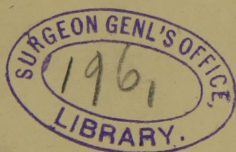


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THE

PENNSYLVANIA JOURNAL OF MEDICINE

Lexington 1831, IV

AND THE ASSOCIATE SCIENCES.



ART. VIII.—*Remarks on a Review, in the May number of the Am. Jour. of Med. Sciences, of "A Treatise of Pathology and Therapeutics, by JOHN E. COOKE, M. D., &c."* By the author of the Treatise.

A TREATISE of Pathology and Therapeutics, published by the writer of the following observations in 1828, and for sale at Carey, Lea and Carey's bookstore in Philadelphia since that time, has been noticed in the last number of the American Journal of the Medical Sciences, (No. XV, May, 1831,) it is presumed from the signature, by one of the collaborators of that periodical. It is proposed to make some remarks upon that review in this journal; and although they may not be as extensively circulated, and will not even be presented to the same readers, as the review, yet it is the best that can at this time be done to set the views of the author in a correct light. If the Editor of the *Am. Jour. of Med. Sciences* should consider them fit to publish in his periodical, his sense of justice will, it is presumed from his reply to the application made to him to publish an answer to the review, cause him to give them a place in his next number.

The Reviewer, after an introduction complimentary to the West, proceeds thus.

"The treatise opens with some judicious observations on the proper method of pursuing medical investigations, in order to arrive at just and permanent principles; and the author next expatiates on the causes which have retarded the progress of the science. The great cause of this evil he attributes to the extreme proneness of physicians to frame hypothetical theories from partial and limited premises, instead of tracing up the varied phenomena of disease through their chain of causes to the first or remote cause of the derangement, in the true spirit of the inductive philosophy. After such an explicit avowal of views which we have been taught to consider as the only sound procedure for the attainment of a correct system of pathology, aided, however, by a knowledge of the laws of physiology

and autopsic inspection, we did not expect to find the author straightway propounding a theory of his own, which we fear will be found to rest on no better data than he had the page before deprecated. This theory is no further novel than in the extensive application made of it to explain the production of the multiform characters of disease, but as it constitutes the key-stone of the system of pathology, which this treatise is devoted to verify and enforce, we shall give it in the author's own words, that the reader may be fairly put in possession of the aim and tendencies of the work under consideration."

With this view the reviewer then proceeds to give an extract from the preface to the treatise. This however the author cannot admit as giving a fair view of his work. That part of the preface given in the review was written to show, not the doctrine of the treatise, but merely the manner in which he was led on to investigate the leading points of that doctrine. And although these leading points are mentioned in the preface, they are barely mentioned; and he is persuaded that every man who takes the trouble to read the two pages of the preface given in the review, and compare them with the whole tenor of the work, will say, that this extract does not "put the reader fairly in possession of the aim and tendencies of the work under consideration."

The assertion contained in the above quotation from the review, that the author has straightway propounded a theory of his own, and the intimation, that he has departed from the proposed plan of investigating the subject, the author thinks far from being well founded.

The first chapter of the work is occupied by a notice of the theories of various deservedly celebrated writers, and an attempt to point out the manner in which the investigation should be conducted. In the 10th and 11th sections it is said,

"The remote causes of disease, operating on the human system produce uniform effects. This must be the consequence of their operating in an uniform manner, and of the system's acting, under their influence, uniformly according to certain laws by which it is governed. The exposition of this manner and of these laws, or a statement of the immediate effects of the remote causes on the system, and of the changes wrought in it in consequence of these effects, will show the whole connexion between the agents ascertained to be the remote causes of disease, and their effects the symptoms; and is the true theory.

"In the following pages I shall attempt the investigation of this connexion, or, *a statement of the immediate effects of the remote causes on the system, and of the changes wrought in it in consequence of these effects.*

The course which, it is believed, the inquiry must necessarily take to be successful, is stated in a few observations which follow."

The sum of these observations is given in section 18 in the following words.

"Applying these manifest truths and inferences to the phenomena of disease, the remote causes of fever, and the whole train of symptoms proceeding from them, denominated a febrile paroxysm, with all the intermediate links, constitute a series of causes and effects: &c."

In section 19 an inference is drawn from section 18, in the following words.

"Hence it is evident that, in order thoroughly to investigate *the connexion between the remote causes and the symptoms of disease*, we may inquire into the effects of the causes ascertained; next into the effects of those effects, or new causes; and so on, descending until we arrive at the ultimate effects, the symptoms in question; or, that we may inquire into the causes of the symptoms, and into the causes of those causes, and so on, ascending until we arrive at the remote causes; or, that these two modes may be combined, and that the agreement of the two in the same result is a strong confirmation of its truth."

After showing at some length that this mode of investigation had not been followed by the authors of the theories that have been promulgated, the chapter concludes in these words.

"I shall now attempt the investigation of this connexion by inquiring into the effects of the remote causes; next into the effects of these effects, or new causes; and so on descending to the ultimate effects, the symptoms in question."

After such an explicit avowal of views which we too have been taught to consider as the only sound procedure for the attainment of a correct system of pathology, it would indeed have been extraordinary to find the author departing from the plan proposed and straightway propounding a theory of his own. But he cannot agree with the reviewer, that this has been done "*in the work under consideration*." On the contrary, the plan proposed has been most rigidly adhered to, it is believed, throughout the whole. It is not intended to say that the book contains no theory. On the contrary, the absolute necessity of a knowledge of the true theory or explanation of the operation of the remote causes of disease in producing their effects, in order to scientific and successful practice, is strongly urged. It is a hypothetical theory of which the reviewer speaks, and which the writer has in view here. The work in question is devoted to the explanation or theory of the operation of the remote causes of fever.

But this explanation may rest on supposition or conjecture, as have many explanations or theories of disease. It was the intention of the author to avoid this entirely; and rigidly to pursue the inductive method of philosophising.

The second chapter commences the investigation on the plan laid down in the first. After showing in the first three sections, that the action of the heart is measured by the force of the pulsation of the arteries, and by the temperature and colour of the surface and extremities; and stating, in the next section certain agents which have been considered remote causes of fever, the author lays down the proposition, (section 85) that "*all the remote causes of disease directly or indirectly weaken the action of the heart,*" without propounding any theory, or intimating to what this proposition is to lead; and in the following sections endeavours to show that this is true of each remote cause separately.

In sections 86-88 it is shown that abstinence from food is a remote cause of fever, and produces weakened action of the heart.

In sections 89-90, the same is shown to be true of fatigue.

In section 91, the same is shown of external violence from a blow or a fall; in 92, the same is shown of the depressing passions; in 93, of want of rest or sleep; in 95-96, of every kind of stimulant used to excess; in 97, of strong tea and coffee; in 98, of intense application of the mind; and in 99 &c. of cold.

The 3rd, 4th, 5th, and 6th chapters consist of an argument, rigidly upon the inductive plan, to show that a dense gas the product of the putrefaction of vegetable matter is the remote cause of hot weather endemics. Several doctrines are noticed in the course of this investigation, and among others Dr. Ferguson's, at length.

In the 7th chapter, section 595, it is shown that "weakened action of the heart is the consequence of respiring the dense gas arising during the putrefaction of vegetable matter."

In the 8th chapter, the winter epidemics, called bilious pleurisy, &c. are shown to be the effect of the joint operation of the same dense gas, and of cold.

The chapters 10 to 16, inclusive, contain arguments to show that contagion is not the remote cause of plague, yellow fever, or typhus, but that these diseases arise in precisely the same circumstances in which the annual hot weather endemics of this continent arise; and that the doctrine, that a general epidemic constitution of the atmosphere

is the cause of the diseases of hot weather, is unfounded. In these chapters the evidence is drawn almost entirely from writers who advocate the doctrine of the contagious nature of these diseases.

In the 17th chapter, section 1204, the following is given as the sum of the argument.

“We have now seen that all the known remote causes of fever weaken the action of the heart, and that weakened action of the heart precedes every fever. (691*) Weakened action of the heart is therefore one link of the chain of causes extending from the remote causes to the symptoms of fever; and all those causes which produce this link of the chain, are themselves remote causes of fever; inasmuch as all those causes which produce it, are remote causes of its effects.”

After a few inferences from this section respecting some other remote causes of fever, occupying less than a page, the volume closes; and the author ventures to say that a man might read the whole and have no conception of the theory to follow.† He has not time to read over the whole work to ascertain the fact, but is confident that in the whole seventeen chapters of the first volume, there is no part of the theory advanced but what has been mentioned; viz. that the remote causes of fever directly or indirectly weaken the action of the heart, and that weakened action of the heart precedes all fevers. The last words of the first volume are, “We next proceed to inquire into the effects of weakened action of the heart:” and with this inquiry the second volume opens.

*The following references in support of the last clause of this sentence are given at section 691.

Boerhaave's Practical Aphorisms, Eng. translation, 563.

Cullen's First Lines, xxxiv, xlvi.

Darwin's Theory of Fever, Supplement, l. 1. 6.

Rush's Works, vol. 3, p. 3, 4.

†In the winter of 1827 the manuscript of the work in question was delivered, with occasional remarks and some omissions, as a course of lectures, the first delivered by the author in Lexington. A number of the intelligent and inquisitive of the class were struck with the novelty of the course pursued, and visited the writer and were very urgent to know what doctrine or theory he had in view—what was to be the end of the discussion they were daily hearing, of principles, the tendency of which they could not perceive. Their inquiries were for some time evaded; they were told to be satisfied to proceed step by step, and inquire whether the successive points discussed were established, or not; so that they might be unbiassed by preconceived notions, when they should discover the conclusions resulting from the doctrines advanced. These visits were however repeated, and at length the visitors became so importunate in their requests, that when the winter was well advanced, the writer gave them a sketch of the whole.

In the first section of the second volume the question is proposed, What is the effect of this cause, weakened action of the heart?

