DR. STEVENS AND DR. JOHNSON.

REJOINDER OF DR. STEVENS.

To the Editor of THE LANCET.

SIR,—It is painful for me to be compelled to appear so often before the public, on purpose to resist the unfounded attacks of an individual, who is no sooner fairly convicted of one misrepresentation than he tries to evade it by inventing another.

The chief accusation contained in Dr. Johnson's last letter, is one which concerns himself fully as much as it does me; for in it he asserts, that the *analysis* of my paper on the blood, which appeared in his journal for July 1830, was written by myself. Now, there is but one charitable construction that I can put on this, and that is, to consider it as a modest intimation to the public, that any author, who wishes to review his own work, will find a place for the same in the *Medico-Chirurgical Review*, of which this accommodating individual is the sole editor.

It is very natural that a certain class of authors might wish to be accommodated in this kind of way, and lately there has been but too much reason to fear that such accommodation is not uncommon; let this, however, be as it may, there can be no doubt that the editor who permits such conduct is just as culpable as the author who wishes to be the writer of his own review. I am sorry to observe, that Dr. Johnson, in the present instance, has brought an accusation of this kind against himself; but in as far as relates to this instance I will stand his friend, and try to prove that he is not guilty.

He asserts that, as he had not heard my paper read at the College of Physicians, I had called upon him, and requested that he would insert a report, or analysis, of the said paper, written by myself. "This," says he, "I did publish, and this is the unfair statement by which he (Dr. Stevens) says I wilfully endeavoured to mislead the public." As Dr. Johnson has fortunately furnished us with a great variety of polite expressions, I will borrow one of his own, and aver, that there is scarcely one iota of truth in all this. I was not present myself, neither did I know whether Dr. Johnson was or was not present when that paper was read; nor did I care. Dr. Johnson's journal may be, like others of the day, a useful medium for disseminating original facts, but those will be sorry to have the doctor himself on their side who have either read his own works, or who know the light in which his *opinions* are considered by the *intelligent* part of the medical profession. It is true, however, that I did call upon him, but not for the purpose he has specified.

Some of your readers may recollect that, in 1814, I published an account of an operation in which I had tied the internal iliac artery in 1812; the patient died in the year 1822, and the parts were preserved. Dr. Van Brackle, who had been present at the operation, and who was also present at the examination of the body after death, wrote a minute account of the preparation, and sent it to his friend Dr. Chapman (the then editor of the Philadelphia Journal), which was published in that work, in one of the numbers for 1822. Believing that this would find its way into the English journals I did not write any account of it myself, but I kept the preparation in my possession and brought it to England with me, when I returned to this country in 1829. And the truth is I considered the preparation at that period as of little value: for I thought the circumstance of my having tied that artery was universally admitted. But in this I was deceived, for, in May 1830, in looking over Mr. Lawrence's lectures, which had then just been published, I found that he had there expressed a doubt about the correctness of the statement which I had published. In consequence of this, I waited on Mr. Lawrence, told him that I had the preparation in my possession, and expressed a wish that he would examine it as early as possible. The preparation was immediately sent to the museum of the College of Surgeons, where it was examined by Sir Astley Cooper, Mr. Travers, Dr. Hume, Mr. Lawrence, and many others, who expressed themselves perfectly satisfied with respect to the correctness of the original statement which I had made.

A few days after this, I received a communication from the Secretary of the Medico-Chirurgical Society, requesting me to draw up a description of the preparation, and to send it to them for the next number of their Transactions. This, however, I could not do, for I was then on the very eve of embarking for America; in consequence of which I requested Mr. Owen to do it for me, and promised to procure him the description which had formerly been made by Dr. Van Brackle, who had seen the parts in their more recent state. On purpose to procure this, I applied to several of my acquaintances, but none of them could give me the volume which I wanted. Amongst others, I went to my friend Dr. James Clark, who told me that Dr. Johnson was the most likely person to be able to give me the journal in question; and, as I did not know Dr. Johnson myself, Dr. Clark gave me a few lines to him, saying who I was, and what I wanted. It was for this purpose that I called upon Dr. Johnson, and not as he

avers, to request that he would insert in his journal an analysis of my paper on the blood written by myself.

