

ON PLACENTA PRÆVIA.

To the Editor of THE LANCET.

SIR,—Your last number contains three cases and some observations "On Floodings before Delivery, arising from Adhesion of the Placenta to the Os and Cervix Uteri," by Dr. Barnes; and as it is a subject in which I feel a deep interest, I have written these few remarks, to draw, as courteously as possible, his attention to my published papers, which I feel charitably disposed to believe he had not seen.

Dec. 10th, 1844, I delivered a lecture on galvanism applied to the treatment of uterine hæmorrhage, published in the *Provincial Medical and Surgical Journal*, Dec. 24th, 1844, in which I mentioned the names of several gentlemen who had adopted the practice of first entirely detaching the placenta. Reference was also made to a number of cases of spontaneous expulsion of the placenta. Afterwards, in the same *Journal*, were inserted a number of propositions intended to point out the precise but modified treatment applicable to different cases of placenta prævia, in which were included cases of partial presentation of the after-birth. The controversial remarks which were afterwards made went to prove to whom the merit belonged of originally suggesting entire detachment as a principle of practice. Since then, I have inserted a case of complicated placenta prævia, with distortion of the pelvis, in which this practice was first adopted, and the child's head afterwards opened. (*London Medical Gazette*, Oct. 24th, 1845, page 1110.) The same *Journal* (Nov. 14th, 1844, page 1246) contains a statement of the sources of bleeding in cases of placenta prævia; and (Nov. 21st, 1845, page 1291) a few observations on the mode &c. of performing the operation. In a recent number of *THE LANCET* (No. ix. Feb. 27th, 1847) will be found further observations on the sources of hæmorrhage in cases of placenta prævia.

Dr. Barnes, I trust, will find that I have already stated, I hope truly, that there are more sources of bleeding in these cases than one. He will also see that I am an advocate that a modified plan of practice should be used in these cases. The use of the plug I warmly advocate, but its abuse I as strongly condemn.

I hope Dr. Barnes will read this letter with the same spirit it is written in, as it is alone my object to draw his attention to the state of facts. And as to any personal concern in the merit of having done anything to draw the attention of the profession to this important subject, I shall say nothing, but leave it to be decided by the respectable, unbiassed, and liberal members of it. I will, however, say, that I shall lay before them, at some future time, a full statement of the whole matter, with references to all publications, in order that merit may be awarded to the party to whom it is due.

I have the honour to be, yours most respectfully,
 Manchester, March, 1847. THOMAS RADFORD.

TREATMENT OF NEURALGIA.

To the Editor of THE LANCET.

SIR,—It is not my intention, nor would it be suitable, that I should notice all objections to any principles or details of treatment which I have brought forward in the "Alphabetical Notices" which you have favoured with a place in your valuable columns. I think, however, a very few observations on a paper of Dr. Allnatt's, which appears in last *LANCET*, may be made practically useful in more ways than one.

Dr. Allnatt objects to me and others treating neuralgia on the "narcotico-tonic" system, as he designates it. I am sorry I have not perused the work which Dr. Allnatt himself informs us he published some years ago, on Neuralgia, and its Treatment; but he tells us the work was intended to establish that "neuralgia almost invariably arises from some aberration of function of the chylopoietic viscera," which doctrine he also acknowledges was condemned at the time, "as of too sweeping a nature."

This part of the subject, however, we dismiss for the moment, to examine briefly the case with which Dr. Allnatt fortifies his own opinions, and treats with some contempt and severity my own. A gentleman called on him a fortnight since, with neuralgia of the nerves of the cheek and brow; "his appetite was good; the functions of the stomach apparently uninjured; the liver secreted a due quantity of bile; the bowels acted with regularity, and nothing could be detected, after the most minute search, to account for the agonizing pain in the peripheral expansion of the sentient nerve." Dr. Allnatt, gave (as he informs us his custom is) a preliminary course of active purgatives, which aggravated to a considerable extent all the symptoms. Disregarding this,

Dr. Allnatt carried on the purgation, and at length, "undetected scybala, in large quantities," were evacuated, and the patient was cured.

There is nothing in the smallest degree remarkable in this case, except Dr. Allnatt's error of diagnosis. At the very moment that "nothing could be detected," and that, "after the most minute search," to account for the neuralgia, "undetected scybala, in large quantities," loaded the abdomen? This case requires no comment. It is one which Dr. Allnatt is quite welcome to class among those due to derangement of the chylopoietic viscera.

But what if Dr. Allnatt's diagnosis had been correct; what if the digestive organs had been, not "apparently," merely, but positively "uninjured;" what if there had been not only no "undetected" scybala, but no scybala at all (to a morbid extent, I mean) in the bowels; what, I ask, in such a case, would Dr. Allnatt have done? Dr. Allnatt will not deny such cases are. If he did, I would dare to say he had not studied neuralgia very extensively. Now it is in such cases that I recommend quinine, iron, zinc, sulphate of copper, nitrate of silver, united with hop, lettuce, hyoscyamus, conium, and even hydrochlorate of morphia; and am prepared to defend the propriety of doing so.

