

A refutation of the remarks on the Institutes of experimental chemistry: in a letter addressed to the members of the Society for the Encouragement of Arts, Manufactures, and Commerce / [Robert Dossie].

Contributors

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REFUTATION

OF THE

REMARKS

ON THE

INSTITUTES OF

Experimental Chemistry;

By Robert Dumas

IN A LETTER

Addressed to the Members of the Society for the Encouragement of Arts, Manufactures, and Commerce.

*“ Ut jam nunc dicat; jam nunc debentia dici
“ pleraque differat, & præsens in tempus omittat.”*

HORAT. Art. Poët.

LONDON,

Printed for J. Nourse in the Strand. 1760.

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TO THE
MEMBERS of the SOCIETY

FOR THE

Encouragement of Arts, Manufactures,
and Commerce.

Gentlemen,

I Beg leave to submit to your consideration this defence of myself and my writings, from an attempt to disparage and depreciate the one through the other: and, by that means, to lessen my pretensions, as a candidate, to the office of principal secretary to your society. I do not, therefore, lay the subject of this letter before you meerly in a literary light; which possibly might not appear so pertinent to you: but as the occasion of the dispute in question arises solely from my being a candidate to an office in your election, and the ground of it is an attempt to influence your opinion of me in relation to that concern, I hope the addressing myself to you in this view, will not be thought improper. To you, as the most fit tribunal, I consequently appeal, with the greatest confidence in your justice and candour. The same spirit of benevolence and patriotism, that links you together as a society, solely in the design of advancing the interest of your country, will, I doubt not, operate also, relatively to a private case, in the breasts of each individual;

and inspire a due sense of equity to decide against, and concern to redress, the injury I complain of. Every honest man must be offended at the appearance of *bad* designs: and every gentleman must be disgusted with those means, for the prosecution of *any* design, that are *insidious* and *deceptive*.

The particular subject, which occasions this address to you, is an attack made on my character, with respect to abilities, in a pamphlet intitled, *Remarks on Mr. Robert Dossie's Institutes of experimental chemistry*: a work some time ago published by me: which, together with two other treatises of my composition, are there censured and condemned, in the most outrageous manner, under the sanction of pretended reasons, that are, for the most part, founded on false representations, and misquotations of what I have advanced. In order, however, to set the state of the case in a clear light, it is previously requisite, to shew under what circumstances, and in what manner, with relation both to these works and myself, this injurious piece of criticism was produced: that the motives of its publication being more evident, it may the more obviously appear, what weight and authority the facts and doctrines composing it ought to have with those, who are not qualified, by a prior attention to this kind of subjects, to judge with certainty of particulars.

The *Elaboratory* laid open, and the *Handmaid to the Arts*, were both published in the year 1758, and the *Institutes of Experimental Chemistry*
about

about nine months ago. They were all given to the world without having any name prefixed to them; or bearing any other mark by which I might be known to be their author. The reason of this concealment, did not however arise from any consciousness, that they contained what I ought to be ashamed of; but from my conceiving that they might equally well answer the end of their publication, which was, the propagating the knowledge of the subjects they treated of, without being the acknowledged work of any particular person: and that I might by this means avoid any hazard of being under a defence of my own character, in relation to them, from the censure and objections, that either prejudice against the opinions advanced in them, or personal dislike to myself, might give rise to. If they had merit to claim the approbation of the world, I presumed that merit would sooner or later procure it: but as it related to myself, I had little solicitude about the event; as I had not then any particular views, in which it could be of much consequence to me. The public was, however, pleased to be favourable to them far beyond my expectations. All the English writers of Reviews, and other such periodical works of criticism, had the candour and generosity to recommend them in the strongest manner. The authors of a foreign work of a parallel kind, were also equally indulgent to one of them: and I had many concurrent testimonies of a private nature, of their being as well received as I could wish, by those, to the assistance of

whose studies, or practice, they were particularly intended. I flatter myself, from all these circumstances, they would still have enjoyed the same advantages, had their author been yet unknown; and that they would have remained uncensured, had they continued anonymous. But an occurrence, of which you are well apprized, induced me lately to declare myself the author of them: and afforded the same motives to others to censure, or rather abuse them, as to myself to claim them. Your society thought proper to institute the office of a principal secretary; and many of my friends intimated their opinions to me, that they thought me a fit person to fill it; and urged me to offer myself a candidate: to which having consented, it was thought necessary, that I should avow such of the works I had published, as had any relation to those subjects, which make the object of your society's consideration; as you might from thence have the most certain method of judging of my qualifications. This was more particularly expedient; because one of them, the **HANDMAID TO THE ARTS**, treated expressly and solely of those matters, which make the object of your care and encouragement; and was published with a design so correspondent to yours, that I dedicated it to you; and pointed out, in the preface, some very strong political reasons for the institution of such a society; which had not, to my knowledge, been before offered to the public. In making this use of the works I had published, I employed none of those artifices,

fices, modernly called *puffs*, in order to prepossess; or even draw your attention to them. My name was only put to the advertisement of the books themselves; and not the least advantage was offered to be taken of them in the others, where I declared myself a candidate: there was indeed but one repetition of that of the books after the others were inserted; which was by accident only, and in a different part of the paper. I meerly signified there, that I offered myself a candidate for that office, without intimating any superiority of pretensions I had over my competitors, either from qualifications of this kind, or from my not being engaged in any other employ: as I thought, it might seem to intimate invidious comparisons: though from the ungenteel treatment which I have since met with, I find my delicacy in this point might well have been spared. But no sooner was my name put to these works, than the most strenuous attempts were made to disparage them, in order to prevent the effect they might have, in pointing me out as a proper person to be the secretary then in question. In order to this, a junto of persons, induced by various motives of passion or interest, whose names I shall spare the exposing here, set themselves down in judgment on the INSTITUTES. But not being however well acquainted with the subject, nor, as will appear from their performance, when carefully examined, very acute in their general conception of philosophic matters, they failed to find an adequate number of real errors and inaccuracies to answer their purpose of shewing

the work defective in the degree sought for. In default, therefore, of sufficient ground of censure from just remarks, they had recourse to artifice; and by blundering over, and mistaking the sense of some passages, and designedly misrepresenting others, together with positive contradictions of demonstrable truths, and unfair censures of the general design and manner of execution of the book unsupported by any assigned reasons, they formed a system of abuse and impertinence, calculated to impose on, and prepossess, such as had either not read the work: or, from their being little conversant in subjects of this kind, were not adequate judges of the points in question. This they reduced to the form of a pamphlet, in the manner of a *letter addressed to the authors of the Review*, &c. under the pretence of reproving them for their false accounts of my works: avoiding, nevertheless, to use the *plural* of the word *review*, lest it should appear, that more than one had, by their approbation, been guilty of the crime that called for this severe reprehension; though in fact all the writers of the periodical works of criticism, then subsisting, had been equally culpable in this point. But before this work was completed, part of it was shewn in manuscript as discoveries of my ignorance, or want of veracity, made by a gentleman who had really, by experimental examination, found my assertions to be false: and the warm friends of the other candidates, as well as those of him on whose account it was written, were not idle in taking
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advantage of it, by instilling prejudices against me on this score. The pamphlet was, nevertheless, in due time printed : but, as an open publication was not the best method of rendering it effectual to its intended end, it was for some time only shewn privately to those whom it was designed to mislead. For had it been published before the end of the instant month, the review writers, in their own justification, might have shewn it in its true colours ; and prevented, by that means, the effect hoped for from it. In due time, however, when that danger was over, it was published, if the once advertising it in the public papers can be called so : but as this was for appearance only, and the members of your society, or their friends, and not the world in general, were intended to be the readers of this work, it was judged a more effectual method to disperse the copies, by giving them away gratis, than to trust to their sale in pamphlet-shops. Accordingly they were sent to the houses of some gentlemen, and given to others at the society's meetings ; and every opportunity was taken by the junto, and those who could be induced to concur in their measures, to speak of this rhapsody of nonsense, as containing a full demonstration, that the *Institutes of Chemistry* was a weak, absurd, and contemptible performance ; and, to use the words of the president of the junto, " that the author was no chemist." As this manner of proceeding is too gross to have any effect, after it is in the least understood, and the contents of the pamphlet