The said paper had been read at the College of Physicians a short time previous to this period, and during this interview it became the subject of conversation, but who introduced it I do not recollect. I will allow, however, that it was probably myself, for I felt then, as I do now, an anxious desire that the facts contained in that paper should be made as public as possible. In the same conversation, Dr. Johnson expressed a wish to get a copy of the said paper, on purpose to be able to lay a correct account of the facts which I had stated before his readers. I told him that I would send him a correct copy, provided he would give me a positive promise not to publish it in its original form; for, as I believed, the College of Physicians retained to themselves the right of publishing any paper which had been read at their meetings. In consequence of this conversation I afterwards sent, or gave him, a correct copy of the original paper, and, in as far as Dr. Johnson has given a statement of the facts contained in that document, they are perfectly correct. He states, however, what is not correct, when he asserts that he can prove that the analysis, and the only statement which he had ever given of my views, were written by myself. But I have only to refer to the very first sentence of the said analysis to convince any impartial individual that I, at least, could not have been the writer of that paper. He begins by stating, that "At a late meeting of the College of Physicians, a paper on the above subject *was read by Dr. Stevens*, who has for many years been a practising physician in the West Indies.

Now in the first place, so far from having read the paper myself, I was not even present at that meeting, and, in the second place, you will observe that even my name is misspelt, which is not likely to have been the case had I been the writer of that article.

It is customary, I believe, for an author who has written a work, to send a copy of it to the editors of journals. I have explained how and when Dr. Johnson got possession of a copy of mine. There is, however, a wide difference between an author giving a copy of an original paper to the editor of a journal, and writing a review of his own work.

Dr. Johnson asserts, however, that he has a document in his possession which will prove that I had written the analysis myself. Now if he has such a document, or any other of mine, except a correct copy of the original paper on the blood, let him bring it forward and expose it to a fair inspection

within fifteen days from the publication of this, and then I will prove either that the said document is entirely a forgery, or confess that my memory has deceived me sadly in this affair. But if he does not produce it within the above period, I shall consider his assertions about this document to be as untrue as the rest of his other misrepresentations, and then leave him to God and his own conscience.

I have stated in my former paper, that I had mentioned to Mr. Greatrex my belief, that the aerial poisons which cause fever, have an acid effect in blackening the blood, but that the alkaline or neutral salts possessed the power of again restoring the red colour of this blackened fluid. Now I can readily conceive how Mr. Greatrex, with whom I had merely a verbal communication on this subject, might give an incorrect view, without any intention on his part of misrepresenting my theory. I can also believe that Dr. Hacket, whom I have never seen, might easily fall into the same error; but I cannot make the same excuse for Dr. Johnson, who had previously given a fair view of my facts and opinions in his own journal, and after having published in his own work a correct statement of my views with respect to the effects of salts and alkalies on the blood. I say again, he had no right, either in the Westminster Society, or in his letter to you of the 5th December 1831, to put insufferable nonsense into my mouth, which he must have known I never could have uttered, or to make me the supporter of opinions which he himself must have been perfectly aware were in direct opposition to my own facts.

Now, notwithstanding all his sophistry, the following is the fair statement of the case. In his journal for July 1830, Dr. Johnson published a correct statement of the outline of the facts contained in my paper. At that period he spoke favourably of my opinions, and represented "the means which I had proposed for combating a dreadful scourge of the human race as both simple and practicable." Secondly, when he found that some eminent individuals in the profession considered the proposal which I had made for the treatment of cholera* as better than his own, he then got