What conceivable object or warrant there can be for a routine "preliminary course of active purgatives," in cases of neuralgia, or of any other disease, in which, by an accurate, not (as in Dr. Allnatt's case) an erroneous diagnosis, the digestive organs are found to be acting well, and the bowels unembarrassed, exceeds my judgment and apprehension. In cases of neuralgia of pure, morbid, sentient innervation, with no seeming or actual derangement of the digestive organs, I omit, most justly, purgatives, and pass at once to metallic tonics and sedatives, separate or conjoined.

Nor can I see how or why, in cases of neuralgia in which purgatives are really indicated, these should occasion aggravation of the symptoms, as in the case reported by Dr. Allnatt. In cases of neuralgia depending on, or accompanied by, a state of plethora, and in which there is reason to suspect the blood is more than usually loaded with the constituents of uric acid; in other words, in cases of undeveloped arthritic neuralgia, (those in which purgatives are conspicuously indicated;) I have never witnessed, even at the commencement of treatment, the slightest "aggravation" of symptoms; but, on the contrary, early, if not immediate, relief.

It is undeniable, and it would only be necessary to refer to Andral and Barras, if each one's own professional experience did not satisfy him of the fact, that some of the most formidable cases of neuralgia have been due to the debility induced by bloodletting. Now, I ask the profession, wherein essentially differs debility induced by bloodletting from that induced by catharsis? and I therefore further inquire if, seeing the many origins and kinds of neuralgia, a plan of treatment which regards purgation as an indispensable preliminary is judicious, is scientific, is free from hazard?

To maintain that neuralgia can have no origin but from some "aberration of function in the chylopoietic viscera;" that there may not be lesion of cerebrum, spinal cord, or nerve, in the neurilema, or in the nervous substance; in origin of nerve, or in the length of the nerve itself,—to maintain, I repeat, that there can be no lesion of the parts just named, except as a corollary to digestive derangement, appears to the writer as one-sided a theory as perhaps has ever been put forth; only equalled, in that respect, by the treatment proposed to be based on it—to wit, invariable, or almost invariable, preliminary purgation!

On Dr. Allnatt's view, neuralgia is wholly, or nearly, a secondary affection; and idiopathic neuralgia—i.e., a pure case of morbid innervation, a thing rare or unknown! Such are the pathological deductions to which Dr. Allnatt's premises lead, which are (to use his own words) "proposed, I fear, without due consideration."

I have not had the advantage of perusing Dr. Allnatt's work on neuralgia, but if the views above detailed form its substance, all I should say is, that the author has left something for a second edition.

I have commented with more freedom on Dr. Allnatt's views than my natural feelings and my inclinations would have disposed me to, but for the needless tone and spirit of his own paper. He speaks of "routine prescriptions;" "of the gentleman having improved on the practice of his predecessors;" of his "having stepped from the ranks," to assure us, "on his own individual testimony," that that is right, which Dr. Allnatt, on *his* individual testimony, considers wrong. For to what are the individual testimony and opinion of Dr. Dick opposed, but to the individual testimony and opinion of

Dr. Allnatt? The public will decide which are the more valuable; but, at any rate, it becomes not either of the individuals themselves to sneer or gibe at the sincere and scientific reasonings of the other.

I shall report, at a future opportunity, two cases of neuralgia for which I have recently been consulted, neither of which correspond with Dr. Allnatt's views. The one is that of a brother physician (previously unknown to me) in an inland county, a man of intelligence and high respectability; he suffers from severe neuralgia in the stomach. The other is that of a gentleman, aged thirty-eight. His case is that of tic in the second cervical nerve, on the left side.—I am, Sir, your obedient servant,

Bentinck-st. Manchester-sq. March, 1847.

ROBERT DICK.

MR. PHILSON'S CIRCULAR TO CUSTOMERS.

To the Editor of THE LANCET.

SIR,—Although I must think you have handled me very roughly in your remarks in the last number of *THE LANCET*, still as you have published the document which has offended the delicate sensibilities of some of my fellow-townsmen, I do not complain, as every one can, by a perusal, form his own judgment of the justice of your comments. My object in writing is to contradict that part of the statement which charges me with distributing my circular amongst the patients of the other medical men in the neighbourhood.

I know not, Sir, who are the parties you refer to as having furnished this piece of information; but this I do know, that they have iniquitously misrepresented my conduct, and I challenge them openly to prove their assertion. The names of the persons to whom the circulars were addressed were taken by me from the ledger, and I do not pretend to say but that they were sent to families where another medical man was occasionally consulted by some of their members.—I remain, your obedient servant,

W. PHILSON.

Hitchin, March 25th, 1847.

THE FATAL ADMINISTRATION OF ETHER TO MRS. PARKINSON.

To the Editor of THE LANCET.

SIR,—It would have been a great advantage to the profession and the public generally, in the case of Mrs. Parkinson, who is said to have died from the effects of ether inhaled by her during an operation, if we had been furnished with a detailed report of the evidence, especially that of the medical gentleman. If some such information as the following had been given, the profession and the public would have been in some degree enabled to form a correct opinion.

