A letter to Dr. David Boswell Reid, ... in answer to his pamphlet intitled "An exposure of the misrepresentations in the Philosophical Magazine and Annals," / by Richard Phillips.

A LETTER

TO

DR. DAVID BOSWELL REID,

&c. &c.

" I am glad (saies *Eleutherius*) to see the Vanity or Envy of the canting Chymists thus discover'd and chastis'd ; and I could wish, that Learned Men would conspire together to make these deluding Writers sensible, that they must no longer hope with Impunity to abuse the World. For whilst such Men are quietly permitted to publish Books with promising Titles, and therein to Assert what they please, and contradict others, and ev'n themselves as they please, with as little danger of being confuted as of being understood, they are encourag'd to get themselves a name, at the cost of the Readers, by finding that intelligent Men are wont for the reason newly mention'd, to let their Books and Them alone: And the ignorant and credulous (of which the number is still much greater than that of the other) are forward to admire most what they least understand. But if Judicious men skill'd in Chymical affaires shall once agree to write clearly and plainly of them, and thereby keep men from being stunn'd as it were, or impos'd upon by dark or empty Words; 'tis to be hop'd that these men finding that they can no longer write impertinently and absurdly, without being laugh'd at for doing so, will be reduc'd either to write nothing, or Books that may teach us something, and not rob men, as formerly, of invaluable Time; and so ceasing to trouble the World with Riddles or Impertinencies, we shall either by their Books receive an Advantage, or by their silence escape an Inconvenience."—*Boyle's Sceptical Chymist. Oxford,* 1680, *p.* 210.

A
LETTER
TO
DR. DAVID BOSWELL REID,
EXPERIMENTAL ASSISTANT TO PROFESSOR HOPE,

&c. &c. &c.,

IN ANSWER TO HIS PAMPHLET,

INTITLED

"AN EXPOSURE OF THE MISREPRESENTATIONS IN THE
PHILOSOPHICAL MAGAZINE AND ANNALS," &c.

BY

RICHARD PHILLIPS, F. R. S. L. & E., &c.

> "Il n'y a point de gens qui aient plus souvent tort que ceux
> qui ne peuvent souffrir d'en avoir."
>
> ROCHEFOUCAULD.

LONDON:
SAMUEL HIGHLEY, 174, FLEET-STREET.

1831.

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A LETTER,

&c.

SIR,

In a pamphlet, entitled *An Exposure of the Misrepresentations in the Philosophical Magazine and Annals, &c.* contained, as asserted, in a review of your *Elements of Practical Chemistry,* you have freely bestowed terms of contempt upon the author of that criticism; denouncing him as a doughty critic, of utter incapacity and cool effrontery; as an infatuated, and, unaccountably, totally and grossly ignorant person; as one who is captious, cavilling, clumsy, audacious, and guilty of gross misrepresentations, &c.

These, Sir, are a few of the phrases which you have applied to the writer of the review in question; and you will perhaps think that I possess more courage than prudence, when, after such a description, I venture to avow, as I now do, that I am the author of, and alone responsible for, every statement which the criticism contains.

I assure you, however, that I do not complain of the language in which you have described me. Nature has provided her creatures with means of defence, suited to the stations they were intended to occupy; and to you (in this respect) she has been peculiarly liberal, supplying you with stores of invective, which, ample as they once were, you must have now nearly exhausted.

I am not aware that I have made use of any expression, in speaking of your work, that is disallowed by the language of fair criticism, I have, indeed, once said that I thought a certain statement culpably inaccurate; and as the result of an examination of an important chapter, I also ventured to state, that you appeared to me not to be sufficiently acquainted with the science which you had undertaken to teach. Whether I was justified in expressing these opinions, remains to be discussed; and in replying to your *Exposure,* I shall more minutely detail the facts upon which my conclusions were founded.

The observation that first excites your displeasure, ought, I think, to have produced a very different feeling. Having supposed it probable that your book did not appear entirely without the sanction of Dr. Hope, to whom it is dedicated, you call it "insinuating that he must have superintended the work;" and yet you say that he has been pleased to "express his approbation" of the plan and arrangements of your lectures. If, then, *sanction* means superintendence, what construction is to be put upon *approbation?* I beg, however, to state my entire belief in your assertion, that your book "had not the benefit of so valuable a revision" as Dr. Hope could have bestowed upon it.

Alluding to me, in the first page of the *Exposure,* you observe, that "this writer has, with an infatuation for which it is not easy to account, condescended to be specific in his charges, and has thus exposed himself in a way that enables me to bring matters to a direct issue, without the possibility of his escaping in loose generalities. For it must be borne in mind that his charges do not refer to mere opinions and speculations, but to matters of fact, where an appeal to experiment, easy of execution, and involving no doubtful position, can at once afford the most unequivocal evidence of the accuracy or inaccuracy of the statement made."

The latter part of this paragraph is excellent. It indicates a vigorous determination on your part, to try that which is doubtful, and to rest satisfied with no evidence but that of your own senses; and yet, so ill do your actions agree with your words, that, among many others, there is one experiment which you might have repeatedly performed in the time bestowed upon writing the passage above quoted, for which you have trusted to authority, and have been deceived. I shall also point out another instance, so strangely discordant with your resolution to examine, that in the *Exposure* you have flatly and positively contradicted a statement contained in the *Elements;* and yet, Sir, the experiment that would have decided the question is "easy of execution." But of this more anon.

I shall now proceed pretty regularly to examine the allegations contained in the *Exposure;* premising, that if some circumstances had not occurred during the investigation, which add to the facts of chemical science, I should not have bestowed so much time in the refutation of your opinions. 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.

You have informed us in the *Elements,* that when a metal, having a strong affinity for oxygen, is acted upon by dilute nitric acid, white fumes arise, which consist of nitrate of ammonia. In my review I stated my disbelief of the fact; you have re-stated it in the *Exposure,* and I have, therefore, performed the following experiment:—I put a quantity of tin into a retort, and poured upon it, at intervals, more than six fluidounces of nitric acid of sp. gr. 1.306, occasionally adding fresh portions of the metal; the gas liberated, which must have contained the vapour of nitrate of ammonia, if any were present, was passed into a fluidounce of water, kept at a temperature of about 50°. After the experiment had been continued for about half an hour, the water through which the gas passed was examined: its quantity was increased to three fluidounces, by the condensation of water and some undecomposed nitric acid; its sp. gr. was 1.008; excess of hydrate of lime was mixed with it, and moistened turmeric paper held over the mixture; but neither by this nor by any other means, was the evolution of ammonia discoverable.

I conclude from this experiment, that the vapour in question is not nitrate of ammonia; and allow me, Sir, to inquire how it could possibly be so? Dilute nitric acid acts rapidly upon tin, and gas is plentifully evolved at 180°, and below it; and yet, in p. 41 of your *Elements*, when directing the mode in which nitrate of ammonia is to be prepared, by evaporating the aqueous solution, you say—"in this manner the last portions of water are expelled more speedily, and full advantage taken of as high a temperature as can be applied with safety." Thus, then, at 180°, nitrate of ammonia evaporates from water, while, at as high a temperature as can be applied with safety, the salt remains and the water is evaporated.