In the next section, it is answered: One consequence immediately flowing from weakened action of the heart, is the diminution of the quantity of blood sent into the arteries; whence follows weakness of the pulse, a very obvious consequence of the diminution of the power which distends the arteries; paleness and coldness of the surface, consequences, as certain, of the failure of the usual supply of blood to the surface; and diminished bulk of the external parts, shrinking of the features, and shrivelling of the skin, all necessarily following deficient fulness of the cutaneous and subcutaneous vessels.

In the following sections, 1211 to 1218, it is shown that another consequence of weakened action of the heart inevitably following in every case, is an accumulation of blood in the great veins entering into the heart; and in the sections following, it is shown that this accumulation cannot be confined to the vena cava, but must extend into the large branches of that vein which proceed from the head, the liver, the spleen, and stomach and bowels, including the hæmorrhoidal veins, the veins from the kidneys, and the veins from the pelvis including the uterine branches of the internal iliac veins.

This being established, section 1248 runs thus;

“Accumulation of blood in the venous cavity” (the vena cava and branches above-mentioned) “being the effect of weakened action of the heart, is another link of the chain of causes extending from the remote causes to the symptoms. The next question is, What are the effects of this cause, accumulation of blood in the venous cavity?”

Chapter XIX is taken up with the answer to this question. It is there shown that accumulation of blood in the vena cava and its branches produces various effects; viz. enlargement of some parts, disturbance of the functions of the head, the stomach, the liver, the kidneys, and the uterine system; which it is unnecessary further to notice at this time, as the object now is to show not the truth or correctness of the various arguments or explanations, but that the proposed plan of investigation is followed. The writer has also in that chapter attempted to show that the alternations of low and high action in fever are the consequence of the operation of sudden accumulation of blood in the vena cava, upon the heart, exciting it to action too great to continue and therefore after some hours falling back into weakened action; thus allowing of a gradual return of the accumulation of blood in the

vena cava, &c. ready, as soon as the heart becomes capable of it, to excite it again to increased action; again to subside into weak action; to be followed by accumulation, and increased action; the whole to be repeated as long as the heart can be thus excited; which, observation shows, is rarely more than twenty days, often not more than fourteen; though sometimes for months.

The twentieth chapter contains the mode of practice founded upon the doctrines advanced in the preceding part of the work. It is not intended to say that the mode of practice laid down is entirely different from that formerly practised by the writer, as well as others, in every case; but that the principles upon which it rests, the reason for the different prescriptions, as well as the object to be held steadily in view, are rendered, at least to the writer, and to many with whom he has had communication, more clear and more intelligible; so that the practice is to them simpler, plainer, and more satisfactory. There are some cases, too, in which the practice of the writer and many of his friends, has been entirely changed, in consequence of the inferences drawn from the doctrines of the work in question. Thus, in menorrhagia, and in threatened abortion, the practice is so opposite to all preconceived notions on the subject, that the writer has always had a difficulty in inducing patients to submit, except when they had previously acquired entire confidence in him—and yet the practice has been altogether successful up to the present time.

The remaining chapters of the second volume are occupied with the application of the general principles in the first twenty chapters to various chronic affections.

It is evident from this account, if it be a correct one, that the author has not straightway propounded a theory such as he has reprobated, but that he has endeavoured to adhere to the mode of investigation proposed. If he has failed in this, he is willing to admit it as soon as he is made sensible of it. Until then he must necessarily rest in the belief that he has not built upon hypothesis, but that what he has advanced consists of facts and fair inferences from them.

The reviewer after giving the extract from the preface of the treatise above-mentioned, proceeds to object that the theory of the author "is much too mechanical for general reception in this age of physiological and vital pathology."

The author had been taught to believe that "the inductive philosophy" is "the only sound method of procedure for the attainment of a

correct system of pathology," and therefore that whatever conclusion this leads to, should be received, without respect to the pre-judgments of any age.

The reviewer says,

"It is no where shown that the liver is actually in a state of congestion, or that the vena cava and its branches seriously suffer from an accumulation of blood in the precursory stage of fevers, though it must be admitted that such accumulation to a certain extent constantly takes place during the continuance of rigors, from whatever cause induced: consequently we are constrained to class this theory, according to the author's own definition, among those hypotheses which have been so long the bane of medicine."

It is shown in sections 1218, 1220, 1229, that accumulation or congestion of blood in the vena cava and the liver is the inevitable consequence of weakened action of the heart, which always occurs in the forming stage of fever; it is known from the manifest enlargement of the liver and spleen in the living body, as well as from dissections of such as have died in the cold stage of fever, that this state of the vena cava and the liver exists; and the reviewer himself in the very passage quoted says, "it must be admitted that such accumulation to a certain extent constantly takes place during the continuance of the rigors, from whatever cause induced;" and *yet he concludes*, "consequently we are constrained to place this theory among those hypotheses which have been so long the bane of medicine."

Speaking of the endeavours of the author to prove that all the remote causes are either directly or indirectly debilitating, the reviewer says;

"On this point we would remark, that whatever may be the mode of operation of these causes, it cannot be denied that fever does not take place until a general or local irritation is set up, which is, indeed, itself a state of fever, whenever the irritation is sufficiently intense to affect the rest of the system."

This seems to the writer to amount nearly to the following. Fever does not take place until fever is set up; the substitution of the word *fever*, for *irritation*, being entirely admissible, if irritation be, as the reviewer says, a state of fever whenever it is sufficiently intense to affect the whole system. If this be incorrect, the passage is not understood.

The reviewer proceeds:

"Besides, the view taken of the operation of the remote causes, necessarily supposes fevers to be idiopathic and general affections, a doctrine fast vanishing from the minds of physicians," &c.

Must the inductive method give way because it leads to conclusions inconsistent with the fashionable doctrines of the day? The reviewer further says;

“We will merely observe in passing, that if it can be shown, as we believe it can be, that the fevers heretofore esteemed idiopathic arise from local irritations, and are nearly allied with phlegmasial affections, that then whatever may be the nature of the remote causes inducing them, that these causes must be either directly or indirectly essentially stimulating in their operation, a conclusion in contravention of the author's theory.”

The forepart of this quotation seems to the writer, to admit that the doctrine, that the fevers called idiopathic arise from local irritation nearly allied to phlegmasia, is not yet established; and the writer fully believes never will be. After this admission, it seems to the writer to be taking rather more ground than yet belongs to the reviewer, to draw his “conclusion in contravention of the author's theory.”

The reviewer next proceeds to examine a little in detail the author's explanation of the mode of action of some of the more frequent of the remote causes of fever. He “admits that the first effect of abstinence is not only weakened action of the heart, but of all the powers and functions of the system. This state of depression, however, is not a state of fever; on the contrary, it is universally considered one of our most efficient means to remove or alleviate an existing febrile affection.”

This state of depression is not said by the author to be a state of fever. Such a state sometimes is not followed by fever, though it generally is. The question however arises, does not this admitted weakened action of the heart cause necessarily an accumulation of blood in the veins entering into the heart? Where else can the blood be accumulated, when the heart is sending on a quantity less than usual? What are the effects of this accumulation? manifestly it must produce increased action whenever the heart becomes capable of it, under the continually increasing quantity of blood pressing into it. And if the heart be incapable of it, the load must continue in the veins, and produce such effects upon the parts concerned, as such an accumulation is capable of producing.

As to the objection, that the depressing operation of abstinence is universally considered as one of our most efficient means of removing or alleviating an existing febrile affection, the writer thinks it is destitute of force. The very same might with equal propriety be made against admitting cold to be a remote cause of fever; which the

What makes the heart
capable of it's

reviewer does not question. Of the depressing effects produced by cold it might be said with equal propriety, "This state of depression is not a state of fever; on the contrary, it is universally considered one of our most efficient means to remove or alleviate an existing febrile affection." True, the depression is not a state of fever; but it leads to a febrile state; and when fever is produced, as it very often is, the state of depression is called the cold stage of the fever. And, notwithstanding that cold is universally and justly considered as one of our most effectual means of removing or alleviating an existing febrile affection, yet is it undoubtedly a remote cause of fever; and the reviewer admits it. These reasons, therefore, for denying that abstinence is also a remote cause, are not valid.

The Reviewer proceeds to say;

"When abstinence is carried to the extent of producing disease, it does so not by weakening the action of the heart, which effect rather wards off for a time the evil, but by changing the character and qualities of the circulatory fluids, and rendering them so acrid and irritating as to produce not only most intense inflammation of the stomach and intestines, but also of other tissues of the system, which, when they prove fatal, destroy life amidst unexampled pain and suffering," &c.

Does the reviewer seriously offer this explanation as one founded upon observation and the inductive method of philosophising? Has he produced any evidence of this acrid and irritating state of the circulating fluids? Has he any to produce? The writer knows persons who suffer excessively from abstinence but for a few hours. Is their blood already in this acrid and irritating state? On eating a few mouthfuls, the pain is removed. Is this by removing the acrid and irritating state of the circulating fluids? The writer has known the painful sensations arising from abstinence to go off in a few hours, though the abstinence was continued, and the person abstaining the whole day, to rest in the latter part of it entirely free from pain. The case stated by Dr. Currie of Liverpool, in his Medical Reports, is entirely opposed to the Reviewer's opinion on this subject.

A gentleman was prevented by a scirrhus tumour of the œsophagus from swallowing. The difficulty gradually increased with the increase of the obstruction. From October 17th "he was able to swallow only a tablespoonful of liquid at a time, and with long intervals. It was with difficulty that he got down seven or eight spoonfuls of strong soup in a day, and this quantity gradually diminished. On the first of November, the passage seemed wholly obstructed." After

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cause of the cold stage*

that he continued to swallow two or three tablespoonfuls of milk daily, till about the 15th November; but though it rested in the œsophagus for some time, it was constantly ejected at last; and for the last twenty days of his life he made no attempt to swallow. After that time an attempt was made to sustain him by the regular administration of clysters of broth. The result of this remarkable case was, that until Dec. 1st "no man had ever perhaps approached death by steps more easy." (p. 212.) This was fifteen days after he had desisted from making any attempt to swallow. On the first of December, however, a change took place. "On the morning of the 1st December it" (the pulse which had for a month previously been healthy,) "became small and frequent; and still more frequent after the delirium commenced; the state of the pulse, as well as almost all the other symptoms from that time forth, very nearly resembled the symptoms of the last stage of fever, when it terminates fatally."

