

The gall-bladder by no means distended ; in it was found green bile.

The urinary bladder empty, and hidden behind the pubes.

The pericardium contained no fluid. Heart distended. Both ventricles, right auricle, and vena cava, contained dark-coloured fluid blood.

The lungs were natural.

The dura mater on the right side was found ossified to the extent of an inch in length by half an inch in width. Vessels on the surface of the brain turgescient. Half an ounce of serum was found in each lateral ventricle.

CHARLES DAVISON, Surgeon.
Newcastle-upon-Tyne, Dec. 8, 1831.

DR. WILLIAM STEVENS

ON THE EFFICACY OF

SALINE AGENTS

IN THE TREATMENT OF

WEST INDIA FEVERS.

BEING A DEFENCE OF HIS PRACTICE
AGAINST THE CHARGES OF DR. HACKET,
DR. JOHNSON, AND OTHER PHYSICIANS.

To the Editor of THE LANCET.

SIR,—I do not mean to lessen the force of the serious charges which Dr. Johnson and my other more able opponents have preferred against me, by dwelling on subjects that are decidedly of minor importance in the present inquiry: but, if you will have the goodness to compare Dr. Johnson's letter and the Trinidad documents with the paper which was sent nearly two years ago to the Royal College of Physicians, you will find either that these gentlemen have but a sorry conception of my opinions with respect to the treatment of fever, or, that Dr. Johnson, at least, has wilfully endeavoured to mislead the public, by giving a very unfair statement of my views upon this subject.

It has long been known that some of the neutral salts possess the power of reddening the blood; but it was not I believe generally known, at least until lately, that every one of the alkaline salts possesses the property of striking a red colour with the black colouring matter, and of converting even the blackest venous into the brightest arterial blood; and as a proof that this fact was not generally known, I may mention that one of the latest writers on the blood in this country informs us that the muriate of soda darkens its colour. It is also a fact that the Royal College of Physicians were so little prepared to admit the asser-

tion, that *all the alkaline and earthy salts*, except those which contain a great excess of acid, redden the blood, as well as some of the other chemical facts which I had there stated, that they would not consent to the reading of that paper, until they had first sent it to one of the ablest chemists of the day, to ascertain, by actual experiment, whether the circumstances which I had stated as chemical facts were really correct.

In the same paper I stated my belief, *that the natural saline ingredients of the circulating current were, in reality, the true cause of the red or arterial colour of the blood; and whatever the colouring matter may be, that black is the natural hue of this agent, but that the whole of the alkaline salts possess the power of striking a rich arterial colour with this black substance.* I also observed that in the last stage of the yellow fever the blood in bad cases becomes black and vapid from the loss of its natural saline matter; but that, by adding a small quantity of any one of the neutral salts to this black fluid, we could again restore it to its red or arterial appearance, even when the blood that we used had been black as ink. The Trinidad physicians do not attempt to deny this; and Dr. Hacket admits that the blood is invariably dark-coloured and diseased in the West India fevers, without there being even "*one single solitary exception to this rule.*"

The fact that the blood owes its red colour to its own natural saline ingredients, is now, I believe, almost universally admitted by those numerous physiologists who have witnessed the experiments which, in my mind, prove beyond all question that such is the case; but of this hereafter. I stated also in the same paper that acids, alkalies, electricity, and, in short, everything else that possessed the power of decomposing or destroying the neutrality of saline matter, destroyed the red colour of the blood, and made it perfectly black, as I believe, by decomposing or destroying that which is the true cause of its red colour.

But it seems, notwithstanding all this, that the Trinidad physicians have discovered that *alkalies* redden the blood out of the body;* but complain that its colour had not been reddened by *soda* in three men who had never taken *even one particle of that alkali*, and who had died after having been carried to the Trinidad Hospital half dead in the last stage of the yellow fever. This new discovery, however, that *soda* reddens the blood either in or out of the body, is not correct; for the alkalies, as I have stated, do not redden, but blacken its colour.

I have, for reasons which will afterwards

* See Hacket's Paper.

be given, long adopted the belief, that carbonic acid, and not carbon, is the cause of the dark colour of the blood in the venous circulation. I have long also been aware of the fact, that the aerial poisons which cause fever have an *acid effect** on the blood; and both darken its colour and give it a deranged appearance, even before they excite the disease. It is probable that I did mention these opinions to Mr. Greatrex; but it was neither soda, nor potass, nor any of the other alkalies, which I recommended these gentlemen to use: and, certainly, it is not my fault, if neither Dr. Johnson nor themselves be acquainted with the difference betwixt a pure alkali and a neutral salt. It may be quite enough, if I am made to be answerable for my own faults; but, surely, I am not accountable for the errors of others; and Dr. Johnson who reviewed my paper on the blood, and ought to have known better, has no right to put nonsense into my mouth, which I never could have uttered; and after having stated that all the alkalies blacken the blood, they have no right to call upon me to prove that soda can redden it, even in opposition to my own facts.

They state, that soda had nothing whatever to do with the success in the above island. Now this is a mere equivocation; for, *I never said that it had*. I am glad, however, to find, that they admit the success, for by admitting this, they prove at least the one-half of my statement to be true. They state also that, after death, the blood of sodafied patients, was found to be precisely similar to that of the unsodafied. Now, if Dr. Johnson had allowed himself time to have reflected on this subject, even for one moment, he would never have uttered such insufferable trash: for soda as soda, or, in other words, in its alkaline form, had nothing whatever to do with the treatment of the Trinidad fevers. It was, as I have said, neither soda nor any of the other alkalies which I advised them to use; but the strongest, and, perhaps, the best, of all the non-purgative *neutral salts*; namely, a saturated solution of the muriate of soda, or, as it is now called, the chloride of sodium, combined with the nitrate of potass. They admit that they did adopt the use of this remedy in consequence of my visit, and still continue, I believe, the use of it up to this day, *both in the proportions and at the times which I had pointed out*; and I will prove, that however numerous the deaths may have been previous to the commencement of this practice, yet from the moment that they began to attend to the diseased state of the blood, and adopted the saline treatment in that hospital, they had but few

opportunities of ascertaining, after death, whether the blood had been *sodafied* or not.

"Permit me," says Dr. Hacket, "to inform you, that every case that dies in our military hospitals must be opened and reported on; and, therefore, the opportunities of the military practitioner for examining the human body, and viewing the morbid changes in its structure, are perhaps greater than *that* of any private practitioner, however great or extensive his practice may be." Dr. Hacket, however, forgets to state that, however numerous the deaths may have been previous to August 1828, yet from the moment that they commenced, not the soda, but the saline treatment, they had scarcely a death from fever: consequently though I admit that, previous to my visit, there had been dissections enough, yet I shall prove, that from that period their opportunities of examining the dead bodies in the Trinidad hospital, could not have been so very extensive as they had been before.