I come now to consider your opinions as to the temperature at which nitrate of ammonia is volatilized and decomposed. I say, *opinions;* for you have expressed two very opposite ones, and with a degree of precision rarely to be found in your writings. In p. 41 of the *Elements*, you say, when treating of nitrous oxide, "the first portions of gas that come away are often impure, *especially if the heat to which the nitrate is exposed is too great, and part of the salt also is then volatilized without being decomposed, and collects occasionally in a solid mass in the beak of the retort.*" I did not remember this statement, when, in reviewing your work, I supposed you to know "that this salt is decomposed at a low temperature and not vaporized." I may, perhaps, be pardoned for not being aware of the extent of your knowledge, but that you should be ignorant of how much you knew, is rather a curious circumstance; and yet such seems to be the case, for the opinion expressed in the *Elements*, is thus characterized in the *Exposure*.—"It must appear not a little singular, that a statement displaying such ignorance of facts, and of works intimately connected with an important part of the history of the science, could gain admission into a journal which had a character to support. *It is not true that nitrate of ammonia, as he states, 'is decomposed at a low temperature, and not vaporized.' It is vaporized without being decomposed at a comparatively low temperature, and it is not decomposed till the temperature is increased to above that point at which it may be volatilized.*" After this you proceed, in a triumphant strain, to inquire whether I ever heard of a gas called nitrous oxide—ever prepared it—ever read of it, &c. Indeed, Sir, if I had not read of it till I met with your accounts of the circumstances attending its production, I should have formed a strange idea of chemical operations; for the following is, I believe, a fair summary of your opinions respecting the preparation of this gas; I submit it to your consideration, and, if approved, for introduction into the next edition of your *Elements*, which, as "five other reviews" have recommended it, must be near at hand. *In preparing nitrous oxide gas, care must be taken to subject the nitrate of ammonia to a sufficient temperature; for it is vaporized without being decomposed, at a comparatively low temperature, and it is not decomposed till the temperature is increased to above that point at which it may be volatilized. These directions must be minutely observed; for if the heat to which the nitrate is exposed is too great, part of the salt is then volatilized without being decomposed.*

Any person who denies the accuracy of the whole of this statement, is a doughty, ignorant, cavilling, captious, clumsy, audacious, furious critic.

If, however, upon reflection, you should find it requisite to dismiss one of your conflicting opinions on this subject; and if, as usual, you should prefer the judgment of another to an experiment of your own, allow me to offer for your consideration the following passage from Berzelius—(Traité de Chimie, tome ii. p. 46.)—"Le sel [nitrate d'ammoniaque] est introduit dans une cornue garnie d'une tube propre à conduire les gaz, et qu'on chauffe à la flamme d'une lampe ou sur quelques charbons. Il commence par se fondre, puis il entre en ebullition, et le gaz se degage en grande quantité. Si'l parait des vapeurs blanches dans la cornue, la chaleur est trop forte, et une partie du sel se sublime."

Should you adhere to the opinion expressed in the *Exposure*, in preference to that contained in the *Elements*, you will, of course, wonder that Berzelius could make "a statement displaying such ignorance of facts;" that it "could gain admission" into the work of an author who "had a character to support," &c. Before, however, you again adopt this language, allow me to relate an experiment which I have performed, and which I advise you to repeat. It is, I assure you, "easy of execution," and "involves no doubtful position;" which are properties that characterize all excellence in experimenting, of which you have talked so much and done so little. I took a glass tube, closed at one end, about 7½ inches long and 8-10ths of an inch in diameter; having noted its weight, I put 350 grains of compact nitrate of ammonia into it, and immersed it, with a thermometer, into a vessel containing melting suet; the heat was gradually raised till the salt fused, which was totally effected at a temperature rather below 380°; the salt was kept in fusion for about 20 minutes, and then allowed to cool; a solid mass of nitrate of ammonia, 1½ inch in length, remained at the bottom of the tube, the upper part being cleared, and the tube and salt weighed, the loss was only 8-10ths of a grain.

From this experiment I conclude, although in opposition to Davy's statement, which you have quoted, that compact nitrate of ammonia becomes fluid, and will remain so, without undergoing either sublimation or decomposition.

The point to be next determined is, whether nitrate of ammonia decomposes or sublimes at the lower temperature. You, as already shown, maintain both opinions; Davy states that it sublimes before it decomposes; while I have ventured to assert, that it decomposes at a lower temperature than that required for its sublimation. To determine this, I put 150 grains of compact nitrate of ammonia into a similar tube to that employed in the above-related experiment; into this I inserted, by means of a perforated cork, a bent tube, about 15 inches long, and a quarter of an inch in diameter, but at the angle, owing to flattening occasioned by the heat used in bending, the aperture was considerably less. The tube and salt, with a thermometer, were, as before, immersed in melting suet; at about 360°, the salt began to liquefy; at 450°, forty bubbles of gas were evolved in a minute; and at 540°, nitrous oxide was copiously evolved. For a short time the heat was raised to 580°, but never higher; when about 160 cubic inches of gas had been received, the process was stopped. Now, if the salt were vaporized at a lower temperature than that at which it is decomposed, a tube of so great length and small diameter, must speedily have been choked with it; but this was so far from being the case, that not the slightest sublimation occurred. 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 requisite to apply a strong heat. I find that the flame of a spirit-lamp, readily produces the effect to a certain extent; though I confess I have not been so fortunate as you, in choking the neck of the retort when attempting to procure nitrous oxide.

The subject which next presents itself for consideration, is the effect produced by passing nitric oxide gas into colourless nitric acid. If I do not mistake, you assert, that in pale nitric acid, of various specific gravities, the different colours of light straw, reddish brown, various shades of olive and green, and almost blue, may be produced. These assertions you make in contradiction to mine, that "strong nitric acid never becomes at all either olive, green, or blue, by absorbing nitric oxide;" and that no "acid of any one degree of strength is capable of exhibiting them all, whatever may be the quantity of nitric oxide passed into it."

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 that 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 20 or 30 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 pennyweights 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," 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.

Observe, that in this experiment the acid assumed a deep orange colour presently after the process began; it then became green; and towards the end of the process it was rather blue than green. But did the acid remain of the same strength? No;

"It had become, by means of this process and the evaporation together, exceedingly weak." Now can any experiment more distinctly prove my assertion, that acid of no one degree of strength is capable of exhibiting all the colours?

The experiment which you next quote is from Davy. This I shall not give at length; for I confess I do not place any dependance upon the result; it was made with only 93 grains of nitric acid, which, it is stated, became of a dark orange colour by absorbing nitric oxide, and yet diminished in sp. gr. from 1.5 to 1.48.