too

absurd to impose on those whose taste for the subject, to which it relates, would incite them to take the pains of comparing it with the book, I should at any other time have treated such an attack with contempt and neglect : But as my being a candidate for an office in your society draws the attention of many on me, who not being conversant in these matters, would be otherwise inattentive to them, the momentary impression which may be made on some of the members, by the confident assertions, and imposing tone of the writers, becomes of considerable consequence ; and lays me under a necessity of justifying my work, and preventing even the temporary effects. Since before just notions of this affair can be propagated in private conversation, by those, who being really adequate judges, may set the matter in a true light, the injurious consequences intended, would have taken place in the influence, the perverted notions of some members of your society might have on the election. It was therefore incumbent on me to give some answer to these fallacious remarks : and to point out, by such means as might have tendency to convince even those not versed in these matters, what the true spirit and intention were, that animated the writers to undertake this work ; and what the means were by which they have proceeded in the execution of it. In order to this, I shall therefore principally confine my observations to such points as do not require the knowledge of abstract principles, nor an acquaintance with nice
and

and complex experiments, to the forming a decisive judgment: and these consequently are such as regard rather the conduct and manner of their work, and their unfair treatment of mine with respect to false quotations, and perversion of the sense, than the discussion of particular doctrines, or examination of facts. Though I shall not omit to touch on some of the more glaring instances of their absurdity and want of veracity, even with respect to those. In the performance of this, to give a more conclusive view of their failure in what they pretend in either way, I shall not follow them through each article, according to the order in which it stands in the pamphlet; but first consider the manner of their work, and what they have said against the general plan and conduct of mine; and then descend to some few of the most notorious particulars.

The singularity of the title, *Remarks on Mr. Robert Dossie's Institutes of experimental chemistry*, is the first thing which strikes the reader with some degree of surprize; and presents, when duly attended to, a sufficient proof of the intention of the book. The unusual manner of putting the author's name, as well the christian as surname, before that of his work, may seem at first only a contemptuous insult; but, on reflection, it will soon be found to have another design also. By rendering the name so conspicuous, the attention of the members of the society, who pass by the windows of shops, or other places where it is put in view, is drawn to the
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pamphlet : as it indicates to them, that the contents of the book, though respecting subjects for which they would otherwise have an absolute indifference, relates to a candidate for their secretaryship : which inspires necessarily a curiosity for further information regarding it ; and they are thus induced to swallow the bait. So that this peculiarity in the title, instead of an impertinence, is, in reality, an artifice truly worthy of the junto who devised it. The form of the work presents likewise a like instance of the low, but shallow, cunning with which it is composed. To write without any seeming reason, but meerly to disparage a person then a candidate for a public office, might have too barefacedly evinced the intentions : A pretence was therefore borrowed from the supposed misbehaviour of the writers of the Reviews, with respect to chemical authors : though part of the criminal transactions, for which they were now so severely reprehended, had gone unpunished for near three years ; and the other part many months ; during all which time the public had been suffered to be imposed on, notwithstanding that great zeal of these writers for truth, which now bursts forth so suddenly and violently. But, according to their own relation, they forbore to interpose sooner ; because they presumed, from the false account the review writers had before given of the *Elaboratory laid open*, and the *Handmaid to the Arts*, they would give a very just one of the *Institutes*. “ *I should have sent you some*

animadversions on them as they made their appearance, if I had not thought yourselves more equal to the task:" see page 17. How clumsy, how inexpert are these writers in their own paltry arts of deception, not to be able to give even a moderate share of plausibility to their pretences, but to blunder, and fall into inconsistencies, as well in their introductory plan, as in almost all the particular subjects of fallacy it was intended to exhibit! These circumstances would be too minute and trivial to touch upon on any other occasion: but at present they are more material, as they display and illustrate the true spirit of the work; and evince the real intention of the writers with equal force, as passages of seemingly more importance.

But to proceed to the particular contents. The first thing that is exhibited by them to our observation, is such an eagerness to condemn, joined, as appears, to a total ignorance in philosophic subjects, as leads our censurers into a most complicated absurdity in their very entrance into criticism. They set out in supposing themselves, or at least intimating to others, that I have offered this work as a "*body of chemical philosophy*," though I have called it *Institutes of experimental chemistry*; and expressly declared, that I have not extended philosophic speculations beyond what related to particulars, further in any part, than was necessary for the understanding the reason of the processes in the experiments. But to this blunder they were led, I suppose, by my mentioning in the
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preface the importance, and, indeed, necessity of such a work, in order to the further improvement and progress of natural knowledge: and intimating my entertaining some thoughts of such an attempt hereafter. When they have however presumed this work, contrary to my own account of it, to be offered as such "*a body of chemical philosophy, founded like the mechanical on general principles,*" they say, "*what he calls general principles, are deduced from particular facts, and by being made general, they are made false.*" Can there be a more evident proof than this extraordinary charge of the total want of abilities of these writers to intermeddle in philosophic matters: or of their unparalleled presumption on the ignorance of their readers? For from whence are general principles in natural philosophy to be collected but from particular facts? Is not this the method, by induction, introduced by my Lord Bacon in the place of the sophistry and verbal systems of the schools, and adopted by Sir Isaac Newton, who lays it down as the sole basis of philosophic knowledge; and by all others who have succeeded in the investigation of physical truths? Whether therefore are we to wonder at, the hardy ignorance of a writer, who censures another at this period, for pursuing a method so obviously right, and universally received: or is this only an artifice to impose on such of the readers, as are not conversant in matters of this kind, in order to answer the particular purpose which this pamphlet was intended to serve?

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Their position, that *principles deduced from particular facts* are made *false* by being made *general*, is equally extraordinary. It seems indeed a contradiction in terms. For the principles themselves are no more than the relations of analogy, which the particular subjects have with respect to facts collectively considered: and, therefore, as far as they are general, they are necessarily true. But I suppose these writers, had they been able to express themselves intelligibly, would have said *universal* instead of *general*, as may be gathered from the instances by which they endeavour to prove what they aim at charging me with; and then indeed had I set up such principles, the charge might have had some foundation: as the analogical link of relation in the properties, as well as forms of particular kinds of bodies, are sometimes interrupted. But I have been so far from setting up *a chemical philosophy founded like the mechanical* on principles that hold good *invariably* and *universally*, that an express passage in my preface, and several others in the body of the work, evince, how cautious I was, not to give room for mistakes of this kind. In the preface I say, in speaking of the nature of that general knowledge we may have of the properties of bodies with respect to their action on each other specifically considered “That the principles of this knowledge *do not extend to all instances with equal certainty, as in the case of the mechanical: but yet hold good in a degree, that makes such an approximation, as answers*

extremely

extremely well in practice, and makes, moreover, a body of speculative science, &c." Preface, page vii: and in page 14 of the work itself also, speaking of the specific attractions of bodies, it is said, "*Most species of bodies, which have so much the same qualities in appearance as to be deemed of the same genus in the general view of nature, have, for the greatest part, the same specific attractions: though this is not without some absolute exceptions to the contrary; and great variations in the degree of almost all from each other.*" Indeed, in every part of the work, it is inculcated, that the generical relations of bodies, which make the principles of the chemical philosophy, are never to be considered as universal: but only as *general, with limitations*; and that the proper object of this science is, the demonstrating the mode of relation which is general: and then noting the several deviations and exceptions from it. Again, in page 4, Vol. I. where I speak of the method of applying the experiments of chemistry to the improvement of natural philosophy, by reducing the subjects to genera and species, I express myself, as will be found, in the most careful terms of limitation.