* I may observe, that though this treatment has not yet got a fair trial in cholera, still, in as far as it has been tried, it has been found to be decidedly the most successful. See the result of this treatment at Sunderland, as well as Sir William Crichton's letter in the supplement of a contemporary journal, in which it is stated, that in an hospital at St. Petersburg, only three died out of thirty patients who had been treated with large doses of muriate of soda: but of this hereafter. It is probable that the cholera, like the yellow fever, never would have been so fatal had it not been for improper treatment. Water, saturated with carbonic acid, an infusion of dried raspberries, mustard eme-

up in a violent passion, and on the authority of certain papers, uttered a tirade of insufferable trash about soda and sodafied blood which he himself must have known to be incorrect; for, as I have said, the whole of it was in direct opposition to my own facts, such as he himself had previously published in his own journal. It was for this that I accused him of wilful misrepresentation; and, having convicted him of this, even by his own evidence, how does he try to get rid of the proof? why, simply by inventing one misrepresentation on purpose to conceal another; and tries to mislead those who do not think for themselves, by asserting that he had never made any statement except that which was contained in his own journal. And though I have admitted the said statement to be correct, yet he labours to prove that this is the ground on which I have accused him; but the truth is, that I never accused him on any such grounds, for I admitted that the facts as stated in his journal were perfectly correct. He asserts, however, that this is the only statement which he had ever given of my views. Now the memory of this gentleman must indeed be very bad, for, independent of what occurred in the Westminster Society, is it not true that in his letter to you of the 5th December he asserts, "that he had in his possession documents from which it appeared that *soda* had nothing whatever to do with the success of the treatment in Trinidad; that *the blood of sodafied patients* after death was found to be precisely similar to that of the *unsodafied*;" and, lastly, he intimates that the success had been owing, not to *soda*, but to the reddening of the blood by the warm-bath.

It was for this that I accused Dr. Johnson of unfair dealing, and wilfully endeavouring to mislead the public; for, as he himself had previously published a correct statement of my views, he must have known that the opinions imputed to me by the Trinidad physicians were directly the reverse of those which I had publicly expressed. Under these circumstances, he had no right to bring forward such documents as affording any proof against either my facts or opinions; and if his having done so be not a wilful misrepresentation on his part, then it is madness; and the person who can be guilty of such conduct, without meaning to misrepresent, is fitter to become the inha-

bitant of a lunatic asylum, than to be, as he professes, an instructor to the public.

Dr. Johnson denies having had any previous communication with the writers of the Trinidad documents, and this perhaps is true. I did not however directly accuse him of corresponding with either of these gentlemen; I observed, "that Mr. Greatrex, apparently in answer to a letter from Dr. Johnson, states, &c." and I did so for the following reason: If you will refer to page 296 of the last number of Dr. Johnson's journal, you will find it there stated, that "the following extract from a letter from Mr. Greatrex (*after perusing a communication of mine on the foregoing subject, and essentially* the same*) I beg leave to append, &c."

Mr. Greatrex's letter, which is apparently written to Dr. Johnson, is dated July 24th, 1831, and in this he states, "that I withheld, as you have seen by Dr. Hacket's letter, all authority, &c." Dr. Johnson, however, observes, that he had "never corresponded with Mr. Greatrex, either before or since." Now, are we to consider this as an indirect admission that he had corresponded with him at that period? And when we find it stated in a journal, "that Mr. Greatrex in answer to a communication of mine on the foregoing subject, &c.," who are we to suppose to have been the correspondent in this case but the editor himself? It is evident from what follows, that it was not written in answer to any communication of Dr. Hacket; and if it was not to Dr. Johnson, to whom was it written? It is possible that some other unknown third person has been making himself busy in this affair: and, if so, Dr. Johnson can only clear himself of the suspicion by explaining to the public who this person is.

Dr. Johnson observes that he had acted the part of a friend, and denies that he had made any attack upon me: "for where," says he, "did I attack him at all?" Now, Dr. Johnson is aware, that when my last paper was written I was under an impression that he had asserted, in the Westminster Society, "that he had a document in his possession which would prove that what I had published were forgeries; and that my conduct was disgraceful even to the whole profession." This statement found its way into the *Morning Post*. It is, however, but fair to state, that when I requested a friend to wait on Dr. Johnson, and to require either a disavowal of, or an apology for, such outrageous conduct,

tics, &c. &c. have lately been found to be successful in the treatment of this disease. Not that there is anything specific in these remedies, for they are more useful merely because they are less pernicious than calomel in *scruple doses*, and the other destructive agents that have been recommended by Dr. Johnson, and used by most of his colleagues who have treated cholera in the East.