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 fluidrachm of nitric acid, of sp. gr. 1.497, I obtained nearly the tints you mention; 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." I proceed, therefore, to consider what you have advanced, in opposition to my statement, that nitric acid, of any one degree of strength, is incapable of exhibiting all the colours which you have enumerated. "I have made the experiment," say you, "frequently, with pale acid of various specific gravities; with acid of the very specific gravity to which he alludes [1.46]; and with acid both stronger and weaker than this. I have seen each of these kinds of acid become not only yellow and red, but also olive and green, and almost blue, as I have described, or, to use Sir H. Davy's expression, blue-green." I shall meet this statement, both with quotations and experiments; and, first, with the former. M. Laugier (Cours de Chimie générale, Leçon x.) makes the following observations—"On prepare de l'acide nitreux en faisant passer un courant de deutoxide d'azote à travers de l'acide nitrique, qui prend des couleurs diverses, suivant qu'il a plus ou moins de densité. Si on le reçoit successivement dans quatre flacons contenant des acides à 1.15, à 1.32, à 1.41, de densité et enfin de l'acide le plus concentré, il se developpera dans le premier flacon une couleur bleuatre, verte dans le second, jaune foncée dans le troisième et brune foncée dans le quatrième. Ainsi la nuance est d'autant moins prononcée qui l'acide a moins de densité et *vice versâ*." Again, and to the same purport, I refer you to M. Dumas (Traité de Chimie, tom. i. p. 327); speaking of the action of nitric acid upon nitric oxide, he observes—" L'effet varie d'intensité en raison de la concentration de l'acide; ainsi l'acide pesant 1.15 est sans effet sur le deutoxide d'azote, celui qui pèse 1.32 en absorbe un peu et devient vert, celui qui pèse 1.41 en prend davantage et devient jaune, et l'acide à 1.510 devient brun en absorbant encore plus de gaz." Now you will observe, that these authors do not quite agree as to the changes produced upon acids of similar specific gravities; but that their general statements are correct, as to effects upon dilute and strong acids, I have proved by the following experiments:— I put into a vial two fluidounces 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 eqtual 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 fluidounces 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 fluidounces 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.

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. I shall, however, notice what you observe respecting the modes of cooling receivers employed in distillations. 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. When the receiver is placed in a basin of water, if not fixed in its place, and kept down by a weight, but allowed to find its own level, it will float unsteadily in the basin;" and to these statements, you add a list of evils attendant upon the plan which I propose, and of advantages secured by your own. Now, Sir, I have already proved that, owing to a defective memory, you sometimes forget in the *Exposure* what you have written in the *Elements;* and I shall hereafter also show, that the same remark will apply to statements contained in different parts of the latter. 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." I will indeed admit that the mode of cooling is not directed in the text, but fig. 62, which accompanies it, shows that it is the plan which I prefer and you now condemn, viz. that of placing the bottle receiver in a vessel of cold water. 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; and you must, therefore, have discovered, in the course of a few months, that common bottles are "extremely apt to be broken when surrounded by cold water;" and the " many receivers broken," must have met with their fate since the *Elements* appeared; for you could not have recommended a plan so fraught with mischief, as that which you first proposed and now condemn, had you been aware of its nature.

We come now to discuss the mode of distilling nitric acid from a flask. In your *Elements*, p. 55, you assert—" it is a very convenient method of conducting the process, and is often preferred to distilling the nitric acid from a retort." When I disapprove of the use of a flask for this purpose, you tell me, in the *Exposure*, that you " have never affirmed that a flask is *more* proper than a retort." Am I, then, to assume, that the flask is " often preferred," because it is *less* proper than a retort?

In my review I objected to the employmentof flasks, that, owing to their form, the acid is more apt to condense and run back than when a retort is employed; this argument you answer thus, "it *must* be obvious to every one who reflects on the nature of the apparatus, that it [the flask] *must* be less favourable to the condensation of the vapours before they have passed the angle than the retort, &c." "there *must* be much less condensation," &c. Assertions so positive caused me to repent of my determination not to try the flask apparatus, and I will confess, that with some improvements, it is not so bad as I expected, and you have not done it justice, for clay is a perfectly sufficient lute without the additional use of plaster of Paris, which you recommend.

I will now trouble you with the details of a few experiments: the capacity of the flask employed was 44 cubic inches, the bent tube was 7-16ths of an inch in diameter, and about 16 inches in length; the height of the apparatus was 12 inches. Six ounces (avoirdupois) of sulphuric acid and nine ounces of nitre were used; the flask was heated by an argand lamp; the quantities of nitric acid, and the time in which they were produced, taking the mean of two experiments, were as follows:—

One hour	3 oz.	54 gr.
Two hours	3 —	425 —
Three hours	4 —	80 —

I then made two similar experiments with a retort, the capacity of the body of which was 50 cubic inches; taking the mean, I obtained of nitric acid, in

One hour	3 oz.	376 gr.
Two hours	4 —	135 —
Three hours	4 —	245 —

On comparing these results it will be seen, that the retort yielded in one hour, within 50 grains, as much acid as the flask did in two hours; and that in two hours, the product by the retort exceeded, by 55 grains, that obtained from the flask in three. These experiments prove, therefore, that your thrice repeated assertion of what *must* happen did not occur; and I leave you to discover any other cause for the failure of your theory than that which I have pointed out, viz. the condensation of the acid and its return to the flask; but to be convinced that I am right, attempt the distillation of mercury in this apparatus. To conclude this part of the subject, I agree with Mr. Faraday, *Manipulation*, p. 217, that "in cases of necessity, Florence flasks may often be substituted for retorts in distillation," but I cannot admit with you that they are often preferred.

Although I have been much puzzled by your reasoning and experiments upon several occasions, a note of admiration which you have employed in one instance, has completely set at defiance my powers of solving a mystery:—the following contains the difficulty which has occurred to me; "He next tells us that a thin tube when bent is very apt to break at the angle!" now whether this portentous! means that I have stated a truth so obvious, that it was useless to mention it; or that I have made an assertion so false that no one will believe it, I really am at a loss to discover, and when you reply to this letter, (and you will find sufficient authorities on record to answer every statement it contains,) I beg of you to explain the meaning of this note of admiration.

I find that I misunderstood part of your flask process; it seems that the tube is not to be fastened into the receiving flask, but to rest "loosely" in it; this word is so characteristic of your usual mode of proceeding, that I confess, I ought to have discovered your intention; but allow me to inquire whether "delicate" gentlemen, are not likely to be incommoded by the escape of gas attendant upon such an arrangement? 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.

I must however insist, that as you have made no general declaration of the kind of weights which you employ, "eight ounces" may mean either troy or avoirdupois, and your assertion that the former is always used in the operations above alluded to, I know to be incorrect; I must also repeat, that no such measure as "water measure" exists, nor does the table at the end of your book mention any measure by this name. You also assert that the student "would take the only liquid ounce measure in use, and which is fixed to contain 455.6 grains of water," in your table you say 456.5 grains; the difference is trifling, and I suppose you to mean the fluidounce of the London College; you will find however, that there is another liquid ounce measure in use; I employ it, and it is mentioned in a work which I recommend to your serious perusal, I mean Mr. Faraday's *Manipulation*, in this he describes the imperial fluidounce, containing 437.5 grains of distilled water.

