G. Chaplin Child
from Dr. D. B. Reid.

AN EXPOSURE

OF THE

CONTINUED MISREPRESENTATIONS

BY

RICHARD PHILLIPS, Esq.

(ONE OF THE EDITORS OF THE PHILOSOPHICAL MAGAZINE AND ANNALS,)

IN HIS

ATTEMPT TO VINDICATE HIMSELF

FROM

DR. REID'S FIRST EXPOSURE OF HIS MISREPRESENTATIONS IN THAT JOURNAL,

BY

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"*Nos nostraque lividus odit,*"—Hor.

MACLACHLAN AND STEWART, EDINBURGH.

SEPTEMBER M.DCCC.XXXI.

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LETTER

TO

RICHARD PHILLIPS, Esq.

ONE OF THE EDITORS OF THE PHILOSOPHICAL MAGAZINE AND ANNALS.

Edinburgh, September 19, 1831.

Sir,

As, from the unaccountable length of time you have taken to answer my former Pamphlet, the charges I there brought against you must have been in a great measure forgotten, I am reluctantly compelled to notice your reply.

In your letter addressed to me, which, you say, is in answer to my former Pamphlet, you have, in every instance, evaded the most serious part of my charges; and when you have attempted a reply, you have either adopted the same system of misrepresentation, of which I formerly complained,—shifted your ground when you found your first position to be untenable,—or endeavoured by some subterfuge to mystify the point at issue: In the annexed Exposure, I have given abundant evidence in proof of these assertions. I have there shown that you repeatedly contradict yourself, representing me, among other charges, as never having made an experiment I described in my book, while, when you try it yourself, you find it to be as I describe; that, after involving yourself in a labyrinth of errors, you endeavour to evade the point at issue by quibbling upon words; that, after affirming in your review that strong nitric acid never becomes coloured in the way I mention by absorbing nitric oxide, and finding that it does on trying the experiment, you invent a method by which the experiment must fail, and then maintain that the colours are not produced; that you quote in your favour an experiment of Dr. Priestley which you do not understand, imagine that I have misrepresented it, and actually adduce evidence against yourself;

that you think it best not to interfere with, or enter minutely into, charges of misrepresentation, and do not attempt to answer even one of all those I have made; that you do not mention the points at issue correctly, and do not scruple to affirm that I maintain what I never maintained, that you may have an opportunity of refuting me; that in renewing your review of my Elements, (the delicacy of this proceeding after what had taken place I do not challenge), you continue the same system of misrepresentation; and that in your anxiety to find fault with my work, you actually go the absurd length of charging me with ignorance, because I did not know in the year 1829 *what you were to discover in the year* 1831.

I may be allowed to add, Sir, that I do not consider this matter altogether of a personal nature; for just in proportion to the great importance of the character of those persons who profess to give a direction to the public taste and judgment, it becomes necessary to make them occasionally amenable to public opinion when they pervert the legitimate objects of criticism, and take advantage of their privileges to sport with the characters of others.

I am, Sir,

Your obedient Servant,

D. B. REID.

RICHARD PHILLIPS, ESQ.
Birmingham.

EXPOSURE

OF THE

CONTINUED MISREPRESENTATIONS,

&c.

It is now upwards of nine months since I felt myself called on to expose the calumnies and misrepresentations of an article, which appeared in the Annals of Philosophy, on my Elements of Practical Chemistry. The charges which I there preferred were too serious to be trifled with, and the proof I led of too substantial a character to be evaded by the usual subterfuges. Accordingly, after taking the extraordinary period of seven months to consider charges, which every one of right feeling must have seen the propriety of answering promptly, if he could answer them at all, this tardy answer at last arrives, prepared by R. Phillips, Esq., one of the editors of the Annals, who acknowledges himself the author of the review. He takes credit to himself for making the acknowledgment, and, whatever may be his motives, he has judged rightly, for at a small expense he makes a parade of openness, whereas, had it been worth the trouble, he might have been easily traced out; and thus he takes little more on himself, than that responsibility which already attached to his situation as editor.

Mr. P. complains of my language; he is of opinion, I suppose, that I should have submitted tamely to have my book depreciated and my character traduced. He has accordingly raked together several quotations to shew that authors are a most irritable and unreasonable generation, that they will not submit quietly to the lash of the critic, but that all those of any promise humbly bow to his decision. We learn, moreover, from these quotations, and several other broad hints, that he himself is a man of a calm philosophic spirit, free from gross passion, who keeps the even tenor of his way, and proceeds, purely from his love of science and zeal for the commonwealth, to clear the land of noxious animals. The merit of this temper is somewhat equivocal; it may be very convenient for Mr. Phillips in his vocation; but it is rather too much for him to expect that authors should bear a wound with the same coolness with which he can inflict it. Mr. Phillips's commonplaces about authors will not lead me to deal in the same kind of language about critics,—for many of them I have a most unfeigned respect; no person has a higher sense of the importance of sound and manly criticism to the interests of literature and science; and I am no less aware of the rare combination of talent necessary for the discharge of its duties. I know likewise its privileges, and am quite sensible of the folly, in all ordinary cases, of contending with those who take advantage of them. General abuse, disingenuous nibbling, caricature, petulance of tone, and all the vulgar arts by which some critics try to make out a smart article, are within their chartered liberties; but surely no man is called on to submit to downright false statements, and gross misrepresentations, which affect both his work and his professional character, when he has it in his power to correct them. Whether, therefore, in the present case I have been over sensitive, or Mr. Phillips has gone such lengths as to justify my Exposure, must be left for the public to judge. This Exposure I have considered the more necessary, as many who have paid little attention to these subjects might be deceived by the hardihood of the assertions, and the confident air with which Mr. Phillips delivers his opinion. Had Mr. Phillips allowed my former statement to appear in the Annals, this Exposure of his answer to it would have been in a great measure unnecessary, but this he thought proper to evade.

In one of the quotations referred to, allusion is made to those who fear inquiry, "while those whose sole object is truth, can have no apprehension from the severest scrutiny." Whether Mr. Phillips or I feared inquiry, the following statement will prove.

When the review of my Elements of Practical Chemistry appeared in the Philosophical Magazine last December, I immediately published a pamphlet exposing the want of candour in the reviewer, quoting in full every passage on which I made any comment. Mr. Phillips wrote to me from Birmingham on the 12th of February, stating that he was the author of the review, and assuring me that he would have great pleasure in publishing my reply in his Journal, provided it were condensed. To this I made the following reply :—

DR. REID'S LETTER TO MR. PHILLIPS.

George's Square, February 19, 1831.

Sir,

I have to acknowledge the favour of your communication, and am rather surprised to perceive that you are the author of the review of my Elements of Practical Chemistry, in the Philosophical Magazine and Annals. From the offer you have made, you seem to be aware of the claim I have to be heard in your Journal, but I don't see that the alteration you suggest can be made without injuring my statement. I have, indeed, with the view of condensing it within as small a compass as possible, passed over several things which I should otherwise have noticed, and confined myself to what was strictly connected with specific charges which you have made against my work, quoting your own words in full wherever I have adverted to them, that the public might be put in full possession of the materials for judging between us. But even had I been disposed to abridge it, the undefined manner in which this is proposed, would leave me completely at a loss as to the limits that would render it acceptable. As you appear, however, to decline admitting it in your Journal in its present form, if you will give me notice of the number of copies that may be required, I shall provide them at my own expense, and forward them in time to be appended to the next number when it is published.

Should you think proper in any way to allude publicly to this correspondence, I have to request that you will publish this letter at the same time. I am, Sir, your obedient servant,

D. B. REID.

Richard Phillips, Esq. Birmingham.

I accordingly put aside copies of this pamphlet for the Journal. It was not to be expected, after writing it solely with the view of exposing the misrepresentations in his Journal, that I was, at the invitation of the author of the review, to condense my pamphlet, and leave out any statement that I considered necessary for my defence, after I had been so unceremoniously attacked. It would have been no more than common justice had I been heard in the Journal in which I was attacked, more especially as from the offer I made, it would have been done at no expense to the proprietors.

Nearly a month elapsed after I sent this letter to Mr. Phillips before I received any reply, when I was favoured with the following communication :—

MR. PHILLIPS'S REPLY TO DR. REID.

Birmingham, March 15, 1831.

SIR,

After mature consideration, I intend, at my earliest leisure, to publish a reply to your "*Exposure;*" should I however alter my determination, I will let you know, and then arrangements may be made for appending your remarks, in the way which you propose, to the Philosophical Magazine.

By thus waiting for a short time, you will have the advantage of more completely attaining your object by replying to any additional errors which I may commit, and also of correcting any mistakes, into which you may yourself have fallen.

I have no intention of publishing the correspondence which has taken place between us. I remain, SIR, your obedient servant,

R. PHILLIPS.

Dr. Reid.

Thus, after mature consideration, he no longer stated that he would have great pleasure in publishing my answer, and postponed indefinitely the insertion of my pamphlet in his magazine, on the plea that he intended to publish a reply at his earliest leisure, a circumstance which ought not to have prevented him from doing me that justice which he at first appeared so anxious to afford me.

The first subject in his letter to which I shall refer, affords a very remarkable illustration of the contradictions into which a person naturally falls in attempting at all hazards to support an erroneous statement; and I beg leave to call the reader's particular attention to it, as it alone will give him an idea of the spirit in which the letter is written, the mode of experimenting adopted, and the nature of the proof he brings forward in support of his experiments and affirmations. This single specimen also will inform him, should he not already have paid special attention to chemical investigation, how absolutely necessary it is that the details should be minutely and deliberately compared, before any conclusion can be drawn from the general statements made along with them.

I. MR. PHILLIPS CONTRADICTS IN HIS LETTER WHAT HE HAS AFFIRMED IN HIS REVIEW.

In the first place, I have to beg that the reader will compare the following statements :—the first is taken from his review of my work; and the second from his "Letter." They refer to the colours produced by transmitting nitric oxide through nitric acid. I had stated in my Elements of Practical Chemistry, p. 61,

"If a current of nitric oxide gas be transmitted through colourless nitric acid, a large quantity of this gas is absorbed, and the acid speedily acquires a light straw-colour, which deepens to a reddish brown, and passes through various shades of olive and green till it at last becomes almost blue."

Mr. Phillips makes the two following observations on this subject :—

Passage from Mr. Phillips's Review.

"We have been already instructed to consider that by nitric acid, we are to understand that which is concentrated, and such we presume is that to be employed in this experiment; and if this be the case, the effects produced are described with extreme and most culpable inaccuracy. In the first place, strong nitric acid never becomes at all either olive, green, or blue by absorbing nitric oxide: and what proves that Mr. Reid never performed the experiment is, that when the colours are produced they do not occur in the order stated by him;" P. 454.

Passage from Mr. Phillips's Letter.

Quotation I.

"I now come to the experiment by which you intend to prove, that in strong nitric acid all the appearances described may be produced in the course of a single minute, by operating 'on a small quantity of acid with a brisk current of gas.' I have performed this experiment, and will readily admit, that by passing a strong current of nitric oxide through a fluid drachm of nitric acid, of sp. gr. 1.497, I obtained nearly the tints you mention;" p. 7.

In his critique, he affirms that my description is extremely and culpably inaccurate,—so far from the truth that he does not hesitate to throw out that I never performed the experiment; while in his letter, it appears that the moment he tried the experiment in the manner I recommend, he obtained nearly the tints I mentioned. Here he has not only denied the accuracy of my statement, but actually charges me with not having performed the experiment, in such a way as to insinuate that I had described as if from personal experience an experiment which I had never performed, and thus wantonly sports with the character of another on a subject on which, notwithstanding the confidence of his tone, it appears he was totally ignorant, and is compelled, after my exposure, to bear testimony to the truth of my statement. This is the person too, who says that he is not aware that he has exceeded the bounds of fair criticism, and wonders that I should take offence at this review.

But let us continue our examination:—

II. CURIOUS QUIBBLE BY MR. PHILLIPS.

Quotation I. continued.—"but the acid, as in Dr. Priestley's experiment, had become 'exceedingly weak,' by the evaporation of nitrous acid, into which the nitric acid had been converted by combining with nitric oxide. Nitric acid, of 1.497, decomposes about 72.8 per cent. of carbonate of lime; but the blue-green acid remaining after the passage of the nitric oxide through it, decomposed only 52.5 per cent. I consider it as completely proved, by Dr. Priestley's and my own experiments, that 'strong nitric acid never becomes at all olive, green, or blue, by absorbing nitric oxide.'" Letter, p. 7.