They endeavour, however, to produce instances of the failure of my attempt to establish principles by experiments: which instances consist partly in false facts, and partly in the mistake or wilful perversion of what I have advanced. Of these false facts, I shall take notice in their proper place: but with respect
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to misrepresentation of the sense; a passage that occurs here merits some notice. I have given a table to shew the subordination of the powers of bodies, of opposite genera, with respect to each other. This table is formed only in that view, to shew the relation the respective species of those two capital genera of bodies, called acids and alkalies, have, with regard to their subordination of attraction, or their power of departure, or dispossession of each other, from any third body of the opposite genus; and not to distinguish which, through the whole species of each genus, will act on any particulars of those of the opposite. But, in order to render the table defective, or faulty, they have confounded the disposition, or want of disposition to attract each other at all, with the comparative degree of force or power of such attraction, which any two may have with respect to a third: and as instances of my mistaking the degrees of attraction, have offered those species, which have attractions with respect to certain others, against those that have none, with respect to the same: as in the case of lead and mercury with the vitriolic acid, and several more. Whereas, there can be no comparison of the degrees of subordination of attraction, or power of dispossession, with regard to a third body, betwixt two others; of which the one will not at all combine with, or attract such third. The facts themselves likewise brought here, respecting these attractions, are most of them false, even considered in any light: as we shall have occasion to see in some

instances below, when we speak of their fallacy in relation to particulars: and were taken on the authority of Geoffroy's table, which is extremely inaccurate, and not on the experimental knowledge of the operative gentleman of the junto, or of another of superior abilities, whose name has been likewise brought publicly in question by them, as having a share in this pamphlet. Though from the multiplicity of errors in point of facts, delivered in a work lately published by him, as from the diary of his own experiments, as also with respect to doctrines, he ought to treat the characters of other writers with tenderness. But these matters are of too nice a disquisition to answer the end of this reply: and I will therefore wave them, and pass on to what is more simple, and generally intelligible; only I would bespeak a little more candour and allowance on any future occasion, for inaccuracies, in large works, that may depend on inadvertencies of expression, from a junto of writers, who, notwithstanding the magisterial tone and judicial character they assume, can, in the second page of so short a piece, according to the literal expression, call a *table* a *doctrine*. "*In the table of attraction (one of the fundamental doctrines of his system) we are taught, &c.*" page 2.

After this attempt to shew, by particular instances, that I have failed in forming such universal principles as never entered into my imagination, these writers venture to advance, by a clear implication, a doctrine of their own: which is, that there are no general principles
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with respect to the specific action of bodies on each other. They say, "*even the deflagration of nitre with inflammable substances, than which nothing seems to bid fairer for a general principle, is, in our author's sense, by no means such: for if the nitre be melted, and a certain proportion of inflammable matters, as antimony freed from a part of its sulphur, be immersed in it, no deflagration will ensue.*" But what can be more weak and absurd, than either the position itself, or the instance by which they endeavour to illustrate it? Is it a consequence, that because the complex relation of a number of things to each other, is not universal, that therefore it is not general; or that because antimony, containing phlogistic matter, may not deflagrate with nitre, supposing it true, that therefore it is not a principle, that phlogistic bodies would deflagrate with nitre, if found, as he allows, to be so, in almost every other instance? Let me ask the *philosophic gentleman* who presides in this junto, if the polar attraction in MAGNETIC bodies ought to be exploded as a principle; because nice observers may discover some local variations in the effects: or, whether the principles of medicine, or other scientific arts, that depend on the powers of nature in minuter systems, stand on a better basis than those I have advanced. But to leave a matter too obvious to require further argument in the general view of it, and turn our eyes to the particular fact my adversaries have unhappily advanced here against the universality of the deflagration of

nitre and phlogistic bodies ; and the use they have made of it against me ; I must take the liberty of saying, that in a philosophic sense, or, as they say, “ *according to mine*,” it is absolutely false ; as well as the application of it to their purpose. I never advanced that the *deflagration*, but the *commenstruation* of phlogistic bodies with nitre, under certain circumstances, was a general principle. For deflagration is no more than a term expressing some sensible effects of the commenstruation of nitre and phlogiston with each other, under those circumstances. And nitre, in many cases, where it is demonstrable from the effect, commenstruates with the phlogiston in compound bodies, without those sensible appearances that are understood by deflagration. Thus particularly, when the quantity of phlogiston in any compound body is so small, that the action is not sufficiently rapid and strong to shew itself in the operation, these sensible marks of action will be wanting, though it be apparent in its effects : as in the very instance produced by our writers ; where there is a considerable explosive effect, while the antimony contains its full proportion of sulphur, or phlogiston ; but, as this effect depends on the phlogiston, it must necessarily be weaker as the proportion of that substance to the other constituents becomes less, till it be no longer sensible. But even then, when there does not remain a sufficient quantity to make any detonation, or explosive appearance ; yet the same action evidently results betwixt the antimony, even in
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this state, and the nitre, on their admixture in a due degree of heat: as is evident from its appearing afterwards perfectly calcined: which shews the action of the nitre on the phlogiston, by the intire separation of it from the metallic earth, with which it was combined in the constitution of the antimony.

The next article consists of an attempt to render my work ridiculous, by applying another passage in the preface to the contents of it; when it is evident, by the subsequent lines, I had no such meaning. This passage is thus.

*“ The author presumes, that he has gone much
 “ farther in the investigation of the general prin-
 “ ciples, on which nature conducts her operations in
 “ the minuter parts of the system, than any writings
 “ already published lead;—and that he has made
 “ several discoveries relating to particular subjects.”*

This they say seems to have an eye to matters in different parts of the work, which they enumerate. But there can be nothing more wanting to shew the unfairness of their proceeding in the treatment of this book, by mutilation of the text, and perversion of the sense, than the quoting the whole of this passage: which is as follows.

*“ Presuming, however, that I have gone
 “ much farther in the investigation of the general
 “ principles, on which nature conducts her operations in the minuter parts of the system, than
 “ any writings already published lead; and that I
 “ have made several material discoveries relating
 “ to particular subjects, which could not be de-*

“ monſtratively communicated, till a more juſt
 “ and regular body of general doctrine than has
 “ been hitherto laid down, were formed ; I
 “ thought it expedient, in order to prevent the
 “ fruits of my labour, ſhould they prove of any
 “ value, from being wholly loſt to the world, to
 “ give, in the mean time, a more compendious
 “ work ; which may be preparatory to, and, in
 “ ſome meaſure, ſubſtitutive for, one more co-
 “ pious and perfect. For ſhould the deſign of a
 “ more complete ſyſtem be at laſt abortive, this
 “ might answer in a leſſer degree the ſame end ;
 “ at leaſt in rendering it practicable to me, to
 “ treat hereafter intelligibly and precisely of thoſe
 “ particular ſubjects, in relation to which, I flatter
 “ myſelf, I am enabled to give new lights that
 “ may be of beneficial conſequence.”