While speaking of the preparation of nitric acid in the *Exposure*, (p. 14) you say, that you "have given a minute account, illustrated by a diagram, of the action that takes place when the proper proportion for obtaining nitric acid is used—the proportion in unison with equivalent numbers." Really sir, I am astonished that you should refer to this "minute account," for in it, as pointed out in my review, you give 96 as the weight ot two atoms of sulphuric acid, instead of 98, and this error you have repeated and "illustrated by a diagram."

I shall now notice your statement, that red nitric acid, is "preferred in many processes from its great strength, being always stronger and more active than the pale acid:" first, as to its greater strength; to ascertain this point, I made the following experiment; I prepared some red acid of sp. gr. 1.541; a portion was diluted, taking care that as little gas should escape during the dilution as possible; it dissolved 73.2 per cent. of carbonate of lime, equivalent to 79 of nitric acid; when however, another quantity of the red acid was diluted, in a bottle containing azotic gas, and the nitric oxide evolved had consequently but little opportunity of becoming nitric acid, it decomposed only 71.3 per cent. of marble, denoting 77 of nitric acid. Now pale nitric acid of 1.5 dissolves 73.7 per cent. of carbonate of lime, equivalent to 79.6 of nitric acid; from these experiments, I am warranted in asserting, that the density of red nitric acid may be greater than that of colourless acid, and its strength less.

As it is reasonable to suppose, that the activity of diluted nitric acid is proportional to its strength, there would not seem to be any occasion to ascertain the former by experiment, after the latter had been determined; as however in my review I thought I could "defy you to point out a single process in which the coloured acid is to be preferred to the pale," you have in the *Exposure* stated that nitric oxide may be procured more readily by employing the former. You have not given the details of your experiments on this subject, but I will state those which I have made relative to it: I diluted 500 grains of red nitric acid of sp. gr. 1.541 with twice its weight of water, and when the mixture had cooled to 60°, I poured it upon 250 grains of copper in a retort, and in about 10 minutes I obtained 50 cubic inches of nitric oxide gas; I then used an equal quantity of pale acid of sp. gr. 1.507, with the same proportions of water and copper; it required however 15 minutes to produce 50 cubic inches of nitric oxide; I therefore admit that red nitric acid produced 50 cubic inches of gas in 5 minutes less time than the pale; this however, cannot be owing to its greater strength, for I have shown that it is weaker; the cause I presume to be, that the diluted red acid, when used quickly after dilution and of a greenish colour, contains as much nitric oxide as it is capable of holding in solution: whereas, the diluted colourless acid, not containing any, occupies some time in being saturated before the gas passes over; that this is the case, is well illustrated, by an experiment of yours on the solution of mercury in nitric acid, and the non-escape of gaseous matter; this fact is stated in p. 17 of the *Exposure*, and I shall have a future occasion for attending to it.

I wish, however, you had given some additional and more substantial reasons for preferring the red acid to the colourless; for I really do think, that for the sake of gaining five minutes in the preparation of 50 cubic inches of nitric oxide gas, it is not worth while to obtain only 3600 grains of red acid, from 16 ounces of nitre and 10.7 ounces of sulphuric acid, instead of 4530 grains of pale acid from 16 ounces each of the salt and acid; these I can assure you are the respective products, and you will observe, that in the latter case, one-fourth more nitric acid is obtained, and at about 1-10th additional cost; added to this, when the sulphuric acid is equal to only two-thirds of the nitre in weight, the mixture is exceedingly apt to froth, and without care would boil over, which never happens when equal weights are employed.

The subject next to be discussed is whether, as I have stated, "when nitric oxide gas is passed into colourless nitric acid it becomes red, and its density is considerably increased." Now, relying, as usual, upon authority to convict me of error, rather than allow yourself to confirm my opinion by experiment, you refer to Davy, and quote an experiment which you must know to be inaccurate; in p. 57 of the *Elements* you state that "Dr. Hope has prepared it [the red acid] with so high a specific gravity as 1.54, though the specific gravity of the colourless acid does not exceed 1.500," and you also inform us, that to deprive nitric acid completely of colour, "it must be exposed to a gentle heat, as long as any nitrous acid vapours or nitric oxide gas is expelled." I must do you the justice to believe, that, knowing the expulsion of nitrous acid and nitric oxide from red acid rendered it pale, and reduced its sp. gr., you must also have been aware, that, by causing pale acid to absorb nitric oxide, by which the nitrous acid is generated, colour must be created and density increased. If you do not admit this, you 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. For experimental proof of the correctness of my assertion, I refer you to page 8 of the present letter, where you will find, that pale nitric acid of sp. gr. 1.497, had its density increased to 1.541, and was rendered red, by absorbing nitric oxide.

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: I put 100 grains of copper, iron, silver, lead and tin, into small vials with glass stoppers, and upon each metal I poured two fluidrachms of nitric acid of sp. gr. 1.500; upon the copper a slight action immediately took place, whereas, it was long before any apparent change occurred with the other metals. After the vials had remained closely stopped for a week, it was evident, that all but the tin had been slightly acted upon; minute crystals of nitrate of copper were formed, and the acid was coloured by holding oxide in solution; I examined only two of the solutions particularly, those of iron and silver; the former when saturated with ammonia, gave 3 grains of peroxide of iron, and the latter gave 1.5 grain of chloride of silver; these statements are sufficient to show, that strong nitric acid of 1.5, acts very feebly upon the metals. I now repeated the experiment with acid of 1.48, the copper was immediately, but slightly, acted upon, and after remaining a week, I procured one grain of peroxide of iron, and the same quantity of chloride of silver; the solutions of tin and lead I did not examine, though it was evident, that some nitrate of the latter metal was formed. These experiments are, I think, sufficient to show, that there is "nothing peculiar" in acid of sp. gr. 1.48, and that when nitric acid is above a certain strength, it is very slowly decomposed by the metals generally.

You persist however, that there are peculiarities in nitric acid of different densities, and you detail the following experiments in proof of the accuracy of your assertion. "Nitric acid, of specific gravity between 1.50 and 1.51 was used. Four glasses were taken, and an equal portion of acid poured into each. A few drops of water were added to one of these, twice as much water to another, and four times as much to a third: the fourth was not diluted. Pounded antimony was thrown into each till it rose to the top of the liquid, when it was found that an effervescence took place in the strong acid, and in the acid to which most water had been added, while the antimony had but comparatively a feeble action on the acid to which the smaller proportions of water had been added. I took acid of the specific gravity 1.48, and added a similar quantity of pounded antimony to it, and it was not nearly so much acted upon as the strongest and weakest acids used in the preceding experiment. The difference was very marked;" *Exposure*, p. 16.

These experiments appear to me to be of much too loose and indefinite a nature, to admit of their serving as the basis for a general law; in three out of five of them, the specific gravity of the acid is not given, nor is the quantity of antimony employed mentioned. In order to determine the action of acid of different densities upon the metal in question, I added separate portions of powdered antimony to two fluidrachms of nitric acid of the specific gravity stated, and obtained the annexed results:—

Antimony.	Acid, sp. gr.	
20 grs.	1.507,	action immediate, soon rather strong, afterwards ceased.
50 —	——	action immediate, soon rather strong, afterwards nearly ceased, but went on again, though not strongly.
150 —	——	action immediate, and exceedingly strong.
20 —	1.480	no action.
50 —	——	action at first slight, then ceased.
150 —	——	action immediate and strong.
20 —	1.441,	no action.
50 —	——	action slight and then ceased.
150 —	——	immediate and strong action.
20 —	1.420,	no action.
50 —	——	action very slight and soon nearly ceased.
150 —	——	immediate but not strong action, soon increased and became strong.