I have already referred to the three cases belonging to the Royal Artillery, where the men had been brought to the hospital in the last stage of the disease, and where, after death, the blood had been, as they say, in a state of solution, but not sodafied. The latest letter which I have from Trinidad is dated 4th April, 1830; that is, about twenty months after the commencement of the saline practice. During the whole of these twenty months the Royals had lost, up to that date, only eight men from fever, even including the three men mentioned by Mr. Greatrex, in his letter to me, as having died from organic disease in the lungs; and of the other five, two of them had died the same day that they had been brought to the hospital; consequently, in the first twenty months which followed my visit, they had only three opportunities of examining the bodies of patients who had been under any thing like fair treatment. These are the only cases where they had a fair opportunity of examining the state of the blood after my visit; and these cases were probably not examined; or if they were, they have not given us an account of the result. These are facts; and from these it appears that, after the month of August, 1828, up to the 4th April, 1830, that is, twenty months after they had commenced the saline treatment, their opportunities of examining the bodies after death, to ascertain whether the blood had been *sodafied* or not, have not been so very numerous as Dr. Hacket's statement would lead us to believe.

I know one private practitioner, who, about the same time, but in a much shorter period, had three cases in his own establish-

* See Dr. Hacket's paper in Johnson's Journal.

ment. One of these was his partner, the other his apothecary, and the third his clerk; they were all treated according to the old system, with bleeding, calomel, &c.; and out of these three cases of yellow fever, which he had seen in the very beginning of the disease, he had, in his own domestic establishment, as many opportunities of examining bodies after death, as they had in the Trinidad Hospital, during the first twenty months after they commenced the saline treatment.

During the first month after my visit to Trinidad, from forty to fifty fever cases were admitted into that hospital; they were all treated with the muriate of soda and nitrate of potass. Out of this number three died; and all the rest, I believe, recovered. These three were the only cases in which they had an opportunity of examining the bodies after death; but I do not consider these three cases, mentioned in Mr. G.'s letter to me, as affording the slightest argument either for or against the saline treatment; for these were not fair cases, either for this or for any other experiment. In the paper which was read at the Royal College of Physicians, I stated that "where proper means are used to protect the organs from the increased excitement during the early stage of the disease, and after the excitement is sufficiently reduced, when proper nourishment is given, and certain saline medicines are timely and judiciously used, the bad symptoms are generally prevented. When proper saline medicines are used, they do not fret the stomach; they act on the intestines as much as is necessary; they keep up all the secretions, particularly that of the kidneys; and enough is absorbed to enter the circulation, and prevent the dissolution of the blood, and preserve it until the fever abates, and all danger is past. This I am warranted to state as a fact, inasmuch as this treatment was commenced in the West Indies in 1827; and since then it has stood the test in several hundred cases of the West India fevers where it has been tried, both by myself and others; and with scarcely a single loss when we were called to the patients within the first twenty-four hours after the attack; and, with very few deaths where we were called in previously to the commencement of the fatal symptoms." But though I said this, I did not say that the saline treatment was capable of reanimating the dead, or of saving soldiers from the grave who are brought to a hospital in the very last stage of the yellow fever, with their fate already fixed from previous neglect. These men were, all three of them, notorious drunkards; they had all of them been undergoing fatigue while exposed to the intense rays of a burning West India sun, and that too during the hottest

season of the whole year. They had been taken ill at a distance; they had not been bled in the beginning of the disease; they had been carried to the hospital during the heat of the day; and were exposed, on their way to St. James's, while the fever was on them, to the direct rays of a burning sun. Two of them were brought into the hospital at a late period of the disease; and from these circumstances, but above all from their not having been bled in the first stage, it is more than probable that some serious injury had been done to the brain; and that the finger of death was already upon them, before any attempt to arrest its progress could have been made. It is also admitted, even by the Trinidad physicians themselves, that it is only within the first twenty-four or forty-eight hours that good can be done by any treatment in these fevers; for, as they say, after this the disease becomes, as it were, a labyrinth most difficult to unravel; consequently, these three cases do not, in my mind, afford even the slightest proof against the saline treatment, or any thing else, except neglect in the beginning, and the imprudence of the manner in which these men had been brought to the hospital.

Twelve hours after death, the blood in these cases was found to be in a state of solution, but dark in colour. Now the truth is, that in yellow fever, death may be produced in one or other of two different ways. In the first place, by injury done to the brain, or to some of the other important organs, during the violent excitement which generally occurs in the first or inflammatory stage of the disease. But even when they escape this, death may be caused solely and entirely by the diseased state of the vital fluid. When they die from the first cause, the blood may still be in a state of solution; as is said to have been the case in these men; but still it is diseased. I am not quite sure, however, that in these cases the diseased or unsodafied state of the blood was the cause of death. It is true, that in these men, saline medicines had been thrown into the stomach; and that too at a time when this organ had probably ceased to perform its functions. Now, has it never occurred to these gentlemen, that if these men had escaped the injury which had probably been inflicted on the brain in the first stage of the fever, and if the diseased properties of the blood had been properly remedied by the saline agents, they would not then have had an opportunity of examining their bodies after death, to ascertain whether the blood had been acted on or not by the neutral salts: for these men, like the others, would not have died if they had been properly bled in the beginning of the disease; and, after this, had

the saline medicines been given at a proper period, when they could have entered the circulation, in time to prevent those fatal changes in the blood, which are, in reality, in these fevers, but too often, though not always, the sole cause of death.

The Trinidad army physicians appear to be perfectly aware that the saline treatment was in reality the cause of the great success in that island; and, in point of fact, they admit almost as much; but they take great pains to conceal the circumstance, that they had learned this practice from me: and the manner in which they do this is the only ingenious part of their whole proceeding. They use the word *saline* as seldom as possible; and endeavour to deceive their readers by talking eternally about *soda* or *other alkalies*; and try to convince the public that these were the remedies that I had recommended them to use.

