road, however.

I want to point attention at present chiefly to a body which is not included in the Royal Commission, I mean the “Faculty of Physicians and Surgeons of Glasgow,” a corporation (of which I am a member) holding its charter from Charles the Second, and in which there are to be found even more than the usual number of abuses. The facility with which boys and others can enter the profession through this *thoroughfare*, has become a matter of serious consequence. The house-painter leaves his colour-pot, the tailor his board, and the blacksmith his anvil, and, with a little of the usual oil, gets himself dubbed “surgeon.” To such persons, in many instances, are the lives of his Majesty’s subjects intrusted in that quarter of the world! In this way the west of Scotland and part of Ireland have become inundated with practitioners—half boys—half men—many of them every way so contemptible both in manners and education, that the writer of this has been unable often to conceal his disgust on coming in contact with them. Very many of the regular practitioners of the place are men of the highest scientific acquirements, and I believe nowhere are better-educated men to be found; but the present practice of throwing loose upon society, with an examination which may be laughed at, swarm after swarm, hundred after hundred, of raw, half-educated lads, is a lamentable illustration of the modern “march of improvement.”

The north has been celebrated as the *officina gentium*, and these modern northern seem determined to make the character appreciable to northern medicine also.

Reform from within will prevent the necessity for reform from without. Perhaps the day is not distant, when some one will be found sufficiently bold and competent to cleanse the Augean stable of medical abuses commonly called “The Faculty of Physicians and Surgeons of Glasgow.” I trust they will take the hint from

ONE OF THEMSELVES.

London, March, 1833.

OCCUPATIONS OF MEDICAL APPRENTICES.

To the Editor of THE LANCET.

SIR,—Amongst the numerous enactments relating to the medical profession, there is none more absurd in character, or more injurious in tendency, than that which enforces an apprenticeship of five years duration upon every candidate for a certificate from Apothecaries Hall, without specifying any course of study to be pursued during that long period.

That some apprentices are fortunate enough to enjoy great opportunities of im-

provement during the term of their indentures, I, for one, am bound to acknowledge; but, on the other hand, it is notorious, that by far the greater portion of them are doomed to pass their whole time “in the wholesome and intellectual employment of rolling pills and papering bottles.” It is strange that such a state of things should ever have existed, but far more strange that, in this enlightened age, it should be permitted to continue. It is undoubtedly right that medical students should possess a knowledge of pharmacy, but cannot that be obtained in less time than five years, and at less expense than the average amount of an apprentice fee? and surely if the pupil is to be bound to do his utmost to promote the interests of his master, ought not the master in like manner to be bound to promote the interests of his pupil?

The English apothecary, it should be remembered, is not a mere mixer of medicines, but the physician of a large and important portion of the community. Considering then the important station which he sustains in society,—a station in which not unfrequently the happiness of whole families depends upon the exertion of his individual skill,—is it right, I ask, that five-sevenths of the time usually devoted to his professional education should be wasted in mere manual drudgery? I have the honour to remain, &c.

Finsbury.

GEO. E. EACHUS.

ADDENDA TO DR. HOWISON’S REMARKS ON THE MALIGNANT CHOLERA.

To the Editor of THE LANCET.

SIR,—Will you have the kindness to insert the following in your valuable publication.

In my former remarks upon epidemic spasmodic cholera, contained in THE LANCET for November 17th 1832, I addressed four queries to the medical profession, to which no individual has as yet thought proper to give any answer. To these I now beg leave to add a fifth:—

Have medical men, in their laudable and praiseworthy attempts to alleviate or cure spasmodic epidemic cholera (I allude in particular to venous injection), benefited the individual to any extent whatever? On the contrary, have they not tortured the latter hours of departing life?