In what I have now quoted, it will be seen, that he appends to the first part of Quotation I. as qualifying the result, a statement of a fact which has nothing whatever to do with the point at issue. In the passage in my Elements, in which this experiment is mentioned, there is not even the most distant allusion to the changes which the acid undergoes in specific gravity by the operation of the nitric oxide. My remarks are confined to the colour alone. Had I affirmed that the acid does not become weaker during the absorption of nitric oxide, then there would have been some reason in bringing forward his objection. My words, however, are simply, "If a current of nitric oxide gas be transmitted through colourless nitric acid, a large quantity of this gas is absorbed, and the acid speedily acquires a light straw colour, which deepens to a reddish brown, and passes through various shades of olive and green, till it at last becomes almost blue." In his attempt to get out of the difficulty in which he had involved himself, by asserting that strong nitric acid does not become of the colours described, because at the same time it becomes weak, he commits the same blunder or play upon the terms he must necessarily use, as he would do were he to say, " strong sulphuric acid does not become weak when water is added to it, because, after the addition of the water it has ceased to be strong." It was necessary for him to have some explanation of the admission he was obliged to make in his letter, " that by passing a strong current of nitric oxide through a fluid drachm of nitric acid of sp. gr. 1.497, be obtained nearly the tints I mentioned," which he had so stoutly denied in his review: and for this he resorts to the curious quibble upon the word *become.*

In the following quotation from the very next page of his letter, he makes use of the verb " become" in the same manner as I do.

Part of Quotation II. (below.) " Two fluid ounces of nitric acid sp. gr. 1.497, similarly treated," (having passed into it nitric oxide obtained from the solution of about 650 grains of copper,) " became red at first, and then brownish red; but did not at any period of the operation, appear either blue or green. Its sp. gr. was increased to 1.541." P. 8.

This statement, explained in Mr. Phillips's way, must signify—acid of sp. gr. 1.497 became red at first, and then brownish red, but having changed its density during the operation, and ceased to be of the sp. gr. 1.497, *acid of sp. gr.* 1.497 *never becomes red at first, and then brownish red by absorbing nitric oxide.*

III. MR PHILLIPS, AFTER AFFIRMING THAT STRONG NITRIC ACID NEVER BECOMES COLOURED IN THE WAY I MENTION, BY ABSORBING NITRIC OXIDE,—AND FINDING THAT IT DOES ON TRYING THE EXPERIMENT,—INVENTS A METHOD BY WHICH THE EXPERIMENT MUST FAIL, AND THEN SAYS THAT THE COLOURS ARE NOT PRODUCED.

Quotation II.—" I put into a vial two fluid ounces of nitric acid, of sp. gr. 1.067, and passed into it nitric oxide gas, obtained from the solution of about 650 grains of copper; the acid soon acquired a slight blue tint, the intensity of which did not afterwards increase, nor did it assume any other colour; its density was raised to 1.110.

" Through an equal measure of acid of sp. gr. 1.420, I passed the same quantity of nitric oxide. It became for a short time yellow, then olive green, and afterwards deep green, without ever appearing red or blue; the density of the acid, after the operation, I found to be 1.403.

" The acid next employed was of sp. gr. 1.465. Through two fluid ounces of this, nitric oxide was passed in the same quantity as in the former experiments. The acid was first red, then olive, and retained the latter tint at the conclusion of the experiment. Its sp. gr. was 1.459.

" Two fluid ounces of nitric acid, sp. gr. 1.497, similarly treated, became red at first, and then brownish red; but did not, at any period of the operation, appear either blue or green. Its sp. gr. was increased to 1.541.

" I think, Sir, I have now proved, both by authority and experiment, the following positions:—That nitric acid, of any one degree of strength, is incapable of exhibiting all the various changes of colour, produced by absorbing nitric oxide:—that when strong acid becomes either olive, blue, or green, it is owing to the evaporation of nitrous acid and the consequent diminution of its strength; and lastly, that weak nitric acid never becomes yellow or red at all."—P. 8.

Weak nitric acid never formed a point of dispute between us. I have already shewn the absurdity of his defence, on the ground that the acid becomes weak; and that he is obliged to admit in his letter, what in his review he had denied. I have now to shew that, in his anxiety to find me in the wrong, he has invented a method of conducting the experiment I have described, by which it must necessarily fail, and concludes, therefore, that I am in error, forgetting, at the same time, that in one page he is actually denouncing as false, what, in the preceding page, he had, by a reference to his own experiment, admitted to be true. The proof of this the reader will see in two of the paragraphs above quoted, which I now oppose to each other:—

Mr. Phillips's Statement in page 7 of his Letter.

"I come now to the experiment by which you intend to prove that in strong nitric acid all the appearances described may be produced in the course of a single minute, by operating 'on a small quantity of acid with a brisk current of gas.' I have performed this experiment, and will readily admit, that by passing a strong current of nitric oxide through a fluid drachm of nitric acid of sp. gr. 1.497, I obtained nearly the tints you mention;"

Mr. Phillips's Statement in page 8 of his Letter.

"Two fluid ounces of nitric acid, sp. gr. 1.497, similarly treated, became red at first, and then brownish red; but did not, at any period of the operation, appear either blue or green. Its sp. gr. was increased to 1.541."

"—— nitric acid, of any one degree of strength, is incapable of exhibiting all the various changes of colour, produced by absorbing nitric oxide."

How curious it is, that it did not occur to Mr. P. that if one fluid drachm of acid of 1.497 gave the appearances I described, when treated in the manner I mentioned "by passing a brisk current of gas through it," he would have obtained the same result with sixteen fluid drachms of the same acid, had he used sixteen times the quantity of nitric oxide to act upon it. Surely, if a brisk current was required for a small quantity, as I had said, no ordinary person would imagine, with a large quantity of acid, a limited quantity of gas should be used for the same experiment. He certainly stands a little in need of that advice which, through his own blunders, he recommends without any grounds to others.

"Really Sir, if you will not take the trouble to examine by experiment in order to be correct, I advise you so far to consult your memory, as to enable you to be at least consistent in error; you will thus avoid the contradiction of stating that to be true on one occasion, which you denounce as false on another." Letter, p. 17.

IV. MR. PHILLIPS QUOTES IN HIS FAVOUR AN EXPERIMENT OF DR. PRIESTLEY WHICH HE DOES NOT UNDERSTAND, IMAGINES I HAVE MISREPRESENTED IT, AND ACTUALLY ADDUCES EVIDENCE AGAINST HIMSELF.

In my first Exposure, I referred to authority as well as to the experiments I had often performed, in evidence of the accuracy of my description. In his observations upon one of the passages I quoted, the following passage occurs.

Quotation III.—"You have attempted to support your opinions in two modes.—First, by experiments, without details; and secondly, by authorities, one of the latter of which, by a most infelicitous accident, proves I am right.

"To begin with your authorities; and, first, with Dr. Priestley. The annexed statement I copy from p. 5 of your *Exposure.*—'Dr. Priestley made many experiments on this subject: the following are the changes of colour which he observed in one of them, of which he makes particular mention, where the acid was placed in a vessel containing nitric oxide. The experiment was made with 'strong pale yellow spirit of nitre.' *'Presently after this process began,' alluding to the absorption of the gas by the acid, 'the surface of the acid assumed a deep orange colour, and when twenty or thirty ounce measures of air (nitric oxide) were absorbed, it began to be sensibly green at the top; and this green kept descending lower and lower, till it reached the bottom of the phial. Towards the end of the process, the evaporation of the acid was perceived to be very great; and when I took it out, the quantity was found to have been diminished exactly one half; for there remained no more than the quantity of two penny weights of water. Also it had become, by means of this process, and evaporation together, exceedingly weak, and was rather *blue* than green.'—*Experiments and Observations on different kinds of Air*, Vol. i. p. 384. Now, Sir, although I had read Dr. Priestley's writings repeatedly, and, as I thought, with some care, yet I protest I did not understand what is meant by the statement that 'there remained no more than the quantity of two pennyweights of water,'

* Here, in my first Exposure, the quotation from Dr. Priestley begins.

none being mentioned in the experiment as quoted by you. On referring to Dr. Priestley, I found the following lines, which threw much light on the subject to me, and may, perhaps, to others:—'Having filled a phial, *containing exactly the quantity of four pennyweights of water*, with a strong pale yellow spirit of nitre, with its mouth quite close to the top of a pretty large receiver, standing in water, I carefully drew out almost all the common air, and then filled it with nitrous air; and as this was absorbed, I kept putting in more, till, in less than two days, it had completely absorbed 130 ounce measures. Presently after this,' &c. as above quoted by you. If I had made an omission of this sort, what an outcry of garbling, &c. you would have raised against me. It is by no means requisite, however, that I should charge you with intentional misrepresentation, to account for the omission which I have detected."

Here then Mr. Phillips has assumed the following position:—

That I have omitted an important part of Dr. Priestley's account of the experiment, (the passage quoted by Mr. Phillips, in which the italics occur), thereby misrepresenting his experiment, and making it appear that it was performed with strong acid, when, according to Mr. Phillips, it was performed with acid diluted with the quantity of four pennyweights of water. Now, in answer to this, I have to shew, that Mr. Phillips has completely misunderstood Dr. Priestley, that the experiment was performed with strong undiluted acid, and that Mr. Phillips has consequently misrepresented the experiment, when he says that the acid was diluted with the quantity of four pennyweights of water. The meaning of the words which puzzled Mr. Phillips, and which he has triumphantly printed in italics, (though not so by Priestley), is, *the phial was capable of containing a quantity of acid equal in bulk to four pennyweights of water.*

First, let us attend to the following passage in the quotation from Dr. Priestley: "Towards the end of the process, the evaporation of the acid was perceived to be very great; and when I took it out, the quantity was found to have been diminished exactly one half; for there remained no more than the quantity of two penny weights of water," that is, the quantity of acid at the close of the experiment was only one half, and it was equal to the quantity of two penny weights of water, or occupied a space equal to two penny weights of water. But the phial was full at the commencement of the experiment; the quantity of two penny weights of water remained at the end, and this was exactly one-half of the whole, therefore, the other half which evaporated must have been equal in quantity to two pennyweights of water; that is, the whole must have been equal to four pennyweights of water. Now, Mr. Phillips says that the phial contained at the commencement four pennyweights of water: I have proved that the whole contents of the phial, whether acid or water, were equal in quantity to four pennyweights of water, and it was full; therefore, the phial was full of water, and could not hold any of the nitric acid with which the experiment was made, and had Dr. Priestley attempted to pour any strong pale acid into it, the acid and water would have run out together and burnt his fingers. Any one might have easily discovered from this passage that there must have been a quantity equal to four pennyweights of water in the phial, and that this filled it, because when two parts are evaporated from the whole, and two remain, the whole must have been four parts, as two and two are equal to four; but Mr. Phillips could not discover this, therefore, to use his own words, he "must be in a condition to understand that, when two are subtracted from four, there remain two; yet unable to comprehend that four will result from the addition of two to two."—*Mr. Phillips's Letter to Dr. Reid*, p. 13.

But there is yet additional internal evidence of Dr. Priestley's meaning. What does he mean by saying "having filled a phial containing exactly the quantity of four pennyweights of water, with a strong pale yellow spirit of nitre?" Here he means to give us particulars as to the quantity of acid employed; if he does not, we have no other data to inform us of it, and the particular statements regarding the quantity left are of no use whatever; and, besides, how absurd it must be to tell us that he diluted the acid with four pennyweights of water, when he gives no clue to the proportion of acid, either directly, or by mentioning the size of the phial. But Dr. Priestley's statements are exceedingly correct and minute, and he is endeavouring to inform us how much acid he employed, and how much remained, that we may have a precise idea of the result; though, according to Mr. Phillips, all that we learn is, that Dr. P. took a phial, put four pennyweights of water into it, filled it with strong pale yellow spirit of nitre, and found that there remained at the end of the experiment only two penny weights of water, and *no acid at all!* How much acid, however, was used, Mr. Phillips's view does not inform us, and all that we learn is, that two pennyweights of water had evaporated with the whole of the indefinite quantity of acid employed; in fact, the experiment goes for nothing,—has no meaning. But, to crown all, he goes on in the next paragraph to speak of the strength of the acid that remains, while, according to his view, nothing remained but water. Taking the other view, however, we learn that one-half of the acid employed had evaporated.

