

have been observed elsewhere. Our examinations at King's College Hospital are entirely in accordance with those which were made with so much care and accuracy by Dr. Parkes, who thus summarises his conclusions:—"The essential symptoms of the algide cholera—namely, the loss of animal heat; the loss of voice, proceeding from the diminished volume of air in the lungs; the colour of the surface; and the arrest of the circulation; coupled with the collapsed state of the lungs after death, their deficiency in blood (although blood may be in the large vessels), the contraction of the left cavities of the heart, with the empty arterial system,—prove satisfactorily, as it appears to me, that there is a failure more or less complete in the transmission of the blood through the lungs."*

Dr. Rees admits that collapse may occur with but a moderate amount of discharges; and he believes that the cholera poison has a depressing influence on the heart somewhat similar to that exerted by tobacco, "the blood passing freely through the lungs and stopping on the left side of the heart." This theory is not in accordance with the morbid anatomy of the disease as I have observed it, and as described by Dr. Parkes and others; neither is it supported by the results of treatment. How can Dr. Rees reconcile with this theory the marvellous temporary benefit from hot injections into the veins, the great and permanent relief sometimes afforded by venesection, and the acknowledged failure of alcoholic stimulants to improve the pulse and relieve the other symptoms of cholera collapse?

Dr. Rees is surprised that one patient who inhaled nitrous oxide gas, and another who inhaled diluted oxygen, got no benefit from the inhalation; and he therefore assumes that the blood undergoes some change "which destroys its attraction for oxygen." This assumption is unnecessary, and it is opposed to undoubted facts. It is unnecessary if we admit that the mass of blood is arrested in the minute branches of the pulmonary artery before it reaches the capillaries, where it should be exposed to the air. The small volume of blood which passes through the lungs is sufficiently aerated: what is required is, not more oxygen to combine with the blood, but more moving blood to take up the oxygen in its passage through the lungs. Again, Dr. Rees's assumption is opposed to the fact that when cholera blood is brought into contact with air it immediately undergoes the usual chemical changes. This is seen when either the hot venous injection or venesection sets free the pulmonary circulation, and thus removes the impediment to the respiratory changes; when a thin layer of blood out of the body exposed to the air at once assumes the arterial hue; and when the lung artificially inflated after death rapidly becomes florid. These are unquestionable facts, and they cannot be reconciled with the hypothesis of a mysterious change which destroys the natural affinity of the blood for oxygen.

Dr. Rees compares the choleraic discharges with the sweats of acute rheumatism. The comparison is a fair one, but there is one important difference between the two diseases. The acid sweats of rheumatism, when they reach the surface of the skin, are out of the body, and they require only to be absorbed and removed by woollen clothing; whereas the choleraic secretions, when thrown into the bowel, may be retained and reabsorbed into the blood. If Dr. Rees has read the book to which he refers, he is aware that I do not propose to increase the choleraic discharges, but only to prevent their retention and accumulation within the bowel; and I would ask him whether a practitioner who looks upon cholera as only an intestinal flux to be arrested by opiates and astringents is more scientific, or likely to be more successful, than one who, believing rheumatic fever to be a sweating sickness only skin deep, makes it his chief business to repress the sweating by exposure of the surface to cold? In both diseases the discharges are a necessary part of that process by which the *vis medicatrix* effects a cure; and the business of the physician is to assist, and not to oppose, the curative process.

Dr. Rees has "not heard that anyone presumes, in the present day, to attack rheumatism with diaphoretics." I presume that the hot-air and Turkish baths must be considered diaphoretics. These means have been largely used by many practitioners in the treatment of rheumatism, and I can assure Dr. Rees that I have found them more efficacious than any drugs, not even excepting lemon-juice. I believe that for cutting short an attack of acute rheumatism we have no means so effective as the diaphoretic action of the hot-air bath, when employed at the very commencement of the disease.

I am, Sir, your obedient servant,

Savile-row, Dec. 31st, 1866.

GEORGE JOHNSON.

* Researches into the Pathology and Treatment of Cholera; by E. A. Parkes, M.D.; p. 105.

CLITORIDECTOMY.

To the Editor of THE LANCET.

SIR,—In your impression of last week, there appears a letter from Dr. Greenhalgh, to which I beg leave to offer a word or two in reply.