These experiments require but little comment; they certainly prove that undiluted nitric acid is decomposed by a small quantity of antimony, while weaker acid is acted upon only by a larger proportion; but they fail most completely in showing that there is any thing peculiar in the nature of acid of 1.480, for you will observe that no difference exists between acid of that density, and 1.441, and very little indeed between it and 1.420; and in addition to this, the experiments prove, that the action of acid, of the same density, depends generally upon the quantity of the antimony employed, a circumstance of which I was not previously aware, nor do I know whether it exists with any other metal.

The subject next to be considered, is a doubt expressed by me, whether nitrous acid is in any case evolved by the action of a metal on nitric acid. You have attempted to establish this fact, both by authority and experiment; into the value of the former, I shall not inquire; but I shall examine a little the accuracy of the latter. In reference to this subject you state, "that there are metals which are acted upon by nitric acid either with a copious evolution of gaseous products, or without the disengagement of a single particle according to the relative proportions in which they are mixed together," and the latter of these cases, or the non-evolution of gaseous matter, you prove by the following experiment, "take a common test tube, fill it half full of colourless nitric acid, and pour into it ten or twenty grains of mercury. In a few minutes, the mercury is completely dissolved, a considerable agitation is observed in the lower part of the liquid, and much gaseous matter is produced; but none of it escapes, being absorbed by the superincumbent liquid before it reaches the surface." You might as well have added, that the gas produced is nitric oxide, and that owing to its absorption, the nitric acid is rendered green; I particularly allude however to this experiment, as proving what I have before stated, that it is not owing either to the greater strength or activity of red nitric acid than pale, that nitric oxide is more readily evolved when it is used, but because the pale acid, as in the above detailed experiment, absorbs the first portion of gas evolved, whereas, recently diluted red acid, already holds as much as it is capable of retaining. As to the formation of nitrous acid, this experiment of yours, shows nothing; proceed we therefore, to the next, which you say "proves most unequivocally the evolution of nitrous acid gas when mercury acts on nitric acid." The experiment is as follows :—"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 blow pipe, 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 ruddy vapours, and affording another instance of their production when metals act upon nitric acid, totally independent of the action of the air."

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. You have quoted an experiment of Dr. Priestley's in which he mentions the evaporation of nitric acid, when exposed to nitric oxide, and the same effect was produced, when you passed a brisk current of nitric oxide gas into a small quantity of nitric acid.

I made however the following experiments on the subject, which appear to me decisive of the question, whether, when mercury is acted upon by nitric acid, any nitrous acid is produced; I passed some nitric acid of sp. gr. 1.318 into a jar of mercury, red vapours of nitrous acid were produced, and consequently, the acid was of a sufficient degree of density for the experiment; I then took 720 grains of the same acid and put it into a retort, with mercury, and received the gas generated into lime water; when the evolution of gas ceased, there remained excess of lime as shown by turmeric paper. I then passed carbonic acid into the lime water, heated it to throw down the carbonate of lime, and afterwards decomposed the nitrate of lime formed, by carbonate of soda; the resulting carbonate of lime weighed 10.5 grains, equivalent to 11.34 grains of nitric acid. Now, 720 grains of acid of 1.318, contain about 320 of real acid; and I leave it to you to decide, whether it is more probable, that 11.34 grains, or one twenty-eighth part of this acid, was vaporized with the nitric oxide gas, or converted into nitrous acid.

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 intirely into oxygen and nitric oxide? 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 perhaps the only metals that disengage pure nitric oxide when they act upon nitric acid?"

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.

The question which next arises is, whether, Liebig's process for detecting the presence of nitric acid is intended for free or for combined acid. You maintain the former, and I the latter opinion; I repeat that I cannot discover the use of the sulphuric acid in the process, unless it is to decompose the nitrate and allow its acid to act upon the indigo; indeed Liebig's own words which you have quoted are direct to my purpose, "Si le liquide contenait un nitrate, il sera décoloré;" I admit that he also mentions nitric acid without stating it to be combined, but on the occasion of proposing the test, the acid, if any existed, must have been in combination with potash, the gases suspected to contain it, having been passed through a solution of the carbonate, for the purpose of subjecting it to the action of the test. This part of his statement, was not to your purpose, and therefore, you omitted to notice it.

To prove the correctness of your opinion, you quote the account of the process as given in the Philosophical Magazine; if I prepared that statement, which was not, I believe the case, I committed a blunder in introducing the words "sulphate of indigo;" Liebig does not mention it, and you must have been aware of this mistake; but provided you can find evidence, you are perfectly indifferent as to its value.

I come now to the last statement contained in the *Exposure:* in my review, I observed that no such substance as oxygenated nitric acid exists, although you have mentioned the mode in which it may be prepared. You now indeed admit 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.

I have now, Sir, concluded a re-examination of such statements contained in your work, as I objected to in my review of it, or rather of such of them as you have defended and repeated in the *Exposure.* Your reply contains however, a complaint that I have examined one part only of the *Elements;* "whether, in thus confining his attention to a single chapter, and passing over the claims of a work which professes novelty of plan, arrangement, and illustrations, which embraces a wide and comprehensive system of operations, where the mass of matter could not be restated from former authorities, but required precise observation and personal experience, and which therefore had peculiar difficulties to encounter,—whether, I say, passing over all these is altogether fair, I do not pretend to determine. I can only state that five other reviews in this country have noticed my work, and that each has brought these points prominently forward. But I am most willing to meet this worthy reviewer on his own ground, and likewise to let my book stand or fall according as I can prove his ignorance, his misrepresentations, and his clumsy and ill disguised efforts to depreciate my work," *Exposure*, p. 2.

If you will refer to the concluding paragraph of my review, you will find, that I was not unprepared to adduce more facts in support of the opinion expressed of your book; challenged therefore, as I am, to proceed with my examination, I shall mention some additional circumstances, which, if I mistake not, will confirm the accuracy of my views.

When treating of the preparation of oxygen gas from nitrate of potash (p. 13,) you say, "if the heat is continued after this quantity of oxygen has been obtained, the whole of the nitrous acid is decomposed, and a large quantity of mixed gases is obtained, consisting of oxygen, nitrogen, nitrous, and nitric oxides," what evidence you may have for the production of nitrous oxide, I know not, but I learn from p. 50 of the *Elements*, what indeed I happened before to know, that no mixture of oxygen and nitric oxide gases can exist; for after mentioning the circumstances under which these gases combine to produce different compounds, you say, "any excess of nitric oxide may easily be detected, by mixing a little air or oxygen with the gas in the jar suspected to contain it; if, on the other hand, all the oxygen should not have been consumed, on introducing a little nitric oxide, ruddy vapours will be immediately produced; hence, nitric oxide and oxygen gas may be employed, each to indicate the presence of the other;" in one word these gases are incompatible, and therefore no such mixture of gases, as that which you describe to be existing largely, can exist at all.