Mr. Phillips professes to have read Dr. Priestley's writings repeatedly, and with some care. How then did he fall into such an extraordinary mistake as to take Priestley's method of mentioning the capacity of the vessel with which he was operating, for his affirming that it *contained* the quantity of water, which he only said it was capable of containing? How did he not discover that through his whole work Dr. P. uses similar modes of expression for mentioning the capacity of a vessel, as in the present instance, and which, in our day, certainly appears somewhat antiquated? If he had read the chapter in question with care, how did he not perceive that at the top of the second page after that in which the experiment is described, Dr. Priestley says, "The above-mentioned experiments were made with the strongest yellow spirit of nitre?" Only one experiment occurs between this passage and the experiment in question. How did he not perceive that in the page preceding the one in which the experiment is described, Dr. P. mentions an experiment in which he takes precautions against the dilution of the acid, and that it was this experiment which led to the one which Mr. Phillips mistakes, as will be seen in the annexed quotation? "In order to observe the full effects of nitrous air in a given quantity of strong nitrous acid, I filled a small phial with it, and then introduced it through the water into a large jar, previously filled with nitrous air, and supported the phial in such a manner as that the water could never rise so high as to get into it."—"I was so much struck with this experiment that I repeated it very often; and the following is a distinct recital of all the remarkable appearances attending one of them, which I select from the rest, as I noted them more minutely than in any other process of the kind." P. 383. Immediately after this comes the experiment, in which, according to Mr. Phillips, a quantity of acid was mixed with four pennyweights of water, and at the end there was nothing left but two pennyweights of water.

Is it not a remarkable circumstance that Mr. Phillips should accuse me of an omission, and say I misrepresented the experiment quoted; and that, in endeavouring to set me right, as he thinks, he should actually himself misrepresent the experiment, and interpret it in a manner that renders it a tissue of inextricable errors; that, not knowing what he is about, he should actually quote in his favour the results of an experiment which he considers as done with an acid mixed with water, which was done with strong acid? How singularly curious is his remark, that this very quotation from Priestley should, by an infelicitous accident, prove his statement, while it actually demonstrates the accuracy of my statement, and of Mr. Phillips's own experiment in page 7 of his letter, which he forgets, and attempts to disprove in page 8. It may be proper to mention, that the passage he alludes to was omitted in my quotation, because, as will be seen, it refers to minutiae in the mode of conducting the experiment, and to the quantity of acid employed, that the amount of evaporation might be known, a matter which had nothing to do with the point in question—the colours produced by passing nitric oxide through strong nitric acid.

V. MR. PHILLIPS'S MODE OF EVADING CHARGES OF MISREPRESENTATION.

Quotation IV.—" In p. 8 of the *Exposure*, you observe, ' the reviewer has also been pleased to comment very freely on the manipulations in some of the processes which I have described; and here, also, I have to point out numerous errors and misrepresentations.' And here, as usual, you have found a quotation to answer a fact, and with this I shall not interfere." Letter, p. 8.

Perhaps he thought it would be prudent not to interfere. See Exposure, pp. 8, 9, 10, and 11, particularly Quotation VI. pp. 10 and 11, where I have shown that he has misrepresented my statements in such a manner, as thereby to make out a case against me which my words would have disproved, had he quoted fairly the passage on which he was commenting. His method of replying to other charges of a similar nature is well illustrated by the following passage, at page eleven of his letter.

Quotation V.—" There is also another subject on which you accuse me of error; I do not think it worth while to enter minutely into it, and will therefore admit, that after having directed the use of equal weights of nitre and sulphuric acid, for preparing nitric acid, you do *not* ' give further instructions on the subject,' (p. 55,) though you afterwards advise the employment of ' three ounces (water measure) of sulphuric acid to eight ounces by weight of nitre,' (p. 56); and I will also allow, that the first mentioned proportions ' were directed for the retort process,' though we are told that ' the distillation may be conducted in flasks;' these I will not dispute to be facts, though I have some difficulty in believing them."

All the circumstances which he refers to in this paragraph were charges, not of error, but of gross misrepresentation, which, above all others, he was bound to enter minutely into and explain, if he wished to maintain the character for candour and fair criticism which he had claimed for his review.

VI. MR. PHILLIPS'S MODE OF MAKING OUT CONTRADICTIONS.

Quotation VI.—" In p. 54 of the *Elements*, you propose to condense vapours by means of a slender stream of water. To this method I objected, and stated that, in my opinion, immersion in cold water was more convenient. In p. 8 of the *Exposure*, you say, ' I have frequently tried this mode of operating. Though apparently more simple to an inexperienced operator, it is in reality much more difficult, and less successful; and the mode which I have recommended was adopted in preference, because I found that the student conducted the operation more easily in this manner." Letter, p. 9.

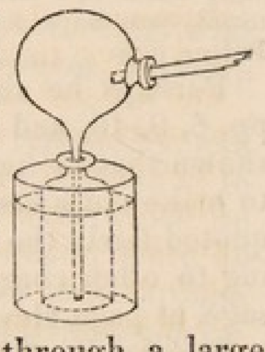
He afterwards goes on to state,

" I am sure you will excuse my not being aware that you had actually employed a common bottle, and a tubulated receiver, and cooling by immersion, when I put you in mind that you have forgotten it. In p. 245 of the *Elements*, you state, that ammonia ' is obtained most conveniently by decomposing muriate of ammonia by slaked lime, receiving the product into water kept cold in a bottle receiver.' "—Letter, p. 9.

Here he affirms, that the statement in the Exposure, quoted above, is a contradiction of the statement which he has extracted from p. 245 of my Elements; and has charged me with forgetting in the Exposure what I have stated in the Elements. He says that I have, in my Exposure, objected to cooling by immersion, when condensing vapours, and that I recommend it in my elements.

Now, it will be observed, that the case where I object to cooling by immersion in cold water, is totally different from, and has no analogy whatever with that in which I recommend the other plan.

In the case in which I object to immersion, and recommend cooling by a slender stream of water, hot vapours pass into an empty vessel; these must be condensed by cold applied to the external surface of this vessel; as the vapours rise to the upper part, it is of great importance that this part particularly should be cool; these ends are best gained by a stream of water which affords a succession of cool portions of water: and besides, by avoiding the inequalities of temperature, the receiver is much less apt to be broken. If we use immersion in this case, the part of the vessel to which the hot vapours go, is the warmest part of the vessel, which is therefore apt to be unequally heated and broken.



Now, in the case of Ammonia, where I recommend immersion, the circumstances are not only altogether different, but quite peculiar. The annexed diagram is the one in my book which I have used to illustrate these peculiarities in the process he refers to.

In the *first* place, the warm ammoniacal gas does not pass into an *empty receiver*, but is condensed by water which must be placed in the receiver, without which, it could not be condensed at all.

Secondly, It will be seen that the ammoniacal gas passes through a large globe, in which its temperature must fall considerably before it reaches the bottle receiver.

Thirdly, There is no special object in cooling the upper part of the receiver, for the warm gas does not go there at first, as in the other case, but is conveyed at once by a tube to the bottom of the receiver which contains the coldest water.

Fourthly, In this process there is no risk of the receiver being broken, as the quantity of water inside prevents the interior from being suddenly or unequally heated.

Thus, Mr. Phillips has failed here also in making out a contradiction. The facts I have brought forward will speak for themselves, and shew his utter indifference to the means by which he endeavours to substantiate the charges he has ventured to make. But let us proceeed with the quotation.

VII. ANOTHER SPECIMEN.

Quotation VI. continued.—" When you advised this method, you had no idea that a weight was requisite to keep the bottle from floating, for none is mentioned in the description, nor drawn in the figure."

Certainly not; cannot Mr. Phillips conceive that a receiver half full of water may be immersed to a certain depth in water, as shown in the above figure, without requiring any weight to keep it fixed in its place, though, when empty, it must certainly float if this precaution be not taken. How unjust is it then not to mention that the receiver is in one case quite empty, and in the other half full of water.

VIII. MR. PHILLIPS SHIFTS HIS GROUND.

Quotation VII.—" We proceed now to consider, whether there is, as you assert, any thing peculiar in the constitution of nitric acid of sp. gr. 1.48. I shall not go through the tedious process of repeating all that you and I have said on this subject; nor shall I refute the charge of suppressing your words, for the sake of misrepresenting your statement. What I understood you to mean is this, (and as usual, you have authority ready to support you,) that, nitric acid of greater or less density than 1.48, is readily acted on by metals ; whereas, when it is of this particular specific gravity, they produce no effect upon it. In order to ascertain these points, I made the following experiments," &c. Letter, p. 13.

Nor shall I go through the tedious process of commenting on his experiments, but must here observe that I made a charge of misrepresentation, which he says he will not refute. Why he should decline refuting this, the most serious part of my statement, he must know best himself. On this subject, he has also completely changed his ground of attack. In the critique, he merely charges me with want of care in admitting into the Elements two statements, one of which contradicts the other. In my exposure I show that the two statements are not contradictory, but that the latter is clearly brought out as an exception to the former, though by the manner in which he couples two different statements together, omitting an essential part of the latter, they appear at variance. In the letter, again, he abandons the charge of contradiction, and says that he understood me to mean that one of the circumstances was a peculiarity in the history of the acid. In his critique, he says I make a statement which is "in direct contradiction" to what I affirm only six lines farther on in the same page, while in his letter after my Exposure of the Misrepresentation, he states he understood me to mean "that nitric acid of greater or less density than 1.48 is readily acted on by metals, whereas, when it is of this particular spec. gr., they produce no effect upon it."

It was evident that when he wrote the review he was not aware that it had ever been alleged that there was any peculiarity in acid of this strength—the subject was quite new to him, and this explains how he at once set down as a contradiction what had been considered a peculiarity.

IX. MR. PHILLIPS AGAIN SHIFTS HIS GROUND.

In the following quotations we have an instance of the readiness with which Mr. Philips can change his opinions, and adapt them to the special end he has in view. I stated that nitrous acid gas is sometimes evolved when nitric acid is made to act on the metals. Mr. Philips, in his review, says,—

"We much doubt whether nitrous acid is in any case whatever evolved. Has not Mr. Reid mistaken the production of nitrous acid, by the action of nitric oxide upon the oxygen of the atmosphere, for its direct evolution by the action of the metal?"—*Review*, *p.* 453.

I quoted several authorities and experiments to prove the correctness of my statement, and among many others the following experiment which I described in my Exposure:

"Take a stout mercurial pneumatic jar from one to two inches in diameter, and from one to ten inches deep ; fill it with mercury and invert it over the shelf of the mercurial trough. Then introduce into it about a drachm of the colourless acid used in the preceding experiment, taking care to avoid the introduction of any air, and using for this purpose any of those numerous modifications of the dropping tube that are now so much employed ;—a common glass blowpipe with the point bent a little upwards does very well. In about a minute in general, after the acid has risen to the top of the mercury in the jar, and when it is of the usual specific gravity, red fumes begin to appear, and the mercury slowly descends, leaving the jar quite full of the ruddy vapours, and affording another instance of their production when metals act upon nitric acid, totally independent of the action of the air." First Exposure, p. 18.

Upon this Mr. Phillips observes,

Quotation VIII.—"I admit the accuracy of the experiment, but I deny that of the inference ; the production of nitrous acid, I believe to have resulted, *not* from the direct action of the metal, but from that of nitric oxide which could not escape, upon the nitric acid undecomposed."—*Letter*, *p.* 16.

In this paragraph he admits the accuracy of the experiment, and being driven from his former explanation, *that the nitrous acid is formed by the action of the* *nitric oxide on the air*, he has recourse to a new method of explaining the same phenomenon, viz. *that the nitrous acid is formed by the action of the nitric oxide upon the nitric acid.*

According to this view, when a metal acts upon nitric acid—at one time, nitric oxide rises to the surface, and, absorbing oxygen from the atmosphere, becomes nitrous acid :—but, if the acid be excluded from the action of the air, the nitric oxide which is formed, determined to become nitrous acid, and being aware that there is no air to supply oxygen at the surface, wisely takes it from the acid in passing through it.

X. MR. PHILLIPS TRIES TO MAKE OUT MORE CONTRADICTIONS.

Quotation IX.—" I will again take the liberty, which I have done on former occasions, of comparing together your own statements. In page 17 of the *Exposure* you assert, ' I know from experiment, that nitrous acid is evolved by different metals when they act on colourless nitric acid in circumstances where no fallacy can arise from the evolution of nitric oxide, and its action on atmospheric air ; and any one may easily satisfy himself of this, by causing iron, nickel, copper, tin, zinc, or bismuth to act on the acid, after adding a little water, in vessels filled with carbonic acid gas, so as to exclude the action of the air.' If this be the case, allow me to inquire, what becomes of the nitrous acid, when, as stated in p. 46 of the *Elements*, that during the preparation of nitric oxide, ' every three equivalents of metallic copper decompose two equivalents of nitric acid,' and these, as shown in your diagram, are separated entirely into oxygen and nitric oxide ?"—*Letter, p.* 17.