1. In reference to a case sent to me for operation by Dr. Greenhalgh, he says:—"We know, on the best and most unquestionable authority, that Dr. West never has advised, and never would advise, clitoridectomy for the object specified, and we are asked to believe that he made an exception to this rule in favour of a patient whose case he had not personally diagnosed, and whom he had never even seen. I deny that I ever recommended removal of the clitoris for self-abuse. The patient just referred to stated that she could not and would not discontinue the habit to which she was addicted. Mr. Brown urged the excision, and promised it would effect a cure. Mr. Brown must know as well as I do that when I was appealed to I declined to express an opinion, stating that I had no experience, and could have no faith in the result thus confidently promised." These are Dr. Greenhalgh's own words on the 26th December, 1866. Nothing material is omitted from the sentence, as your readers will see by referring to your last number. My reply is also in Dr. Greenhalgh's own words, written in May, 1865, and, of course, sent to me before I ever saw the patient. I enclose you the original document:—

"76, Grosvenor-street, Grosvenor-square, May 24th, 1865.

"MY DEAR BROWN,—I am ashamed at this tardy reply to your kind and satisfactory note, which is all I can wish, and believe me removes any and every unfavourable impression I entertained concerning your transactions with me.* I hope to bring you a case for removal of the clitoris in a few days. The subject is a single lady, about 44 years of age, who has been under Simpson, A. Farre, and, I think, Dr. West, for many months without any permanent, and but slight temporary, relief from pruritus, from which she has suffered about eight years. Simpson some time ago divided the nerves of the clitoris on one side without the slightest benefit. I have exhausted every remedy, I think I may say, ever suggested for the cure of pruritus, so there is no other course open but removal, which you perform with such great dexterity. I enclose her niece's letter.

"Believe me, sincerely yours,

"R. GREENHALGH.

"P.S. Please return the niece's letter, which is sent confidentially."

Is a single word of comment necessary? I regret deeply that any disagreement should have arisen upon a matter of facts; but I was not the aggressor.

Into the other matters in Dr. Greenhalgh's letter I shall not enter. Indeed, the most material parts of my complaints against his conduct he absolutely admits. Dr. Greenhalgh attended the Home for three years and a half, and paid us, as the book will show, numerous visits: he would have conferred an obligation upon me, if, on any occasion during that long period, he had pointed out anything in any way objectionable.

I am, Sir, your obedient servant,

Harley-street, January 1st, 1867.

I. BAKER BROWN.

To the Editor of THE LANCET.

SIR,—I am sorry you have brought me into the clitoridectomy controversy. My convictions were clearly stated before the Obstetrical Society; nor is it needful that I should again refer to them. Upon one point, however, I trust you will allow me to reply. I stated that I believed clitoridectomy was only extensive circumcision, and the following reasons will, I hope, bear me out. By reference to Dr. Savage's work on the Female Pelvic Organs, you will see the following views, both figured and detailed, and I gather from subsequent conversation with him that such are his opinions. (See plates ii. and iv., with explanatory notes thereon.)

The clitoris consists of the two corpora cavernosa ("the latter greater in proportion than in the male"). Owing, however, to the position of urethra only a small portion of the corpus spongiosum extends into it. The analogues of the

* This was in reference to a disagreement between Dr. Greenhalgh and myself, in which he had imputed to me a breach of professional etiquette.

corpus spongiosum and intermediate veins of communication with the corpora cavernosa are to be found in the double bulb of the vagina, the venus plexus round the urethra, the plexus around the vagina, the various plexuses connected with the sides of the uterus, and the pampiniform plexuses connected with the bulb of the ovary. (See explanations, plate iv.)

During the venereal orgasm it is not the clitoris or the crura alone which become erected, though, like the glans penis, the clitoris may be the most sensitive part, but the entire of this erectile system becomes turgid; and it is by means of this turgidity that the vagina is made tighter, the uterus erect to receive the semen, and the Fallopian tubes brought down in apposition to the ovary so as to receive the ovum. If these views are correct—and I imagine Dr. Savage's plates are confessedly correct,—until you can remove all these parts named, with the clitoris, you have not removed the analogue of the penis in clitoridectomy.

I am, Sir, your obedient servant,
Montague-square, Dec. 26, 1866. C. H. F. ROUTH.

To the Editor of THE LANCET.

SIR,—At the recent meeting of the Obstetrical Society the speakers in the somewhat stormy discussion on clitoridectomy confined themselves almost solely to the relation of cases that had come under their own observation, and no attempt was made to explain the results which Mr. Brown professes to have obtained from his practice.