Dr. Hacket confesses that he is stupid enough not to comprehend how *soda* should produce a similar effect on the blood in the living body, to that which occurs when *soda* is mixed with the same fluid out of its own vessels. He observes also, that "considering, too, the very minute quantum of *soda* that can possibly pass from the stomach, direct and unchanged, into the mass of circulation, how small must be the means to produce such mighty effects!" Now, what will the reader think of all this, who has seen by their own statement that I never had advised these gentlemen to use even one particle of *soda*: and, however successful this sort of logic might have been with Mr. Greateux, who was under his command, yet is it not an insult on the common sense of their medical readers to talk in this way, in the very same paper in which they admit that it was neither *soda* nor any other alkali that I had recommended them to use; but, the strongest of the non-purgative neutral salts? Is there even the merest tyro in the profession who does not know that *alkalies* and *neutral salts* are, in their effects, as opposite to each other as day is to night; but, above all, in their effects on the blood: for, the former, as I have said, blackens arterial blood, and the latter converts even the darkest venous into the brightest arterial? Now, if all this be true, it then follows, either that these gentlemen are grossly ignorant of this, or that they have tried to deceive the profession by the same sort of reasoning which they had found, perhaps, to be successful with those who are totally unacquainted with medical subjects. I will not for the moment say one word about *soda*: for, with this, in my practice, I had nothing to do, nor they either in theirs; but, with respect to the action of the neutral salts on the blood, my belief is, that this sort of scepticism may be carried a great deal too far.

That the salts do enter the circulation is beyond all question: for, healthy blood in every part of the world where it has yet been analyzed, has been found invariably to contain a given, and that too a large proportion of saline matter, to which it is indebted for its red colour, and in some degree for its stimulating power; and without this, the blood, as in cholera* or the last stage of the yellow fever, becomes totally incapable of performing its functions.

If Dr. H. really knows anything in his profession, he ought to be aware that when strong salts, such as those which I recommended them to use, are taken into the stomach in excess, that the greater part enters directly unchanged into the circulation, through the medium of the vena portæ; and produces an instant effect, and suddenly changes the properties of the blood; and when used in the last stage of fever, a part of the salts is retained in the circulation, particularly of those that are natural to the blood, while the excess, after having passed through the circulating current, is removed by the secreting organs without having been decomposed either in the stomach, or changed in its properties by the vital principle. Had Dr. Hacket taken away small quantities of blood in the last stages of the yellow fever, from patients who had been treated under the old practice, as recommended either by Jackson or Johnson, he would have found that the blood every hour was becoming more diseased in its properties, and blacker in its colour, chiefly from the loss, or at least from the great diminution, of its natural saline ingredients; or, had he made the same experiments on patients who had been treated with the active salts, such as they used in consequence of my advice, he would have found in that case that the blood was hourly becoming not merely more red in its colour, but less diseased in its appearance; or, had he injected a small quantity of the same mixture into the vein of an inferior animal, he would have found that even in living vessels the vital principle presents no obstacle to the reddening of the whole mass of blood. He would have found also that the action of the heart was not only augmented, but that all the secretions were immediately increased; and this even he himself admits to be a great object in the cure of fever. If, on the other hand, he had adopted a false theory (which has already been the cause of great mortality), and used any of the acids, as Chisholm and others had done, with the view of reddening the blood by means of their oxygen, he would then have

* See Dr. O'Shaughnessy's analysis of the blood in cholera, in one of the late numbers of THE LANCET.

found that the nitrous and other acids which contain oxygen in a state of loose combination, not only blacken the blood out of the body, but, when used in the last stage of the yellow fever, kill, not so quickly, yet cause death as certainly as if the patient had received a bullet through the head or the heart.

The Trinidad army physicians talk of having used the sulphate of quinine in the yellow fever, but forget to tell us at the same time that this was neither a discovery of theirs, nor a new remedy in 1828. The practice of giving this salt in the yellow fever was commenced by the French physicians in the island of Martinique, and with considerable success, as far back as the year 1823. The sulphate of quinine, like all the other alkaline salts, reddens the blood out of the body, but it possesses this power in a much inferior degree to the muriate, or the carbonate, of soda, the nitrate, the chlorate, or the carbonate, of potass. The sulphate of quinine has been long used, both by myself and others, in the islands of St. Thomas, St. Croix, &c., and we have found it a useful remedy; but we could only trust to it during the convalescence; and even the Trinidad physicians very properly did not use this remedy until, in ninety-nine cases out of a hundred, the patients were out of danger; that is, until the diseased state of the blood had been previously corrected by other salts, that are much stronger in their saline property.

Dr. Hacket states, that when I visited the hospital at Trinidad, I admitted the similarity of their practice to my own. Now this is a mistake, for I knew, from the rate of the previous mortality, that there must have been some radical error in their practice. I saw clearly what this was, and pointed it out to Mr. Greatrex. They make me to say also, that the only difference in our treatment could not be great, for that we were both in the habit of using *alkalies*. Now this is also a mistake, and I again repeat that it was *not alkalies*, but the non-purgative neutral salts, which they afterwards used in consequence of my recommendation. They say that even before my visit they had used the supertartrate of potass as recommended by Jackson; this, however, is a most improper remedy, particularly where there is, as in these fevers, an excess of acid in the stomach. It is also true that the salts which are formed with vegetable acids are decomposed in the gastric organs, and, consequently, are not to be trusted to in these diseases. The Trinidad army physicians, however, appear to be totally ignorant of this. Dr. Hacket states, that it was the wish of Mr. Greatrex to employ the stronger salts as recommended by me, and that he had consented to their use.

He is not perhaps, however, aware how many lives were probably saved by this great condescension on his part. It is also stated that I approved of bleeding in the warm-bath, and that this in the practice of both was our *sheet-anchor*. Now, though I will not follow the improper example of these individuals, and assert that this is a misrepresentation, yet I do say, that in saying this they have fallen into a great error: for the fact is, that though I consider the free use of the lancet as essential in the first stage of the yellow fever, and said so to Mr. Greatrex, yet, with respect to the warm-bath my conviction was then, and still is, that immersing patients in hot water is a most improper measure, particularly at the period at which they used it; that is, in the first or inflammatory stage of that disease.

In an epidemic of the yellow fever which commenced in the island of St. Croix in July, 1817, after having found the mercurial and other methods of treatment in common use to be of no value, I then gave the warm-bath a fair trial; but in every case that I used it the patient died; and from that moment to this, to the best of my recollection, I have not, even once, used this measure. As I was anxious to prevail upon these gentlemen to give my treatment a fair trial, I had no wish to fret them by finding much fault with theirs. But, under the above circumstances, I could scarcely have said that bleeding in the warm-bath had been my *sheet-anchor* in St. Thomas's; and it is well-known in that island, that in place of *this measure*, I invariably used iced water when it could be procured, for the purpose of removing the morbid heat from the body. I told Mr. Greatrex that ever since I had adopted the *saline treatment* I had not lost even one case, where I had seen the patient early in the disease. In place of this, they make me to say that I had lost no case of fever since I had adopted the practice of *bleeding in the warm-bath*.