The reviewer denies that famine is a remote cause of fever on the ground that a famine in France in 1817, produced certain chronic diseases, but that there was a remarkable exemption from febrile diseases. The author has, however, equally good testimony to show that famine has produced epidemic disease. Russel, in his history of the plague at Aleppo in 1760, 61, and 62, says, (p. 9) "Through the summer of the year 1757, grain of every kind bore a very high price, and as the winter approached, became scarcer and dearer; insomuch, that from the month of December in that year, till the following June, most parts of Syria and Mesopotamia might be said to have suffered all the miseries of extreme famine. In the month of February, 1758, a malignant petechial fever made its appearance at Aleppo, and advancing rapidly with the spring, raged throughout the summer and part of the autumn. This fever reigned with influence not less extensive than the famine, and both together produced every where a mortality little inferior to that of the true plague."

Here then is evidence on both sides; but with this material difference. That of the Reviewer is negative; that of the author positive. The former proves only that in the case specified, famine did not produce fever. The latter proves that fever did accompany famine; and the capability of an agent to produce certain effects being shown in one instance proves that agent to be one of the causes of those effects. Indeed, it would be no difficult matter to multiply proofs that famine produces fever. Numerous instances might be cited of famine in pla-

ces besieged being followed by pestilential fevers; and these have ever been considered as so inseparable, that if famine prevail, pestilence is confidently expected shortly to make its appearance.

Moreover, the testimony produced by the Reviewer is not only inferior in character, but it is by no means inconsistent with that produced by the author. An agent universally admitted to be a remote cause of fever may and often does produce *chronic* and not *acute* epidemic disease. Of this the occurrences on board of the Channel Fleet of England in the years 1794, 95 and 96, afforded a striking example. In 1794 an epidemic fever prevailed on board the fleet, in 1795 an epidemic scurvy, and in 1796 there were numerous cases of both scurvy and fever among the crews of the ships.*

The reviewer thinks that when fever is produced by excessive bodily exertion or by stimulant drinks, that it is during the excitement and hurry of the circulation. This the author would not call a state of fever. The fact is, that febrile excitement frequently follows the debility which the Reviewer admits to be often consequent on excessive bodily exercise; as well as the debility left after a night of intemperance. This however, is a matter respecting which every man must form his own opinion from his own observation.

The Reviewer says; (p. 129)

“In making contusions from external violence, as falls and blows, the cause of fevers, the author has drawn his argument from the analogy of their first effects to the concussions of the brain from the same causes.”

The author is not aware of having done this. The whole he has said on the subject is given in section 91, as follows.

“External violence from a blow or a fall produces fever and various morbid symptoms, besides the local symptoms from contusion. Blows have the effect of reducing the action of the heart in a remarkable manner. A violent stroke, not on the head only, but on any part of the *body*, will in an instant stop the action of the heart. Falls have the same effect precisely, it being immaterial whether a body be impelled against a man, or he against the body. I have seen a person who had fallen from a horse continue for some time with a feeble pulse, pale countenance, and yawning and stretching, as in the cold stage of fever.”

The author did not intend in this passage to represent *contusion* as the cause of fever. He has seen fever follow the state of debility produced by a fall, when there was no contusion properly speaking. He has had a case of nephritis produced by a fall from a horse in which

* See Trotter's *Medicina Nautica* vol. i.

there was not the slightest appearance of contusion, and the patient it is believed lit upon her feet.

Neither does the author, in making blows and falls, remote causes of fever, draw his argument from analogy, &c. He merely states the fact that they do produce fever; as may be seen by referring to the first sentence of the quotation given above. Argument in such a case is superfluous. Every man who has not yet made up his mind on this subject, must do so, if at all, from observation. The whole passage above quoted, after the first sentence, is occupied with showing that blows and falls weaken the action of the heart—and in support of this, reference is made to observations made by the author; but no argument is used.

The author rests upon the alleged matter of fact for the truth of his theory, so far as there stated. The reviewer in objecting, combats alleged matter of fact, not by an appeal to observation, but to an argument founded upon a hypothesis, in which a case is supposed that was not in the view of the author at all when he wrote the passage in question; viz. a case of contusion in which the part is so injured as to become “the seat of irritation and fluxion.” So far from having such in view, the author expressly excepts that case in the first sentence of the quotation; and the tenor of the remainder of it, accords with that meaning. He will allow, however, that another word instead of *besides* might have conveyed his idea more clearly to the mind of the reviewer. The passage might have been expressed thus. “External violence from a blow or a fall produces fever and various morbid symptoms, *independently of*, or *over and above* the local symptoms proceeding from contusion.” Nevertheless the word *besides* does certainly bear the same signification with the other words used above; and that this was the intention and meaning of the author is evident from the tenor of the subsequent part of the passage, from the express declaration in section 1858, latter clause, and is well known to all those who have heard his views at large.

The Reviewer says; (p. 130)

“Cold is one of the most constant and evident causes of disease, and the author endeavours to prove, chiefly from the facts furnished by Currie, that it invariably does so by weakening the action of the heart.”

This is a singular misapprehension of the passage, it appears to the writer. The author in speaking of cold, does not, as he had done in the case of the previously mentioned causes, say any thing in the

commencement of his remarks on cold, of its being a remote cause of fever. He enters immediately upon the subject of its power to reduce the action of the heart. This he endeavours to show by reference to the well known experiments of Currie, to those made by Dr. Stork of Bristol, and Drs. Spooner and M'Donnel of Edinburgh, and to experiments and observations of his own. (Sections 109, 111, 112, 113.) This argument occupies 16 pages, in which there is no mention of the production of disease, except incidentally; and no effort made to prove from Currie that cold produces disease at all. The whole object in those pages is to prove from the facts stated by Currie and others, that cold weakens the action of the heart. This being established, the author states it as the conclusion at which he had arrived, in section 138, and in 139 proceeds to state the position, for the first time, that "Cold is one of the most common, and is the most immediately active of the remote causes of fever; in many cases producing fever in a few hours, independently of the assistance of other remote causes; as in the cases mentioned above from Currie, 122, 124." These two cases are incidentally mentioned by Currie in the statement of his experiments and observations, and are above alluded to.

There is no attempt made, the author thinks, to prove that cold produces fever by weakening the action of the heart. But the propositions are considered separately and independently; 1, Cold weakens the action of the heart; 2, Cold is a remote cause of fever. On this last no proof was offered, as being undisputed. The object was, here as elsewhere, not to anticipate, but to treat each proposition on its own merits; and the reader, *from the work itself*, cannot at that stage of the investigation perceive the object of the author, or what his theory is.

The Reviewer then proceeds to give his views and those of some others on the mode of operation of cold, with which the writer conceives he has at present nothing to do, his business being to show that what he has stated as fact, is true, and what he has inferred, is correctly inferred, and no more.

The Reviewer proceeds thus; (p. 130)

"We think we have said enough to show that the author has not succeeded, to use his own language, 'in tracing the remote causes through their chain of effects to the symptoms of disease,' by assuming weakened action of the heart, from the nature of these causes, to be an indispensable precursor of all fevers, and we shall not therefore, examine into the operation of the remaining causes with reference to this subject."

The quotation in the body of this extract, though not, it is thought, in the author's own words, comes near enough for the present purpose.

As to the opinion of the reviewer, in the same extract, that he has said enough to show that the author has not succeeded in his object, the author does not pretend in that part of the work to have traced "*the connexion between the remote causes and the symptoms of disease,*" but only to have proceeded so far in the accomplishment of his object as to have shown that weakened action of the heart is an effect, directly or indirectly produced, of the remote causes of fever thus far mentioned, viz. those mentioned in the second chapter of the work; the reviewer's remarks, when this opinion was expressed, having been confined to those two chapters and the preface.

The Reviewer, in the same extract, above quoted, speaks of the author's "*assuming* weakened action of the heart, *from the nature of the remote causes,* to be an indispensable precursor of all fevers." The following are the words of the author, section 691.

"It appears from the preceding pages that weakened action of the heart is the effect, directly or indirectly, of the remote causes of fever. This, therefore, is a link of the chain of causes and effects extending from the remote causes to the symptoms of fever. (18) This conclusion is confirmed by the fact long since ascertained, that all fevers are preceded by weakened action of the heart."*

From this passage it is evident, that the author, *in the first sentence,* infers *from the preceding pages,* (in which, as he had proposed in section 85, he has adduced various facts and arguments to show its truth, of the several remote causes,) that weakened action of the heart is the effect, directly or indirectly, of the remote causes of fever. *In the second,* he infers *from this conclusion,* that weakened action of the heart is a link of the chain of causes and effects extending from the remote causes to the symptoms of fever. And *in the third,* he cites in confirmation of this conclusion, as "*a fact long since ascertained,*" that all fevers are preceded by weakened action of the heart," and quotes Boerhaave, Cullen, Darwin and Rush, in a note, in support of *this fact long since ascertained.* This is very little like *assuming* this doctrine *from the nature of the remote causes.*

The Reviewer in his observations on the chapters on the remote

*"Boerhaave's Practical Aphorisms, 563.

Cullen's First Lines, xxxiv, xlvi.

Darwin's Theory of Fever, Supplement, 1. 1. 6.

Rush's Works, vol. 3, p. 3, 4."

causes of the epidemics of hot climates, shows a disposition to speak favorably, which, the author thinks, would have been exercised more frequently but for the clashing of his facts with the reviewer's hypotheses. There are one or two remarks, however, which it is necessary to make. The reviewer says, p. 131.

"The author, *in his eagerness to exclude animal matter from any agency in the production of these epidemics*, has, we think, passed over too lightly the proofs of the power of exhalations from putrid animal matter to produce febrile diseases. Numerous instances of their agency in this respect, are not wanting in the annals of medicine, and we ought not to reject them *because they do not square with our own theoretical preconceptions*," &c.