The weight of a cubic inch of water (p. 29) is stated to be 252.525 grains, instead of 252.458 grs. and you have given 0.625 as the specific gravity of steam, instead of 0.62349 as ascertained by Gay-Lussac, and generally admitted to be correct.

In treating of the preparation of nitrate of ammonia (p. 42,) you inform us, that "every 54 parts or one equivalent of real nitric acid (contained in 72 of the common liquid nitric acid) combine with 17 of ammonia, and give 71 of dry nitrate of ammonia." Now if you had tried this experiment, you would have found that 72 of the nitric acid contain 57.6 of real nitric acid, which combine with 18.1 of ammonia, and about 9.6 or an atom of water, which is essential to the existence of nitrate of ammonia, even in its compact state; I speak from direct experiment, when I say that nearly 84 of dry, though not anhydrous nitrate of ammonia were obtained, by saturating 72 of nitric acid of 1.5 and evaporating the solution. The diagram therefore, by which you illustrate the position, that 71 of dry nitrate of ammonia, will yield 95 cubic inches of nitrous oxide, will lead the pupil into error; the product will be less than 85 cubic inches.

I ascertained the composition of nitrate of ammonia, also by analysis; 100 grs. of the compact salt were decomposed by boiling in water with lime, the excess being thrown down by carbonic acid; the nitrate of lime formed, gave 63.4 of carbonate by carbonate of soda =68.4 of nitric acid, requiring 21.5 of ammonia for saturation and leaving 10.1 for water. That this is the composition of the salt has already been shown by Berzelius, and Dr. Thomson; I am not aware however that it has been clearly stated, that the crystallized salt, contains no more water than the compact; yet this is the case, for I find, that when treated with lime, they yield equal quantities of nitrate; and the crystallized salt fuses without losing water.

The errors which you have committed with respect to the composition and decomposition of this salt, are not, comparatively, of a very glaring character; still however, the mistakes are too important to be overlooked when challenged to investigation by the author of a work which "professes" to contain "a mass of matter that could not be restated from former authorities," and lays claim to "precise observation and personal experience."

For the preparation of nitric oxide gas (p. 46) you advise the use of copper and "nitric acid diluted with one and a half times or twice its bulk of water," and you afterwards assert, that should a more diluted acid be employed, "heat is necessary for the preparation of this gas." Now Sir, I have the "cool effrontery" to declare either that you never tried a weaker acid, or that you obtained nitric oxide gas without the application of heat. Nitric acid diluted, as you propose, with twice its bulk of water, contains at least 34 per cent. of real acid; in experiments which I have made on the production of nitric oxide gas, I generally employed acid of about 1.150 sp. gr. and which contains less than 22 per cent. of acid; this acts readily upon copper without heat, and at one period of an experiment, I obtained 20 cubic inches of gas in five minutes.

I shall now pass over about 20 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.

The operation is thus further explained "four equivalents, accordingly, of sulphuric acid are required to convert one equivalent of mercury into persulphate, two of these affording oxygen to the metal, while the other two combine with this oxide as it is formed." As then, you think that protoxide of mercury is procured by boiling three parts of sulphuric acid with two parts of the metal, and peroxide when only two and a half of the former are employed with the same quantity of mercury, you must admit the anomalous circumstance, that when *six* equivalents of the acid are used, only *one* equivalent is decomposed, while when *five* equivalents are employed, *two* suffer decomposition.

You have stated it as a fact, in p. 80, that sulphuric acid forms compounds "with most of the earths" which "are very insoluble." If you will examine authors on this subject, they will furnish you with statements nearly as follows:—

Sulphate of Barytes, } insoluble.
——— Strontia, }
——— Lime, sparingly soluble.
——— Alumina, soluble in less than its weight of water.
——— Magnesia, very soluble.
——— Glucina, deliquescent.
——— Thorina, soluble.
——— Zirconia, slightly soluble in cold, but readily in boiling water.
——— Yttria, soluble in 30 or 40 times its weight of water at common temperatures.

You must find better evidence than your assertion, that two are more than seven, if you mean to prove it.

The composition of carbonate of lime is given wrong at p. 117; you state that "every 100 grains of marble contains 46 grains of carbonic acid." Now as 50, the atom of carbonate of lime, yield only 22 of carbonic acid, it is almost self-evident, even supposing the marble to be perfectly pure, that it cannot contain 46 per cent. of carbonic acid.

In order to prepare acetic acid (p. 160) you advise the student to "pour sulphuric acid carefully, and in small quantities at a time, on twice its weight of the acetate of potash in a glass retort, waiting till the ebullition ceases after each successive addition of the acid;" and you say that this process is the best; now I assert on the contrary, that if there is any exception to its being the worst, it is the process of the Edinburgh Pharmacopœia. The necessity of adding the acid a small quantity at a time, is of itself an insuperable objection to the method, and your flask apparatus, which is "often preferred" in other operations, it would be impossible to use in this. It is many years since I first attempted this process, but I have now tried it again, and the opinion which I have above stated is the result of this new trial.

When giving directions for preparing chlorine (p. 176), you mention that the best method is "mixing one part of the peroxide of manganese with four times its weight of muriatic acid in a glass retort," and in the next page you observe that "the common liquid muriatic acid containing more than its own weight of water, even in its most concentrated state, it is necessary to make allowance for this, and hence the large quantity which is directed to be mixed with the manganese." Now I confess I have yet to learn, how a given weight of any compound can contain more than its own weight of one of its ingredients, and yet according to you the strongest liquid muriatic acid, is in this extraordinary predicament. If you mean that the strongest liquid muriatic acid contains more water than real acid, it is certainly so.

Your diagram of the preparation of chlorine shows that 74 of real muriatic acid are required for 44 of peroxide of manganese; the liquid muriatic acid, prepared according to your directions, in p. 190, contains 34 of real acid in 100, 74 will therefore require 217, very nearly five times the quantity of the peroxide of manganese, instead of four which you represent as a large proportion.

I shall now pass over several pages of your *Elements* without offering any remarks upon their contents; not, I assure you for want of opportunity, but because several of the errors which they contain are the result of mere carelessness, and will I doubt not be corrected in your next edition.

Your statement respecting iron and its compounds, calls for some notice; the peroxide of iron you inform us "is the principal product of the combustion of iron in oxygen gas, and it may be procured also by transmitting watery vapour over metallic iron in the manner directed for the preparation of hydrogen gas." M. Gay-Lussac, (Ann. de Chimie, tome lxxx. p. 164) gives a different account of the oxides obtained in these modes, "on obtient l'oxide au second degré, toutes les fois que l'on brûle du fer dans le gas oxigène, ou dans l'air à une haute température, et mieux encore toutes les fois que l'eau seule est décomposée par le fer, soit à froid, soit à une chaleur rouge. Il est composé de

Fer	100.0
Oxigène	37.8"

But protoxide of iron, as indeed you have stated, is composed of

Iron	100
Oxygen	28.5

Crystals of sulphate of iron do not contain 7-8ths of their weight of water of crystallization, but less than half their weight. There are also some particulars regarding copper which I shall not pass by without remark. From the following passage you seem to suppose that when copper is immersed in a solution of ammonia, water is decomposed : " put some metallic copper into water of ammonia and allow them to remain mixed for some hours ; the copper acquires oxygen from the water and a small portion of oxide is formed which is speedily dissolved by the ammonia, and the usual blue colour appears," p. 288.