Dr. Hacket does not deny that Trinidad is a sickly island; and admits, that the cases which are met with there, are of the severer type of tropical fevers. In a document which is dated Tobago, 23rd of October, 1830, he states that the fevers which he had been accustomed to see in *Trinidad*, for the last five years, were of the *aggravated congestive form*; and they do not pretend to say, that the cases that occurred during the first twenty months after my visit, were more *mild* than those which had been met with previous to that period: but they forget to state that *before this*, under their old Jacksonian practice, the mortality had been great, and that, *after this*, they were most successful. Now there must have been some cause for this sudden change; and though I may admit for a moment

that my theory may be wrong, yet when we find out a method of treating the West India fevers, where in the garrison of a large and a sickly island, during the period of twenty months, out of probably about from seven to eight hundred cases of the various fevers to which the soldiers were subject, and out of this number there were only about eleven deaths, and of these eleven only three died where the men had been under anything like fair treatment,—if this be true, and I believe that it is, we need not quarrel with the saline treatment, merely because there may be some little doubt about the *modus operandi* of these agents.

But it seems now, that the saline treatment had been combined with other measures which rendered it doubtful how much of the success was to be ascribed to the one, and how much to the other. But may I be permitted to ask, at what particular period did these gentlemen become acquainted with these other measures? Did they know of them before the month of August, 1828; and, if they did know of means, previous to that period, by which they could cure or lessen the mortality of the West India fevers, was it not criminal in them to allow the mortality to continue up to the very moment of my visit to that island? And if they did know, previous to that period, of means or measures for preventing the mortality, what excuse can they make for having allowed so many of the soldiers to die? For there is one most important fact which these gentlemen have entirely forgotten to mention, namely, *that the mortality had been very great in that garrison up to the very moment of my visit.*

From the beginning of that year, up to the middle of August, it is a fact, that the Royals alone lost, in that hospital, upwards of forty men from fever.* It is now, I believe, generally admitted, that the blood is a living fluid; and if so, it must, then, like everything else that possesses life, be subject to disease. It is also true that the mortality ceased only from the moment that I had not only directed their attention particularly to the circumstance that, in the fevers which they had to contend with, the diseased state of the blood was the chief cause of death; but pointed out the means by which they might greatly lessen the mortality, by preventing those fatal changes in the vital current, which are, in reality, in these fevers, the great

cause of death. It is also true that from that period they scarcely lost even a case, from fever, except the three men already mentioned, which men had been beyond all redemption before they were admitted into the hospital, and the other two men who died on the very day that they were brought in. But I know well what their practice was both before and after the middle of August, 1828; and I know the result. I know also what those other *unnecessary measures* were which I did not recommend, and which they combined unnecessarily with the saline treatment; apparently, for the purpose of taking to themselves as much of the credit as they possibly could. They knew, however, of all those other measures to which they allude, previous to my visit, yet they allowed the mortality to continue up to that very period: but, I will afterwards prove that some of these measures were too insignificant to have had any effect on the result, while others, but particularly the hot bath, were not merely inert but actually improper; but, above all, when employed at the period at which they used it.

But it seems that the hot bath also reddens the blood. Be it so! and even this is no discovery of theirs: but, for this very reason, of its reddening the blood, the hot bath ought not to be used during the hot stage of a burning fever. It is our duty at this period of the disease not to redden the blood by a hot bath, but to reduce the excitement by means of the lancet, and if the one be proper the other must be quite the reverse. Fortunately, however, they used this measure *only once*, and that too, as I have said, during the hot stage of the * climate fever, when the blood was already but too red; and, then, in the dangerous stage of the disease, when there was great risk of death from sudden diseased changes in the vital current, they did not then trust the reddening of the blood to the hot bath, or to soda either, but to the strongest, and, perhaps, the best, of all the non-purgative neutral salts. This is what they did trust to after my visit, for preventing the blood from becoming black or more diseased in its properties: and, after this, they had, as I have said, scarcely even one fair opportunity of examining the bodies after death, on purpose to ascertain whether the blood had been sodafied or not. Had they not *however* used the saline treatment, it is very probable that their dissections would have continued, after my visit, to be just as numerous as they had been before.

* It was in the first battalion of the Royals that this number was lost from fever: their actual strength was, I believe, about three hundred men, consequently they had lost about one-seventh of their number within a few months previous to my visit. So much for croton oil, active measures, and their old Jacksonian treatment.

* This is also a disease of the blood, but it is not produced by a *poison*; and one of the characteristic marks of the climate or seasoning fever is, that the venous blood is almost as florid as the arterial.

That the mortality was lessened after the middle of August, 1828, is beyond all question; not that the season became more favourable for the British soldier; for August, September, and October, are frequently the three worst months in the whole year; and in twenty months these soldiers had been exposed to every variety of tropical seasons. The mortality did cease, partly, as I believe, because I had warned them against the improper remedies recommended by Jackson, Johnson, and a multitude of others; but, as I have said, chiefly *drawn their attention to the diseased state of the blood, as the cause of death*; but, pointed out the proper means by which this fatal effect could be prevented, in almost every instance where the patients were brought to the hospital early in the disease. These gentlemen admit, that great success did follow my visit to their hospital; but like Corporal Trim, "piqued for the reputation of the army," they claim the sole merit of it themselves, except what is due to Dr. Jackson, Inspector Taggart, and other army physicians. Their great dependence, they say (or one of them at least), both before and after my visit, was promptness and decision, bleeding in the warm bath, castor oil, and quinine. Now these, *except the warm-bath*, are all very proper; but these of themselves are no more a match for the ardent and malignant fevers of the West Indies, than the mercurial practice of Dr. Chisholm, or the leeches and gum-water of M. Broussais. And may I ask, if Drs. Jackson, Johnson, Mr. Taggart or any one else, had taught them how to cure the West India fevers previous to my visit to that island; or, if the warm-bath, croton oil, vitriolic emetics, calomel, sugar of lead, quinine, or alkaliized toast-water, can cure these diseases, may I again ask why they allowed so many of the soldiers to die, and why their dissections were so frequent, up to the very moment of my visit?*

Dr. Hacket, after giving an account of their other measures, gives, I must say, a most unfair view of the nature of the practice which they had adopted on my recommendation, and which has been attended with such signal success; he then states that the sole cause of the cure may as well be attributed to any one of the other means, as that a *little soda* had caused all the advantages which they had obtained, that is, after my visit. The reader will himself see the art with which all this has been

got up. Had they trusted in reality to a little soda, they would not have been more successful after my visit than they had been before; or if Dr. H., who is, I believe, still in the West Indies, will only return to Trinidad, and again give their other measures and a little soda a fair trial, he will find, without the assistance of the active saline agents which they have since used in consequence of my visit, that their old Jacksonian practice and other measures, will again be just as unsuccessful as they had been previous to the month of August, 1828.