I also beg leave to modify to a considerable extent, my ideas of the general fatality of the epidemic spasmodic cholera, as thrown out in my former paper. It is now my belief that the spasmodic epidemic cholera, when it first attacks a place, rages with excessive violence; and no individual

seized with it at that period ever recovers. By degrees, however, after a duration of greater or less extent, the virulence of the disease gradually becomes mitigated of its own accord; and at that period individuals so attacked recover, although in a small proportion. At length the disorder wears itself out, ceasing to exist altogether. And the above changes take place independently of medicine and of medical men.

Your obedient servant,

W. HOWISON, M.D.
Edinburgh, 9, Nicholson-square,

CRANIOLOGY—ARMY MEDICAL ABUSES, &c.

To the Editor of THE LANCET.

SIR.—For a considerable length of time I have been in the habit of taking your valuable publication; and in the Number for January 19, 1833, under the head of the *Westminster Medical Society*, when discussing the science of phrenology, I find the chairman, Dr. Copland, alluding to the ancientness of craniology, and mentioning having seen a book published more than two hundred years back upon that subject, of which he has not been able to see another copy. I have a copy of a work published upon the same subject, and much about the same time, and should feel obliged by your informing me whether it be the one the chairman alludes to. The title-page I have copied here.

"De Humana Physiognomonia, Joannis Baptistae, Portae Neapolitani. Libri IV.

"Qui ab extimis, quæ in hominum corporibus conspicuntur signis, ita eorum naturas, mores et consilia (egregiis ad vivum expressis Iconibus) demonstrant, ut intimos animi recessus penetrare videantur.

"Omibus omnium ordinum studiosis lectu utilit maxime jucunda.

"Editio postrema, priori correctior Cum duplici Rerum et Verborum Indice longè locupletissimo.

"Francfurti, Apud Nicolaz Hoffmannum, impensis Laredani Jacobi Fischeri.

"Anno MDCCXVIII."

Knowing how open your Journal is to the correction of abuses, I think a few pages might be beneficially dedicated to the abuses in the army medical department, showing how the best appointments are distributed to Scotchmen, and by favour, and also how little merit and long service are rewarded.

I have lately witnessed some cases of cholera spasmodica, in which all the different modes of treatment were tried, and unsuccessfully, with a single exception, that of croton oil, which was administered to a most enormous extent. In the post-mortem examinations, I found one case where the bladder contained urine. I have the honour to be, Sir, your most obedient servant,

W. W. B. DAVEY, Surg.
Beccles, Suffolk, Feb. 12, 1833.

MR. SMITH'S DISPENSARIES.

PETITION OF MR. SMITH, OF SOUTHAM, TO
THE HOUSE OF COMMONS.

"Sheweth,

"That it is the practice of overseers of the poor in many parishes, to contract by the year for attendance on their poor, when sick or hurt, and that this practice is productive of the following evils:—

"It induces many to apply to a parish for their surgeon, who otherwise would not seek gratuitous relief; and who are thus taught more readily to make application for food and clothing.

"It causes those, who are paupers, to be inadequately attended, as the surgeon finds an assiduous discharge of duty is followed by an increase of trouble without any increase of remuneration.

"It causes many to continue chargeable to a parish during convalescence, in consequence of not coming under the notice of the surgeon.

"It may produce a disposition (or the suspicion of such a disposition) to use medicine of a superior quality, often causing the poor to sell their necessaries for supplies of quack medicine, by which relief appears so cheaply offered, and is so temptingly recommended.

"Where midwifery is included in the contract, it is subversive of proper feeling, by obliging the poor women to change, with each change for parochial arrangement, their accoucheur.

"It is a great impediment to the extension of vaccination, for as vaccination is not usually included in the contract, the cases accumulate till the parish is alarmed by the introduction of the small-pox,—when, if vaccination is resorted to, it is hurried and partial, and has given rise to many of the doubtful and mixed cases, that have so much divided and perplexed the public mind.

"It is unkind to the poor, who are by this practice confined to a particular practitioner, who, experience has convinced them, is unacquainted with their disorder; who may not have seen its commencement or progress; which must necessarily be the case so long as they are turned over at Easter, or perhaps quarterly, in the midst of their afflictions, to a new contractor, chosen by individuals whose object it is to get the work done at the lowest price, and who cannot make any estimate of the skill or education of the candidates.