The writer has marked in this extract two places in italics, as objectionable. The author, so far from being eager to exclude animal matter from the list of causes, was a believer *in their agency in the production of these epidemics*, even after he had concluded, as he then thought, his investigation of this subject; and it was on a reconsideration of the whole, just before publication, that his present judgment was formed. Evidence of this may be seen in an essay of his, published in the Medical Recorder for July, 1824. In that paper, after citing a number of cases on record, the object of which was to show the power of exhalations from dead animal matter, in producing fever, the writer concludes thus. "These show unquestionably, that exhalations from the putrefaction of animal matter also produce epidemic fevers." So far, therefore, was he from rejecting the theory because it did not square with his theoretical preconceptions, that *he rejected his theoretical preconceptions*, because he conceived that they were not supported by facts.

With regard to the criticism on the nature of the gas, the author agrees with the reviewer that the argument is not conclusive, and that "this branch of the inquiry is still *sub judice*." This he was sensible of very shortly after that part of the work was put to the press, in 1827, insomuch that he did not deliver that opinion to the class of 1827 or to any subsequent one.

The remarks of the Reviewer upon the subject of winter epidemics are duly appreciated. The reviewer is correct in conjecturing the meaning of the author respecting the operation of miasmata and cold in jointly producing these epidemics. If he will carefully re-peruse sections 599 to 603, he will find that they are attributed to the operation of cold upon persons *who are not yet freed* from the consequences of exposure to miasmata *during the time of their production*.

With regard to the inference to which the reviewer objects, it is sufficient to observe that, *alone*, as he has given it, the author is willing to admit that it has, perhaps, no force. But in connexion with the inferences immediately preceding, he thinks it has. We have not space to enlarge on this minor point.

The writer cannot agree with the reviewer in his remarks upon the subject of contagion. "No American physician knows enough of plague to be able to lay down, with that certainty which science demands, the laws which control its origin and propagation." If this assertion of the reviewer be correct, no country physician who has not visited places in which yellow fever prevails, knows enough of that disease, to be able to lay down with that certainty which science demands, the laws which control *its origin and propagation*. It is true, as the reviewer observes of the plague, and equally true of the yellow fever, that "on this subject, he must be content to receive his knowledge from the report of others." But having received this knowledge of the facts relating to the origin and propagation of these diseases, from others who are well qualified to report them, the physician who never saw either can form a precise and correct opinion of the circumstances in which, according to their report, these diseases always arise and prevail and terminate; and therefore can form a correct opinion of the laws which control their origin and propagation.

The reviewer proceeds to make some remarks on the second volume.

"The second volume opens with an exposition of the effects of weakened action of the heart, which is considered, as we have seen, the *immediate* and invariable consequence of the operation of all the remote causes of febrile affections, and indeed of nearly all the diseases incident to the human frame."

The reviewer is in error, in the use of the word *immediate* in this sentence. The author expressly says that weakened action is the effect of the remote causes *directly* or *indirectly*, (see section 691) and speaks of this effect as being gradually or suddenly produced. (See sections 1260, 1262, 1264.)

The reviewer says, (p. 135) that the author attributes the accumulation of blood in the vena cava and its great branches, "in a great measure to these veins being destitute of valves to aid in propelling the blood, and to prevent its regurgitation, whilst the external veins are guarded against such accidents, by being furnished with them throughout their extent, to accomplish these purposes."

The author attributes the congestion in the vena cava, &c. to the weakened action of the heart: he did not intend to express the idea that the want of valves in the vena cava, &c. *was the cause of the accumulation in them*; but that the want of valves in them and the presence of them in the other veins, *was the cause of the confinement of the accumulation to the vena cava, &c.* (See sections 1222, '23.)

It is impossible, in the compass necessarily assigned to these remarks, to notice all the objections of the reviewer to the reasonings of the author on the subject of the various effects of congestion on the human system. The reviewer, however, is in error in supposing that "*every physician, the least acquainted with disease at the bedside, will coincide*" with him in his remarks. The views given on that subject are the result of long and painful observation at the bedside of the patient. The writer, from the commencement of his practice in 1804, has been too much and too ardently devoted, for his own good, to these investigations, literally at the bedside of the patient; and the conclusions he has drawn from his observations, without pretending to claim for them entire correctness, nevertheless are so near the truth, that they have led him and an increasing number of friends and acquaintance, to practice in several diseases new to them, at least, and as successful as new. And whatever may be the doubts on this subject of the reviewer, and his friends, there are literally hundreds of persons who, without even calling on a physician for advice, are advantageously following the practice, even in that disease which the editor of the Am. Jour. of Med. Sciences has selected for his share of this correct representation of the author's practice, viz. dyspepsia.

The reviewer has occasionally expressed himself in a manner indicating a proper disposition in the office he has taken upon himself. The author is sorry to say, however, that in his remarks on the *Therapeutics* of the work under consideration, the reviewer has allowed himself a latitude that he cannot but esteem entirely unjustifiable. The reviewer in speaking of the second indication laid down in the work, viz. "to excite the weakened action of the heart," uses the following language.

"The second may be accomplished in febrile affections, by warm drinks, warm applications externally, and the exhibition of emetics, to throw the blood on the surface, and in intermittents, during the apyrexial period, by giving permanent stimulants and tonics. The author however, prefers in these latter diseases, as well as in chronic affections, the repeated use of cathartic medicines, as calomel, aloes, rhubarb, jalap, scammony, &c. given during the time of the

lowest stage of weakened action, in order to keep up and stimulate the heart's action, and to produce their evacuant effects in the after stage of excitement."

The reviewer has not in this passage given such a view of the practice, that a person unacquainted with the work, can have a correct idea of what is advised. Emetics are spoken of as the most effectual means to produce immediate increase of the action of the heart. (See sections 1551, 1555.) The mercurial cathartics are relied upon for continued use; but not in the unlimited way the quotation from the review leaves the reader to suppose. After speaking of various remedies commonly used in intermittent diseases, the author proceeds thus in section 1558.

"These remedies, however, I have made little or no use of for some years; and have derived more benefit than is usually experienced from tonics, quinine, or the solution of arsenic, from the administration of cathartics at a particular period of the remission. Cathartic medicines are necessary in all such cases; and by giving those which are stimulant, in the decline of the action of the heart towards the lowest point, the action is sustained and even raised, so that it does not fall to that point at which a chill appears; meanwhile the cathartic operation is effected at a period when it can be best borne, viz. during the increased action, and when it is most serviceable in reducing it."

In the two following sections, he proceeds to mention *expressly*, calomel, aloes, rhubarb, jalap and scammony as the cathartics intended to be used; nor has the author ever used any other with this view. The reviewer, however, by the use of the words *cathartic medicines* at large instead of "*cathartic medicines which are stimulant*," as the author has it; and by the addition of &c. at the end of the list of cathartic medicines given by the author, has represented him as speaking with a latitude he never intended, and does not use.

In the treatment of chronic diseases with these cathartics, there is a very particular attention paid by the author to the quantity and the quality of the discharges produced. This caution runs through the whole work, so as to constitute a remarkable feature in the treatment recommended. The cathartics employed are selected on account of producing such consistent discharges from the liver as lessen congestion, and thereby relieve the symptoms proceeding from it; (see sections 1566 to 1579) and others are rejected on account of their producing watery evacuations, which are continually objected to as weakening and injuring the patient and *increasing the disease*. (See sections 1593 to 1596 and 1602 to 1612.)

Of all this the reviewer gives his reader no notice, but leaves him under impressions respecting the views and practice of the author which.

are very far from being correct. The reviewer for example, speaks of the author's "constant resort to reiterated purgation, not to be restrained even after it has *induced bloody discharges.*" The last words are in italics in the review, and certainly afford no idea of the author's views or practice. His remarks on the subject are contained in sections 1632 to 1637, and are as follows.

"1632. It is proper to mention here, that in some cases, while attempting to procure the necessary evacuations from the liver, instead of those usually observed there is a discharge of black blood in considerable quantity. This sometimes occurs early in the disease before any other discharge from the liver, sometimes in the course of the treatment; occasionally also when no medicine has been given. It always alarms the patient and his friends, and sometimes the physician. It ought seldom to alarm either if such a judgment may be formed from the following facts.

"1633. Among the whole number of my patients thus affected, I have not known more than five deaths. In one of these cases the patient was extremely ill, with the highest grade of bilious fever, and was almost entirely neglected; in fact left to his fate. I saw him not until near his death, on the 7th day, when he was covered with petechiæ. In another the bowels were not evacuated at all, though cathartic medicines were given very freely. The hemorrhage became less and less, and was never considerable. In the other three cases the hemorrhage disappeared during the use of cathartic medicines, and the patients died some time after through gross neglect of the directions given.

"1634. In all the other cases which occurred since the autumn of 1821, cathartics have been used freely, and while they were in operation the discharge ceased: and this was the case whether the discharge preceded or followed the administration of the cathartics. (1632)

"1635. In some cases the hemorrhage was manifestly and immediately beneficial. In one case the patient had been lying in a fever, with weak pulse, stupor, and pain in the abdomen for ten days. Cathartics had been given daily; when the hemorrhage appeared, the blood was discharged pure, black and fluid, five or six times in the day, in quantity equal to a moderate passage. On the next day the patient was up, and well, and walked out.

"1636. On one occasion the patient being ill, and discharging bilious matter every day, began at length to pass black, fluid blood. The physician alarmed was about stopping the hemorrhage by an opiate. Being consulted, I advised him to proceed with his plan of purging, the hemorrhage being not at all injurious. He did so, and the patient recovered, and became very fat.

"1637. On another occasion the patient having taken several cathartics without effect, at length discharged a very large quantity of blood, black and fluid. The cathartics were notwithstanding continued: the discharge of blood also continued till the third day, apparently produced by the action of the medicine; the evacuations occurring in time, and quantity, suited to the time of administering the medicine. On the third day a passage appeared of a doubtful character. It was nearly like black bile, but did not give a yellow

colour to the side of the white vessel which contained it, as bile does, however black. The subsequent passages were however more and more like bile, and in the course of the day they were decidedly so. They in the following day became less and less dark, and finally yellow; and the patient recovered."