Can it be more explicitly and clearly ſaid,
 that the diſcoveries, here alluded to, were not a
 part of this book ; but intended to be treated of
 hereafter ? To have an opportunity, however, of
 raiſing prejudices againſt ſeveral parts of the book
 collectively, by a deluſive form of cenſuring them
 as pretended diſcoveries, this falſe meaning
 is given to my words. But had I nevertheless
 intimated in this paſſage, that the book did con-
 tain ſeveral diſcoveries, even in very important
 points, as well philoſophic as practical, conſider-
 ing every thing to be ſo that has not already
 been publiſhed in any other work than my
 own, I might perhaps be well juſtified in it :
 but modeſty forbids I ſhould enlarge on this
 point. The doctrines, which they have made
 a part

a part of the detail of those discoveries they treat with contempt, are, however, as far as they are asserted by me, supported by valid arguments in the parts of the work where they are delivered; to which I beg that recourse may be had, by those who are desirous of being satisfied in these points, as they take up too much room to be recited here. As I would not, however, be wanting in candour even to my adversaries, I beg leave to mention what appears to me some excuse for their speaking of them in this light manner. I mean their wanting capacity to understand them; which is evident, by their continual misconception of things, even of a much less complicate nature. I shall refer to the work itself therefore, for the defence of my opinion, concerning the putrid and vital ferments in the blood; which, however it may be a *reverie*, according to these writers' witty turn, is not offered as a *theory*, as they fallaciously intimate; but as an *hypothesis*, which it is expressly called, that well merits consideration. I shall do the same with respect to all those other points of a general nature; where having alledged no reason for their ridicule or censure of them; nor displayed the particular manner of their own mistakes about them, no answer can be given: unless that I hope, what I have said will be found to be well warranted by the arguments, contained in, or annexed to, the very passages themselves; or to be found elsewhere in the book. But as they enlarge a little, in relation to some sup-

posed notions concerning animal and vegetable substances, I will endeavour, as far as the confusion of their manner, and obscurity and want of precision in their expression, will suffer me, to follow them through that part. In speaking of what I have meant in the passage of the preface above quoted, they enumerate among the other things, “ that *I have an eye, to the project for explaining the phænomenon of the animal and vegetable œconomy, from the water, salt, phlogiston, and earth, into which the parts of animals and vegetables are resolved by putrefaction, and by fire.*” If by this they mean, a project to explain *all* the phænomena of the animal and vegetable œconomy on these, or indeed any other, principles, such certainly never entered the imagination of any but themselves. On the contrary, I have observed, in more than one part of the work, that, in the vital œconomy of animals and vegetables, nature acts by peculiar principles, that seem to break through the general analogical relations, which bodies have according to their respective generical properties. But if they mean, that it is ridiculous, to attempt to explain *any* of the phænomena of the animal and vegetable œconomy, from the known properties of water, salt, phlogiston, and earth, the contrary is obvious: as may be seen by many passages of that work. They then proceed, however, to go deeper into this matter; and, in so doing, shew how much they are bewildered whenever they attempt to reason: and prove indeed, what they have be-

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low said, “ *that nothing can be more dangerous in chemistry than the spirit of generalizing,*” to be extremely true, when taken with this limitation, “ *where nature has denied the power.*” The manner in which they thus enlarge on this subject, is as follows. “ *The author himself admits, that by this ultimate resolution or destruction, nearly all the parts are reduced into the same principles. How then can we deduce from those principles, even their gross differences, from one another, much less their different actions in the body of the living animal?*”

What with the defect of the grammatical expression, and what with the inexplicitness of the sense, it is impracticable to settle what they really intend here: whether to deny that all the parts of animals and vegetables are not reducible into the substances enumerated; or that the differences are not owing to the properties of these elementary bodies in the compounds: for either construction seems equally absurd: or do they mean, *by deducing*, explaining the modus of action in every case?

To say that these bodies, adding to them air, or some elastic fluid resembling it, are not all the substances into which the parts of such bodies are reducible, is a contradiction to what is known to all, who are conversant in experiments of this kind: and if these are all the substances, that enter into the constitution of animal and vegetable bodies, from whence can the differences found, be deduced, but from the combination of these bodies; their action on each other; and the new properties generated
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in the respective compounds? But the great difficulty here seems to lie in their ignorance of a most material point, from which they might have been free, had they read the *INSTITUTES* with a better design, than raising idle cavils against what they did not comprehend; which is, that many properties are generated in compound bodies, by the commensural combination of the simple bodies or elements, of which they are constituted, that had no existence in such elements while in their simple state: while others, which are found in the elements, are wholly suppressed in the compounds, as is explained, and demonstratively proved, page 7, and 8, of the *Institutes*. So that, from a few simple and elementary bodies combined in various manners, and proportions, an almost infinite number of compound bodies, may be produced; displaying all those differences that make the object of this objection. But these writers illustrate this extraordinary notion, that the differences of animal and vegetable bodies, are not to be deduced from the properties of the bodies of which they are formed, by this argument. “*The same heat that liquefies the glutinous matter of the animal solids, coagulates that of the fluids: and what water, salt, earth, oil, and phlogiston, can be discovered in the one more than in the other?*” If I understood the application of this query, in any manner consistent with common sense, I would give some answer to it: but it seems to me to imply, that the parts of the solids and fluids, being wholly composed of the same substances, have differences, nevertheless; and that,

that, therefore, these differences must be owing to the properties of something of which they are not composed. But this is equally rational with the passage that follows it: which is, “*the author indeed acknowledges that nature, in many instances, deviates from the laws he has established: or, in other words, that he has himself deviated from the laws established by nature:*” an interpretation truly worthy the candour, and good sense of these writers: and which exhibits, in its true colours, the spirit of the whole. But this is supported in the usual manner by a query; the force of which lies, according to the practice of these writers, in a sophism, formed by the fallacious change of the word *deviation* into *repugnance*. “*How can he pretend to have discovered the principles on which nature conducts her operations, when many of these operations are directly repugnant to the principles he has advanced?*” By the substituting such blunders and misconceptions of the genuine sense, as are found in some parts, and such sophistical commutations of words as are shewn in this and others, the most perfect work might be made to appear inconsistent and absurd.

They afterwards observe, that, according to a misquoted and mutilated part of my preface, I pretend to have settled the distinctions of the genera, and species of several of the kinds of natural substances, which had been either neglected, or unsuccessfully attempted by preceding writers: and by a malicious insinuation, supported by several pretended instances of failure in my attempts, they endeavour to take
away

away all the merit to which this part of the work can have any claim. As I do still insist upon what I have advanced on this head in my preface ; and believe I have reason to be satisfied, in the manner I there declare, with the result of my labours on so important a point, I will not pass over what they have said on this score, without setting in a just light, the absurdity and falsity of it. Their first objection lies against my specification of earths, which I say is “ to be
 “ further incapable of analyzation or decomposition ; insoluble in water ; infusible without vitrification ; incombustible ; fixt in every degree of
 “ culinary heat ; and of a pulverine texture, or at
 “ most to have only such a slight degree of cohesive tenacity, as renders them very friable.” These qualities, taken collectively, give the true specification of earths ; and if any body in its *natural state* want any of them, though it possess all the others, the absence of such quality affords an equally sufficient mark of discrimination, or difference from, those of any other genus. In order, however, to shew this specification of earths to be faulty ; and that it was applicable to bodies not to be deemed of the same, with any propriety, but belonging to another genus, they first made a false quotation, by suppressing the preceding words, particularly *very*, and saying only *friable*. And then affirm, that, according to this specification, POWDERED platina is an earth. But could any person, except themselves, have offered so ridiculous and weak a proof of the imperfection of my definition. The design, in laying
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down the generical characteristics, was to shew, what the apparent and interior qualities were, which the several species of each respective genus *have in common*, and which constitute them of such genus. It is, therefore, such qualities only as are *natural*, and inherent in the *proper and general form* of the bodies that can come in question. But, in order to shew, that a body allowedly belonging to another genus, will fall under this description, and thence prove that this definition fails in excluding all others, these ingenious writers practice an artificial operation. They *powder* the platina: and, when it is made to vary greatly from its *natural* form with relation to texture, because it happens to coincide, in respect to other properties, with earths, it is brought as a proof of its being an absurdity to say, that bodies, which in their natural state are not tenacious and cohesive, but pulverine and very friable, are specifically different from those which are of contrary texture. But even with the medium of this confusion of all principles, the matter would not turn out right without the usual aid of misquotation: for, if the words *very*, and those preceding, had not been taken away from friable, the difference betwixt platina and earths would still have been striking; but in this, and most other instances, we see artifice joined to blunder. The choice of platina does not, however, seem very lucky for this purpose. For it happens to recede greatly, in its texture, from earths: and to be so far from being very friable, that