I think we are bound to assume, at the very least, that when a surgeon of Mr. Baker Brown's status brings forward a number of successful cases, a large proportion of his operations must have been followed by either a temporary or permanent relief. But it is by no means necessary that we should also accept his theory or explanation. There are several other modes of accounting for the results; and it is to one of these, which I believe has not been sufficiently dwelt on, and which seems to me capable of explaining a large number of his cases, that I would venture to call attention.

It is quite certain that a great majority, if not all, of his patients were women of a highly nervous, susceptible, and hysterical temperament. Now we well know that if we can only succeed in impressing a patient suffering from any form of hysteria that a given method of treatment is certain to be successful, it follows, as surely as night follows day, that when that method is resorted to a cure will be effected. It is this principle which accounts for the occasional success of which any strange or irregular method of treatment can always boast.

I need only trouble you with one example, and I trust Mr. Brown will pardon me the illustration. There is, or was, not long ago, in Brighton, an individual who professed to cure paralysis and white swellings, and a variety of other formidable complaints, by frictions along the spine and the application of strong adhesive plasters. And he certainly did effect some very remarkable results, which must have seemed almost marvellous to a non-professional observer. One case of hysterical paralysis which recovered under his care I myself know of, and it might almost vie with the similar one which Mr. Brown narrated at the Obstetrical Society. But surely no medical man could be found to maintain that the rubbings or the plasters had anything to do with the cure.

On the same principle, if you say to an hysterical patient, "You are suffering from peripheral irritation, and if you submit to a somewhat formidable operation, which consists in the removal of a portion of the body which is the *fons et origo mali*, you are quite certain to get well," is it matter for surprise that a successful result, either temporary or permanent, should follow? I think I may safely venture to say that it would baffle the ingenuity of man to invent a treatment more likely to produce a strong mental impression on the unfortunate class of patients for whom it is recommended than this operation of clitoridectomy.

Whether it be justifiable to resort to a method of treatment which not only involves a distressing and irreparable mutilation, but which also in its very essence, if this theory is correct, depends for its success on an appeal to the imaginative faculties, the morbid condition of which is at the root of the disease, or whether such a cure is likely to be permanent, are very serious questions which I must leave for abler hands to discuss.

I trust it may not be entirely useless thus to draw attention to one amongst the many explanations which may be given of

Mr. Brown's cures; for I am very sure that if those of us who are opposed to his treatment can only show that it is based on erroneous theory, and is altogether unscientific in its nature, we shall attack it much more effectually than by vague denunciations, or even by the narration of unsuccessful cases.

I am, Sir, your obedient servant,
W. S. PLAYFAIR, M.D.

Curzon-street, Mayfair, December, 1866.

To the Editor of THE LANCET.

SIR,—I perfectly agree with Mr. I. Baker Brown that "one fact is worth a dozen theories," but I think he never made a more unfortunate mistake to prove it than by quoting the case of Miss B——, mentioned in Mr. Smith's letter from —, and which, with your permission, I will endeavour to show. This Miss B—— was placed under my care at the end of July, 1865, suffering from hysteria, associated with amenorrhœa, which, after a lapse of eight months, ended in an attack of decided hysterical mania, in consequence of which I required the parents to "certify" to her being of unsound mind, so that I might retain her, legally, in my house, or else she was to be removed immediately. This latter step was, with regret, adopted, and, by their wish, I accompanied her to some apartments in London, which had been prepared. Much to my astonishment, I discovered also that arrangements had been made for this young woman to have the clitoris excised, as, *in less than twenty minutes after our arrival* at the house, a nurse from the "Home" came with the elastic bandages and other appliances necessary for restraint, and, in spite of my assurance to Mr. Brown's usual assistant (Mr. Brown himself not having at that time arrived, but did so a few minutes after I left) that it was a clear case of hysterical mania arising from amenorrhœa, and that my acquaintance with mental diseases satisfied me that she would come right when the catamenia became regular, this young woman, aged twenty, was, nevertheless, there and then operated upon by Mr. Brown, who had never seen her in his life before, and knew nothing of the case but what, I understand, he might have gleaned from her father! So much for the "zeal" one can display when an opportunity offers for the practice of any particular "theory." And now, Sir, let me show the mistake Mr. Brown has made by quoting Mr. Smith's letter as a proof of the case of Miss B—— being a "fact" likely to corroborate his views, as stated in his reply to Dr. West's letter.