The representation of the reviewer is the more objectionable, 1, because all the fatal cases are stated, with circumstances showing that the cathartics used, could not possibly have produced the hæmorrhage; (see section 1633 above quoted) 2, because a number of cases are stated which show that, although the hæmorrhage came on during the administration of cathartics, in the way usual in bilious fever, the cathartics could not have been the cause for two reasons.

1. The hæmorrhage from the bowels was not of that cast that could be attributed to severe purging, but a copious flow of black blood unmixed with fæces; sometimes accompanied and *even preceded by a like discharge from the mouth*; and often accompanied by petechiæ of various sizes, from that of a pea to that of a dollar, and of a purple colour, some of which being opened with a needle dark coloured blood flowed out. 2ndly, The hæmorrhage in almost every case preceded the use of cathartics, and ceased during the administration of them. These circumstances are stated in section 1634, &c. (quoted above) only in a general way, because the author was there laying down general principles of practice. But the same is stated fully and particularly in the chapter on *Hæmorrhages* in sections 2084, &c.

The reviewer proceeds with his remarks, thus;

"Little reliance is placed on promoting discharges from any other organs than the chylopoietic," &c.

The reviewer should bear in mind that the work under consideration is yet unfinished, that it embraces only those chronic diseases in which the chylopoietic functions are known to be disordered, and the remedies are therefore adapted to relieve them from the cause of almost all their disorders, viz. congestion. Hence, *in the diseases mentioned in the work*, "little reliance is placed on promoting discharges from any other organs." The reviewer will probably find other remedies for other diseases, when they come to be mentioned, used with as much perseverance, in their proper place.

The reviewer says;

"This purging is quite a passion with our author, and employed on nearly all occasions, and to answer opposite and contrary indications. To check menstruation when too profuse and to bring it on when suppressed or scanty."

As to the remark respecting the administration of cathartics *on all occasions*, the last observation of the author is sufficient notice of it.

With regard to that respecting their being "*employed to answer opposite and contrary indications*," the reviewer must have seen, if he read the work, that the author does not believe that "opposite and contrary indications" exist in the cases spoken of; and therefore that he does not employ cathartics *for the purpose of fulfilling opposite and contrary indications*. So far from this, he shows at length that the cause of excessive and of deficient menstruation is the same, and consequently that the indication of cure in the two cases is, *not opposite and contrary*, but the same.*

The reviewer continues;

"He never fears any untoward effect from the most active and stimulating materials of this class of remedies, but gives them with the same confidence in uterine hæmorrhages in the latter months of pregnancy, and in hæmorrhages of the intestinal canal, as others would employ them to relieve an impacted state of the bowels."

This passage, in connexion with the last quotation from the review, (which it immediately follows,) is calculated to represent the author as so passionately attached to purging on all occasions, as not to be deterred from the use of the most active and stimulating cathartics, in circumstances in which, according to common opinion, they would probably be destructive of life; viz. *notwithstanding the presence of uterine*

*The reasonableness of the doctrine is not here to be discussed. The writer will barely suggest to those who think it cannot be true, that congestion in the liver, produced by the remote cause of the autumnal diseases, is accompanied by increase of the secretion of the liver in some cases, and in others with a deficient secretion. Moreover, that the object in the treatment being to relieve congestion, cathartics are given when there is no secretion, as well as in many cases in which there is an increased secretion. So likewise has the writer known, in some autumnal seasons, increased uterine discharges to be common, and in others a deficiency of the same to prevail, in places noted for the prevalence of autumnal fever. Here is a striking similarity. The same remote cause prevailing, the same internal state therefore existing in those who are in a morbid condition, that internal state evincing itself by a number of its usual effects, *some* are affected with an increase, and others with a diminution of the secretion of the liver, among other symptoms; and *others* in the same situation in all respects, have the additional symptom, an increased uterine discharge, or a deficient one. With this view of the case fully explained in the treatise, the author conceived the indication was the same in the latter case as in the former; and the extraordinary and continued success of the practice, confirms him in the belief of its truth.

hæmorrhages in the latter months of pregnancy. The effect of this representation unexplained, would be to make the reader believe that the author was a madman. But the impression on his mind would be very different, perhaps, if the reviewer had done his duty in the office he has assumed. Had he performed that duty, he would not have represented cathartics as given on all occasions, without being restrained even by uterine hæmorrhages in the latter months of pregnancy, but *as given with the express view of putting an end to those hæmorrhages.* He would have examined the grounds and the arguments on which the author relied for venturing upon a practice so desperate as he seems to consider it, and he would have inquired into the result. Having done this, he would have made a faithful representation of the whole in as condensed a form as possible, that the reader might be able to judge of the propriety of the practice; and he might then have called his account of the practice recommended in the work under consideration, *a review.* As it now stands it may more properly be called *a caricature.*

The author fully sensible of the importance of the subject, and of the opposition he might expect if he published his views and his practice, gave with great particularity a statement of many cases of the treatment of uterine hæmorrhages with cathartics. He says, "From the foregoing considerations, perfectly convinced of the truth that menorrhagia is the effect of accumulation of blood in the venous cavity, I was led directly to the inference that those medicines which evacuate from that cavity would cure this affection. This was immediately tried, and the result was a speedy and perfect cure; insomuch that for some years" (now nine years) "I have been treating this disease with cathartics alone with complete success. *As this is a very important matter, a number of cases are stated.*" (Section 2135)

A number of cases are then stated in detail, in which the success was striking, immediate, and so complete, that it is believed that to this day there has not been one failure. In every case but one, mentioned in section 2149, the subject of which removed when almost well of a long continued and habitual menorrhagia, the cure is known to have been perfect.

With regard to hæmorrhages in a state of pregnancy, the author, after discussing the subject of the remote causes, &c. observes as follows. "Resting confidently on the correctness of this train of reasoning, and on the conclusion, that menorrhagia in pregnant women is

produced by the same cause that produces it in women not pregnant; (2172) and confirmed by the successful issue of the practice of evacuating from the venous cavity through the liver in the latter, I gave the same cathartics in the first case of the former that occurred, and with the same complete success." (Section 2188)

A number of cases are then stated with the same regard to giving the reader a complete account of the patient in every particular. The practice was eminently successful. Of these, as well as of the statements made of the cure of menorrhagia in women not pregnant, it is sufficient to remark, that some of them relate to some of the most respectable females in the country in which they reside; and that the cases are well known to many, as may be gathered from the extraordinary nature of the practice, and the opposition which was almost uniformly made to it when first recommended; both of which excited the attention of many to the result. Something of this is intimated in the work, (sections 2145, 46, 91,) as well as the effect of the success of the practice on those at first opposed to it. (See sections 2192, 95.)

The reviewer proceeds with his remarks thus; (p. 139)

"It would seem that in his view hardly any other medicinal effect is capable of removing disease. Has the exhibition of cinchona or the nitrate of silver cured a case of dyspepsia, it was by acting as a purgative. Has tansy or lime-water warded off the gout, or calomel and squills removed a dropsy, it was still by their purgative qualities without reference to their operation on the other emunctories of the system."

To the first sentence of this extract a sufficient answer has been twice given in the compass of the last three pages, rendered necessary, perhaps, by the repetition of the objection.

As to the remarks concerning the mode of operation of cinchona, the nitrate of silver, &c. they are calculated to make a false impression on the mind of the reader and to mislead him entirely.

The author has shown that dyspepsia is produced by congestion of the vessels of the liver, stomach, &c. by a train of reasoning which *the reviewer* says nothing about. Congestion being the cause of the disease, producing not only the derangement of secretion commonly observed, in such cases, in the liver, but in the stomach also, the fair inference is, that diminishing congestion will lessen its effects, the symptoms.

This was soon tested, and the justice of the conclusion fully supported by the result; and that the reader may have the fullest opportunity of forming a judgment for himself, a number of cases are stated

at great length and with minute particulars, from day to day, and among these were all those which were most unfavorable in their termination.

The first case, it is believed, in which the practice was adopted was the following. (See sections 1833, 1834.)

The patient had been a number of years dyspeptic, had consulted various physicians in different parts of the country, in conformity with whose advice he had made many strenuous efforts to obtain relief, without success. He had first tried tonics in vain; afterwards alterative doses of mercury and Epsom salts, with the same result; then again tonics with alteratives, and finally the Bedford water, all in vain.

By the advice of the writer, being now in an emaciated state and greatly deranged as to the secretion of both stomach and liver, "he commenced with scammony, aloes, and calomel, in equal quantity, in pills, with directions to procure about three consistent passages every day." Following this plan he soon improved, but on his second call "the discharges being rather thin, he was directed to take pills of rhubarb, aloes and calomel, instead of those first mentioned." The rhubarb was here substituted for scammony, in consequence of a condition of the evacuations continually adverted to by the writer, which is never to be allowed to continue in chronic diseases, because it reduces the patient's strength, whereas the cathartics which operate in the way proposed, bringing away consistent passages two or three times a day, may be continued to any necessary extent with a constant increase of the strength.*

*In confirmation of the above see section 1612 of the work. In the case there stated, the patient was a lady, a little above thirty years of age, of a slender frame, of sedentary habits, the mother of many children, and on all these accounts in bad health. She was advised to take the course above recommended, and long refused; but at length, when she was so reduced in strength, as to be afraid to venture into the street for fear of faintness and of falling, commenced the use of them. "She took them several months. When she commenced, she walked along slowly, with the appearance of infirmity, and upon any exertion, as on ascending a flight of steps or a little hill, her respiration was hurried, and she could scarcely proceed. After a month or more, she walked from one side of the town to the other, in a quick and lively manner, and ascended a hill without difficulty or delay." She continues in good health to this day. (See section 1612; also 1811, 1837 and 2037 in connexion; 1813 and 1841 in connexion; and section 1842.) Cases of this description, in any reasonable number, are at the service of the reviewer with names and dates. Two more relating to medical men shall here be mentioned.