This question presents no difficulties ; and if he had compared my statements as he states he had done, instead of *contrasting parts of them*, which was all that he could have done, he also would not have been puzzled by them. In the action represented by the diagram he alludes to in page 46 of my Elements, a *diluted acid is alone spoken of, consisting of the strong acid diluted, as I state, " with one and a half times, or twice its bulk of water."* While in that case in which I have affirmed nitrous acid is disengaged, *strong nitric acid alone is referred to ;* nor is any mention whatever made of water being added in the paragraph in which this statement occurs. In my Exposure again, where I do not allude, as in the Elements to two metals, but to half a dozen, I direct the addition *of a little water.*

Quotation IX. continued.—" Permit me again to ask, how can nitrous acid result from the action of mercury or copper upon nitric acid, since we find, (Elements, p. 47,) that ' mercury and copper are the only metals that disengage pure nitric oxide when they act upon nitric acid ?' "—*Letter, p.* 17.

Here also Mr. Phillips is equally unfortunate in his comparison as in the preceding instance, an acid diluted with one and a half times, or twice its bulk of water, having been employed for the pure nitric oxide, while, as I have shown above, a strong acid, or one with a little water added to it, gave the nitrous acid.

XI. MR. PHILLIPS'S MODE OF MENTIONING THE POINTS AT ISSUE CORRECTLY.

In the remarks which I have made upon Mr. Phillips's letter, I have adopted the same plan as in my former Exposure, quoting fully every passage of his on which I make any comment. Had he done the same in his Critique and in his letter, there would have been no occasion for either this or my former Exposure. In page 2 of his letter, he says,

" In performing my task, I shall not in every case quote the words of your statements, nor of mine in opposition to them ; but I shall endeavour to mention the points at issue correctly, and appeal to authority or experiment, or to both, as the case may seem to require."

Without doubt he had his own reasons for not always adopting a mode of procedure, which alone could prove his determination to lay both sides of the question fairly before the public. We shall see immediately another proof that he does not "mention the points at issue correctly;" and that he has no scruple in affirming that I hold opinions which I never entertained, with the view of proving me in the wrong.

In my first *Exposure*, page 20, while alluding to his observations on oxygenated nitric acid, the following sentence occurs.

"As I have expressed no opinion whatever as to the precise nature of the compound, all the remarks of the reviewer refer solely to the name which I have given it, and from this all his conclusions are derived. How then are the terms that Thenard adopts—*liqueurs oxigénées acides, liqueur oxigénée nitrique, &c.*, to be translated? These expressions, besides the awkwardness of their literal translation, convey no precise idea of the constitution of the compound, nor have they to my knowledge ever been literally translated in this country. Dr. Turner gives these compounds no name whatever; and Dr. Ure, in the fourth edition of his valuable dictionary published a few days ago, employs the original terms which I have still retained."

Upon this Mr. Phillips remarks.

Quotation X.—"You now indeed admitted that Thenard considers the compound alluded to merely as *liqueur oxigénée nitrique;* your excuse for rendering these words *oxigenated nitric acid,* is rather a curious one, viz. 'the awkwardness of their literal translation;' by thus giving a false name because a true one would be awkward, you have adopted a principle in the art of translating, of which you may, I believe, be considered as the first promulgator, and to you its use will probably be confined." Letter, p. 18.

Here, then, Mr. Phillips asserts that I allege the awkwardness of a literal translation as my reason for translating *liqueur oxygénée nitrique*, oxygenated nitric acid, while, on examining my words above quoted, it will be seen that the awkwardness of the literal translation is only one of the circumstances given as my reason, that it is given as one of secondary importance, and that the principal considerations are, that the literal translation conveys no precise idea of the constitution of the compound, and that it had never been literally translated by authors in this country, the term that I adopt being the only one at present in use by English authors, who have given the compound a specific appellation.

Again, in the first part of Quotation X. he says, that I "now indeed admit that Thenard considers the compound alluded to merely as *liqueur oxygénée nitrique.*"

One would suppose from this statement that I had affirmed that Thenard did *not consider the compound as* "*liqueur oxygénée nitrique,*" whereas in my Elements Thenard's name or opinions are not mentioned, or in any way alluded to in the short paragraph that occurs on this subject. See pages 19 and 20 of my First Exposure, or the passages referred to there in Thenard's Traité de Chimie.

Thus, from these two specimens in one short paragraph, it will be seen how far Mr. Phillips has endeavoured to "mention the points at issue correctly," and how far it was prudent in him *not in every case to quote the words of my statement.*

XII. MR. PHILLIPS'S EXPERIMENTS!

His remarks on the nitrate of ammonia I ought to have alluded to before. Mr. Phillips labours with his usual finesse to disguise his former inaccuracies, and is obliged to have recourse to a series of new experiments to help him out of his difficulties. He mixes this up likewise with rather a clumsy attempt to be playful, which it is not easy to understand, for his reasons often look so like jokes, that it is not easy to say whether he is in earnest or not.

The following is the passage in his letter in which he sums up his views, alluding to experiments which he there details.

Quotation XI.—From these experiments I conclude with Berzelius, that nitrate of ammonia first fuses, then boils, and afterwards decomposes; but if the heat be too great, it then sublimes. In order to sublime nitrate of ammonia, it is required to apply a strong heat."—Letter, p. 5.

I have made experiments not once or twice for twenty minutes, but often, and for hours together on this subject, and he must pardon me, therefore, when I state that nitrate of ammonia can be volatilized at the temperature below that at which it is decomposed. As to the passage from Berzelius which he refers to, the circumstances show that it is merely incidentally introduced by this eminent chemist in a chapter upon another subject (nitrous oxide). Mr. Phillips does not refer to any specific memoir of Berzelius on the subject; but Davy's experiments, to which I referred, and who affirms that nitrate of ammonia can be volatilized at a temperature below that at which it is decomposed, were made expressly on this subject. See his " Researches," &c.

XIII. MR. PHILLIPS REVIEWS MY ELEMENTS AGAIN.

With these remarks then, I conclude my examination of the first part of Mr. Phillips's letter. He must have been sufficiently satisfied that my first pamphlet had driven him from the positions he had maintained in his review, and that all his turnings and windings had been exposed, when he commenced a second review, an honour which I could scarcely have anticipated, as scarcely nine months have elapsed since his first review. I shall here be equally able to prove that the same system of analysis has been pursued, and accordingly I proceed to the examination of this part.

XIV. MISREPRESENTATIONS CONTINUED.

He has quoted a passage from my Elements, in which I state that sulphate of mercury is formed during the preparation of sulphurous acid gas, by the action of mercury with sulphuric acid; and opposes to this, as if in contradiction to it, another statement, in which I say that persulphate of mercury is prepared by the action of mercury with sulphuric acid, carefully omitting to mention the circumstances which render the two operations different.

Quotation XII.—" I shall now pass over about twenty pages of the *Elements* without observation, for they contain the chapter on nitric acid which has been already considered, and proceed to notice a statement of yours respecting the preparation of sulphurous acid gas (p. 71.) " When it is prepared on the small scale, 200 grains of mercury, and 300 of sulphuric acid, (about 3 drachms by measure,) may be taken and put into a retort," and after giving some further directions, you say, " the theory of this process is very simple; one equivalent of sulphuric acid (composed of three of oxygen $=24+16=$ one equivalent of sulphur) loses one equivalent of oxygen (8,) which combines with the metallic mercury, and the rest of the oxygen comes away in combination with the sulphur in the form of sulphurous acid gas; the oxide of mercury combines with another portion of sulphuric acid which is not decomposed, and is converted into sulphate of mercury." If then only one equivalent of oxygen be separated from the sulphuric acid, the oxide of mercury formed, though you do not distinctly say so, must be the protoxide, composed of 200 mercury + 8 oxygen; having some suspicion that this statement is contradicted by " precise observation," I boiled together the assigned quantities of metal and acid; the sulphate procured was treated with muriatic acid, in which it was so nearly soluble, that only 3 grains of protochloride of mercury were formed; the oxide produced was therefore, peroxide; in support, I will not say in proof, of the accuracy of my experiment, I shall quote an author, to whom you must not object, although I may sometimes be inclined to mistrust him, I mean *yourself;* in page 338 of the *Elements*, you make the following statement, " persulphate of mercury is prepared by boiling two parts of metallic mercury to dryness with two and a half of sulphuric acid," and the action which occurs is illustrated by the following diagram:—

	Before Decomposition.		After Decomposition.
Sulphuric Acid	Sulph. Acid	32	32 Sulphurous Acid.
	Oxygen .	8	
Sulphuric Acid	Sulph. Acid	32	32 Sulphurous Acid.
	Oxygen .	8	
Sulphuric Acid	. . . . .	40	
Sulphuric Acid	. . . . .	40	
Mercury	. . . . .	200	296 Persulph. of Mercury."

Letter, p. 20, 21.

In the passage where I stated that the compound formed is a sulphate of mercury, I have simply directed the materials to be *heated together*, and the object being not to collect the mercurial compound that is left, but the sulphurous acid evolved, I have said nothing further respecting it, than was necessary in explaining what became of the mercury. Mr. Phillips seems to forget that the usual mode of procuring sulphate of mercury, is by heating metallic mercury and sulphuric acid together. To use Dr. Thomson's words, "This salt may be formed by heating one part of mercury in from 1 to 1½ parts of sulphuric acid. Sulphurous acid is disengaged abundantly ;" or as Mr. Brande says, "the acid and the mercury are to be digested in a moderate heat ;" while to procure the persulphate, the acid is to be boiled to dryness with the metal, or at least have a considerable heat applied for some time. Now, here Mr. Phillips alleges that one statement in my Elements contradicts another, and, in attempting to make out his case, leaves out, in his old way, a part which should have gone along with one of the quotations, which forms an essential part of the paragraph, and would have at once disproved his assertion. For, in the line immediately preceding the first quotation which he makes, that regarding the preparation of sulphurous acid, I direct the acid and metal to be "heated by a chauffer or spirit lamp," and *make no mention whatever of boiling to dryness, or even continuing a strong heat for some time, while, in the process for preparing the persulphate, it is specially directed to "boil to dryness."*

Thus he has collated the circumstances, that in each case the acid and metal act upon each other; carefully keeping out of view the circumstance, that the product varies according as they are "heated together," or "boiled to dryness." No remark is necessary here, except just to remind the reader, that this is contained in his second examination, at the commencement of which he refers to his review, where he says, "that he is not unprepared to adduce more facts. Of facts such as these, this and my former exposure show, that he has within himself a very ample stock.

XV. MR. PHILLIPS DISCOVERS A NEW MODE OF CRITICISM.

Although we have already noticed several rather curious feats of Mr. Phillips, the next one in the second review we are about to advert to surpasses them all in originality, and we willingly bear testimony to the ingenuity which it displays. He has, indeed, found a principle in the art of criticising, of which, to use his own words, he must be considered the *first promulgator*, "and to him its use will probably be confined." (Letter to Dr. Reid, p. 18.)

This discovery of his consists in the happy thought of charging me with ignorance of facts which were not discovered till upwards of a year after my book was published, and which are made public for the first time in the number of the Philosophical Magazine for this month. (September 1831.)

The following is the passage in which this is done.

Quotation XIII. "In p. 339, you make the following observations, and give the annexed diagram, explanatory of the mutual action occurring between bipersulphate of mercury and water: 'Throw half an ounce or an ounce of the persulphate of mercury, heated to the temperature of 400 or 500, into five or six pounds of boiling water, in a large glass flask or earthen basin. A yellow coloured precipitate will be immediately thrown down, composed of one equivalent of sulphuric acid and one of the peroxide of mercury, another portion of the peroxide remaining in solution with an excess of acid. I am not aware that the latter has been very accurately examined ; the annexed diagram gives a precise view of the nature of the reaction, supposing the salt that remains in solution to contain only one more equivalent of acid than the persulphate ; the yellow coloured precipitate is usually termed SUBSULPHATE OF MERCURY, or TURPETH MINERAL.