1. What apparatus was used; whether the patient breathed into and out of the same vessel?
2. How often had the person administered the vapour before, and in what cases; the size and nature of the vessel in which the vapour was generated?
3. The size or bore of the tube; the quantity and quality of the ether—if washed or unwashed?
4. The specific gravity of the ether?
5. If the apparatus was placed in hot or warm water—and if so, the temperature?
6. The temperature of the room in which the operation was performed?
7. Was the apparatus placed above or below the mouth of the person inhaling the ether?
8. If the pupils retained their contractility?
9. Was the mucous membrane of the nostrils still sensible?
10. If the mouth and nostrils were perfectly closed?
11. The state of the pulse before, during, and after the operation?
12. The breathing—if slow, laborious, or natural?
13. The appearance of the face, head, and lips—if congested, pale, or livid?

As a member of the profession, I feel deeply interested in the subject; and if by any means the information here suggested could be supplied, we should then be in a much better position than at present to form a judgment.—I remain, Sir, your obedient servant,

ALEX. FAIRBROTHER,
Senior Physician to the Bristol General Hospital.

Bristol, March, 1847.

The following answers have been given to the more important queries proposed by Dr. Fairbrother:—

1. The woman breathed out of, but not into, the vessel.
2. Six or seven times, several cases being extraction of teeth, and one the removal of a portion of a toe.

3. About an ounce of pure washed ether was prepared for inhaling; but a portion remained after the administration.

4. The specific gravity was 733 to 765.

5. The apparatus was not placed in either hot or cold water.

6. The temperature of the room was moderate.

7. The apparatus was placed below the mouth.

8. The pupils became dilated.

9. The sensibility of the nostrils was not examined; but the conjunctiva was sensible.

10. During the inhalation preceding the operation, the mouth and nostrils were closed; but they were not during the second inhalation, after the operation had commenced, from the difficulty of keeping the head still.

11. The pulse was natural in the morning, being about 76. It rose to 84 previous to the inhalation, (probably from the appearance of the surgeons, and expectation of the operation,) and during the inhalation rapidly increased to 140; but before inhalation ceased, it became small and feeble.

12. The breathing was rapid for a short time, when the effect of the ether was first produced, but then became natural.

13. The face was pallid; the lips natural, but slightly congested.

* * We shall be glad if Dr. Fairbrother will pronounce his opinion upon the case now that the answers to his queries have been obtained. As we do not doubt that in each question he proposed to himself some specific object of inquiry beyond the mere verbal query,—more specific probably than would be attached to them by "the profession and the public,"—his opinions upon the various points raised in his letter would be very desirable, and are, indeed, in a manner called for.—Ed. L.

ON THE USE OF THE LONG FORCEPS, AND ON TURNING THE CHILD WHERE THE PELVIS OF THE MOTHER IS GREATLY DISTORTED.

To the Editor of THE LANCET.

SIR,—In the *Edinburgh Monthly Journal of Medical Science* for March, 1847, there is a case related by Dr. Simpson, of great distortion of the pelvis, in which, during the labour, he made the woman inhale ether vapour, and while under its influence, he performed the operation of turning. The pelvis of the woman, it is stated, was greatly contracted in its conjugate diameter, from the projection downwards and forwards of the promontory of the sacrum. It was her second confinement. "Her first labour had been long and difficult: she began to suffer on a Monday, and after a protracted trial of the long forceps, was at last delivered by craniotomy, late on the subsequent Thursday night. Even after the cranium had been fully broken down, a considerable time and much traction had been required to drag the diminished and mutilated head of the infant through the contracted brim of the pelvis, and she was long in recovering." From the circumstances thus recorded it is impossible for any one to doubt that the treatment employed during this woman's first labour was highly injudicious, and diametrically opposite to the rules laid down for the employment of the forceps by all the greatest systematic writers on midwifery in this country. It must have been obvious from the first that nothing but mischief could result from the use of the long forceps; and her tedious recovery proves that she was injured by the labour being allowed to continue from the Monday till the Thursday, and the treatment adopted, when it must have been obvious from the first that a living child could not pass, or be dragged with the long forceps, through the pelvis.

On the 19th of January last, this woman was in labour with her second child at the full period, having refused to allow premature labour to be induced. "The pains of her second labour commenced in the forenoon of the 19th. I saw her," says Dr. Simpson, "with Mr. Figg at six o'clock in the afternoon, and again at seven. The os uteri was pretty well dilated; the liquor amnii not evacuated; the presenting head very high, mobile, and difficult to touch; and a pulsating loop of the umbilical cord was felt floating below it in the unruptured bag of membrane. From five to nine o'clock the pains seemed only to push the circle of the os uteri further downwards, without increasing its dilatation, or making the head in any degree enter into the pelvic brim. Assisted by Dr. Zeigler, Dr. Keith, and Mr. Figg, I, shortly after nine o'clock, made the patient inhale the ether vapour. As she afterwards informed us, she almost immediately came under the