When Dr. Johnson first announced the Trinidad documents, he promised to lay before the profession information of the greatest importance. That they do contain facts of great value is perfectly true, but these do not prove exactly the points which he wished them to establish; for, so far from proving that the saline treatment in fever is of no use, they prove beyond all question that this is by far the most effective practice that has been tried, and infinitely superior to the improper remedies which he has been recommending all the days of his life. There is also another important fact contained in these documents—namely, that the same treatment had been tried, and with a similar result, in a fever of a fatal character which appeared in the island of Tobago in 1830. As to the other parts of their information, if there be anything good in these documents, it is merely a repetition of what I have stated in my paper on the blood. These gentlemen have evidently benefited by my practice, but I confess that I can see nothing else for even Dr. Johnson to admire, except the temerity with which one of them, at least, mistates the facts, and the great ingenuity which both of them have shown in endeavouring to conceal from the public the true cause of the great success which certainly followed after they had adopted a new practice, and treated fever on a new principle. As to the rest, their papers are actually beneath all criticism. They have given an account of the derangement in the solids, such as they had met with after death; but they do not seem to be aware of a fact, which I will afterwards endeavour to prove, namely, that all this derangement in the solids is merely an effect of the diseased state of the blood.

I have stated, "that in a malignant form of the yellow fever, which is sometimes met with in the West Indies, the blood was nearly as thin as water, and, literally speaking, black as ink." In fact, it was so thin, that even before death it could scarcely be retained in its own vessels; and when taken from the heart after death, it was so thin and black that it could only be distinguished from the black vomit by a che-

* The statement which I have made of the previous mortality under the old Jacksonian treatment, and the subsequent success under the new practice, give on the authority of an extract from the regimental books, about the correctness of which there can be no question.

mical test. All this is known to be true, to others, as well as myself; and even Dr. Hacket admits, that during the disease the blood is sometimes so thin and dissolved, that it oozes from the gums, eyes, &c. &c., even before death, as stated by me. They admit also that this state of the blood which I have described as peculiar to a malignant form of the yellow fever, which is *sometimes* met with in the West Indies, had also been observed in the military hospital at Trinidad; yet, after having admitted this which I *have* said to be true, they make me to say, on their own authority, that I have asserted that this appearance is *never absent*; and, on the strength of this, their own misstatement, they venture to impeach me for want of candour. But the truth is, that if these gentlemen had only been good enough to have confined their impeachments merely to what I *have* stated, they would then have had *nothing* to impeach; and finding this to be true, they fix assertions upon me which I have not made, and then on these their own misstatements, and these only, they have the temerity to accuse me of errors which I have never committed.

Whatever the immediate cause of the essential fevers may be, it is very clear that this cause must invariably exist. Inflammatory affections of the brain, or derangements of the stomach, which are often, but not always, met with, are merely accidental; and when they do exist, they are only an effect of the diseased condition of the vital current. In the paper which was read at the Royal College of Physicians, I stated my belief that derangement of the blood was the immediate cause of fever, and that death in *bad fevers* was *often*, but not always, caused by its dissolution. Dr. Hacket admits that, on examination after death, "the appearances the blood presented were invariably, and without a *single solitary exception*, dark-coloured, viscid, and grumous." That this is often the case in certain forms of the yellow fever, I have long known to be true; for, under the old treatment, patients frequently died before the whole of the fibrin was completely exhausted, and the imperfect coagulation of this, gives to the blood a grumous, or, perhaps, even when exposed to the air, a viscid appearance. All this, therefore, is nothing new; and I will afterwards prove to Dr. Hacket that the diseased state of the blood in these fevers was better described than he has done it, long even before he was born; but admitting every word he has stated to be true, yet it only confirms, and does not contradict even one syllable of, what I have said, namely, that a deranged state of the blood is the cause of the disease, and a dissolved or diseased state of the blood was in

these fevers often, but not always the cause of death.

I have said that the blood is invariably diseased. Dr. Hacket admits that there is not even a single solitary exception to this rule, yet he denies my statement, which is similar to his own, and then he appeals to others to approve of his veracity; and refers even to Dr. Johnson himself, who I believe has never seen the West India fevers, and who is, I may venture to say, totally unacquainted with this subject.

In the paper which was read at the Royal College of Physicians, I stated that acids, alkalies, electricity, and everything else that decomposes or destroys the neutrality of its saline matter, makes the blood black and vapid. I have also formerly stated that those aerial poisons which act as the remote cause of fever, have an acid effect on the blood, and not only blacken its colour, but derange its properties, even before they excite diseased action in the heart, or any of the solids. I believe, also, that the black colour in the circulating current is not produced by an excess, or by a diminution, of the natural carbonic acid of the venous circulation, for this blackness exists even in the arterial blood. This darkness is, as I believe, caused in the first period of the disease, by the effect of the poison on the blood acting as an acid, or perhaps even as an alkali, and darkens the colour by destroying or decomposing the saline matter. The blood, however, becomes more red during the hot stage; but these fevers run their course with great rapidity, and in a very short period the blood is completely black from the loss, or at least from the great diminution, of its natural saline ingredients, which are, as I have said, the true cause of not only its red, but arterial colour. Now, when the blood has almost entirely lost this, as in the last stage, it is not by acids or other agents that contain oxygen in a state of loose combination that we can restore the vital fluid to its healthy appearance, for this object can only be effected by throwing those saline ingredients into the circulation which are essential to its healthy properties; and are not only the true cause of its red colour, but one chief cause of its stimulating power. These have also, as I will afterwards prove, not only a specific effect in preventing or destroying the action of the aerial poisons, but they are, in fact, more essential to its healthy state than either the fibrin, the albumen, the colouring matter, or any of the other ingredients, of the circulating current, except, perhaps, its vital principle. These are some, at least, of the opinions which I have adopted, and will only relinquish them when I think them to be wrong. Yet Dr. Hacket, who, I believe, has never even read my paper on the blood, undertakes to