"It is a continual source of dissention amongst the worthiest men of the profession. Their confidence in their art is weakened by it, as they receive no support in difficult cases from each other, and their

estimation and character are thus lowered in the eyes of all the district in which they reside.

"In conclusion, your petitioner further declares, that it is consistent with his belief, as well as in some degree with his actual knowledge, that if the sums paid by the parishes according to their contracts, with such sums as they pay on account of unforeseen cases, not included in them, together with the various sums collected from the poor for that medical assistance which to themselves appears the most cheap and efficacious, were judiciously employed in the formation of District Dispensaries, they might be provided without distressing their best feelings in regard to independence, at their own homes, or at the Dispensary, which would assure the most speedy and effectual re-establishment of health, combining likewise, when it might be necessary, the skill of all the practitioners within the district, and promoting unanimity amongst the profession, whilst in all probability it would ultimately remove one third of the pauper population from the parochial funds.

"Therefore your petitioner prays this honourable House, that the subject, in all its various bearings, may be forthwith referred to the consideration of a committee of the House.

"H. L. SMITH."

* * * This petition is inserted, as promised, but we are compelled, from want of space, again to defer our remarks upon it.

LONDON FEVER HOSPITAL.

MALIGNANT SCARLET FEVER.

(Written and Communicated by Mr.
PROTHEROE SMITH.)

A CASE of malignant scarlet fever, which terminated in purulent deposition in the joints, lately occurred at this hospital, under the care of Dr. Tweedie. The patient, a young woman apparently about 20 years of age, was brought into the Fever Hospital on the 3d of January last, in a state of wild delirium, and consequently unable to give any account of her previous history. The skin was cold, and partially covered with a dark copper-coloured efflorescence; the voice was hoarse; the eyes dull and suffused; and the pulse rapid, but feeble and indistinct. The state of the throat, from the violence of the delirium, could not be ascertained; the tongue was coated with a dark-brown dry fur, and the lips and teeth with sordes. She had also a discharge from the vagina, conjectured to be venereal. She

was ordered to have beef-tea freely, and a dose of castor-oil.

On the following day, the 4th, the rash was more fully developed, and of a dark, livid hue; the pulse 104, and very soft; she was nearly comatose, and only muttered unintelligibly when roused; her voice was hoarse, and she swallowed with much difficulty; the evacuations by urine and stool were passed unconsciously. The nourishment was ordered to be continued, and a draught, containing ten grains of carbonate of ammonia in camphor mixture, to be taken every four hours; a blister to be applied to the back of the neck, and a cold lotion to the head.

On the 5th she was reported to have passed a very restless night, having become so violent as to require restraint. The pulse was 110 and soft, and the breathing rapid. The same remedies were continued, and a draught, containing thirty drops of the solution of the muriate of morphia, to be given early in the evening.

On the 6th the pulse was 108; she had slept well, and was more composed and sensible; the eruption was fading; the tongue was still dry, and furred in the centre, but moist at the edges. She had one involuntary stool. *Contin. med.*

On the 7th the pulse was 112, she had passed a good night, was collected, and expressed herself to be better and stronger. One stool was passed consciously; the skin cool and desquamating; the nourishment and remedies to be continued. Next day, 9th, she had passed another good night, and was quite sensible; the bowels were twice relieved. She complained, for the first time, of both ankle and wrist-joints, attended with redness and swelling, which, on the succeeding day, were much increased. A similar tumefaction, with redness of the integuments, was then observed on the dorsum of the right-hand. She had slept but little; there was great prostration of strength, and the pulse was rapid and feeble. A pill containing one grain of calomel, with half a grain of opium, was directed to be taken every four hours, in addition to her nourishment and other remedies.