When, however, copper and a solution of ammonia are put into a retort, the aperture of which is put under a jar of water in the pneumatic trough, no gas whatever is evolved ; but on the contrary, the oxygen of the air contained in the retort is absorbed, and the solution soon becomes and remains colourless, until fresh air is admitted ; absorption again takes place and the solution is again rendered blue for a short time. When ammoniated copper is exposed to the air, it is not mere dry sulphate of copper which remains, as you assert; if you will add a solution of potash to it, you will find ammonia plentifully evolved; indeed this must evidently be the case, unless sulphate of ammonia evaporates at common temperatures. Binacetate of copper crystallizes in oblique rhombic prisms, and not in octahedrons as you mention; and I take this opportunity of recommending more attention to the forms of crystals, than you have generally shown.

With respect to antimony you tell us, that when it is treated with " hot nitric acid " the peroxide may be obtained by " evaporating the solution to dryness, and heating what remains to the temperature of 500 or 600, to expel any water which it may still contain." The fact however is, that only about 1-30th of the antimony is dissolved, and therefore, the directions for evaporating the solution to dryness are useless ; the remainder of the metal is converted into an insoluble compound, which probably contains some nitric acid.

Of this metal, you also state, that " the sulphate is easily prepared by boiling metallic antimony reduced to a very fine powder, with twice its weight of sulphuric acid;" and you add that "the metallic antimony attracts oxygen from part of the sulphuric acid, and sulphurous acid is disengaged, the oxide of antimony that is formed combining with part of the sulphuric acid that is not decomposed ; the excess of acid is expelled by the heat."

You then give a diagram to illustrate the action which takes place; by this we learn that when an atom of antimony 44 is boiled with 98 or two atoms of sulphuric acid, one of the latter gives an atom of oxygen to the antimony, that 32, an atom of sulphurous acid, is evolved, and 92 an atom of sulphate of antimony formed.

In the first place, it is to be observed, that to produce these results, in the text 44 of antimony are to be boiled with only twice their weight of sulphuric acid, or 88, while according to the diagram 98 will be required; then in the text we are instructed that the excess of sulphuric acid is expelled by the heat, while the diagram shows that there is a deficiency of the acid.

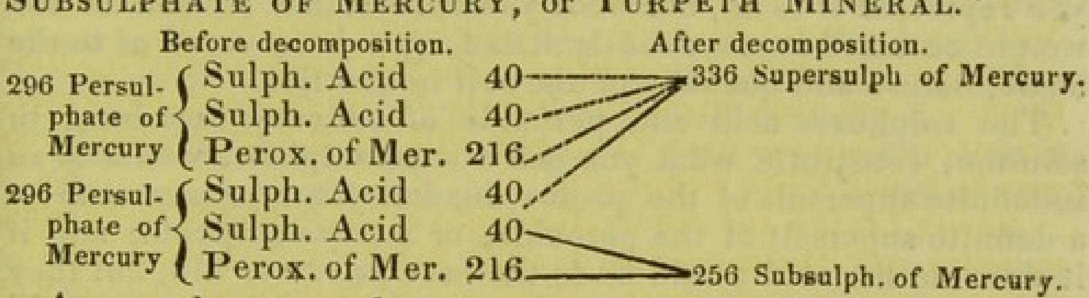
A stream of sulphuretted hydrogen gas, you state in p. 300, throws down a solution of emetic tartar of a " deep reddish brown colour," and in p. 303 you state that the same reagent produces " a very deep and characteristic reddish brown precipitate even in solutions containing a very minute quantity of this salt." I assure you that I have always found, as others have done before me, that the precipitate is of a bright orange colour instead of a deep reddish brown; consult Henry, Thomson, Dalton, Brande, Murray, Turner, Christison, Thenard, and *experiment.*

In describing the properties of bismuth you have given a diagram to show, that when the nitrate is decomposed by water, a dinitrate is precipitated; if you will try the experiment you will find that it is a trisnitrate.

Before I make any observations on your statements respecting mercury and its compounds, I shall briefly notice some of the inconsistencies and errors of your nomenclature. In p. 233 when treating of carbonate of potash, you say " it is usually termed *subcarbonate* of potassa, as it possesses decided alkaline properties, the acid which it contains not being able to neutralize the alkali; I have hitherto" you continue "adopted the term that is generally made use of in speaking of it, that it might not be mistaken for the bicarbonate, which is called carbonate of potassa by those who term the present compound subcarbonate, but as the nomenclature of the different compounds is regulated by their atomic constitution, I must here use the terms of carbonate and bicarbonate." So soon however, does your determination to be accurate evaporate, that in less than two pages, you tell us that " if the *subcarbonate* [of potash] is required quite pure and perfectly dry, it is prepared most easily from the bicarbonate by fusing it in a platina crucible;" so also in p. 267, *subcarbonate* of potassa is thrice mentioned, and in p. 342 it occurs five times; similar incongruity exists with respect to carbonate and subcarbonate of soda.

Inaccuracies of this description are not confined to the compounds of carbonic acid with potash and with soda; you will find by carefully revising the chapters on the metals and their combinations, that the rules of nomenclature have been exceedingly neglected. In some instances neutral, super and subsalts have been confounded, and in other cases distinctions are inaccurately made. You have also frequently omitted to denote the state of oxidation of the base in metallic salts, so that the student will be much embarrassed in his researches; these observations apply particularly to the saline compounds of mercury, on which I now proceed to offer some remarks.

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 bason. 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."



As you have stated the composition of the precipitated salt positively, I do not see why you should have hesitated, as to the nature of that remaining in solution; for unless you can conceive sulphuric acid or peroxide of mercury to evaporate, you must be right in supposing the soluble salt to consist of three atoms of acid and one atom of base.

In order however, to investigate the accuracy of these statements, I put 200 grains of bipersulphate of mercury into about a quart of cold water; the yellow sulphate precipitated weighed 141.1 grains; the solution was then heated, by which 8.4 grains more were obtained, and afterwards sulphuretted hydrogen threw down 14.5 grains of bisulphuret of mercury; but if your diagram were correct, I should have obtained less than 87 grains of yellow sulphate, instead of nearly 150, and more than 78 grains of bisulphuret, in the place of only 14.5.

To ascertain the composition of the precipitated yellow sulphate, I boiled 100 grains in a solution of soda; the peroxide of mercury separated weighed 86.9 grains; to the solution, after supersaturation with muriatic acid, muriate of barytes was added, and 37.3 of sulphate were precipitated, equivalent to 12.6 of sulphuric acid.