In the summer of 1828, Dr. Owen of Alabama, came to Lexington for medical

The patient in question by following this plan of treatment soon recovered.

The author proceeds to give a number of cases with minute particulars and called on the writer for the purpose of obtaining it. He was exceedingly weak, pale, emaciated, and dyspeptic in the extreme, so that he had been compelled to lay aside all thoughts of continuing practice and had given it up. He was advised to commence immediately with pills of rh. al. and cal. in sufficient quantity to produce two or three consistent passages every day. He followed the plan proposed and improved fast; insomuch that he went to dine out, and this indulgence was followed by a vomiting &c. and great prostration. This he at first attributed, with the writer, to the improper diet used at dinner: (among other articles were tomatoes and boiled corn.) But afterwards, having discovered that he had been taking calomel, he attributed the bad effects to that medicine. He had *objected*, at first, to the use of calomel, and the first dozen pills made for him contained none. But after the first, every parcel was made by the young gentleman in the office in the usual way, viz. with calomel, the objection not being known to him, or not being recollected. He had therefore *followed* the plan proposed, though he objected to it. His objections to continue the course, resulted in his leaving town, and at a friend's house, not far off, he treated his case in his own way. Hearing that he was likely to die, and that he was considered as my patient, I called to see him; and finding him on the bed and very low, I requested as a favor of him that, as he was not following my advice, he would let it be understood that he was not my patient; particularly as he was using some articles that in my view would not by any means benefit him. We parted in a friendly, but somewhat cool way, and I saw no more of him for perhaps two months, when I was surprised to see him ride up to the door and get down with more strength than I ever before saw him exert. Meeting him at the door, he came, he told me, to do me justice. He told me that he had persisted in the course he was following when we parted, until he was convinced he should die; and determining to make a last effort, he had fully adopted the plan originally proposed to him, had gradually regained health and strength, and was now going home. Moreover, he had, with candour and generosity by no means common in this world, mentioned every where the circumstances, determined that the truth should not be injured through him. This gentleman, the writer is informed, has become fat and healthy, and has resumed the practice.

Dr. Clarke of Missouri, called on the writer in the summer of 1829, having come to Lexington for the purpose of obtaining advice in his case. He was emaciated and so weak he could with difficulty get here. He complained of pain and disorder in the abdominal viscera. On the writer's observing that he need not ask advice how to treat that disease, he replied that he knew what would relieve it, viz. cathartics; but that the disease was so obstinate, that to cure it he was compelled to use them so often that he should be destroyed by the remedy. To this the reply was immediately made, Then you could cure the disease if you had a cathartic that could be taken without reducing your strength. Certainly, he answered. Then said the writer, I can tell you of such an one. The rh. aloes and calomel pill was then proposed to him, to be taken in the usual way. He adopted it, and in a fortnight had gained strength, and was so much improved that he returned home, confident that he could cure himself, or at least keep the disease down, so as to be comfortable.

culars from day to day, including among them all those cases which were most unfavourable in their termination. These cases occupy forty pages; and the author thinks that the numerous facts stated in them, so extraordinary in the estimation of the reviewer as to deserve the name of "*infatuation*," are deserving of other notice than a sweeping denunciation. They are not thus to be obliterated from the recollection of the writer, nor of the numerous physicians who have adopted this mode of treatment, nor of the still more numerous class of persons who have heard of it, from the contests relating to it, and have adopted it, from seeing the success attending it in their friends and neighbours, without consulting any practitioner; thereby materially affecting the practice of some physicians as the writer is well assured from the best authority, and among whom he ranks himself.

Having stated so many cases in which the mode of practice recommended, had relieved the patient in a remarkable manner, the author observes in section 1843; "These cases are sufficient to prove that dyspepsia is to be cured by medicines which produce free evacuations from the venous cavity, as above mentioned. The following cases go further, and show not only that such evacuations are effectual, in restoring health to dyspeptic persons, but also that tonics, when they are salutary, *sometimes*, and *probably often*, act in the same way."

The author then proceeds to give in the two following sections, two cases. The persons were his own friends, who suffered excessively under long continued dyspepsia. Both had made great and persevering efforts to obtain relief without success. One of them particularly, was a man of extraordinary energy, and "used tonics in the freest manner." But all in vain. These two men left that part of the country and the writer saw them no more for several years. When he met them, they were both hearty men. This excited his surprise, and he immediately inquired how they had been restored to health. One stated that he had been cured by bark, which had *purged him every day for three months*; the other that he had been cured *by bark and Epsom salts*.

From these remarkable facts the author drew *the guarded inference* stated above, that "tonics, when they are salutary, *sometimes*, and *probably often* act in the same way;" viz. by producing a loose state of the bowels.

How different the representation of the reviewer in the passage quoted. "Has the exhibition of the cinchona or the nitrate of silver cured

a case of dyspepsia, it was by acting as a purgative." This sentence would naturally lead the reader to the conclusion, that the author's passion for cathartics not only led him to give them in dyspepsia, but that he asserted without limitation, that *when bark cures dyspepsia it is by acting as a cathartic*. But the author does not make this sweeping conclusion relating to *tonics*, from the good effects of *cathartics alone*; but from observing, in cases in which tonics had long been tried and failed, and years after succeeded, that in the latter trials they had produced a continued loose state of the bowels, he concluded that they *sometimes* and *probably often* prove beneficial in this way.

As to the remark respecting the nitrate of silver, the writer will merely state the facts, and leave the reader to form his own opinion. The reviewer says, "Has the nitrate of silver cured a case of dyspepsia, it was by acting as a purgative;" thereby representing the cases in which this medicine was administered by the author, as cured by it. The following are the circumstances of the only cases in which it was administered.

The patient in the first stated, (section 1836) after long suffering the utmost extremity of the disease, was brought to the writer for assistance. No hopes were given of her recovery, but as she and her friends were desirous that an effort should be made to save her, it was made. She commenced taking pills of scammony, aloes and calomel on the third of February and continued them until the 17th. On the 17th ptyalism having come on, she took instead of calomel a quarter of a grain of nitrate of silver with the aloes and rhubarb. These medicines were continued until the 1st March, she taking every day or nearly so, pills containing from half a grain to three quarters of the nitrate with the rhubarb and aloes.

The effect of these medicines while the calomel was administered, was to produce green and black discharges daily: the effect after the nitrate was given, was to produce the same, but *much more abundantly* throughout, but particularly on the 19th, 25th, and 27th February. All this is minutely stated in the work.

The reader will observe that this is one of the two cases the author has spoken of above, as the most unfavorable. If the nitrate cured this case, as the reviewer intimates, most assuredly the author has a right to conclude that it did it by acting as a cathartic, seeing that after it was given the discharges were increased in number and in quantity, and seeing that *the nitrate of silver is an active cathartic*.

The patient in the second case commenced taking scammony, calomel, &c. on the 28th November, and continued with slight intermissions and some irregularities in the use of improper cathartics, viz. such as weaken by producing watery passages, until the 6th February, when the attendance ceased. In the course of this time the calomel having affected the mouth, on the 27th January, *two months after the treatment with cathartics had commenced*, she took nitrate of silver; and in that and the five following days used eight and a half grains of the nitrate, when she returned to the use of calomel with the cathartic. The violence of the struggle, which commenced on the 8th January, when she was very ill through imprudence mentioned in the case, was over by the 12th, and from that day she improved regularly; viz. fifteen days before the nitrate was administered. And the reader will observe that all this is minutely stated in the detail of the case from day to day.

What renders it more remarkable that the reviewer should attribute the cure in this case to the nitrate alone, in contradistinction to cathartics, is, that the progress of the disease and the way in which it was treated by various physicians, is noticed in a very particular manner in section 2037, and the whole shows in a remarkable manner the efficacy of cathartics throughout the whole period, and the inefficacy of every other mode of treatment that was tried.

The reviewer is not more correct in his remark respecting the author's views of the operation of tansy and lime-water in gout.

The author's observation of the causes and the nature of gout, led him to the conclusion, that the immediate cause of the symptoms is congestion, and consequently that evacuations from the liver by the bowels, should relieve the patient. His own experience in years previous to the time of his investigation was in full accordance with this conclusion; and the experience of many deservedly esteemed medical writers was equally so.

Dr. Cheyne says he knew a quack to administer sixty grains of rhubarb disguised with cochineal, every morning for six weeks, to a gouty person, who had no symptom of the disease for four years afterwards.

Dr. Rush says he knew two persons who had been accustomed to two fits of the gout a year, who had been preserved from them several years by taking sulphur to obviate costiveness.

Dr. Clark states that he had a patient who had had the gout ten

years, for two months of the twelve in some of the latter years, who drank every day a quart of lime water, which purged him two or three times every day; and latterly when it seemed to lose its laxative quality, he directed him to take four ounces more, after which it purged him as before. He had had no fit nor any other ailment at the end of ten years, when the statement was made.

He states another case, in which a gentleman, who had the gout so severely as to be confined every winter two or three months, and sometimes a month in summer, took near a pint of strong infusion of tansy every night, and had two easy passages every day. He had no return of the gout in seven years, but once slightly when he strained his ankle.

He mentions another case in which the same remedy had a good effect, and the patient had two easy passages every day.

He mentions another in which a patient was entirely relieved by taking half an ounce of the elixir sacrum, every second night. This dose contains the substance of seven and a half grains of rhubarb and four and a half of aloes.

From these statements, all made in the work, the reader may judge whether the author was right or wrong in his opinion.