On the 11th the pulse was 140; she had passed a very restless night; her breathing was rapid; the bowels had been twice moved; the joints were more swollen and painful. Only the pills and opiate draught to be continued.

12th. She had been on the whole more tranquil, but in other respects there was no material alteration.

On the 13th the pulse was scarcely perceptible, and she was in a state of profound coma. She died in the afternoon.

Autopsy.

I examined the body fourteen hours after death, when the following appearances presented themselves:—

Chest.—All the viscera healthy.

Abdomen.—The liver was somewhat enlarged, and exhibited the *nutmeg* appearance. Several detached portions of the sub-peritoneal coat of the jejunum and ileum, particularly the latter, were highly injected and vascular. There was no disease of the mucous follicles.

Pelvis.—The bladder was very thick; its mucous surface very much injected, and in some spots ecchymosed; the peritoneal surface of the uterus was of a deep-red colour and very vascular, as was also the internal or mucous coat, which towards the os tincæ presented some dark-coloured striæ; the mouth of the uterus was very much swollen, and of a purple colour; its aperture was almost closed; the fallopian tubes were very much distended with a brown glairy fluid, and the fimbriated extremities adhered to the ovaries, which were healthy; the mucous surface of the vagina was much inflamed, was of a dark colour, and besmeared with a muco-puriform secretion.

The right ankle-joint was much swollen and œdematous; on dividing the integuments some serum escaped, and on examination of the joint I found a small quantity of pus within the capsule. Its serous lining was very red and vascular, particularly where it invested the cartilages, but was not at all abraded, nor were the cartilages softened. The right wrist-joint was also swollen. On cutting through the skin and fascia, a quantity of pus escaped, and on dissection I observed the following appearances. A deposition of thick pus, a quarter of an inch deep, on the metacarpus, under the extensor tendons of the fingers, and a formation of matter within the capsular ligament, without any lesion or softening of the perichondrium.

Head.—There was sub-arachnoid serous effusion, and the ventricles were distended with a similar fluid. The fauces bore signs of inflammatory action. Dr. Tweedie, at the lecture subsequent to this examination, took an opportunity of drawing the attention of his pupils to this case, and remarked, that though in the present epidemic, scarlet fever was on the whole of a mild type, yet he had seen several malignant cases, both in the hospital and in private practice. He particularly alluded to this case, which presented one or two points of peculiar interest; the rapid and severe form of the inflammation which had taken place in the joints; its termination in a few hours in purulent deposition within and around the articulations, having been able to detect fluctuation within twenty-

four hours of the first appearance of the local inflammation. He remarked, that such cases, fortunately of rare occurrence, were invariably fatal; he had only met with three cases of a similar kind, one of which is detailed in his "Clinical Illustrations of Fever," the second occurred in private practice, and the third forms the subject of the preceding history.

10, Kirby-street, Hatton-Garden.

LONDON HOSPITAL.

INFLAMMATION OF THE ARM FOLLOWING CONTUSION.

J. R., ætat. 30, a labouring man, of extremely intemperate habits, was admitted into this hospital on the 25th February, under the care of Mr. Andrews.

He stated, that about a week before his admission, while in an extreme state of intoxication, he fell down, and struck his elbow violently. Soon after the accident the part began to swell very much, and became extremely painful. He continued following his employment the next day until obliged to desist from the excessive pain, he at the same time drinking a quantity of beer and gin. He has been in the habit of drinking several quarts of beer, and frequently a pint of gin, in the course of the twenty-four hours.

On examining the parts at the time of his application for admission, for some distance above and below the elbow-joint the integuments were inflamed, and of a dull dark-coloured appearance; there was a sense of fluctuation communicated to the finger, but by no means well marked. Mr. Andrews, however, made an incision about three inches in length through the integuments below the elbow, which gave exit to a quantity of pus, and which could be squeezed from the cellular membrane in every direction. He was directed to keep his bed, and bury the part in a linseed-meal poultice, to take 40 drops of laudanum at bed-time, and to have a pint of porter daily.