Before Decomposition.			After Decomposition.
296 Persul-	Sulph. Acid	40	336 Supersulph. of Mercury.
phate of	Sulph. Acid	40	
Mercury.	Perox. of Mer.	216	
296 Persul-	Sulph. Acid	40	
phate of	Sulph. Acid	40	
Mercury.	Perox. of Mer.	216	256 Subsulph. of Mercury."

Letter, p. 25.

Then follows a description of experiments to ascertain the composition of the subsulphate and of the salt that remains in solution, word for word the same as in the Philosophical Magazine, premising in his Letter, that he makes the experiments to ascertain the accuracy of my statement, and in the Magazine, that he does so to ascertain the accuracy of the general opinion, of the correctness of which he had some doubts. At the conclusion of the experiment, in speaking of his discovery of the real composition of the subsulphate, he says, "This is so unusual an atomic constitution, that I have not admitted its existence until after many analyses;" and addressing me, concludes in these words, "Your diagram, representing the decomposition of bipersulphate of Mercury by water, is a tissue of errors."

In giving the composition of the substance generally termed subsulphate of mercury, I stated its constitution according to the analysis which had been generally adopted by chemists, which was the one that Mr. Phillips himself must have referred to as the most worthy of credit till he made his own analysis (published this month), and the one adopted by the latest authority on the subject, (except Mr. Phillips,) Dr. Thomson, in the seventh edition of his System of Chemistry, published only within the last two or three days.

As to the salt which remains in solution, he describes my statement of its composition as another error, on the same grounds; forgetting also that in the page before, he quotes a passage from my Elements, where I state, *that its composition has not been accurately examined.* With regard to the nomenclature which he has objected to at the same time, I considered it better to employ the terms generally adopted. What would Mr. Phillips say were I now to review his article on Nitric Acid, in his translation of the Pharmacopœia, published in 1824, where he adheres to the established doctrines of the day, and assures us that he had confirmed them by his own experiments, and to charge him with a "tissue of errors," and culpable ignorance, because three years afterwards he considered that these experiments were inaccurate, and could not be depended on.

Such then is the extraordinary and very original mode of criticism invented by Mr. Phillips. HE ACTUALLY CHARGES ME WITH A TISSUE OF ERRORS, BECAUSE I HAVE NOT THE GIFT OF SECOND SIGHT, BECAUSE I DID NOT IN THE YEAR 1829 KNOW WHAT HE WAS TO DISCOVER IN THE YEAR 1831 ! ! !

From what I have now shewn the public will be able to judge, how far Mr. Phillips's Review and Letter are written in the spirit of fair and manly criticism; how far he has been able to vindicate himself in his answer to the charges I preferred against him, and what shifts he has had recourse to for this purpose.

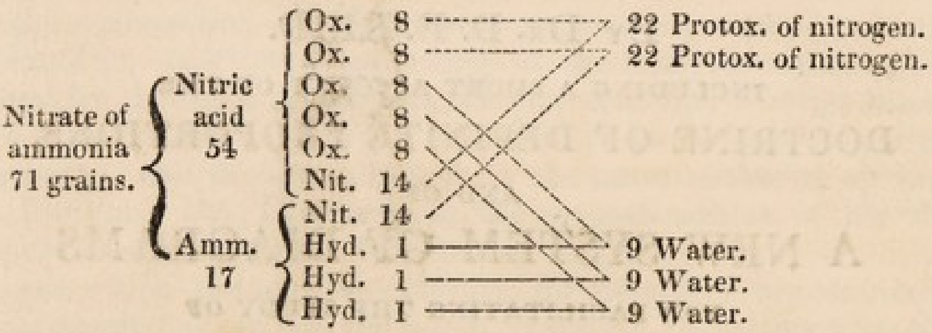
I have only farther to observe, that he has not even attempted to meet any of the direct charges of *misrepresentation* which I brought against him.

D. B. REID.

"That he stands equally high in our estimation as formerly, may be inferred from our recommending the present volume of practical instructions to the student of chemistry, and also to those who may wish to become practically acquainted with this all-engrossing and delightful branch of science."—*Jameson's New Philosophical Journal, July* 1830.

"In the whole work, Mr. Reid shows not only thorough acquaintance with every part of his subject, but a good deal of facility in the manner of explaining the different processes. To show how much order and precision he observes, it is merely requisite to say, that in all the experiments, the proportions of the different substances are stated according to the principle of chemical equivalents, or, in other words, upon the grounds now well established, that bodies combine in certain definite proportions. The changes effected by chemical action he illustrates very ingeniously by the use of diagrams, expressing the proportions in which the different ingredients exist before and after decomposition;—it does great credit to the ingenious author; it promises to be extremely useful to the student, who is desirous to possess more than speculative knowledge of the interesting science of which it treats."—*Edinburgh Medical and Surgical Journal, vol.* 33, *p.* 212.

"Besides the method of preparing each simple or compound substance, a number of experiments are described, showing its most important properties; and the double decompositions or changes, from elective affinity, are illustrated by very simple and ingenious diagrams, such as the following, which shows the changes which take place during the preparation of nitrous oxyd gas from nitrate of ammonia:



"The first part of the table (to the left) represents the elementary composition of the fifty-four parts of nitric acid and the seventeen of ammonia, existing in seventy-one parts of the nitrate, and the other shows the new arrangement which these enter into, and the compounds produced. The three proportions of hydrogen in the ammonia combine with three of oxygen from the nitric acid, and the remaining proportions of oxygen come away with the nitrogen, both of the nitric acid and the ammonia, in the form of nitrous oxide.

"In conclusion, we strongly recommend the work to our readers, as one containing much useful information, not to be found in the best systematic works on chemistry, to which it may form a very valuable accompaniment."—*Lancet, January* 1830.

"We have examined it with considerable attention, and hesitate not to pronounce it one of the very best practical guide-books to the experiments conducted in the chemical laboratory, that has yet been published. The methods of conducting the different chemical processes are fully described, and the theories of their actions explained, in a very clear and simple manner, by the aid of diagrams."—*Edinburgh Literary Journal, December* 1829.

AN INQUIRY

INTO

THE AVERAGE MORTALITY

IN

LITHOTOMY CASES;

WITH

A FEW REMARKS ON THE OPERATION
OF LITHOTRITY.

BY ALEXANDER MILLER,

FELLOW OF THE ROYAL COLLEGE OF SURGEONS OF EDINBURGH.

EDINBURGH:

PRINTED BY ANDREW SHORTREED,

MDCCCXXXI.

TO

ROBERT KNOX, ESQ.

LECTURER ON ANATOMY, EDINBURGH,

WITH GRATEFUL REMEMBRANCES FOR PAST FAVOURS,

THIS ESSAY

IS RESPECTFULLY INSCRIBED,

BY HIS FORMER PUPIL,

THE AUTHOR.

NOTICE.

I DEEM it necessary to state, that the following remarks were originally composed for the consideration of the President and Council of the Royal College of Surgeons of Edinburgh, when I was a candidate for admission into that body. For reasons best known to these gentlemen, they refused to accept this as a Probationary Essay, consequently I found it necessary to withdraw it altogether, and get up for them a few remarks on a different subject.

I take this opportunity of declaring, that this Essay is not now given to the public by way of advertisement,* nor with a view to raise or injure

* There are many ways of advertising in this world. One man attracts the notice of the public by affixing a placard at the corner of every street, whilst another man, equally respectable, courts notoriety, by calumnious attacks upon individual members of certain societies, more especially if these individuals possess any reputation.

the reputation of any individuals ; nor is there any personal feeling towards any party connected with its ~~their~~ publication ; but the sole object I have in view in submitting ~~them~~ it to the public is the same that first induced me to enter upon this subject, viz.—an attempt to arrive at the truth with regard to a most important surgical question.

AN INQUIRY, &c.

It can be said, with tolerable accuracy, that several centuries have elapsed since surgeons began to inquire into the best method of removing a stone from the urinary bladder, to effect which, with a certain degree of safety to the sufferer, almost every invention, and every method that human ingenuity could suggest, has been put in practice. Governments, even, have condescended to reward the discovery of improved methods of treatment in this afflicting disorder; and though they seldom notice the labours of the purely scientific or professional man, the discovery of a supposed solvent for calculus has not passed unrewarded. The attempts to break down stones in the bladder, without the use of cutting instruments, have already been judiciously noticed and encouraged; there is something, therefore, in the nature of this malady that has called forth the sympathies of mankind, and these sympathies seem to have extended to the operation itself when performed with cutting instruments. If cutting into the bladder, for the purpose of removing a calculus, were an easy, safe, and comparatively successful operation,* it may be asked, Whence this anxiety to avoid it? Deeper and more extensive incisions than those required in lithotomy, are daily made by surgeons into the human body, without either exciting in the mind of the patient, or of the public, any of that dread and anxiety which precede and follow the operation of lithotomy, however eminent the talents of the operator.

There are persons who affirm, that the average mortality in lithotomy cases, in their hands, is as one in forty-seven,† one in forty-two;‡ and there may possibly be some who give credit to such

* Sir JAMES EARLE, in his comments on Dr Austin's opinion with regard to the operation of lithotomy, says,—"It must be admitted that there are few (operations) so difficult as lithotomy, and that, unscientifically executed, it may be very dangerous; but I trust there are many of our profession capable of 'performing it dexterously;' and when skilfully performed, the almost certain success attending it is the best proof that it is not so dangerous as the author has represented it to be." See *Practical Observations on the Operation for the Stone*, p. 10.

† EARLE's *Practical Observations*, p. 95.

‡ MARTINEAU, *in Med. Chirurg. Trans.* vol. xi. p. 406.

affirmations. There are other surgeons somewhat more moderate in their pretensions, who admit that they have lost one in forty,* and even one in fifteen;† and others, still more moderate, have admitted that they have lost one in ten and a-half,‡ and one in eight.§ In my estimation, the latter computation, however low in comparison, is not worthy of credence; and I think, that professional persons should not suffer themselves to be persuaded that so favourable an average as even the lowest of the above has ever been obtained, at least in any considerable number of cases. Considering the importance of the subject, I have thought it one which might be submitted in the shape of an inquiry to this very learned body, (the Royal College of Surgeons,) even though it were only expressing my doubts as to these reported successes—doubts which, I fear, I entertain in common with most men, whether professional or not.

* GREEN, in *Lancet*, vol. i. p. 61. for 1827–28.

† LISTON, in *Edinburgh Med. and Surg. Journ.* vol. xxix. p. 236.

‡ CHESELDEN'S *Anatomy*, p. 332.

§ CRIGHTON, in *Edinburgh Med. and Surg. Journ.* vol. xxix. p. 230. Mr Hodson, of Lewes, in Sussex, is stated by the editor of the *Lancet* to have operated on thirty-four cases, losing but one; but, as to particulars, no mention is made.

The present inquiry, then, has a reference almost solely to the result of the operation, as practised by surgeons with cutting instruments for the removing of a calculus from the urinary bladder, and to the average success attending such an operation. I shall take the liberty to add a few words regarding the success of lithontritic cases.

My utmost efforts will be used to avoid every thing like harsh criticism; and I trust, therefore, that this learned body will do me the justice to believe, that the documents and materials I have examined in the composition of this Essay, have been reviewed by me in an impartial manner, and that it originates in no unfriendly feeling towards the profession, nor even towards the professed lithotomist, that I thus endeavour to reduce this imaginary success to its real value, and justify, as far as lies in my power, the opinions of those, who, with me, (for many there must be of similar opinions,) consider lithotomy and lithotrity hazardous and dangerous operations, and fatal to an extent of which even the public are not aware.

It is reported of Hippocrates, that he required of his pupils to abstain from the practice of lithotomy; from whence it is probable that, even in those early times, there were professed lithotomists, and from that period to the present, the lithotomist, in some measure, still holds his ground,—at times, itinerant and strictly empirical, and, though brought within the pale of the profession, always affecting mystery and concealment of method; and, above all, persevering in endeavours to prove that his operations are uniformly successful. In such instances, if a fatal case does force its way into public notice, the misfortune is denied to have had any connection with the operation.* Every surgeon endeavours, to the best of his power, to explain, in a manner favourable to himself, the cause of death in such cases as may turn out unfortunate in his hands; and this is a privilege to which he is fully entitled; for it is reasonable that such explanations should be given as frequently as possible. Although it be true that, occasionally, explanations have been given of the cause of death in lithotomy cases, which set all reasoning at defiance, and would be irresistibly ludicrous, were it possible to lose one's gravity in so serious a discussion;† however

* See Sir J. EARLE, at p. 97 of his *Practical Observations;* and a number of cases recorded by Mr SYME in the *Edinburgh Medical and Surgical Journal,* to be noticed more particularly afterwards.