The author was so far from attributing the good effects of calomel and squills in dropsy to their cathartic operation *without reference to their operation on the other emunctories*, that he had used that remedy for years *with reference to its operation on the kidneys alone*, notwithstanding that he continually observed it to purge the patient. And when his investigations led him to believe that it was by its cathartic effect, so far from having no reference to its operation on the kidneys, and throwing it aside for a mercurial cathartic, he gave the calomel and squills in the first case that occurred, and watched the effect on the patient. "They operated on the bowels; the discharges were precisely such as are usually produced by pills of jalap, aloes and calomel. After a day or two, the latter were used and continued until the disease was entirely removed; which was in a short time. The patient was young, and the disease has never returned. This was above five years ago. This was considered, as it is, a strong confirmation of the truth of the doctrine; it being evident that the medicine used as a diuretic had produced its good effects by purging the patient. In consequence of this it was determined to make a full trial of the pills of jalap, aloes and calomel in the treatment of anasarca, ascites and hydrothorax. The

success of the practice was such in the first cases, as to hold out a strong inducement to continue it." (See section 2407-S.)

A number of cases are then stated, which support this practice in the strongest manner, and show beyond all doubt, that whatever other medicines may do, those advised are very useful in the treatment of dropsy.

The reviewer continues to make remarks upon the character of the work, which he does not support by reference to particular passages he has in view in making them; and the author therefore, can only say, in general terms, that the doctrines advanced in the work are *not* rested upon his own *ipse dixit*, nor upon the authority of great names. The author cannot spend his time in searching through the whole two volumes, to make sure that there is not some slight reference to authority which might be construed unfavorably to the following declaration, but *he fully believes that there is not a solitary reference to authority for any doctrine.* There is undoubtedly reference to great names *for facts* upon which the doctrine is built; but the author is yet to learn that authors of great and deserved celebrity are *not to be quoted for what they have seen or heard.*

As to the remark about novelty, ("*Well may the author observe that the treatment is, as far as he knows, new!*") the author will only observe, that he recollects no part of the work in which claims to originality are obtruded on the reader. There is a note to section 1537, in which he states the simple fact about a matter of small moment, not with a view to such a claim but as a reason for not quoting as the author of it, a writer who has also advanced the doctrine, when he did not derive it from him. The remark of the reviewer is made in connexion with his observations on the treatment of dyspepsia; and on turning to that chapter the following passage was observed at the close of the first case. "The unquestionable importance of the case, and the successful termination, will, I am persuaded, fully justify the space occupied by it. I shall state others as fully, because one case is not to settle the question of the propriety of *a mode of treatment novel as far as my reading extends.*" Surely nothing can be more inoffensive than this manner of mentioning a new practice. There is no claim of credit for the novelty of the treatment, but its novelty and extraordinary nature, admitted by the reviewer, is simply mentioned as the reason which induced the writer to put the reader in possession of abundant materials to guide him in his judgment of its propriety.

“This similarity (says the reviewer) is not, we suspect accidental. At any rate we have no where the least intimation that the author is acquainted with the labours of Parry, Armstrong, and Abercrombie of Britain, or with those of Louis, Andral, and Laennec of France, with a host of others, who have contributed to base medical science on principles unknown to former times.”

Does not the reviewer perceive that this very fact, that there is in the work no such intimation, is evidence that the author was not then acquainted with those authors? This would surely be a rational conclusion for a plain, sincere man to make. The reviewer professes to know nothing of the author. Knowing nothing of him but from his work, and finding nothing in the work to indicate that he was acquainted with the authors he mentions, what are the grounds of *his suspicion*? Is such conduct *universal* in authors? Certainly not. From what internal feeling, then, springs this *suspicion*?

The preface to the work seems, for the most part, to have been lost to the reviewer. He might have gathered from it, that the writer was for a long time, (until the beginning of 1822,) engaged in a laborious country practice, in which of course he had little time for reading; and during which, he in fact had few books to read. “The long and solitary rides he was frequently called upon to take, afforded opportunities for undisturbed reflection,” (p. vi) but little for any thing else; and the result of his investigations was, that “The doctrines advanced have been familiar to his friends *since the spring of 1822.*” (p. viii) The case of hæmorrhage stated in section 2085, occurred in February 1822; the case of menorrhagia (2136) in May of the same year. In that year and the following, many such, with cases of dyspepsia, hysteria, gout, &c. were treated in the same way. On the other hand, of the works mentioned by the reviewer, the writer had never then heard, certainly never seen one except Armstrong.

The French works on this subject which the reviewer mentions he has never yet seen, and does not believe have been published in this country; Parry's work, it is believed, has never been republished in this country, and the author never saw it until he met with it in the Lexington library since the publication of the work in question, and more than six years after the date of some of his cases; Abercrombie's work was published since the treatise of the writer: the preface to that work is dated in 1828, and the book was republished in this country in 1830; *the treatise* was printed in Winchester, Virginia, before the writer was called to Lexington, in 1827, (except about fifty pages,) as stated in the advertisement prefixed to the pre-

face. The reviewer's suspicion with regard to these therefore is unquestionably groundless. The work of Armstrong alone remains to be mentioned.

This work was republished by J. Webster in 1821, who sent the author a copy in the following winter, with three other books mentioned in the bill, of which one was "Surgical Essays, by Cooper and Travers," to which is prefixed a dedication to Dr. Davidge dated Nov. 1, 1821. *The bill is dated Dec. 4, 1821*: one of the author's cases is dated Feb. 1822, and in May of that year the practice deduced from his investigations, was in full operation in Winchester, Virginia. (See sections 2085, 2136, &c.) At what time in the winter of 1821, the work of Armstrong reached the writer cannot be ascertained. But whenever it was, he was in no condition to sit down deliberately and commit a robbery. In December and January his family were in great distress from sickness of himself and others. February, March and April were spent by him, as is partly mentioned in *the preface*, (p. viii,) in travelling to seek a place of residence, in removing his family to Winchester, (preface, p. viii) and on a bed of sickness in it, from which he was not expected to rise. In this time of harrassing care and laborious exertion, resulting in severe illness, it was impossible *to read carefully* such a volume; much less to digest it, with its views directly opposed, in capital points, to those of the author, into a system leading to a practice so novel, and to give it such "*extensive application*" as the reviewer allows it. (p. 123) In truth the practice alluded to had been fully adopted before a page of that work had been read; a circumstance not remarkable when it is considered that the subjects treated in it are fevers, and that the attention of the writer was then particularly turned to the chronic diseases.

The author has reserved for this place a passage of the review, because the editor of the Journal has appended to it a note, with a reference to an article of his own on the subject, which, alike in spirit, may be noticed together. The reviewer, in remarking upon the treatment of individual diseases, after mentioning blood-letting, says; (p. 139)

"But in general the great reliance is on purgation, repeated again and again *without looking to any ill consequences that such a persevering course may induce*. The infatuation with which the use of pills of aloes, jalap and calomel, is persisted in day after day, till the patient has taken in a case of dyspepsia, not grains but ounces, and we might almost say, pounds, is really incredible and consternating."

The reader would scarcely believe from this and similar passages of the review, that the author *had continually in view* limiting the purging to two or three daily discharges of such a character (consistent and not watery) as *to prevent weakening the patient*, and that this is adverted to in *many* of the cases and *often* urged upon the reader as indispensable.

The writer cannot conceive the propriety of talking about the quantity of medicine a man takes in a long continued chronic case. The reviewer would not object, perhaps, to giving bitters, steel and bark, for months, and years with Dr. Whytt*. He would probably make no more objection than the author, to "the almost unremitting continuance of purging in hydrocephalus;" or to the use of cathartics day after day, without interruption, for several months successively, in epilepsy; or to copious purging in the confirmed marasmus of children, notwithstanding the great debility present in those cases; all recommended by Dr. Chapman;† or to the long continued purging with jalap and crem. tart. in the disease of the hip joint, recommended by the venerable Physick. The quantity does not startle him in these cases, although he may here also easily reckon it by ounces, and almost, perhaps altogether, by pounds. And why? Because he is persuaded that it is the proper course in the respective diseases. So here. The question is, What is the proper course? The author has reason to believe that *two or three daily discharges* from the liver through the bowels, will, often in a few weeks, most effectually relieve the diseases treated of in his second volume. He moreover proposes, as the best prescription which his observation and experiments have led him to adopt, *as the mildest and least exhausting*, the pill of aloes, rhubarb and calomel. In most cases of dyspepsia, two pills, containing ten grains of this compound, produce the desired effect; sometimes one; and seldom are four required. The few exceptions, two or three, certainly not half a dozen in nine years, were cases which ran into an epidemic season in which extreme difficulty of moving the bowels was a feature of the prevailing epidemic; in which case they, as has often been observed of diseases at such times, assumed this feature; and, like the other cases of disease then occurring, required larger doses to produce the usual effect; and the only

*Whytt on Nervous Diseases, p. 337.

† See for the former, Phil. Jour. Med. Sciences for Feb. 1827; and for the two latter see Mat. Med. vol. i, pp. 169, 178.

choice left the physician and the patient, was to give up the hope of relief, or seek it in an increased quantity of the medicine. Such was the case which the reviewer alludes to, and which the editor of the Am. Jour. of Med. Sciences has selected for his animadversion, and to represent as a specimen of the author's practice; with what propriety the following remarks will show.

Remarks on an Editorial article in the Am. Jour. of Med. Sciences for May 1831, relating to the treatment of dyspepsia. By JOHN E. COOKE, M. D.—The editor of the Journal abovementioned, in a note at the bottom of one of the pages of the review of C. D. (p. 139) says, "In our periscope, under the head of American intelligence, we have inserted two cases of dyspepsia related by our author. *They afford a specimen of his purgative practice in that disease.*" In the place referred to, the editor observes, "In the review of the Pathology and Therapeutics of Dr. Cooke, which will be found in another department of this number, the extent to which the author of that work administers purgatives in the treatment of dyspepsia, is spoken of, but the quantity said to be taken in some cases, is so extraordinary and unparalleled in the records of medicine, that it would be impossible for the reader to form any notion of the practice, without an example: we therefore extract two cases from the second volume of the work, and *that the author may be fairly represented*, we shall quote them *at full* in his own words."

The editor then states two cases, which certainly do not afford the reader a specimen of the ordinary practice at least, of the author in dyspepsia; neither is he thereby fairly represented.