One hundred grains yielded therefore of

Sulphuric acid	12.6
Peroxide of mercury	86.9
	99.5
Loss	.5
	100.0

I consider this salt as constituted of

Three atoms of sulphuric acid ..	(40 × 3) =	120	or 12.2
Four atoms of peroxide of mercury	(216 × 4) =	864	87.8
		984	100.0

This is so unusual an atomic constitution, that I have not admitted its existence until after many analyses.

In this case of decompositions there is much which calls for animadversion: you term the salt subjected to the action of the water, persulphate of mercury, but as it consists of 2 atoms acid + 1 atom peroxide, its proper appellation is bipersulphate; the salt precipitated by the water you represent as consisting of 1 atom acid + 1 atom base, and you denominate it a subsulphate, which means a subprotosulphate, but in fact it is neither what you represent it to be, nor what you have called it, and as the weight obtained is erroneously stated, you have erred as to the name, nature and quantity of the salt in question.

The sulphuric acid and peroxide of mercury remaining in solution, constitute what you call a supersulphate, which is an indefinite supersalt of the protoxide, but you represent it to be a definite supersalt of the peroxide, or a terpersulphate; now if it were so, the acid would be to the base as 120 to 216, but they are to each other as 120 to 50; the actual quantities, as you will find by calculation being, very nearly, 36 grains of acid and 15 grains of peroxide; in short, your diagram, representing the decomposition of bipersulphate of mercury by water, is a tissue of errors; it describes changes which do not occur, compounds that have no existence, and salts, which, whether real or imaginary, are all incorrectly named.

You also give a diagram which shows that carbonate of mercury may be formed by decomposing protonitrate of mercury with carbonate of potash, here incorrectly called by you *sub*carbonate; I have repeatedly endeavoured to procure this salt, but always without success, and my belief is, that no such compound as dry protocarbonate of mercury can be formed; the precipitate at first thrown down is of a yellowish colour, but when excess of carbonate of potash has been used, it becomes eventually as black as that formed by caustic potash. The yellowish precipitate at first formed, appears to be merely subprotonitrate of mercury; and although the black precipitate contains, I believe, carbonic acid, it is lost by drying, even in the air. I dissolved 200 grains of it in a weighed vial of dilute nitric acid, the loss of weight, which was evidently one of manipulation, was only 6-10ths of a grain. This affords another instance of your want of "precise observation and personal experience;" I must confess however, that it does not belong to "the mass of matter which could not be restated from former authorities," for Dr. Thomson in his "*Attempt, &c.*" vol. ii. p. 397, mentions a white sesquiprotocarbonate of mercury, obtained by precipitating a solution of nitrate of mercury by carbonate of soda.

I have already shown that mercury has been a fruitful source of error to you, yet other mistakes on the same subject remain to be noticed. You instruct us that percarbonate of mercury is formed when a solution of *sub*carbonate of potash is added to one of corrosive sublimate, and you have with a profusion of parade employed two diagrams to illustrate one error. In this case also Berthollet would have informed you better; he states (Mémoires d'Arcueil, tome iii. p. 80) that the precipitate contains muriatic acid, but no carbonic acid whatever; and you will find the same observation made by a certain "doughty critic" in the Philosophical Magazine for February, 1830. I must do you the justice to admit your having mentioned that pernitrate of mercury as well as corrosive sublimate, will yield percarbonate of mercury: I added a solution of the pernitrate to carbonate of soda; the precipitate was of an ochre yellow colour; when dried by exposure to the air, it lost 4.4 per cent. by dissolving in dilute nitric acid, and by decomposing the solution with soda, it gave 96.1 of peroxide. This salt therefore, consists of,

Two atoms of peroxide of mercury $(216 \times 2) = 432$ or 95.2
One atom of carbonic acid .... $= 22$ or 4.8
454 100.0

Such then are your statements respecting the carbonates of mercury; the protocarbonate although illustrated by a diagram does not exist,—the percarbonate though elucidated by two diagrams cannot be formed in the mode represented, and when it is obtained by another process, its composition is totally different from that which you have assigned to it.

To discover corrosive sublimate in calomel, you direct the latter to be boiled in a solution of muriate of ammonia, and you state correctly, that if any bichloride of mercury be mixed with the calomel, it will be dissolved, and a white precipitate obtained on the addition of potash; but you do not however, seem to be aware of the fact mentioned by Mr. Hennell (Quarterly Journal, vol. 18, p. 295) that when calomel is boiled in a solution of muriate of ammonia, it may totally be decomposed into mercury and corrosive sublimate, and consequently the test cannot be relied upon, since it occasions the formation of the compound, the presence of which it is intended to detect.

There are some statements contained in the tables of weights and measures given in the *Elements*, upon which I shall make a few remarks.

The *Table showing the weight in grains of various measures of different fluids*, contains numerous inaccuracies. In the first place you do not mention whether the pint is ale, imperial or wine; I presume however, it is the last mentioned, because 7305 grains, the weight of water which it is stated to contain, comes nearest to it. This statement is however, incorrect, as will readily appear according to the data furnished by yourself; as 277.274 cubic inches of water weigh 70000 grains, the eighth part of 231, or the wine pint, must contain 7290 grains within a fraction of a grain; this table consequently exhibits many errors, of which this may be regarded as the source. But the most remarkable oversight which you have committed, is in stating the sp. gr. of muriatic acid gas compared with atmospheric air, instead of that of liquid muriatic acid compared with water, and the error is carried on from the weight of a pint, to that of a fluidounce, a fluidrachm, and down even to that of a minim of muriatic acid gas.

I have now, Sir, concluded, although not for want of matter to continue it, the re-examination of your *Elements of Practical Chemistry*. In my original review, I was satisfied with noticing what I considered to be the inaccuracies of one chapter only; in thus limiting my remarks you think that I acted unfairly towards you; this cause of complaint I have now removed, and yet I am apprehensive, that you will not altogether approve of my additional observations.

You have appealed to the public, and expressed your willingness that your book should stand or fall by their decision; I also am perfectly content that the same tribunal should determine, which of us has substituted words for facts, or as you express it, " mere audacity of assertion for proof."

In the *Exposure* you have asserted, that my review of your *Elements of Chemistry* originated in personal hostility; but you must be perfectly conscious, that no such motive could have actuated me, unless you can conceive enmity to arise without an acquaintance with the object of it. Until your name appeared as the author of the work in question, it was unknown to me, and I am not even now aware of its previous association with science. Under these circumstances, allow me to recommend to your notice, the following passage from a work of one of the most eminent philosophers of the present day:—" There exists," says Mr. Babbage *(Reflections on the decline of Science in England*, p. x.*)* " amongst the lower ranks of science, a '*genus irritabile*,' who are disposed to argue that every criticism is personal. It is clearly the interest of all who fear inquiries, to push this principle as far as possible, whilst those whose sole object is truth, can have no apprehensions from the severest scrutiny. There are few circumstances which so strongly distinguish the philosopher, as the calmness with which he can reply to criticisms he may think undeservedly severe."

I am, Sir,

Your obedient Servant,

RICHARD PHILLIPS.

Birmingham, 15th Aug. 1831.

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