that it is, according to those who have described it, not only very hard, but even, in some degree, malleable, when pure. I imagine, therefore, that some of the apyrous stones, which according to my doctrine, vary generically from earths, would have answered the purpose better with the help of *powdering*; but that operation would be still necessary. One may reasonably conclude from this passage, that these writers have cultivated sophistry much more than science: otherwise they could scarcely have fallen into so gross an oversight, as not to be aware, that bodies deprived of their natural qualities, could no more be proper objects of generical relation to each other, in a philosophic view, than those deprived of certain parts or members, could of such as make the principle of the distinction in natural history. LINNÆUS, in his system of distribution, has made the generical characters of beasts, in some cases, depend on the form and number of teeth. Had he unhappily fallen under the criticism of my acute adversaries, they would have supposed the teeth knocked out of some kinds: and where would then have been the difference of these, from those which had naturally fewer: and yet this operation, or that of cutting off ears, or tails, in the case of Linnæus, are equally allowable with the *powdering* the platina in mine? They are not content, nevertheless, with impeaching my definition of earths, by endeavouring to shew, that it does not exclude platina, a metallic body; but they say further, that it does exclude *chalk*, and all the *calcareous*

rious earths. They have not, however, given their reasons for this: and, I will venture to say, either have none, even in their own imagination; or have made some other blunder like that of powdering the platina: and, if this latter supposition be true, and I may take the liberty of guessing where the trip has been made; it lies in confounding calcarious stones with calcarious earths: and ascribing to them a failure, in one of the characteristics of my specification, from the default of incombustibility.

Another charge of this kind is brought in the case of gummous and sulphureous substances of vegetables; which I have distinguished as two different genera; but which, these writers say, do not differ according to the principles of specification, that I have laid down. They insinuate in this view, that I have made the distinguishing character of the sulphureous to be, that "*they flame in a certain heat,*" and, they say, "*how does this distinguish them from the gummous, of which it is also said, that in a certain degree of heat they will flame and glow?*" The whole of this seeming defect lies in false quotation: and is, therefore,, best removed by giving the respective passages, as they stand in my work. In the specification of the gummous substances, I say, page 18. Vol. II. "gums are not of so sulphureous a nature as to burn through accension, without being previously decomposed by heat; for, as they contain no essential oil, they cannot be accended till they be burnt black: that is, till their constituent oil
" be

“ be rendered ethereal by the action of heat : ”
 and in the next, page 15, Vol. II : “ On their being
 “ subjected to heat with access of air, they do not
 “ (as was observed before) *accend* or *take fire*, as
 “ resins, till they be, in some degree, *decompounded*
 “ *by the action of heat* : and then, after flaming
 “ moderately, and glowing for a *skort time*, *ashes*
 “ *are left, which, besides the earth, contain lixivi-*
 “ *ate salts.*” In the specification of sulphureous
 substances, page 21, Vol. II. I make use of these
 words, “ all such substances as may be deemed of a
 “ sulphureous nature have, for their essential cha-
 “ racter, that they will, when heated to a certain
 “ degree with the access of air, burn from the
 “ heat *generated in themselves*, till their whole
 “ *substance be consumed or dissipated*, leaving either
 “ *none*, or but a *very small quantity, of ashes or*
 “ *recrement.*” It appears, on comparing these
 quotations, that the specific difference betwixt
 gums and sulphureous bodies, besides some
 other variations shewn in other passages, con-
 sists in two points ; the one, that gums will
 not accend or take fire, till their nature be
 changed by the action of exterior heat ; and
 the other, that when they have suffered the ut-
 most effect of heat or fire, there remains not
 only earth, but lixiviate salt : whereas sulphu-
 reous bodies will accend, without previous de-
 composition through the action of heat ; and will
 support, by the pabulum contained in them-
 selves, a burning state, till their whole sub-
 stance be dissipated, or only a small quantity of
 recrement remain.

Are not these sufficient criterions of the difference of the two genera? but these writers, by a suppression of one part, and a change of expression in the other, have most shamefully perverted the clear and obvious sense, to prove that my distinctions of genus and species are imperfect. I hope, nevertheless, that what I have delivered of that kind is so founded on truth and nature, that it will bear any test: at least I may justly presume, from these poor efforts, that I may remain secure from having the weakness of it exposed by men of the level of genius of my present critical adversaries.

The plan of my work is likewise furiously attacked; and it is said, that "*however well a work conducted on it might be executed, it would not be a system of chemical philosophy; for surely it is not the business of chemical philosophy to mould common facts into the form of processes.*" As these ingenious writers here, as well as before, have made this work what it was never called by myself, nor intended to be, "a body of chemical philosophy;" they have a right to treat it in that view just as they please: and the reason they offer for condemning it when taken in such a light, that "it is not the business of chemical philosophy to mould common facts into the form of processes," may be as good as any other; for blunders are most suitably defended by nonsense. They subjoin accordingly, that "*the point ought rather to be, to deliver the simple truth, divested of that insignificant parade.*" Of the manner of

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doing which, they have given an excellent example throughout the whole of their own performance: the simple truth being every where delivered, according to the genuine sense of my expression, in all their quotations, without *moulding the body into new forms till the spirit be evaporated* (if I may borrow an expression used by themselves); or making any parade of deep judgment and sagacity, in discovering what never existed but in their own sophistical imaginations. They give, however, one more extraordinary observation on my plan, that "*if the system were to be completed, it would extend to several scores of volumes.*" By which, if they mean, that if every thing that might be properly investigated by experimental chemistry were to be fully examined and recited, so many volumes might be filled, I am willing to allow it; but is it an inference, that because there is great extent of subject for discovery, and a great multiplicity of facts already known, no further improvement should be, therefore, attempted; nor any collection or digest of the most useful part made? It were equally reasonable to affirm, that because a man cannot know every thing, he should therefore learn nothing.