The reader will observe that the author mentions two or three daily loose passages, as the object to be aimed at, and thin and watery and abundant passages as most sedulously to be avoided, *in many places* throughout the work; not in laying down general principles only, but in many of the cases stated occasional changes for the worse are pointed out, and attributed to the patient's taking cathartics not recommended, or more than he was directed of those which were, whereby he was purged too freely and injured.

The reader will further observe that the editor says not a word of all this; but gives *as a specimen of the author's practice*, two cases

which are striking exceptions to the general rule as to the quantity of medicine; and the reader not having the rule of practice laid down by the author, but only the exceptions, is naturally led to consider the exceptions as a fair specimen of the practice under the rule; and what renders this more exceptionable in the editor, is the fact that the author speaks expressly of these cases in the following terms.

“It is proper to observe that *these extraordinary doses have not been necessary in any other cases than those recited.* In general four pills of rh. al. and cal. or of jal. al. and cal. act sufficiently. To these common doses no one would object, if good arise from the use of them. If objection be made to *the extraordinary doses*, the following is the point to be decided. When ordinary doses fail to produce an effect, which when produced will certainly be followed by improved health, shall we give extraordinary doses, or allow the patient to continue to suffer? The decision rests on the same ground in this case as in autumnal fever. Evacuating bilious matter is necessary. If one, two or three doses fail, we do not stop, but persevere until the operation is effected, without limit as to quantity. If we succeed in purging the patient, he recovers; if not, he dies; and numbers are now saved by perseverance who but a few years ago would have been left to die.” (Section 1840)

The author, moreover, not only speaks of them in these terms as extraordinary cases, but in relating the cases referred to in 1840, (viz. the two quoted by the editor, in sections 1837, 1838, and another in 1839) mentions the reason of this extraordinary state of the bowels in them. In relating the second case stated by the editor, the author observes “that the necessity for large doses was not confined to him; but *that in this season it was a general observation among physicians, that uncommonly large doses were requisite to produce the usual effect on the bowels.*” (Section 1838, p. 247.) This was the 10th of August: he took in the commencement, in April, but two pills daily. The first case stated by the editor as well as one of the same character not noticed by him, occurred in 1824; the former in the close of the autumnal season, the latter earlier. In the commencement this patient took moderate doses for several months with great advantage; but like the subject of the last case mentioned by the editor, “about the first of August after getting wet he became worse, and the season being sickly the disease assumed something of the character of the prevailing epidemic, and required even larger doses than in the former case.” (Section 1839) The year 1823 was a remarkably sickly year in Winchester and the neighbouring country, and in 1824, 25 and 26, there were considerable epidemics.

and particularly in the last. In all these, great difficulty of moving the bowels was experienced.

It is evident from these extracts that the editor has not given a fair representation of the author's practice in stating the two cases above-mentioned. So far from exhibiting a fair specimen of the practice, it is evident that the author felt the difficulty of his situation *in being compelled by the urgency of the case and of the patient to go beyond all his previous experience in order to procure relief*. Thus in the last case stated by the reviewer, *the patient was so relieved when the passages were obtained and so oppressed when they were not, "that he begged for something to bring off the black matter, which he was always glad to see."* And in the case not stated by the editor the same was observed. "His discharges were dark; his bowels were very difficult to move; if a discharge from them was not obtained, he suffered very much from all the symptoms, at the same time his complexion becoming darker; *he was therefore continually anxious and urgent that this should be effected*, and if the medicine failed, sent off without hesitation a long distance to give information and get more medicine. *Thus situated it was absolutely necessary to effect a passage, even for present relief.*" (See section 1839.) This patient was himself a physician, and stated to the writer that he had, before applying to him, consulted a physician, whom the editor would admit to be one of the first in Philadelphia, and had perseveringly followed the advice he received for eleven months, without any material benefit.

These observations shall be closed with a few remarks on the cases stated by the editor.

In the first, it is to be remarked that almost from the commencement, the patient took, in aid of the medicines recommended, large doses of magnesia, which is specially objected to in more than one place; (see sections 1566, &c. 1609 to 1612) and which was frequently objected to by the writer in the progress of this case. Notwithstanding this irregularity, however, although the treatment commenced late in November, by the 9th of December she was much relieved, and the attendance ceased for the time. (See the case stated by the editor.)

Having neglected the advice given her, to continue to procure two passages daily, she had on the 14th of December a return of the symptoms with great violence. She immediately commenced taking merc. cath. pills, without magnesia. "These medicines operated pretty well, and she improved daily in every respect," until the 19th; when the attendance again ceased for a time.

From the 19th to the 29th she took medicine occasionally only. "The acid gaining head, she took on the 30th magnesia with a little soda, and twelve grains of aloes;" and until the 3rd of January took magnesia and senna. "The discharges from these medicines were all light coloured and thin, and she was getting worse; so as again to request my attendance."

January 3rd to 8th, took merc. cath. and occasionally magnesia, almost in every instance of her own prescribing, instead of repeating the merc. cath. when the first dose failed to operate. The consequence was, "that the discharges continued thin and light coloured, and she was rapidly getting worse." This had been predicted, and was urged strongly, and "she was at length convinced, to use her own words, 'that nothing but thorough work would do,' and determined to give up the magnesia." "Jan. 8. She was by this time very low." From the 8th to the 11th, inclusive, she took merc. cathartics alone, resolutely withstanding the inclination to take magnesia, which had by its speedy operation always afforded her temporary relief, but which had no lasting good effect, because it did not evacuate from the liver, and at the same time weakened her continually. (See sections 1612, and the 3rd case in the note at the bottom of p. 310 of this Journal.) This abstinence was rewarded, and as soon as the merc. cath. produced black discharges she obtained relief, and these being continued, she improved every day. After the 12th there was no acid. Feb. 6. The dose was reduced to three pills. "The discharges became natural in consistence about the 21st January. The appetite had been for some time very good; no acid; no pain; no waterbrash; strength returning; colour good and healthy. Continued to take pills of rhubarb and aloes occasionally, after my attendance ceased." She continued well in September 1829, (near five years afterwards) by the occasional use of the pills.

This abstract shows in a striking manner the effect of cathartics in dyspepsia, and *the difference between cathartics of different kinds*; and places in a strong light the fact, that stating cases *without the rule that governed the practitioner in the conduct of them*, is not the way to represent him fairly, or to give a true specimen of his practice.

In the statement of the second case, the editor does not "quote it at full in the author's own words," as he proposes to do.

The editor remarks that "the author gives a very brief detail of the progress of the disease from the attack, up to the summer of 1825,

which as it does not bear immediately upon the treatment subsequently pursued, we shall omit."

The author gave all the facts he could extract from the patient in repeated conversations, taking notes, reading them to him and hearing his corrections; and gave the whole, though he was afraid they would be considered long, (occupying four pages, 1812, 1838) because he considered the most of it as "*bearing immediately upon the treatment subsequently pursued;*" and showing in a remarkable manner that in the course of ten years, in the hands of various physicians, every remedy that produced a loose state of the bowels, was of service to him, and every one that did not, was of little or none. Among others is the statement that "a box of pills and a mixture of rhubarb and salt of tartar, *which together produced three or four dark green passages a day*, had a very good effect, and he thinks if he had continued them he should have been cured. But he got so much better that he went to work, laboured very hard in preaching, and became as ill as ever."

The editor regrets that, as the dyspepsia followed an attack of fever, the treatment of the fever is not mentioned. For, he says, "If the purgative plan recommended by the author, was employed, there would be much ground to suspect that the disease was induced by the use of the remedies; as one of the most common cases for which we are here consulted, *by patients from the South and West*, are the various forms of chronic irritation and disorganization of the abdominal organs, induced by the remedies employed for the cure of the fevers of those sections of our country."

What can be the object of a writer, who with the book in his hands, casts such insinuations in direct opposition to the facts stated which he has thought proper to omit?

In the case stated *in the treatise*, it is said that this disease commenced after a fever in 1807. At that time the author lived in the East, the patient lived in the East, and was treated by Eastern physicians, taught in Philadelphia, if taught at all. This the editor with the book before him must have known. (See preface and section 1812.) Moreover, this is true of almost every case stated. Physicians taught, directly or indirectly, in Philadelphia, managed the fever in every case, and treated the dyspepsia that followed.

The writer has only room to remark respecting the close of this case, that it is not stated in such a manner as to give a correct idea of it. The patient by the middle of September had become able to eat freely

of mush and milk. By the use of toddy from early in the morning, he lost his appetite, and on giving up this indulgence it became as good as it was before. He continued improving under *the occasional use* of the pills; and in November the disease returned, the patient after attendance had ceased, having returned to his indulgence in toddy, (see pp. 251-253 of the treatise, vol. 2) and was given up in despair.

As to the insinuation respecting the practice of the physicians of the South and West, it is arrogant and presumptuous, and altogether absurd. 1. In presuming that the practice recommended by an individual in a publication in 1828, should have had the extensive influence the editor seems to attribute to it. He ought to know that there are very many distinguished practitioners throughout the South and West, whose practice was not to be formed by so recent a publication. Has the editor's practice in dyspepsia greatly increased since the publication of the work under consideration? 2. In presuming, at the distance of 600 miles, to say how the physicians of the South and West ought to practise; and that by the remedies used by them for the cure of fevers, various forms of chronic irritation and disorganization of the abdominal viscera, are induced.

The editor in the last three lines of his notice of the treatise observes;

"It must be remarked, that, it is acknowledged by Dr. Cooke, that these extraordinary doses have not been *necessary*, except in the cases which he cites, and two of which we have copied."

It is rather late at the close of a notice of six close pages, intended to give edge to a review preceding it, to make this solitary remark. The editor has deliberately, at full length, given the character of *extraordinary* to the practice of the author, in connexion with the charge of *infatuation*, (see p. 317) and endeavoured to support it by stating two cases as a specimen of that practice; and even when he admits in three lines at the close that they are stated as extraordinary cases. he says nothing about the ordinary practice; and neither in the review, nor in the editorial article is there any precise statement of what the author proposes to effect and what he considers it necessary to avoid.