† See SYME, *Edinburgh Medical and Surgical Journal,* vol. xxxv. p. 248.

trivial the reasons may be, which are sometimes given with respect to the fatality of such cases in the reports of operations, this course is infinitely preferable to their total omission, of which fault, I fear, it will not be easy altogether to exonerate the lithotomists of this or of any other day.

When we consider the sources of information regarding the average fatality in lithotomy cases, we readily enough discover that these are two, viz. the result of private and of public practice. In so important a matter as this, it is with regret and reluctance that I feel it necessary to decline putting any reliance upon the documents furnished by the private practitioner, as being the result of his private practice; neither shall I enter here upon the reasons that have determined me to do so, as they will no doubt suggest themselves in abundance to the reader of the present Essay. Your most excellent fellow-member, the late Dr Brown, in his admirable critical inquiry into the efficacy of physic and of physicians in shortening the duration of fever, admitted, if I remember right, into that inquiry the records of public practice only, wisely calculating that such documents alone could afford correct materials for drawing legitimate conclusions. Confining myself, therefore, altogether, or nearly so, to hospitals, and the reports made by hospital surgeons, I find, that of the surgeons of whose operations we have any thing like a tolerably accurate statement, the first is Cheselden, who, at St Thomas's Hospital, performed the operation two hundred and thirteen times.* Of this large number twenty died; but we are at the same time informed, that several more died of smallpox; and these deaths from smallpox, whose number he does not think fit to mention, were, in all probability, numerous, and have been set down by him to the successful side of the average; and yet it must be obvious to every one, that an average only of the success in these cases should have been placed on the successful side, and an average mortality on the opposite. We may with safety, then, I think, reduce the average success in Cheselden's cases to somewhat less than one in ten and a half, the average which he himself has given. But though I have, in accordance with all medical writers, spoken of the history of Cheselden's cases as given by himself, I do not think it proper, giving to the whole subject that cautious examination which it requires, to admit as a document, entitled to a full consideration, statements made by surgeons

* CHESELDEN'S *Anatomy*, p. 332.

of hospitals in which proper records were never kept. It must be known to every one, so far at least as I am aware, that authentic, strict, and official records of cases have never been kept in any London hospital. I should, nevertheless, feel happy to be corrected if in error on this point. I have made strict inquiry of gentlemen who have been educated in these hospitals, and find from them that no such records are ever kept; and that no official document could be produced, as to a matter of this kind, by any London hospital. It is admitted by Cheselden, that, previous to operating in these two hundred and thirteen cases, he had lost four in ten,* and, in the high operation, one in seven, exclusive of two in which he cut into the general cavity of the peritoneum: the result of this peculiar style of operating has not been mentioned. Making all allowance for the quaintness of the style peculiar to the times in which Cheselden wrote, it must be admitted that his statements are not strictly professional, and that his anxiety to be thought the most successful lithotomist of his day, might have induced him to exaggerate, and disregard precise accuracy. Speaking of his success, he says, "What the

* CHESELDEN'S *Anatomy*, p. 329.

success of the several operators was, I will not take the liberty to publish; but, for my own, exclusive of the two before mentioned, I have lost no more than one in seven, *which is more than any one else, that I know of, could say;* whereas, in the old way, even at Paris, from a fair calculation of above eight hundred patients, it appears near two in five died." After a statement of this kind, I hope I shall not be exceeding the bounds of fair criticism, if, setting aside his pretended averages, we take the average fatality, even in his hands, to have been similar to that of the Parisian surgeons, two in five, or nearly so.

Mr Smith of Bristol, in his *Statistical Inquiry into the frequency of Stone in the Bladder in Great Britain and Ireland,** furnishes us with a tabular statement, shewing the fatality of lithotomy cases at the Bristol Infirmary, which, although the earliest established provincial hospital in England, appears to be the one that has contributed most to science, by having kept more correct registers than any other yet established. This report merits our most implicit confidence, not only on this account, but also on account of its having been published, not with the view of

* See *Medico-Chir. Transactions of London,* vol. xi. p. 7.

making any one surgeon appear the greatest lithotomist of his age, but solely for the purpose of presenting the world with an authentic statement of the success that has attended the operation in that hospital since its foundation. From that report we learn, that three hundred and fifty-four patients have been operated upon, of whom seventy-nine have died, making an average of one in four and a half. In the same paper, we find a letter from Mr Barnes, of the Devon Hospital, in which is given a table, to shew the average proportion of stone cases cured, to the whole patients admitted into the hospital; we are, moreover, told, that the latter part of this return, namely, for the last seven years, may be considered quite correct—an admission that the first part is not to be depended on, that nine patients have been admitted, and all dismissed cured. Any one, I trust, who reads this statement carefully, will agree with me, that all farther notice of it is unnecessary. We do not wish to know how many cases of lithotomy passed through the hands of this or that surgeon; but, in the pursuit of a surgical question of great importance, we desire to know, how many were admitted into the Devon Hospital, and how many Mr Barnes did not cure.

From Mr Smith's paper we also learn, that at the Birmingham Infirmary, during a period of thirty-eight years, seventy-two operations were performed, five of which occurred in females; and, of the whole, fifty-nine were cured, and thirteen died. Now, this gives us one in five, after subtracting the five female cases from the whole number.

In the same paper, there is a communication from Mr Oakes, of Cambridge, where we are told, not more than one in twenty died from the operation; but then, again, we are inclined to suspect the state of the hospital records, as we find Mr Oakes, in the beginning of his letter, stating, that, as far as he *recollects*, the average number, both in public and private practice, is rather less than four annually.

Mr Smith's paper also furnishes us with a correct copy of the surgeon's books at the Leeds Infirmary. Here the report appears to be official, and, during fifty years, one hundred and ninety-seven operations were performed, in twenty-eight of which the patient died, giving an average of one in seven. This is all the information Mr Smith's excellent paper affords that bears on our subject, with the exception of Norwich, and the result is, that at

Bristol, the deaths are .	1 in $4\frac{1}{2}$
Birmingham, ...	1 in 5
Leeds, ...	1 in 7

According to Dr Marcet's* return of the cases operated on at the Norwich hospital, from the year 1772 to 1816, four hundred and seventy-eight males were cut, sixty-eight of whom died, making an average of one in 7.22. Mr Martineau,† of that place, has also published a list of cases operated on by himself, from the year 1804 to 1820, inclusive, amounting to eighty-four, ten of which occurred in private practice; and out of this number, he says, only two died; wishing the reader to believe that this was the result of the success that attended his practice. On inquiring into the cause of this apparently most unaccountable success, we find that Martineau has followed very much in the footsteps of Cheselden, and only given the result of a certain number of years' experience, and not that of his whole professional career: of this we are convinced, first, from an expression of his own, where we find him stating, " that during the first years

* See Marcet *on Calculous Disorders.*

† *Medico-Chir. Transactions*, vol. xi. p. 402.

of my practice I was not very successful;" in the next place, from the return above mentioned, by Dr Marcet, which must have included all the cases of Martineau, except those that occurred during the four last years of his report, the average mortality was one in 7.22. Now, from this, if we admit Mr Martineau's statement to be correct, we must conclude that his predecessors must have been very unsuccessful, and that it was a very hard case for him, that his merits as a successful lithotomist should be so curtailed by the result of his practice being combined with that of other surgeons so much his inferiors, and his success, by this means, made to appear to be one death in 7.22, instead, as he has given it out to be, one in forty-two. These must, no doubt, have been the reasons that induced Mr Martineau to publish his own account of this matter; but upon this point we are put right by Dr Yellowley, who never published any report that could not bear the most impartial scrutiny. In a note to his valuable paper in the *Philosophical Transactions, for* 1830, we are told by that candid physician, that the whole number of Mr Martineau's cases amounted to one hundred and forty-seven, with seventeen deaths out of that number, making the average one in 8.11. Since we find a surgeon so far wishing to impose on the profession, by publishing a partial account of this kind, of his hospital practice, what reliance is to be placed on the relation of the ten cases that occurred in his private practice, all of which, we are told, were successful. Here, then, we find the average mortality of Mr Martineau's cases rise from one in forty-two to one in 8.11. But to return to Dr Yellowley's statement, we find from it that six hundred and eighteen males have been cut in the Norwich hospital, eighty-seven of whom have died, giving an average of one in 7.1, which agrees to a fraction with the average given by Dr Marcet, who, as will be seen above, makes it one in 7.22.

Mr Crighton,* of Dundee, has published a list of seventy patients operated on by him, nine of which died, making an average of one in eight; but we are led to believe that Mr Crighton has performed the operation a hundred times, from what he himself states, and also from a statement made in Mr C. Hutchison's paper in the 16th volume of the *Medico-Chirurgical Transactions of London*; but from Mr Crighton not having kept an account of his cases, more especially those that

* See *Edinburgh Med. and Surg. Journal*, vol. xxi. p. 226.

occurred in the early part of his practice, and as we do not admit as evidence into this inquiry any thing that is derived solely from memory, I do not feel disposed to include Mr Crighton's cases in this paper.

Mr Liston, of this place, has favoured the profession with the result of his practice, by means of a tabular view,* containing twenty-nine cases of lithotomy; to which he has added, explanatory memoranda, and notes, with reference to some peculiarities in the after treatment, or to the cause of death in the few fatal cases that occurred, these being only two. As the operative part of surgery cannot be carried to a higher degree of excellence by any one, it may, I think, be presumed, that the average success of Mr Liston must be taken as the very highest standard of success in the profession, with a reference, of course, to lithotomy cases. Wheresoever a higher average has been supposed to have been effected, it is reasonable to presume that the mode of operation must have been quite peculiar to the individual; and in order to secure credence to, and confidence in, his statements, it seems but fair, that any greater success than that claimed by

* See *Edinburgh Med. and Surg. Journal*, vol. xxi. p. 236.

Mr Liston, must and ought to be supported and substantiated by explanatory statements of those peculiarities to which I have alluded.* But to return:—The average success seemingly claimed by Mr Liston, is as fourteen to one, being nearly the double of that claimed by any authentic statement on record. To the accuracy of this high average and great success, I beg leave to offer the following objections:—1st, The cases are mostly those of private, not of public practice. 2d, The very different and discordant views which may be taken by different private practitioners of the same case. I shall cite the following as an instance:—Mr Liston operated on Mr J. M.;† the case proved unsuccessful; but Mr Liston argues, that this could not be reckoned a death from the operation, since, as he observes, "the patient died suddenly apoplectic fourteen days after the operation; the urine had resumed its

* We believe Mr Liston does not select his cases of lithotomy, by declining, as Mr Green has done, unfavourable ones. Were surgeons to do this in every disease incurable by other means than an operation, (to which class stone in the bladder belongs,) the practice of surgery would, no doubt, become eminently successful, but would cease to be extensively useful to humanity: we should, in such cases, have to commend the judgment of the surgeon; but severely censure his want of feeling.

† See *Edinburgh Med. and Surg. Journal*, vol. xxix. p. 238.

former course." The actual facts connected with this fatal case of lithotomy differ so widely from this statement, that we do not hesitate to affirm, that Mr Liston must have been very seriously misinformed, and his judgment and confidence altogether abused. " Mr M. *never had the slightest apoplectic symptoms*—he gradually sunk from the moment of the operation; the wound shewed no disposition to heal, *and the urine did not come by the natural passage;* he was watched with the utmost care, and every attention bestowed on him by the surgeon in attendance, and he died exhausted on the fourteenth day. The examination after death shewed extensive inflammation of the bowels, and the contents of the pelvis adhered every where."