The directions for the construction of the apparatus, and the conduct of the operations, are in the most unfair way censured, as being all wrong and improper; and I am moreover condemned for saying, that no directions had been hitherto given, for the completely forming and furnishing an elaboratory for experimental purposes:

purposes: which they intimate to be absurd, on account of the attempts of Glauber, Vigani, Becher, and Dr. Shaw. Whoever knows any thing of the history of that art, must laugh at the three first instances: as they were authors who wrote while the practice of that art was in a most crude and uncultivated state. He might certainly have much better opposed to me some of the late writers, who have had the advantage of the modern great improvements. With respect to Dr. Shaw, I never knew that he had given to the world any thing offered as a complete system of instructions, for the accommodating the utensils and instruments of chemistry solely to experimental purposes; or attempted to teach in what manner an elaboratory for speculative uses only should be furnished: though he has indeed published some accounts of particular parts of such an apparatus, to which I have always ascribed the merit due to them. But after all the defect and failure on this head, which I am charged with by these writers, it seems my capital error lies in the saying, that Windsor loam and Sturbridge clay may be substituted for each other, where only one can be obtained. "*For who that is in the least conversant in chemical experiments, could think, for instance, of taking Windsor loam and Sturbridge clay as equivalent to one another, whether for lutes, furnaces, or vessels.*" I never said they were equivalent to each other for all purposes: but I advised, what is really practised, that where one could not be obtained, the other

might be taken in its place; and the peculiar properties they have in common, and in which they differ from other clays, viz. the being cohesive while in a moist state, and unvitriifiable when exposed to a violent heat, fit them more to be substituted for each other, than any of the substances of the same kind that are to be generally procured. To the end of the last quoted passage they subjoin, "*or of coating retorts for a sand-heat*"; which taken with the preceding part of the paragraph is, "for who, that is in the least conversant with chemical experiments, could think, for instance, of coating retorts for a sand-heat?" Another attempt to shew I was not versed in experiment: but no reference is made to the page where they suppose this is said; and if it be not meant as a delusive falsity, as the omission of the reference usually made by them reasonably suggests, it must be a blundering construction of this passage, where I expressly say the contrary. "The greatest part of the distillations in retorts may be made in a sand-heat, which indeed is most suitable to them; but where a very intense degree of heat is required, the retort being first *coated with a proper lute*, must be hung in the *open* furnace." Correspondent directions are given in other places: so that there is not the least foundation for the surmise, that I have been mistaken in this point, either from the want of experience, or any other cause. I thought this vindication of myself from the insinuations, that I treated idly and vainly of subjects of which I had no distinct

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knowledge, and where the adequate means of information had been wanting to me, to be necessary, with respect to those persons who had not looked into my writings ; or were not, from a previous knowledge, judges in this case. But to those in the least acquainted with subjects of this kind, who have looked into the *ELABORATORY LAID OPEN*, and the *HANDMAID TO THE ARTS*, where a great variety of chemical subjects are practically treated of, must perceive that it is not from the want of an application to experiment in a very extensive view, that defects of this kind in my works, when such are found, owe their cause. Having thus endeavoured to justify the plan and execution of the *INSTITUTES* from the imputations and censures of these writers in a more general view, I will proceed to do the same by particular parts. But not to be tediously voluminous, by wading through so long a tract of fallacy and absurdity. I will select a proper number of instances of the want of veracity, or intelligence, of these writers : which, in order to the more effectually displaying them in their true colours, I will distinguish into two classes. *MISQUOTATIONS* and *MISREPRESENTATIONS*, or *PERVERSIONS OF THE SENSE*, and *FALSE FACTS* or *BLUNDERS*.

With relation to the first class, one of the most notorious instances is found in this passage, page 8. of the *REMARKS* ; where they make me say directly the contrary of what I have affirmed, in the clearest manner of ex-

pression, in several parts of the work: referring most confidently to one of them. The passage is, “*that calx of bismuth is not vitrescible, whereas there is no calx, except, perhaps, that of lead, which vitrifies so easily.*” So far from denying that calx of bismuth is not vitrescible, I say, page 264. Vol. II. that “being acted upon by a strong fire, bismuth may be sublimed in flowers: and with a less degree duly continued, converted first into a CALX, and afterwards into a VITREOUS body:” And in the passage referred to by them, page 132. Vol. I. INSTITUTES, “the calces of metals are *vitriifiable* in very various degrees of heat; those of tin, antimony, and *bismuth*, are very refractory: those of copper and iron, of a more yielding disposition; and that of lead extremely prone to the vitrefactive change.” In both these passages it is positively affirmed, that bismuth is *vitrescible*; and though there is some variation betwixt what they and myself have asserted with relation to the degree, comparatively with the calces of some other metals; yet whoever will try the experiment, will find it more easy to convert copper and iron, than bismuth, into glass. How base, how illiberal, for persons who assume the character of gentlemen, to endeavour to work their ends by such fallacies!

Another extraordinary instance of a glaring falsity is found in page 6 of the REMARKS, where it is said that in page 310, Vol. I. INSTITUTES, I make the spiritus marinus coagulatus to be the same with sea-salt: whereas the expression I use is this. “If spirit of salt be combined
“ with

“ with lixivate salt, a neutral salt is produced
 “ GREATLY RESEMBLING sea-salt.” Surely the
 words GREATLY RESEMBLING do not imply
 them to be the same, but different: though this
 resemblance is extremely great.

In page 8, REMARKS, I am made to say, “*that*
 “ *the pigment, called Prussian, is in its whole sub-*
 “ *stance no other than a blue fixt animal sulphur,*”
 whereas in the very passage referred to, my words
 are; speaking of the fixt sulphur of animal sub-
 stances; “ It is this sulphur which is the ting-
 “ ing matter of the pigment, used in painting,
 “ called Prussian blue: and which indeed being
 “ combined with the earth of alum, forms with it
 “ the whole of that substance, when genuinely and
 “ rightly prepared.” Page 397, INSTITUTES.
 Is this asserting that the fixt animal sulphur is
 the whole substance of Prussian blue, when I
 expressly say, such sulphur is only the tinging
 matter, and that it contains, besides, the earth of
 alum; which is indeed the proper basis of the
 pigment? But this is too gross to need a comment.

In page 8, REMARKS, it is in like manner re-
 presented I have said, in page 377, INSTITUTES,
 “ *that the conversion of iron into steel depends on*
 “ *the expulsion of mineral sulphur.*” I do not,
 indeed, there say directly the contrary; but
 something so different, that it makes their asser-
 tion most notoriously false. My words are “ the
 “ principle of the conversion of malleable iron into
 “ steel by cementation, is solely the EXCHANGE of
 “ the mineral sulphur remaining in the iron for a
 “ purer kind, attracted from the coal in the ce-
 C 4 “ ment.”

“ment.” Certainly there is a great difference betwixt the *expulsion*, a term that does not in any manner occur in the passage, and “the *exchange* of one kind of sulphur for another:” nor can I consider this, and some other similar instances of such flagrant misquotation, as mistakes, but as designed fallacies intended, by the sacrifice of all truth and honour, to serve very bad purposes of envy and interest.

There is another misrepresentation, page 3, REMARKS, of what I have said with respect to the fusion of nitre. “But the place in which it is to be found, is not referred to: for, unluckily, it is not in this work, as I imagine, but in the ELABORATORY LAID OPEN. It is, however, in this passage charged upon me, that “*depending on the universality of the principle (that nitre will deflagrate with inflammable substances) I ASSERT, that nitre cannot be melted in vessels made of the deflagrable metals.*” I suppose by melting is meant fusion: for, in common use, it means, either the liquefaction by heat, or solution in fluids: and these writers, who seem to understand the language of chemistry as little as the facts, want precision in the terms to such a degree, that it is not easy to fix the sense of their words, so as to form a proper answer to what they advance. But presuming from the context, that fusion is intended by melting, I do not conceive that ever I asserted, in any manner, that “*nitre could not be so treated in vessels made of the deflagrable metals.*” The only place where I can find that I have had occasion to touch on the fusion of nitre in metalline vessels, is in
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the ELABORATORY LAID OPEN, page 181, where I caution against the fusing nitre in the preparation of the sal prunellæ in vessels of iron, on account of their deflagrating power; and surely this is neither absurd, nor conveys any falsity, to say that deflagrable metals will deflagrate in those circumstances where that property necessarily takes place; and, consequently, that the vessels, destructible by fused nitre, will be subject to be destroyed by it, in the operation in question. There are many other similar misquotations, and attempts to misrepresent the sense of what I have advanced; but these may suffice to shew the veracity and fairness of these writers in these points. There is, however, one that, for particular reasons, I beg leave to take notice of: which is what they intimate, page 14 and 15, REMARKS, of my misrepresenting and mistaking Dr. Lewis's experiments on platina; and illustrate thus by a pretended instance.