The patient continued for several days without any symptoms requiring particular notice, until the night of the 29th, when he became delirious, and extremely violent. He was immediately ordered 40 drops of laudanum, to be repeated until sleep was induced.

On visiting him the following morning, the pulse was feeble and tremulous; countenance sunk, with a glairy appearance about the eyes, but when spoken to, he answered questions tolerably rationally. Tongue furred, but preternaturally clean and red at the tip and edges. The inflamed part appears much

the same; the discharge is of a healthy appearance. He was ordered to take two pints of porter and half a pint of wine daily, and the *decoc. pulv., and tinct. cinchon., c. tinct. opii ℥ij, 6tis hor.*

31st. Much better to-day; he has passed a comfortable night, and is now free from delirium. Countenance improved; skin warm and moist; pulse more regular. The discharge is of a healthy character, and flows freely from the incision.

It would be needless to give a daily report of this case; it will be sufficient to remark, that he continued improving. On the 25th February it was considered unnecessary to continue the allowance of wine; it was accordingly omitted, when finding his porter also taken away, he requested permission to leave the hospital.

DISEASED KNEE.—PREMATURE DISMISSAL.

The subject of this disease is a young woman, ætat. 24, married, of spare habit and sallow complexion; was admitted under the care of Sir W. Blizard. She has had disease of the right knee for about two years. She attributes its origin to a blow received on the part, followed by great pain and swelling; for which leeches, cupping, and blisters, were employed, with the effect of relieving the pain. About two months before she applied here, the part again became so extremely painful as to prevent the least motion being used; the slightest pressure on the joint in walking, caused excruciating suffering.

On examining the limb the whole joint was found to be very much enlarged; the leg and thigh were greatly emaciated. The usual prominences about the joint could not be readily detected, owing to the thickened state of the soft parts. She experienced great pain on making pressure on any part of the joint, but more particularly over the internal condyle. The treatment adopted in the first instance was, the application of leeches daily; after which, the sour wash, and confinement to bed. The leeches were used every morning for about a week, when a blister was ordered to each side the joint, the blistered surface to be dressed with the *unguentum potassæ hydriodati*. The blisters were removed as soon as the surface had healed. The constitutional treatment consisted in the exhibition of the *decoc. cinchona, c. sodæ carb.*

Under this plan of treatment the swelling and deformity of the joint became considerably diminished, and the woman's general health considerably improved. It was gratifying to observe the amendment which was taking place, when—the knight thought proper to dismiss the patient! Had she been allowed to remain some time longer, there

was very little doubt that she would have left the hospital with a very useful limb; whereas now, in all probability, by moving about, all the suffering and misery which the poor woman has so long endured will be reproduced.

FRAGILITAS OSSIUM.—There is a woman now in this hospital, ætat. 72, who is suffering from this disease. Her bones are so extremely brittle, from the absorption of the animal matter, and consequent too great proportion of earthy substance, that while lately carrying a pail of water, she slipped a little on one side, and fractured the left thigh. On the 21st, while being turned in bed, although great gentleness was used, she felt the right humerus fracture.

ERYSIPELAS.—Mr. Hamilton has been trying the effect of bandaging the leg in a case of erysipelas in this hospital, with decided benefit. The redness and tension rapidly gave way to this plan of treatment.

QUININE EXTERNALLY.—IN THE LANCET of Feb. 23, I observed a statement relative to the external application of quinine in ague, by Mr. Christian, the discovery of which he attributes to Dr. Purcell. In the third edition of the "Study of Medicine," vol. 2, page 149, Dr. Good suggests the application of finely-pulverised sulphate of quinine, mixed with serate, to a blistered surface, in cases where the stomach is very irritable, and ascribes the discovery of it to a M. de Martin, whose communication on the subject may be seen in the *Revue Médicale* for Sept. 1827.