criticise it; and gives, I must say, a most garbled and unfair view, both of my theory and practice; after this, he easily, and, as he seems to think, very cleverly, knocks down the fabric of snow which he himself had made, and to show the very great extent of his reading, he makes a very learned quotation from a very rare work, namely, Dr. Paris's *Pharmacologia*, where it is mentioned that in a fever at Leyden, certain physicians in that place had consigned a great number of their patients to an untimely grave, from a belief that a prevailing acid was the cause of the disease; and consequently they had lost their patients from having trusted the cure entirely to absorbents and testaceous medicines. Now in answer to this, which Dr. Hacket pretends to be quite sufficient to upset the whole of my views, I have only to say that though this sort of logic might have satisfied his grandmother, or convinced even Mr. Greatrex, who had like himself an interest in believing, or at all events in trying to make others believe, that they were solely and entirely indebted to themselves for their own success; yet this kind of reasoning will scarcely be satisfactory to any well-informed members of the medical profession, particularly to those who have seen that the Trinidad physicians, by their own admission, allow that the remedies which I had recommended them to use, and which they did use, were neither *absorbents* nor *testaceous* medicines; and had they attempted to redder the black and saltless blood, which we find in the last stage of the yellow fever, with chalk, or any other inert testaceous matter, in place of the active non-purgative saline agents which I had recommended them to use, they would then have been after my visit just as unsuccessful as the physicians of Leyden in 1699; or as they themselves had been in Trinidad with their other active measures, combined with their old Jacksonian treatment, previous to that period.

Whether the saline treatment was or was not the cause of their great success, or whether the treatment which I have recommended be or be not a better practice than that of either Jackson or Johnson, may still be with some a matter of doubt. The Trinidad physicians may think that it is not better: I think it is; but this is a mere matter of opinion, on which I will still retain the right of thinking for myself; and they have as little, perhaps less, ground for accusing me of want of candour for thinking that it is, than I might have for charging them with misrepresentation for thinking that it is not.

It appears that Dr. Hacket is only acquainted with the contents of my paper on the blood, through the medium of Dr. John-

son's Journal; or perhaps it was through the medium of the Doctor himself that Dr. Hacket received information respecting those misrepresentations and pointed assertions, on the strength of which he has founded his attack, and which, I do say, are not to be found in that paper. It is only after he had already made a most unfounded, and I must say a most ungrateful attack, that he then states, "It is painful to me thus to express myself; if I do an injustice by my interpretation of Dr. Stevens's statement, I am truly sorry for it, and would call on him to correct it, or to set me right."

Now I am in possession of more facts on this subject than these gentlemen are probably aware of, and before I have done, the chance is that I *will set them right*; and when Dr. Hacket finds that in place of the misrepresentations and pointed assertions which he says I so unblushingly made in the paper which was read at the Royal College of Physicians, and on the strength of which they have founded their attack; what will he say when it is proved, and it will be proved, that they themselves have admitted every syllable to be true which I have stated in that paper? But, above all, what excuse can he make, when it will be proved, I trust, to the satisfaction of the public, and that too merely in consequence of their own ungrateful conduct, that even if I had made a claim to a share of the successful treatment of fever in that island, I might have done so with the most perfect propriety. If this be proved, and I trust that it will, even to their own conviction, is it not then probable that one of them at least will repeat his own poetical quotation, and wish that he had coined his heart, and dropped his blood for drachmas, rather than have been guilty of such conduct?

It has been stated, that I have founded the superiority of the saline treatment on the result of this practice in Trinidad. This, however, is not correct, for the previous result of my own practice in the island of St. Thomas was of itself quite sufficient to establish the superiority of the saline over the old mercurial, and, in fact, every other treatment that has yet been tried; even from the first I considered the Trinidad practice as a mere secondary concern. At the time that my paper was read at the Royal College of Physicians, I was then on the eve of a long and unexpected visit to the United States of America, and as I could not at that period publish the whole, I gave merely an outline of the facts; but I did not pretend, in that short paper, to give a minute detail either of my own or of their practice, I merely gave an outline of the facts, chiefly with a view of calling the attention of the profession to an essential part of the living body, which I do think

has been too much neglected ; and, unless we attend to this more than we have done, my conviction is, that, in so far as fever is concerned, we shall continue to go on in the dark to all eternity.

I may now state, that it was only in the year 1827, and after a long residence within the tropics, that I found out what I believe to be the fatal error which had been, in reality, up to that period, the true cause of one-half, at least, of the great mortality from fever in the West India Islands. It was then that I commenced a new practice on a new principle, and the success was such as to be almost incredible, for from this period there was scarcely one death from fever in the island of St. Thomas, except in the practice of those who were unwilling to admit any improvement which they themselves had not made. After this period, those who adhered to the old practice continued to be most unsuccessful. One of these gentlemen lost, at one period, in some cases, three out of four ; while, in the same epidemic, Dr. Stedman and myself scarcely met with a loss, after we had adopted the new treatment.

In 1828, after I had given this practice a fair trial, in the island of St. Thomas, we received intelligence that the yellow fever was raging in Trinidad, and particularly fatal amongst the soldiers. It was then that I put myself to some inconvenience and went to that distant island, in the very middle of the hurricane months, with the hope of being able to prevail on the army physicians of Trinidad to adopt the method of treatment which I had found to have been so successful in another island.

On my arrival in Trinidad, and after having explained the nature of my practice to Mr. Greatrex—I may be mistaken, for I quote from memory and from a conversation which occurred more than three years ago—but, in so far as I recollect, he told me that they had tried the various modes of treatment, as recommended in books, but that none of them had been successful ; and that under these circumstances he should feel himself perfectly warranted in giving a fair trial to any practice that had been found to be successful in another island. Mr. Greatrex, who at that period had charge of the hospital, promised that he would give the method I recommended a fair trial, and let me know the result. It was, however, only after a considerable period had elapsed that I received any information from that island, and even then it came in an indirect way. It was only about the end of that year that one of the merchants of St. Thomas's showed me a letter which he had received from a friend of his, a Captain in the Royals, dated Trinidad, 15th December, 1828, of which the

following is an extract:—"If you know Dr. Stevens, pray give him my very best regards, and tell him, that since he was here (four months ago yesterday) the Royal regiment *has not lost a single man*, which I attribute mainly to the lesson which he gave to our assistant-surgeon, and for which God bless him."