* From this it would appear, that the said document has undergone certain changes since Mr. Greatrex had seen it; and of this I am very glad, for I can scarcely believe that Mr. Greatrex would have lent his sanction to the improper language which has since appeared in Dr. Johnson's Journal.

that he signed a document in which he affirms "that the above report is totally incorrect, and that he had never made any charge of the kind."

Such being the case, and though the editor of the *Morning Post* still insists that their original report was correct, yet I will wave what occurred at that meeting, and simply refer to his own letter to you of the 8th December 1831, in which he states, that he "had documents in his possession charging me with misrepresentation of facts, also with garbling of extracts so as to suit my own purpose, and other grave charges, that were, in his mind, calculated to do injury and even disgrace to the medical profession." Now, if Dr. Johnson has any sense of propriety yet left, let him read this; and then he may himself answer his own question and inform the public "where he had made any attack against me at all."

Dr. Johnson, as the editor of a journal, did perhaps but his duty to the public as well as myself by publishing the documents which he had received from Trinidad; but he had no right, at a time when he had seen only one side of the question, to make himself the partisan of either party, and, nearly a month previously to the publication of the Trinidad papers, to prejudice the case and endeavour to make a strong impression in the public mind against me, by asserting that he had papers in his possession containing grave charges of disgraceful conduct, charges which the documents themselves, *even in their present form, do not contain.*

You will see, by referring to his own letter of the 5th, that he did use language which he had no right to have used; and you know also, that he did make an impression on the public mind which the Trinidad papers themselves never could have made, had they *merely appeared before the public in their original form, and without any previous comment.* Yet, after all this, he has, to use his own elegant expression, "the affrontery" to state that he never made any attack against me, and that he took no part in the controversy, "either *pro* or *con*." Poor innocent man! He has really been ill used. He took no part in the controversy; he made no attack whatever; he only accused me of misrepresenting facts, garbling letters, and other conduct that was most disgraceful to the whole profession; and then, like a good, simple, honest editor, he had merely published *both* sides of the question, although he is perfectly aware that, at this moment, the Trinidad documents are circulating against me all over the world, and uncontradicted, at least in as far as his Journal is concerned. His next number, however, will show whether he is or is not deserving of the good character which he has given of himself.

I have formerly said, that when my paper was written for the College of Physicians, I had a thorough conviction that every syllable in that document was most religiously true, and I believe that neither Dr. Johnson nor his Trinidad correspondents have disproved any one of the *facts* contained in that paper. With respect to my opinions, I trust that the profession will think charitably on this subject until I can find time to bring forward the numerous experiments and facts with which my views will be supported hereafter; and if in the mean time a Prout, a Christison, a Bostock, or a Brande, should find fault with my theory, I will receive their observations with thanks, and try to profit by their superior knowledge; but I will not be schooled; nor occupy any more of my time in refuting the observations of men who argue the propriety of using a hot-bath in the hot stage of yellow fever, or who believe that a neutral salt, and a pure alkali, are one and the same.

If Dr. Johnson had had any knowledge of the subject about which he was writing, he would have seen, at one glance, that the Trinidad documents were a tissue of errors from beginning to end; and if he had had even one particle of fairness in his composition, he would have written to his correspondents and requested them, for their own sake, to contrast the views which they had given of my opinions with those which he had previously published in his own journal. He would have said, "It is evident, even from your own showing, that Dr. Stevens has never advised you to use even one particle of either soda or any of the other alkalies, yet you are constantly confounding the alkalies with the neutral salts, and talking eternally about soda and sodafied blood, while you admit at the same time, that you had never used one particle of either soda, or any other pure alkali, in your hospital." Had he done this he would have delayed the publication, as he himself says he had intended to have done, and by doing so he would have acted the part of a friend to his foreign correspondents; but in place of this, and on purpose to secure a little momentary triumph over opinions which he found to be gaining ground, he gets up in a violent passion, and for fear that any other method of treating cholera should be found to be better than his own, he makes use of the Trinidad documents, to impede for a period the progress of truth, and in place of any reasonable argument, which he was totally incapable of using, he brings forward assertions in place of facts and statements, about soda and sodafied blood, which he himself must have known at the time to be mere folly. "A man may walk the routine of ordinary practice, converse with hysterical wives, and whistle as he walks; but he who has

witnessed the sufferings of such an awful disease, without feeling an anxious desire to understand the cause and alleviate the misery, has neither a head nor a heart worth the caring for."