Mr. Smith states that "Miss B—— had then (i. e., about Nov. 15th) been with him over six months, and was *now* calm and quiet," and, in fact, a reasonable being; "but when she first came to him she was suffering from all the symptoms of hysterical mania"—that is to say, in other words, that Mr. Smith found her some *two or three months after* the operation in the *same* condition as she was on March 26th, when Mr. Brown received her from me; so that, altogether, it seems to have taken a period of *eight months* from the date of the operation before Mr. Smith could give such an account as appears in the letter to his friend Mr. Brown, dated the 15th ultimo.

Now, Sir, I will not comment upon this long interval which elapsed, but let me ask why, in all fairness, there was no allusion made to the return of the catamenia?—irregular, as I am aware it has been, but which had shown a slight appearance just prior to Miss B—— leaving me, and, as some encouragement, I assured her parents might eventually become perfectly right; and I think Messrs. Brown and Smith can testify to such having been the case, although at distant intervals, during their acquaintance with her. But again I ask, why not tell us of this change instead of confining the attention of the profession to an operation which I rejoice to see has so very few supporters?

With many apologies for thus trespassing on your columns,
I am, Sir, yours faithfully,
Margate, Dec. 10th, 1866. ALBERT P. OWEN, M.R.C.S.

To the Editor of THE LANCET.

SIR,—My name, in connexion with those of Sir J. Simpson and several others, having been alluded to in your last impression respecting the above subject, I feel called upon, and I think it the duty of every person who has performed the operation, to lose no time in publicly stating the result of his experience, in order, if possible, to bring this vexed question to a speedy

termination—feeling that our friend, Mr. B. Brown, who, by his scientific and bold operations, has proved himself a public benefactor, is just now in anything but an enviable position.

As regards myself, I must confess I have not met with the success that, from printed statements, I had anticipated; but I have great pleasure in bearing testimony to a case on which Mr. Brown operated which resulted in a *perfect* cure.

I am, Sir, your obedient servant,

Dover, Dec. 28th, 1866.

ALLEN DUKE.

BIRMINGHAM.

(FROM OUR OWN CORRESPONDENT.)

OVARIOTOMY has been twice performed here during the past month, no cases having been operated on at either of the hospitals for about twelve months previously.

The first case occurred at the General Hospital, under the care of Mr. David Bolton, and proved very favourable for the operation. The cyst was of fair size, unilocular, and free from adhesions, so that on the completion of the incision the tumour was easily drawn out of the abdominal cavity. The pedicle was treated with the clamp, and the wound was closed with a loss of scarcely an ounce of blood. But, as if to show the uncertainty of the result, even under the most skilful hands, peritonitis followed, and the patient died on the fourth day.

In the second case, Mr. Gamgee operated at the Queen's Hospital. The tumour was moderately large, and scarcely, if at all, movable, either before or during the first part of the operation. One cyst was evacuated, the operator carefully preventing the passage of any of the fluid into the peritoneal cavity; and then a second cyst, which dipped deeply down into the right iliac fossa, was found to be the cause of the immobility of the mass. When this cyst was drawn up, the residue, which consisted of pseudo-colloid matter, was easily turned out of the abdominal cavity. No adhesions were encountered; the pedicle was treated with Mr. Clay's clamp. The patient's pulse fell to 80 a few hours after the operation, and afterwards never rose above that rate. The clamp was removed on the third day, the last suture by the sixth. The recovery has been throughout uninterrupted. The chief interest of the case, apart from the operation, consists in the striking manner in which it illustrates the difficulty of founding any opinion as to the existence of adhesions on the mobility of the tumour.

The profession in Birmingham have long felt the want of a Medical Reference Library, and attempts have from time to time been made to form one. Some years back the Pathological Society amassed a considerable number of books, which on the Society's dissolution were transferred to the Birmingham Library, in the hope that new medical works would be added annually. This hope, from causes difficult to determine, was not fulfilled; and year after year the old books grew dustier, and fewer new ones found their way to join them. In 1860, the Midland Medical Society arranged to hold its meetings in the medical room of the Birmingham Library, and took charge of the books, with the intention of rendering the collection more complete. The task was too heavy a one, however, for a society with only seventy members, and was at last of necessity abandoned when the Society was compelled to vacate the room to make more space for the general subscribers of the Library. The medical books have since then been shut up in a small room. A few months back a Book Society was established at the Queen's Hospital, with the object of collecting all the British and the chief French and German medical periodicals, as well as books, in the hope of thus forming the nucleus of a future library. This Society still exists, and promises to prosper. Another noble attempt has just been made at the General Hospital by the offer of Dr. James Johnstone, the consulting physician to the institution, to give the whole of his large library to the hospital. Mr. Crompton has generously followed his colleague's example, and the Hospital Committee have determined to fit up a suitable room for the reception of the books. If a united effort of the profession in the district could once be obtained, and the materials now collected in different places be gathered together, a good reference library might be quickly founded. The task, too heavy for any single institution, would prove easy if every practitioner in the town would lend his aid.