I had had the pleasure of meeting the writer of the above letter, as well as most of the other officers in that garrison during my short residence in Trinidad, but I had never seen him before, and I have not seen him since. It is true that he is not a *medical authority*, but his opinion is, perhaps, not the less valuable on this account, for he was not an advocate for any particular practice of his own, and not biassed by any theory. He was not the partisan of either party ; he was guided only by facts and his own common sense. He knew that under their old treatment, and previous to my visit, they had been, in their fever practice, most unsuccessful ; for the battalion to which he belonged had lost, from fever, about one-seventh of its actual strength within a few months previous to that period : he knew also that they had adopted a new method of treatment, in consequence of my recommendation ; he knew that from that period the mortality had ceased, and from these premises he had drawn his conclusions—premises which I believe to be more correct, and more in accordance with the facts, than the conclusions which have been drawn by two, at least, of the opposite party.

I believe I have formerly stated, that in a letter which I received from Mr. Greatrex, dated Trinidad, May 12th, 1829, he admits that they had adopted the saline treatment, in consequence of my visit to that island. He says that they had used a solution of the muriate of soda and nitrate of potass in the proportion, and at the times, that I had recommended. He states that this my *sodaic mixture*, as he calls it, was given, even during the night, and continued until in ninety-nine cases out of a hundred (even including cases of the yellow fever) the fever had abated. And now when the danger was past, they gave two large doses of the sulphate of quinine during the convalescence.

The great mortality previous to the month of August 1828, and the result after, prove that there must have been some essential change in their practice. It was not merely that the soldiers had become all of a sudden more seasoned to the climate ; for the same treatment had been previously tried and equally successful in the island of St. Thomas, even in robust sailors and other strangers who had just arrived from a cold country. In the same letter in which Mr.

Greatrex admits that they had adopted the saline treatment in consequence of my recommendation, he then states,—“the *above system*” (that is, the new system in which the use of the active non-purgative neutral salts constituted the great and the only essential difference betwixt their new and their old practice) “has been applied to three hundred and forty cases or thereabouts.” Now this was the number of cases which had occurred since the period of my visit, and on which the new system had been tried, “and out of this number during the last seven months there had only been three deaths from fever.”

I had received Mr. Greatrex's letter previous to the period that my paper was sent to the Royal College of Physicians, and in that paper I merely stated, that “in August 1828, at a time when there was a good deal of sickness in the garrison at Trinidad, *this practice* was adopted in the military hospital of that island; *that is to say*, they bled freely, and used active purgatives in the commencement, to reduce the excitement; afterwards the saline medicines were administered until the fever abated; and during the convalescence, the quinine was given in large doses. In a communication which I received from Mr. Greatrex, of the Royals, who at that time had charge of the hospital, he states “that the *above system* has been applied to three hundred and forty cases, or thereabouts, including both the remitting and yellow fevers admitted into the hospital, after the fever had existed variously from six to seventy-two hours antecedently to an application to the hospital, with such success, that during the last seven months, not a case had died.” This document is dated about seven months after the commencement of this practice. Mr. Greatrex also states, that within that time *three** men died having the remitting fever; but they had also abscesses in the lungs, and purulent expectoration. As these three cases were complicated with extensive organic disease in the lungs, it is probable that they would have been fatal under any treatment. But, out of the three hundred and forty cases of essential fever, which had been treated in the manner described, there was not even one death in the Royals, from the time that this practice had been adopted; and I may add, that in the West Indies, Trinidad is generally considered as one of the most sickly islands.”

Now after all the noise that they have made, will the reader believe, that in the

paper to which they refer, this is every syllable of what I have stated with respect to Trinidad; or after all the improper language that they have used, will it be credited, that they themselves admit every syllable of this to be true. Dr. Hacket admits that this was “*really their practice* ;” and Mr. Greatrex admits the result to be correct as stated by me. I did not say, as I might have said, that I had gone to that island, with the hope of being able to lessen the mortality by prevailing on the army physicians to adopt a practice which I had found to be so successful in St. Thomas's. I did not say that the saline treatment, or any other was the cause of the great success which followed my visit to their hospital. I merely stated such was their practice, and such the result. But if I did not say then that the success which followed my visit to that island was in some measure owing to that circumstance, *I do say so now with the most perfect conviction that such is the fact.*

If I did not, in that paper, notice the error into which Mr. Greatrex had fallen, about the blood not having been reddened by soda in the men, who had never taken even one particle of that alkali; or about his calling the saline practice, as he had used it, the treatment of Jackson, or if I did not notice his still more serious mistake about reddening the blood with a hot bath in the inflammatory stage of the climate fever, it was merely because I wished not to injure him, by pointing to what I considered as his errors, but rather to do him good; for, whoever the author of this successful practice may be, I believe that the run of success which they have had in Trinidad has no parallel in the army practice in any other sickly island in the West Indies; and as the merit of having *adopted* this practice was due to Mr. G., I thought I might serve one who appeared to me, when I saw him, a very interesting young man, by directing the attention of the army medical board to this circumstance. Such was my conduct, these were my motives, and God knows, I have had my reward.

In a letter written by Mr. Greatrex, apparently in answer to a letter of Dr. Johnson, dated July 24th, 1831, he states, “I have withheld, as you have seen by Dr. Hacket's letter, all authority for the publication of a part of my letter. Still Dr. Stevens has published a garbled extract from it.” Now, whatever Mr. G. may have said in his note to Dr. H., or in his letter to Dr. Johnson, I do most solemnly declare that I have never received any note or any communication from Mr. G. except the document which Dr. Johnson has published, and which I may here observe is incorrectly printed in his journal.

* Dr. Johnson in his version of Mr. Greatrex's letter leaves out the word *three*; thus leaving it to the imagination of the reader to suppose any number that he pleases. This may be an error of the press; but I think proper to notice it, lest it might be supposed that the error was on my side.

Now, mark this. The Trinidad army practitioners admit, even by their own confession, every syllable to be true which I have stated in the paper to which they refer. They admit that their practice was such as I had described. They admit also, that my statement was correct with respect to the result. They admit all that *I have said to be true*, and yet in the same document, and at the same time, they accuse me of having made *pointed assertions and misrepresentations*, which I do say *I did not make*.

I am afraid that Dr. Hacket has been most miserably deceived by some of his correspondents in this country; but, at all events, it was the imperious duty of that gentleman, before he had ventured to make such an attack, first to have proved that I had in reality made even *one assertion* that was not correct, or drawn even one solitary conclusion which I had no right to have drawn. Had he done this, he would then, at all events, have been right in his premises, however wrong he might still be with respect to his conclusions; but the fact is, that he is wrong in both: for I do deny that I either drew a conclusion which I had no right to draw, or made even one single assertion which I had no right to make, and if I have, let them point out where it is, for I cannot find those pointed assertions which he says are not true, in my own paper, neither are they pointed out, much less proved, in his,—it is only on the strength of mistatements which exist merely in their own imaginations, if they do in reality exist even there,—it is only on *assertions* of their own fabrication, that they have founded the whole of their attack. It is only on their own idle assumptions which they are pleased to call my assertions, it is merely this imaginary or baseless cause of complaint which they themselves have conjured up, which they consider as quite sufficient for accusing me of misrepresentations and want of candour.