A. N.
18, Giltspur Street.

COMBINATION OF ACETIC ACID AND ACETATE OF LEAD.—In the 14th Part of the Cyclopædia of Medicine, article "Menorrhagia," the following remark occurs:—"It is very satisfactory that Dr. A. T. Thomson has lately proved, that the addition of acetic acid to the acetate of lead, so as to make an excess of acid, entirely prevents the deleterious property." This passage reminds me of a case which lately fell under my observation, where this practice was fairly tried, but without any such satisfactory result. The case was one of hemoptysis. The acetic acid was given very freely, with the double purpose of correcting the deleterious effect of the lead, and gaining the benefit of its astringent property. After using it a short time, the acetate of lead produced a spasmodic affection of the sphincter vesicæ, so violent as to prevent

the introduction of the catheter, until it was allayed by opium and other remedial measures.

JOHN MITCHELL.

Leeds, Feb. 26th, 1833.

ATTENDANCE AT THE COLLEGE OF SURGEONS.—Some members of the College of Surgeons, who attended Sir CHARLES BELL'S excellent introductory lecture on the 5th inst., consider that the number present was greater than was implied in THE LANCET of that week. The statement in the report was, nevertheless, strictly correct. Those who criticise the passage must have had their seats at the lower part of the theatre, which was, of course, fully occupied. But the upper benches, the last filled, were so far from being crowded, that from thirty to forty persons might have found comfortable, though close, sittings on them. Lest this estimate, however, should still be considered as incorrect by those who may have thought the theatre well filled, because in their own immediate neighbourhood there was no superfluity of room, the following fact may be quoted:—A greater number of students attended than could find room in the gallery, and when the cry was set up, of "Sit closer;" "If you will sit closer there will be plenty of room,"—some of the students contemplated scaling the barrier between them and the amphitheatre, and filling the vacant seats beneath. A dread of interruption, however, prevented the movement. An examination of the central panel in the partition was first of all made, in the expectation that it was a door, but this proved not to be the case, or a mixture of students and members would assuredly have been made.

MR. GRAINGER.—A Webb-street student makes the following complaint:—"Mr. Grainger greatly neglects his surgical class, inasmuch as that gentleman has only lectured one fortnight during five months from the commencement of October to the present time, the lectures thereby devolving on Mr. Pilcher, an arrangement which is by no means satisfactory. Mr. Grainger's constant plea is 'ill health;' but if that be the reason, why did he allow his name to

be inserted in the advertisement for this season, when he knew that his health did not permit him to attend last season? Menaces have also been held out, that if the pupils are not regular in their attendance, certificates will not be granted. How can Mr. Grainger expect punctuality in others, when he sets so bad an example? This complaint is not made from any feelings of pique, but for the sake of having justice done to my fellow-students and myself."

HONOURS TO SIR ASTLEY?—Louis Philippe, king of the French, has most graciously bestowed the Royal Order of the Legion of Honour on Sir Astley Cooper, Bart., which he received from the hands of Prince Talleyrand.—*Court Circular*, March 12th.

LATE ELECTION AT THE MIDDLESEX HOSPITAL.—A correspondent, under the signature of "*Justitia*," writes to us as follows:—"March 11th, 1833. I observe in the last week's LANCET an allusion to the election for the assistant-surgeon at the Middlesex Hospital, allow me to add, as a matter of strict justice, a few words:—the friends of Mr. Shaw charge Mr. Mayo with using, not nepotic influence, but having recourse, according to report, to misrepresentations and untruths; and this not to show his love for Mr. Tuson, but to gratify a paltry and little-minded spite against Sir Charles Bell."

CORRESPONDENTS.

MIDDLESEX SOCIETY. The report last week should have been dated the 12th inst.

Juvenis. We never insert such statements unless they are authenticated. The publication, if untrue, might expose us to an action for libel.

C. We shall not relax our exertions.

Z. Z. Z. "Scraps" will be always acceptable. "Pig-headed Roderick" in the newspaper is good.

Our remarks on Clot-Bey's mode of performing lithotomy, must again be postponed.