The readers of THE LANCET have already been informed of the deplorable *fracas* at the Lying-in Hospital. The non-definition of the duties of the Ladies' Association has undoubtedly been the cause of the mischief, but the present crisis in the affairs of the hospital has long been imminent. The charity has hitherto done very little work in proportion to its income, as shown by the fact that in the year 1865 an expenditure of £1085 only sufficed, with *unsalaried* medical officers, to relieve 103 in-patients and 1690 out-patients. Six skilful practitioners in the town might have been found to attend all the out-patients at a considerably smaller cost, while the general hospitals could have easily accommodated the cases of disease. If the institution survives the storm, a new *régime* must begin; if not, the maternity departments of the General Dispensary and the Queen's Hospital will supply any want. For the honorary surgeoncy, vacant by Mr. Orford's resignation, Drs. Warden and Earle and Mr. Hoffman have announced themselves as candidates; Dr. Warden's election is certain.

Birmingham, Dec. 31st, 1866.

P A R I S.

(FROM OUR OWN CORRESPONDENT.)

THE annual public meeting of the Academy of Medicine took place on the 11th inst., with the usual accompaniments of this solemnity. The house was crowded, and at the foot of the tribune sat a circle of ladies. M. Dubois' report on the prizes which had been distributed during the last academic year was attentively listened to—such is the grace which this talented gentleman gives to all the subjects, even the most barren, which he handles. But the great attraction of the day was the discourse of the secretary of the Academy for the year, M. Béclard. Rarely has an orator met with such a sympathetic audience and received so complete a triumph, and, it must be added, so well-merited a triumph. The subject of his discourse was the panegyric of Gerdy, the well-known medical author, surgeon, professor, and academician, who died in 1856, and whose eulogy had been twice before pronounced at the Faculty of Medicine and the Society of Surgery, by M. Nélaton and M. Broca. The choice of this subject, says the *Union Médicale*, by a rising orator, and for such a public occasion, surprised some people. The peculiar character of Gerdy, and the circumstances which it sometimes entailed, made it a difficult and delicate subject to handle; whilst the nature of his works and the general outline of his life did not afford matter for oratorical effect. If so, M. Béclard's merit must be magnified. His discourse was a model of true eloquence: remarkable for elevation of thought, warmth and sincerity of feeling, and an exquisite sense of propriety, couched in beautiful language. His valuation of the scientific career and discoveries of Gerdy—of his works, which embrace such a wide field of medical literature—of his abilities, more remarkable for boldness of conception and ingenuity of thought than for depth or power of mind; his estimate of Gerdy's character, a man of singular habits, of austere life, of great independence of thought and character, an enthusiastic lover of truth and justice, but whose aggressive and inflexible temper and want of moderation spoiled his better qualities; his estimate of the orator, who had been during twenty years one of the most active and eloquent combatants in the debates of this Academy, but whose violent outbreaks and unparliamentary language had more than once perturbed the even course of these debates,—evinced such impartiality of judgment, was accompanied by such ingenious remarks, and conveyed in such delicate and well-chosen terms, that it drew forth the warm applause of the audience. But where the orator reaped his brightest laurels was at that point of his discourse when, referring to the different *concours* at which Gerdy had earned his professorial chair and other scientific honours, M. Béclard expressed his admiration for the system of public competition, and his deep regret at its abolition. He attributed to this abolition the many evils which have impaired the prestige of the Faculty, the decline into which it is falling, the decay of free teaching in Paris, and the absence of all emulation. The Professorship was no longer a vocation, but a stepping-stone to worldly advancement. No, the *concours* was not, as its detractors would have it, an easy prize for speaking men, to the detriment of thinking men; and even if it were so, why should the mass of men capable of teaching be sacrificed to a few exceptional natures who may be wells of erudition but