It is not the statement which *I have made* that they attempt to deny. It is only conclusions which I did not draw, misrepresentations which I did not make, and pointed assertions which *I had not made*, that Dr. Hacket has been pleased to contradict “in the most pointed and unqualified manner,” and no person has a better right to do this than himself; for all the assertions that are not correct, and the whole of the misrepresentations which have been made on this subject, are entirely of their own fabrication. It is they themselves who have mistated facts, by asserting that I have made pointed assertions which I did not make, and that I had drawn conclusions which I did not draw, and then, after mistaking these their own idle assumptions for facts, they commence an attack upon false

premises, and in conducting this, two of them at least use language that would not be creditable even to a corporal—language which is certainly not either becoming or proper in deciding on the treatment of an awful disease, and which would not have been at all necessary if they had merely been under the influence of the warm glow of truth, in place of an evil passion which Dr. Johnson candidly admits to have been the principal reason why he attacked me, and that too without the slightest provocation on my part.

Dr. Johnson states, that for the honour of the profession he had made up his mind not to publish the Trinidad documents; “but,” says he, “my attention being roused by the announcement of Dr. O’Shaughnessy’s paper, and more especially by the very strong terms in which Dr. Stevens’s discoveries were spoken of in a contemporary, in which it is stated that Dr. Prout considered these discoveries as of the last importance to mankind, I felt that it would be culpable, if not criminal in me, to withhold from the profession the counter-statements of the army medical officers of Trinidad, from which it appears that *soda* had nothing whatever to do with the *success* of the treatment in the above island.” Now, if Dr. Johnson cannot bring forward any better reason than this for such an attack, he had better perhaps have kept it to himself, particularly if he has no other documents than those which he has hitherto produced, to prove the serious charges which he made against me in the Westminster Society, and which, I must say, were most untrue. So much for the manner in which Dr. Johnson proposes to maintain the honour of the medical profession.

As I have said that Dr. Johnson has been guilty of wilfully endeavouring to mislead the public with misrepresentations about *soda* and *sodafied blood*, it may be but fair to show, by his own evidence, that this is the case, I therefore beg leave to refer you to his review of my paper on the blood, in which you will find the following extracts, contained in his number for June 1830.

“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. As the views which Dr. S. entertains are novel, and the means which he proposes for combating a dreadful scourge of the human race are *simple and practicable*, we have endeavoured to collect the substance of the writer’s observations as accurately as possible, to lay before our readers.

“Our author observes that one common property of neutral salts is that of giving a rich arterial colour to venous blood. This property is common to them all, and the de-

gree to which they possess it is, perhaps, the best test of their purity as saline agents. To ascertain the effects of different agents on the blood, he made a number of experiments, in which it was observed:—

“1st. That all the acids give a dark colour to healthy blood, and in proportion to their strength, change it from red to black, as certainly as they change vegetable colours from blue to red. Even the vegetable acids so completely blackened the blood, that the addition of a little water converted the whole into a fluid exactly resembling the black vomit. *Secondly, the pure alkalies have a similar effect with the acids, in changing the blood from red to black, though not in the same degree.* Thirdly, the neutral salts immediately changed the venous blood from a dark modena red, to a bright arterial colour. Even those salts that contain a slight excess of alkali, the sub-carbonate of soda for example, immediately give to venous blood a beautiful bright arterial colour. The effects of these experiments are best seen when made on healthy blood. The agents ought first to be dissolved in a little soft water, and then well mixed with the warm blood before it begins to coagulate.

“4thly. When the neutral salts are mixed with the dark and dissolved blood that had been taken from the hearts of those who had died of yellow fever, even the black and dissolved fluid was instantly converted from a black to a bright arterial colour.

“The nature of this paper (said Dr. S.) prevents me from entering minutely on the important effects which this saline impregnation produces in the vital fluid; but, in a work which will soon be published, I shall endeavour to prove, first, *that the blood owes its red colour to this saline impregnation.* Black appears to be the natural colour of the colouring matter; for, when we take a clot of blood, and deprive it completely of its saline matter, by immersing it in fresh water, the colouring matter soon becomes so black, that even oxygen has no effect in changing its colour. But, when we immerse this black clot in an artificial serum made by dissolving some saline matter in water, the black clot in this clear fluid assumes almost immediately a beautiful bright arterial colour. Secondly, that to this saline impregnation the fibrin owes its fluidity, &c. &c. Thirdly, that the change of form which this saline matter undergoes, when the blood changes from arterial to venous, and from venous to arterial, alters its capacity for caloric, and gives it an influence in supporting the temperature of the system. *The saline impregnation also adds to the stimulating quantity of the blood, and assists, even in a high temperature, in adding to its powers of self-preservation.*”

“As we have no doubt that Dr. Stevens will pursue this interesting inquiry further, and lay the results of his observations before the profession in a more extended form, we shall abstain from any comments on the present occasion. We have laid a very full and faithful account of the paper before our readers, and leave them, for the present to draw their own conclusions.”

As I trust that many of your readers will feel an intense interest on this subject, even though divested of every thing like unbecoming personalities, and as most of them have not seen the West India fevers, it may be proper for me to make some observations on the nature and treatment of these diseases, but above all to point out the *fatal error* in the old practice, which has been, as I believe, the true cause of one half of the mortality in the West India fevers, and perhaps even a great deal more.

This I will do as early as possible, and lay it before the public as soon as I can.

I am, Sir,

Your obedient servant,

W. STEVENS, M.D.

Albany Street, Jan. 9, 1832.

THE LANCET.

London, Saturday, January 14, 1832.

How just is nature in all her laws!
How wise in her decrees! Were it possible that guilty men could perpetually stalk abroad in the guise of honesty, who would be shielded from the hand of the assassin? Who enabled to protect himself from the midnight bandit? Nature proclaims to all men who commence a course of iniquity, “Thus far undetected ye shall proceed, and no farther. Go beyond this boundary but one step, and ye shall stand before the world, self-confessed of your crimes.” That step, that fatal step, has been taken by the JUNTO,—the self-perpetuating, criminal-prosecuting JUNTO, who, alarmed at the prospect of tumbling headlong into the gulf which yawns beneath them, “confess” in the hour of their peril, that their “Regulations” have had “a tendency to ENCOURAGE both teachers and students to direct violation of the law, and to estab-