“ *He describes, for example, a process for separating platina from gold by dissolving the compound in aqua regia, precipitating with fixt alkali, and washing the compound. The doctor's experiment, from whence this process is deduced by our author, proves that the platina cannot be separated by this means.*” I am charged with mistaking and misrepresenting Doctor Lewis's experiments; and, in support of this accusation, an instance is produced of my forming a process from one of his experiments, where it does not in the least appear, that I have deviated in any circumstance from that experiment. But

I have

I have, it is intimated, applied this process to a purpose, which Doctor Lewis's experiment shews it cannot answer. Certainly, however, this is not either mistaking or misrepresenting Doctor Lewis's experiments; but being mistaken myself with respect to the conclusions, I have drawn from the facts evinced by the experiments. I must, however, tell these peremptory writers, notwithstanding their assertion, *that Dr. Lewis's experiment proves that gold cannot be separated from platina by this means*, the contrary seems true from the experiment itself; and the principle, on which I found this opinion, is so obvious, that any one may comprehend it. Gold being mixt with platina, and the compound being dissolved in aqua regia, on the addition of alkaline salts, both the metals are separated from that menstruum, and fall to the bottom of the vessel, in the form of a magistery or precipitated powder. So far there appears no means of the separation of the gold from the platina. But the platina being in this state soluble in water: if repeated quantities of that fluid be added, and then poured off, the platina being redissolved in it, will, by degrees, be thus separated from the gold; which the water cannot in the least dissolve: and it seems, that from thence a method must result, which might be applied to the separating these metallic bodies from each other, in a more gross way, where great quantities may come in question. It is true, Doctor Lewis did not suggest this method: mentioning only the facts on which it was founded. But it is neither *mistake* nor *misrepresentation*.

sentation of what has been given by him, to make a practical application of a principle deduced from the facts he has related; tho' neglected by himself. I shall now proceed to the other class, and exhibit a few instances of the more egregious BLUNDERS and FALSE FACTS respecting particulars.

In page 3, REMARKS, he says, that I have made "*the attraction of fixt alkalies* (by which I suppose he means fixt alkaline salts, for lime itself is a fixt alkali) *greater than to lime or metals:*" and it is very true that I have done so. But he says, that this "*is true or false according to the circumstances of application.*" I insist, nevertheless, on the contrary; and that there is no compound of lime or metals with acids, where a dispossession or depart will not be produced of such lime or metal from the respective acid: the proper proof, that the attraction is always greater in my sense. Indeed he has joined another case to this, which is, that of phlogiston and acids, where the attraction does depend on certain circumstances, as I have shewn in table page 27, Vol. IV. INSTITUTES: when, speaking of the attraction of alkalies comparatively to each other, I first mention "phlogiston in the state of fixt sulphur in animal and vegetable coal, under that degree of heat which will flux salts." Why then is this, and the case of lime and metals with acids, joined together, and the observation made on them promiscuously, that both are true or false according to the circumstances of application, when I myself have explicitly shewn the same thing with respect to the phlogiston? and
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it is not in the least true with respect to the other. Is this the consequence of blunder: or the practice of a mean artifice, effected in this manner? They take two things, of which, one being true, and having been said by me, is admitted to be so; and mentioned there as if I had said the contrary; and the other, which, being also said by me, is denied to be true against the reality of the fact; and they couple them together in such manner, that nothing can be denied or affirmed of the whole proposition: from whence they consequently obtain a deception in both points, by making it seem that I have asserted what was false with regard to one; and not delivered what was true with regard to the other.

Another remarkable assertion of two glaring falsities is in page 7 where it is said to be "*demonstrable that Prussian blue is iron; and that no sulphur of any kind is contained in animals.*" As this, though the assertion of one period, comprizes two very different propositions, it is necessary to consider them separately. The first, that "*Prussian blue is iron,*" may possibly have the authority of some French writer: and is therefore brought here in contradiction to my position, that it is constituted of the fixt sulphur of animal or vegetable substances, and that earth which is the basis of alum. It is nevertheless false, in fact, as may be collected from the manner in which Prussian blue may be produced. For though green vitriol, which contains iron, is used for the more advantageous preparation of this substance as a pigment, yet it may be obtained from
blood,

blood, or any other animal substance, and alum, by the assistance of the lixivate salt and water, without the addition of any matter whatever that contains iron ; and therefore cannot be iron, unless that metal can be formed by combination of substances, which are well known not to have the least relation to it. There is, however, a yet more simple and certain manner of demonstrating, that Prussian blue is not iron : which is, by practising the reductive operation on it : which operation may be thus performed. Commix the Prussian blue with powdered coal ; and subject it in a covered crucible, to the degree of heat that will fuse iron. When this is done, if the Prussian blue was formed of that metal, or any other metallic body which is reducible, it will resume its proper metallic form, and run into granulæ, that will be easily distinguished, from the other matter remaining commixt with it, by the aid of a magnet of any sort. The other proposition, that “ *no sulphur of any kind is contained in animals,*” is delivered by these writers, solely on their own authority : for certainly none but themselves could have made such a random, wild, assertion. By sulphur, in the sense I use it throughout the whole of my work, and which use of it, is justified by Sir Isaac Newton, and indeed almost all other modern writers on philosophic subjects ; is meant inflammable matter, or that substance in any compound body, which renders it combustible. Now, can any thing be so preposterous and absurd

fur'd as to say, that there is no inflammable matter in animal substances? Would not the very candle, by the light of which this strange assertion might perhaps be written, evince the contrary; as well as a multiplicity of facts continually before their eyes? But this can only be resolved, as I observed before, into the ignorance of these daring writers in the language of chemistry: for they certainly confound the PHILOSOPHIC sense of the word sulphur with the OFFICINAL meaning; where it signifies mineral sulphur or brimstone. Though had they read the INSTITUTES with care, they could not have fallen into this blunder, as I have always used the term MINERAL sulphur where I speak of brimstone; and have sufficiently explained in more than one part, how sulphur, in the abstract sense, differs from mineral, or even other fixt sulphurs.

In page 13, *Remarks*, another extraordinary mistake appears, in asserting that the earths of metals are further decomposable. The whole passage is thus. “ *By metallic earths are meant, the common calces of metals, which being all capable of further decomposition, are, according to the definition, not earths.*” There is nothing so certain, as that the calces of metals, when by that term is meant the earths of metals divested of the phlogiston or sulphureous part of the respective metals, do not admit of further decomposition. But first these writers deviate from the simple word earths, that I have used, and which admits of no ambiguity; and then,

then, having introduced that of calces, fall into one of those blunders so common to them, from a loose and improper sense, in which some of the German and French writers, and after them perhaps some English, have used that word. For they confound together, under this same term, the powder obtained by precipitation ; which is, in fact, the whole substance of the metallic body, only reduced to a pulverine form, and which I have distinguished by the name of magisteries ; with the proper calces or earths, that make the basis of the metals, freed from the sulphureous part. By thus introducing an equivocal and ambiguous word, as synonymous to that distinct intelligible one which I have used, they have an opportunity of contradicting what I have said. But I must insist, that if they take calces in this diffuse sense, the earths and calces of metals are not the same ; and that, though it may be affirmed, allowing the magisteries to be calces, that some of them are decomposable ; yet it will still be false to say, that all of them are. For in this sense, gold and silver may be reduced to the state of a calx ; which, nevertheless, admit of no decomposition. So that in my sense of the expression “ metallic earths,” they say extremely wrong in affirming, that any can be further decomposed : and in the only sense in which the term calces of metals can be taken to render it true of them, they are equally wrong in saying, that all can be any way decomposed. Since, taken with this latitude of meaning,

meaning, it extends to those which cannot. But such blunders are the unavoidable result of superficial reading on complex subjects; especially by those, whose parts are not equal to the subtlety and clearness of conception required.