The cases I shall next notice, are those published in the *Reports of the Edinburgh Surgical Hospital.** There we find mention made of thirteen cases. Of these thirteen cases recorded, six deaths took place, or an average mortality of nearly one-half. Mr Syme has his own peculiar views of this operation, but it is proper and right to observe here, that these views were

* See *Edinburgh Med. and Surg. Journal,* vol. xxxii. and following. There are several jobs in Edinburgh, as in all large towns, called hospitals.

adopted, and the following statement made, before the occurrence in his hands of what might almost be deemed a sort of catastrophe in surgical practice. " The operation of lithotomy," says Mr Syme, " as now performed, is one of the *simplest* in surgery; and the importance which is still attributed to it by the public, depends upon the recollection of the shocking and protracted tortures which attended the old method of operating with the gorget. The patient above mentioned is the only one I ever lost * from the operation, and his death may, I think, be ascribed fully as much to old age as to the injury inflicted." Besides these fatal cases, being six out of thirteen, there is a very singular instance recorded, (or, as Mr Syme himself terms it, " a very unusual case in several respects,") of the bladder having been opened as in lithotomy, and one calculus removed, another having been left in the bladder; the patient was shortly afterwards operated on by Mr Liston, and ultimately recovered from this double lithotomy. In the first notice of the case, which appears in the

* Mr Syme has since lost five out of ten; the tortures Mr Syme speaks of as occurring from the old method of operating with the gorget, have been much abreviated, no doubt, in his own hands, and in those of some modern surgeons; and in some respects, this is an advantage, since a speedy relief from suffering is what is greatly desired by all.

fifth hospital report, under the article *Lithotomy,* as a successful case of that operation, we are told it occurred in the private practice of the narrator, in the son of an artist ten years of age, and " that it was very unusual in several respects." " On attempting," says the lithotomist, " to introduce a sound proportioned to the usual size of the urethra at this age, I met with an obstruction about three inches from the orifice, which required a great deal of pressure to admit the entrance of a small instrument. I felt a calculus in the *neck of the bladder,* or rather anterior to it; and putting my finger in the rectum, ascertained that this was really its situation. I performed the operation next day in the ordinary way," * (I presume the ordinary way of performing the operation of lithotomy,) " and extracted a mass of calcareous matter about the size of a walnut, which seemed to have originally consisted of two nearly equal concretions." In the altered and amended statement made in the sixth report † we find, under the head, *Urinary Calculus,* the following relation,—" In last report, I mentioned a case of stone that

* See *Edinburgh Medical and Surgical Journal,* vol. xxxiv. p. 239.

† Ibid. vol. xxxv. p. 245.

had occurred in private practice, in which I extracted *two large stones* * from a sac formed by dilatation of the membranous part of the urethra." From the time of the first report to that of the second, the stone of the size of a walnut, situated originally in the *neck* of the bladder, or *immediately anterior* to it, had grown into *two large stones*, situated in a *sac* formed by *dilatation* of the *membranous part* of the *urethra.* When the patient came to be operated on by another surgeon, Mr Liston, some ignorant persons asserted heedlessly, and without having read with sufficient attention Mr Syme's first report, that the operator had neglected to examine the bladder which he had just cut into, and thus, by mistake, subjected a human life to very great risk, and to the torture of a second painful and much dreaded operation. But Mr Syme remarks, that "he then examined the bladder by a sound, and ascertained that it contained no other calculus." † Notwithstanding this examination, we would venture to suggest, that another calculus must have been present at the time, since Mr Syme himself admits, that "after

* The first report speaks only of one.

† See *Edinburgh Medical and Surgical Journal*, vol. xxxiv. pp. 239, 40.

the urine ceased to flow through the wound, which happened about the end of a fortnight, when he began to complain nearly as much as before." In the remarks which follow, I fear I misunderstand Mr Syme. He cautions the reader that, in removing stones from the situation that this one occupied, the surgeon ought to ascertain whether or not there are any in the bladder. Mr Syme next goes on to state,—" For though it may be of *little consequence* to the patient, in respect to pain or danger, whether they be taken away together or separately," (by which, I presume, is meant, whether you make one or two operations to cure the disease,) " he will, in the latter case, have all the horrors of two operations, and be ready to listen to any suggestions calling the skill of the surgeon into question ;" * and he will have good cause so to do. I cannot here venture, from a want of experience, to decide on the correctness or incorrectness of the opinions which, hastily viewed, might be considered as advocating the doctrine of the " deux temps" operation, but more complete in its nature than that originally contemplated, being performed, not only at two times, but in

* See *Edinburgh Medical and Surgical Journal,* vol. xxxv. p. 246.

two different places, and by two different operators. Good surgeons have, so far as I recollect, always been averse to any delay in completing the operation; and, on the bladder being opened, it has usually been deemed advisable to remove all the calculi which it at the time contained.

The history of the case has, perhaps, been given from recollection by Mr Syme, since there are points in it which cannot be reconciled. It is stated, for example, at first, that the symptoms were those which usually attend stone in the bladder; that the patient had complained for five years; and that, latterly, they had confined him to the house in the greatest misery.* In another place, we find the following remark:—"Another important fact which it (this case) illustrates, is the comparatively small uneasiness generally occasioned by stone in the bladder in young subjects: this boy, after the concretions were removed from the *urethra*, walked about as usual, slept undisturbed during the night, and made no complaint, except when he voided his urine. It was this extreme *mildness* of his symptoms that led me to attribute them entirely to the stricture."†

* See *Edinburgh Medical and Surgical Journal*, vol. xxxiv. p. 239.

† Ibid. vol. xxxv. p. 246.

Perhaps it would be proper to set aside this case altogether, as being a case of stone, neither in the bladder nor out of the bladder. If it must be considered ultimately as a lithotomy case, we presume Mr Liston may be allowed to insert it as one of his in the next tabular view with which that distinguished operator may favour the public; and this will allow for Mr Syme the respectable average mortality in his lithotomy cases, of six in twelve, or one in two.

It is a laudable and excellent inquiry on the part of the surgeon to examine most carefully into the causes of death after lithotomy; and this part of the inquiry has not been neglected by Mr Syme, his opportunities for doing so having been very considerable. The causes of death in these six fatal cases, (their authenticity, and the authenticity of the average success as taking place in a public hospital, in the hands of distinguished surgeons, rendering them extremely valuable,) merit particular attention.

In the first of these fatal cases Mr Syme says, "The patient above mentioned is the only one I ever lost from the operation; and his death, I think, may be ascribed fully as much to *old age* as the injury inflicted." I hope we may be permitted to add, in the mean time, that the patient died of the immediate effects of the operation, since it is not any where stated that he was moribund when brought into hospital, nor when put on the operating table. *Very old* persons do frequently die of severe and extensive injuries, from which those who are younger occasionally recover. In the second case, Mr Syme does not give any opinion as to the cause of death; but we find him stating,—"We found, on dissection, a diffused suppuration in the cellular substance, exterior to the left side of the bladder;" and he should have added, the patient died of extensive inflammation, followed by abscesses within the pelvis, the immediate effects of the operation. In the third case, no explanation of the cause of death is given; but we again find it stated, that, "on dissection, there was not the slightest trace of disease in the cavities either of the abdomen or pelvis, neither was there any appearance of inflammation in the bladder; and the only part that seemed to suffer from disease was the prostate, which was greatly enlarged throughout, but especially upwards towards the cavity of the bladder." Such cases are by no means uncommon; a similar one happened in the Royal Infirmary during the autumn of 1828. The patient, a healthy, middle aged man, was operated upon by

Mr Liston, with his accustomed dexterity, and the stone, which was of a moderate size, removed in a minute from the commencement of the operation; yet the patient, who seemed to do well for a few days, began shortly afterwards to decline in health; slight fever arose, referrible to no distinct cause, and he died, to the best of my recollection, ten or twelve days from the time of the operation. Dissection shewed no disease within the pelvis; the wound in the prostate had never closed, and its edges were slightly greenish; no cause for death could be discovered any where. Such cases are well known to the profession, and they are uniformly set down as cases in which death immediately results from the operation; they are the most provoking of all to the surgeon, since they seem to be connected with some peculiarity of constitution.

In the fourth fatal case, death is accounted for in the following manner:—" The fatal result in this case, may, I think with most probability, be ascribed to the effect of suddenly removing a source of extreme irritation, in a very irritable system. In ordinary cases of stone, this diminution of irritation constitutes the patient's safety by counterbalancing the irritating tendency of the operation; but the irritation in this instance, being of extraordinary intensity, while the operation, from the small size of the stone, was *gently* and *easily* performed; it is conceivable that the actions of the system might, from the cause alleged, fall into disorder, and produce the results that have been described." We have seen Mr Liston and other surgeons remove a calculus from the bladder, by two incisions, and at one grasp of the forceps, and the operation might have occupied a minute, or two at the most. One would be inclined to call such an operation, (if any operation could be so denominated,) an elegant and easy operation;* and if operations of this kind were to prove fatal, owing to the rapidity with which the patient was at once relieved from the calculus, and from all his sufferings and anxiety, we are at a loss to imagine how any of Mr Liston's patients, or those of Mr Green, could possibly escape; but we beg leave to remind Mr Syme, that the operation in question, in which he imagines he lost his patient in consequence of

* Since this was written, I have again witnessed Mr Liston perform this operation, and feel bound to repeat, that if any operation can be called elegant, it is this one, as performed by this surgeon; in so far as an unbiassed judgment enables me to venture an opinion, Mr Liston has no equal amongst living operators.

too sudden relief from suffering, was not precisely an operation of the kind I have been speaking of. Two if not three incisions were made into the bladder, the stone having been found to have been too large to be removed by the first incision; at the moment of performing the second or third incisions, the forceps which grasped the calculus were left in the bladder, and instead of being given to an assistant, were allowed to hang dangling down, in front of the perineum, and that to many gentlemen present, of great candour and honour, the operation did not seem a gentle one, nor performed with great ease to the patient. But I do not by this in any way deny, that, compared with some which I have seen and heard of, it may have been in this view easy and pleasant, both to the surgeon and patient.*

* The cases more particularly referred to here are, first, the one recorded by Dr John Thomson in his appendix to a proposal for a new method of cutting for the stone, where this gentleman and his assistant, after being completely exhausted poking in the patient's bladder, put him to bed for five days, when it was "conceived that we might safely repeat *our search* for the stone." Dr T. having called together a number of his friends to witness the second attempt, goes on to state, that "after various trials, in vain, to touch the stone with the finger, I at last felt it with the forceps, and endeavoured to seize it. I had hold of it three several times with the points of the forceps; but on each attempt at extraction, it escaped from

In the fifth fatal case, it was at first conjectured by Mr Syme, that " the patient had died in con-

between the blades. Overcome with fatigue, and feeling myself at that moment beginning to be agitated with emotions of anxiety, I put the forceps into the hands of Dr Brown, and requested he would have the goodness to extract the stone for me. He did so, after some difficulty in seizing it," &c. This patient recovered, and might be brought forward by Mr Syme to support his doctrine. I will just inquire, if none of the bystanders suggested the employment of a hornspoon on such a trying occasion, for we have been informed that, aided with this useful instrument, some celebrated lithotomists have succeeded in extracting the stone, under similar circumstances, after all other means had failed! Second, the cases recorded by Mr Fletcher, in his *Medico Chirurgical Notes and Illustrations*, a quotation from one of which I shall here give: " A great deal of force was immediately applied, and that not in the best direction, but to no purpose; the stone would not pass; the operator rested; the patient was calm, and complained not; the labours of the former now recommenced with redoubled vigour, and an air which imported a dreadful determination to succeed. His right foot was placed in preparation for this awful struggle against a chair, which was supported by a pupil; the scene became animated, though horrible; the straining and creaking of the forceps, as they occasionally lifted the suffering wretch from the table, (they twice lifted him off it,) his wild agonizing shrieks and entreaties for forbearance, after continuing nearly two hours, gradually became more faint, and sunk at last into a piteous moan; and when the stone was shewn him, it was doubtful whether he saw it, or was even conscious, that a period had at length arrived to his sufferings. He expired a few minutes after being carried to bed. The body was not examined."

sequence of peritoneal inflammation, excited by the loss of a great quantity of blood during the operation :" there is no mention made of the probable cause of death in this case in the published report of it ; but we find it stated, " that on dissection the peritoneum was found more red than usual, and at some points small spots of extravasated blood were perceptible," and that a large anomalous artery was cut across as in Mr Shaw's case.