It is the duty of an editor, or of a professed critic, to lay before his readers a clear view of facts, and, if necessary, to reason from these as fairly as he can. It is one at least of the essential characteristics of a gentleman not to attack the honour of another until he is fairly convinced that he deserves such treatment. I have, however, stated the facts, and whether Dr. Johnson has acted with wisdom towards himself, or fairness towards me, I shall now leave to the decision of the public.

By inserting the above you will oblige, Sir, your obedient servant,

WM. STEVENS, M.D.

Albany St., Regent's Park, Jan. 23, 1832.

EFFECTS OF STRANGULATION

ON THE ORGANS OF GENERATION.

To the Editor of THE LANCET.

SIR,—In reference to several communications which have appeared in *THE LANCET* on the effects of hanging on the male and female organs of generation, and the sexual functions of the cerebellum, permit me to lay before you a short statement of what has fallen under my own observation in relation to this subject.

In October, 1820, I was accoucheur to a lady who, as soon as she was delivered, anxiously wished to know the sex of the child. Accordingly, in absence of light, I ascertained by feeling with my hand the sex of the infant, and told her that it was a fine boy. But what particularly arrested my attention on this occasion was, a strong penial erection in the child, who seemed to have been almost strangled in the birth. The presentation was natural, and the pains vigorous, and at short intervals; the head was very much compressed, and the labour was finished nearly in an hour. Since my attention has been directed to this subject many opportunities have occurred to me of verifying the accuracy of the position of some of your correspondents. But the circumstances in which the accoucheur is most likely to recognise its truth are, when the labour is vigorous, the head strongly compressed, and retarded for a time in the passage through the pelvis, but the child ultimately born without much resistance from the soft parts. Sometimes, however, the rigidity and resistance of the soft parts produce nearly a similar effect. The fact may be noticed in very protracted labours, and in most instances the child's face is turgid

and livid, and respiration often does not take place for some minutes after delivery.

It may be now three years since I attended the following case, of a woman, about 30 years of age, in child-bed. The labour was severe, though not remarkably so, for in two or three hours she was delivered of a female child without any particular assistance. It was on the second day after delivery, when I called to see my patient, that she directed my attention to some marks of blood on the child's linen. At first I thought lightly of the matter, supposing the appearance to have been produced by some drops of blood from the umbilical cords. She however spoke significantly on the subject, and talked of it as catamenial. In confirmation of her view of the subject she remarked that this was the second female child which she had had that exhibited this appearance during the first and second day after delivery. This was quite new to me; at the same time I endeavoured by a careful examination to ascertain the source of this singular phenomenon. No fluid blood was discovered, but around the root of the clitoris, some sanguineous matter was found in a dry, granular form, whence it is presumed the discharge, if it deserved the name, had proceeded. I took an early opportunity of inquiring of a professional gentleman who had been nearly 40 years in the practice of midwifery, if he had ever noticed anything similar to what has been just mentioned, but he frankly acknowledged that he had not. In a conversation which I had on this subject with a midwife of extensive practice in this place, she stated to me that she had noticed the appearance referred to several times in the course of her business. Since I began to make inquiries on the subject, some other cases of the kind have been mentioned to me.

About seven weeks ago I was consulted in a case of apoplexy. The woman had been remarkable for regularity with regard to the recurrence of the catamenia, but while she was labouring under this disease, which proved fatal on the tenth day, the menses appeared on the beginning of the third week, and continued two days, being nearly a fortnight sooner than usual.

In severe epileptic fits, when the face is turgid, livid, and the veins of the head are ready to burst, seminal emissions are, as I understand, not uncommon. Now in all these different kinds of cases the cause of the phenomena seems to be a mechanical effect which is produced on the cerebellum, which, through the medium of the nervous system, becomes apparent in the organs of generation, and bears a striking analogy to the consequences which hanging has upon eunuchs and criminals. I am, Sir,

Jan. 12, 1832.

A.*

* This signature has been authenticated.—*ED. L.*