The sixth and last of Mr Syme's fatal cases is thus recorded, " The result of the following case, though operated upon under less promising circumstances, was more fortunate." The patient, aged eighty, was relieved of twenty-three stones on the 15th May, not without difficulty, owing to the great capacity of his bladder : the report goes on to state, that " he never had a bad symptom from the operation, but was for several weeks so extremely feeble, that great apprehension was felt of his sinking ; the wound is not entirely healed yet, but he is, and has been all along, free from pain." What is meant by the expression " was more fortunate," I am quite at a loss to understand, unless it is, that this patient had the good luck not to die on the day after the operation, as his predecessor did, but to die after lingering for some weeks : we are also told, that " the wound is not entirely healed yet;" and, it may be added, that the wound never healed, but remained open until the death of this "more fortunate" patient, which event *had taken place*, at the time that the proof sheets containing this report passed through the hands of their author. Mr Syme, with his usual candour, takes no notice of the event: the reader is led into the belief that this was a successful case, the operator being at the same time aware that the patient was dead.*

There is a passage in the *Practical Observations on the Operation for the Stone*, by Sir James Earle, which, as often as I read it, excites my

* I am borne out in this assertion by what has been published in the *Lancet* since this was written. In reviewing these cases, it is stated, "The second case (alluding to this one) was *successful*, although the patient was eighty years of age," &c. This case illustrates well "Mr Syme's *indefatigable exertions and talents*, as well in the furtherance of this excellent institution as in the *cultivation* and *improvement* of chirurgical knowledge." The reader of these remarks is requested to contrast the above laudatory critique of the *Lancet* with the atrocious and unfounded calumnies published by the editor of the same Journal, when a case similar to Mr Syme's fifth fatal one happened in the hands of Mr John Shaw of London. The reader is requested to contrast the observations, and then to consider what epithet in all truth could be applied to the editor of the Journal in question.

astonishment and admiration. That distinguished surgeon says, "That his first operation for lithotomy was performed in 1770; that he recollects of forty-seven cases in which he operated, which all did well excepting one, which died; and, as there were peculiarities in the case of that person, in justice to the operation they should be noticed." Here Sir James expressly states, that "he conceives the loss of this patient cannot fairly be ascribed to the operation," notwithstanding this event took place on the fourth day after the operation, and the "bladder, on examination after death, was found thickened and diseased, bearing evident marks of continued inflammation." Sir James would wish the reader to believe, that the death was caused by the lithontriptic medicines that were used previous to the operation, and gravely remarks, that "this case leads me seriously to recommend not to perform the operation of lithotomy in less than a month from leaving off a course of what are called lithontriptic medicines." Might not the peculiarities in this fatal case have been, perhaps, connected with a too sudden relief to the patient, by removal of the calculus, owing to the operation having been performed in too easy, gentle, and simple a manner?* For it would seem agreeable to the opinion of

* See quotation from *Edin. Med. and Surgical Journal*, at page 28.

some surgeons, that a certain quantity of delay, and consequently of torture, must be inflicted on the patient undergoing the operation of lithotomy, to render that operation safe. Now, here arises a nice question for the practical surgeon, as to the maximum and minimum of suffering which a patient ought to undergo, in order to have the operation of lithotomy safely performed, the time taken in its performance ranging from two minutes, to somewhat more than double as many days;* and to graduate nicely, according to the constitution of his patient, what quantity of suffering ought to be inflicted on him. When a limb is amputated, or an extensive tumor, situated amidst dangerous parts, has been removed, and the patient gradually sinks from the time of the operation, and dies after a period varying from one to ten or fourteen days, we say he has died of the shock of the operation; and though this language does not convey any very precise idea to the mind, still it is generally understood, and generally employed; and were I, in relating a case in which a limb had been amputated for some painful and distressing affection—such as, for example, happens in those kinds of white swelling, wherein the patient, worn out, irritated, and exhausted for want of sleep, borne down with the anxiety of carrying about with

* See *Edin. Med. and Surgical Journal*, vol. xxxiv. p. 12.

him an incurable malady, and harassed with unceasing pain, at last consents to the removal of the limb—should it happen that this patient, instead of gradually and steadily recovering from the operation, as so generally happens in all chronic cases, gradually sinks and dies, the wound never closing, nor any symptoms appearing to indicate he had, in any way, recovered from the shock of the operation,—would any surgeon in the world believe me, or would it be credited that I believed myself, in the statement made, that the person died, not from the immediate consequence of the operation performed upon him, but from my having improperly adopted the double flap operation, whereby the time occupied in its performance came to be only about four minutes, whereas, had I adopted the good old method, which, young as I am in the profession, I have often witnessed, protracting the operation to forty or fifty minutes, my patient might easily have recovered?—But to return to Sir James Earle and his extraordinary success: This distinguished surgeon says, he has " an account of forty-seven cases, but the total amount, unfortunately, I have no means of ascertaining; for, in the earliest part of my practice in St Bartholomew's, not foreseeing that one day I should wish to recollect them, I was not attentive to make memoranda of every case that occurred." Now, in what London hospital, I repeat, have public documents ever been kept of fatal or successful cases? There is a part in the history of Sir James Earle's success, which we do not well understand: that gentleman was still operating in 1814 and 1815, and had for colleagues Sir Gilbert Blane and Sir Ludford Harvey. But we have been assured, on good authority, that, during that session, there did not occur a single lithotomy case.

To Mr Green's great success I have two objections to offer, the mere statement of which will, I think, be sufficient to set aside that surgeon's average. In the first place, Mr Green selected his patients; and, secondly, Mr Green can have no authentic documents to substantiate his statement; for we find, in Mr C. Hutchison's *Inquiry into the frequency of Stone in the Bladder in Seafaring People*, "That Mr Green, at St Thomas's Hospital, where *there is no official register kept*, states, that the *sister* (hospital nurse) who attends all the lithotomy cases, *says, she thinks* a young man about twenty was of the seafaring line."*

* See *Medico-Chirurgical Transactions*, vol. xvi. We should like much to see a military or naval surgeon, in the service of any European government, reply to a query put to him by his government, as to the mortality or success in his operations, by returning an answer founded on the *recollection* of nurses and hospital servants.

LITHOTRITY.

LITHOTRITY is the name given to that operation, by means of which stones are so broken down within the bladder, that they may be extracted through the natural passages; thus avoiding the use of cutting instruments, and the necessity of incisions into the bladder.

This operation has attained a high degree of perfection in France, in the hands of Civiale and Leroy. The operation has also been practised by some British surgeons; some itinerant foreigners have likewise exhibited the operation in British hospitals.

It is natural for these men, claiming as they do the merit of discoverers, not only to hold up the fair side of the question to the public, but to endeavour, in as far as they can, to throw lithotomy into the shade, and to induce the substitution of their supposed improved operation for it. In considering the merits and demerits of the lithontritic operation, we encounter, again, the same attempts that we have seen made by the lithotomists, to prove their operation to be simple, and of easy execution, causing no great suffering to the patient; and, above all, successful. That they have not hesitated to exaggerate success and palliate failures, and even, with great effrontery, to falsify reports, which the public hastily were led to believe official, is what, I am sorry to say, has happened almost under my own observation. But, least I should be supposed personally prejudiced in this matter, and that I may have, without due consideration, extended to the lithotritist my disbelief in the statements, and my want of faith in the professional character of the lithotomist, I shall take the liberty of supporting what I have to say, on the authority of a distinguished foreign surgeon, who, residing in the French capital, must have had daily opportunities of verifying the cases of the lithotritist, and who, moreover, was delegated to inquire officially into the correctness of the statements made by these persons to the Institute of France.

What I myself have seen has impressed me with an exceedingly unfavourable opinion of lithotrity. From what I witnessed of this operation in the Parisian hospitals, during the greater part of the year 1829, in the hands of Leroy and Civiale themselves, I had come to the conclusion, that it was in no way preferable to the operation of lithotomy; for, although I did not see so many deaths in the one case as I had previously witnessed in the other, still, taking into account the length of time the patient was exposed to suffering of the most severe description, at each application of the instrument, (for every one must be aware that it cannot be finished at one attempt, but requires sometimes so many as seven or eight,) the violent attacks of inflammation that follow it, and the weakened and exhausted state in which these attacks too frequently left the patient, so as often to render his dismissal from the hospital absolutely necessary; and even sometimes causing death itself. For these reasons, and the appearances that I witnessed, on dissection, of two fatal cases, and the result of many others, I was thoroughly convinced that it was a painful, very uncertain, and too often fatal operation. In neither of the fatal cases witnessed by me was the stone found to have been entirely removed from the bladder, and, I may here add, that I never was convinced that a single patient was dismissed cured by this operation. The first of these fatal cases occurred in the hands of Civiale, on a patient about thirty years of age, who died on the second day after the first attempt to break down the stone.* On

* And yet Civiale *selects* his patients with the greatest care.

dissection, the bladder was found to contain a pretty large stone, which had been seized with the forceps, and slightly touched with the perforator; the mucous coat of this organ was highly vascular, and in some places reduced to a pulpy mass.

The second case, which occurred in the hands of Leroy, was the case that is considered by every one to be most favourable for the operation, namely, where the stone is small, and the disease of short duration. The patient, a peasant, during the summer of 1829, had been amusing himself by passing a straw into his urethra, a portion of which remained in the membranous part of it; this, on an attempt being made to extract it, was forced into the bladder; there it remained for about three months, during which time a calcareous deposit had formed around it. On the first and second attempts to remove this by means of lithotrity, the patient suffered excruciating agony, followed by severe attacks of inflammation of the bladder, and neighbouring parts,* so that, when

* We find it stated by the reviewers of FLETCHER's *Medical Chirurgical Notes and Illustrations*, in the last number of the *Edinburgh Medical and Surgical Journal*, that "it is a singular fact, that, in the cases operated on in Paris by Civiale, so little irritation seems to be created, that the patients have often walked home afterwards; and in *none* of the cases have any alarming symptoms of inflammation taken place." I have not the smallest

placed on the table for the third time, no one could have believed it to have been the same individual that lay there six weeks previously, had he not been in the habit of seeing him from day to day; for, instead of the fine, healthy, robust peasant, we beheld a man worn out by disease, and hectic. Under these circumstances, who can wonder that he never rallied after the third attempt, but gradually sunk, twenty-four hours after the operation. I may add, that, at each attempt, small pieces of the calculus were removed, and even some fragments of straw. On dissection in this case, the bladder was found highly inflamed, in many places ulcerated, and contained the remains of the calcareous deposit surrounding the straw.

With reference to the opinions of Baron Larrey, we find them embodied in the following report, an extract of which I shall here quote as we find it given in a French journal:—" M. Larrey commenced, by presenting the academy with a succinct analysis of the memoir laid before them, by M. Civiale. In one of the cases operated on

doubt of this being Civiale's statement; its repetition, however, can only be made by one totally innocent of the slightest knowledge of this operation.

by this surgeon, the stone was exceedingly large, and nearly filled the bladder, but was, according to the author, attacked successfully with the lithontritic instruments. In the case of an old man, the stone was situate behind the prostate, but was also crushed. Another patient offered the complication of excessive irritability, but he was also cured. M. Civiale next gives a notice of the present state of lithotrity, and remarks, that its principles are every where adopted. The reporters on this memoir, while they profess entire reliance on the honour of M. Civiale, at the same time thought it right to repair to the Hôpital Necker, where they obtained precise information on the results of all the calculous cases treated in that establishment, and according to which M. Civiale had framed his memoir. Baron Larrey, however, in the name of the commission, expressed his regret that M. Civiale had only put forward the advantages of lithotrity, and the successful cases, while the information obtained at the Hôpital Necker, proved that the fatality was as great as in the section cases at the other hospitals in Paris. On the whole, M Larrey spoke very sharply of M. Civiale; he reminds him, that the greatest surgeons were not wont to conceal their unfortunate cases; he added, that M. Civiale has decidedly failed to prove the superiority of the lithontritic method ; but he concluded, that the facts advanced by the author of the memoir were sufficiently valuable to entitle him to the recompense awarded by the academy."*

I trust it has been made sufficiently clear to this learned body, that, judging of the documents submitted to the public, and reviewing impartially the time, place, and circumstances of their getting up, it cannot be truly said that lithotomy has been a tolerably successful operation in the hands of any surgeon, dead or living. It has been shewn to be an operation whose average fatality is at least one in seven ; but my own belief is, that even this is too high. We have just witnessed it as low as six in twelve, or as one in two. That an operation should be fatal in the ratio of six to twelve, and had such a lamentable result happened in the hands of an ignorant person, and of one unacquainted with the anatomy of the human body, it would perhaps not have excited much surprise ; but occurring in the hands of a surgeon of much experience, considerable reputation, and

* See *Lancet* for May 21, 1831, p. 229, extract from *Lanc. Franç.*

possessing anatomical knowledge far above mediocrity, renders the whole subject one of the deepest interest to the surgeon, and to the public; and if we have no other ground for our attacks on the reputation of surgeons of former times, than the dread which the public still feels for the operation, a dread which has been ascribed to "the recollection of the shocking and protracted tortures which attended the old method of operating with the gorget," let us hope that these calumnies will cease, since the cause of that dread, notwithstanding the substitution of other instruments for the gorget, remains in full force to the present hour, —ay! and that, too, in the hands of Hospital Surgeons.

THE END.

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