Another notorious blunder is made page 13, REMARKS, with respect to lime and plaster of Paris: where I am charged with some implied mistake, in making the same stones produce both. The whole passage is, "*Among the calcarious (earths), or such as BURN into lime, are reckoned those which burn not into lime, but into plaster of Paris.*" But I deny the fact; for I have not reckoned among the calcarious stones, such as "burn not into lime;" though I have included such as are by the proper means, though not by *burning*, convertible into plaster of Paris. The foundation of this false position of these writers, seems to lie in two mistakes. The one, that plaster of Paris is produced from the gypseous stones, affording it, by *burning*, as lime; whereas it is made by subjecting the stones to that degree of heat only, which will evaporate the water contained in them. The other, that when the same stones are urged with a greater and continued heat, such as is sufficient to convert other calcarious earths into lime, they do not become lime; though they really do. So that in classing the stones that will afford plaster of Paris by proper means among the calcarious, I do not "*reckon those which burn not into lime,*" but those which really burn into lime; and make therefore a proper part of that class. As
to

to those which *burn* into plaster of Paris, they, together with the *decompoundible earths of metals*, *Prussian blue constituted of iron*, *indeflagrable phlogiston*, and other such ideal substances, may properly make a class by themselves, in the confused imagination of these writers. But though I am so charitable as to believe, that some of the seeming mistakes are the real effects of misconception; yet I cannot help believing others of them proceed less from their own ignorance, than from the design of taking advantage of that of others. For the nature of the stones affording plaster of Paris is so clearly explained, with regard to the foregoing particulars in the INSTITUTES, that they could not escape receiving a sufficient elucidation if they read the book: which appears from their quotations: unless they looked into particular parts, only to lay hold of the first passages that occurred, to contradict or misrepresent it, without the least regard to truth or plausibility.

In page 8. *Remarks* is asserted, in contradiction to what I have advanced, a fact notoriously false; that spirit of wine dissolves myrrh and amber equally. The whole passage is this: they say that I affirm "*that spirit of wine dissolves myrrh, and does not dissolve amber; whereas it really dissolves one as much as the other, attracting only a part from both.*" That spirit of wine will dissolve myrrh is well known in the common practice of pharmacy, by the preparation of the *tinctura myrrhæ*: but after many repeated trials, even with the strongest alcohol, and aid of other

media said to produce that effect, I never could dissolve any sensible part of the amber, nor make any other extract than such a proportion of either the amber itself, or some constituent element of it, as was sufficient to communicate a scent to the spirit. How bold is it, therefore, in a matter so simple and easily examined, to affirm such a falsity, that spirit of wine will dissolve amber as much as myrrh.

I shall only observe one more instance of blunder, or fallacy, by the assertion of a fact believed to be otherwise. It is found in page 15, *Remarks*, speaking of the failure of the criterions which I have given for the specification of bodies when applied to some particulars. The passage is thus, "*The specific character of ethereal oils is, that they rise with less heat than that of boiling water: and yet the oily matter in burnt sugar and burnt gum is called ethereal, though it will not rise with double that heat.*" I deny the allegation, that the burnt oil in sugar and gums will not rise with the heat of boiling water: though it is true, indeed, that the matter of which the ethereal oil is formed by the action of heat will not rise with that heat; but it is not in that state in an oleous form. The blunder here, therefore, lies in considering that, which will produce by distillation in close vessels the ethereal oil of the sugar and gum, for the oil itself when formed: which is not my mistake, but that of these writers. For I say, p. 19, Vol. II. INSTITUTES, that gums, "being
"decompounded by heat, on the principle of in-
"calescence, afford ethereal oil:" and the same,
page

page 11. speaking of sugar : and not that they contain ethereal oil in their natural or undecomposed state ; for had it been so, I should have called such oil *essential*. When the oil is produced by the action of heat in these substances, it will rise afterwards with the heat of boiling water ; and is therefore ethereal according to my specification.

As I am accused, by these writers, of *contemptuous vanity*, with respect to other authors ; and even of injustice, in the characters I have given of them ; I think it necessary to vindicate myself against this charge. The method taken by the writers of *The Remarks*, to support this censure, has been by collecting all the observations that it had been necessary for me to make in treating of the different opinions and facts advanced by others, which stood in the way of the fundamental truths that were requisite to be established in my own system : the greatest part of which observations will not be found to concern the general characters of the writers, but only their errors or defects in particular points. But these are thrown together in *Italics*, and placed after the names of *Becher*, *Stalh*, and *Boerhave* ; as if said of the whole or some of them, in the manner quoted. It will appear however, on examination, that I have done great justice to all of them : and have ascribed to each the particular merit they may claim. I have in the preface called *Boerhave's* work “ an ample and valuable collection of the practical processes of chemistry :” and have said that *Becher*, and *Stalh*, “ were extremely well versed in experiment ;” and that “ the latter, espe-

cially, greatly extended the knowledge of the relative qualities of several genera of substances." A great part, as it seems in the representation, of the abusive manner of treating these authors, given in *Italics* as quotation, was not used of any author; but are words, which I may have introduced on various particular occasions, forced together with this malevolent view; and the last part was neither said of *Becher*, *Stalb*, nor *Boerhave*; but of *Homberg*, who is now treated in the same manner by his own countrymen; and of some other authors of memoirs in the academy. The ill manners with which this collection of censures is applied to my works, and the abusive paragraph concluding the pamphlet, deserve rather my contempt than my resentment: as they must be more advantageous than injurious to me, by illustrating in the most effectual manner that true spirit of malice and design that inspires the writers; and defeating consequently, in some degree, their intention to impose on their readers.

I will wave the consideration of the remaining articles, by which the writers of the *REMARKS* attempt to shew the *INSTITUTES OF EXPERIMENTAL CHEMISTRY* to be a contemptible work, as they have called it; though on their undergoing a like comment, the far greatest part would prove equally unfair and absurd, with those we have already examined. I think it proper however to acknowledge, there may be among them one or two mistakes in point of facts; into which I have, however, been led by con-

confidence in an authority that might well justify me: and one inaccuracy, in the substituting the name of one salt for another; which it is obvious, from other parts of my works, was owing to inadvertency, and not ignorance. In all other points touched upon by these writers, as faults or defects, I undertake to justify myself where there is no typographical failure. I do not pretend, nevertheless, that it is to the perfection of my work solely, that I owe so complete a triumph in the vindication of it; the want of knowledge of the subject in my censurers, and perhaps even that of natural abilities, have been, I must confess, very favourable to me. I am not so arrogant as to imagine, that in a work, of which the design is so great and new, comprizing several thousand articles of the most various nature, many of them extremely nice and complex, there is not to be found a sufficient number of inaccuracies and errors, relating to particular subjects, to fill with comments on them, eighteen pages, the quantity of the Remarks: and yet I should not think even this would prove the work to be *contemptible*, considered abstractedly; and much less, in comparison, even to the latest published by others.

I hope, therefore, you will now see this *anonymous* pamphlet in its true light, and be convinced, that it was written with the unfair and malevolent design of injuring me in your opinion: which the very circumstances of its being published at this particular crisis, and without a name, are of themselves sufficient to suggest.

gest. For whoever takes upon himself to censure books on philosophic and practical subjects, which the author has publickly acknowledged, especially where facts and experiments are in question, ought to put his name to such work of censure; otherwise there is room to suspect that his motives are ungenerous, and his criticism unjust; as there can be no reason for any man to be ashamed of standing forth in the cause of truth with his face uncovered, when he vindicates her by candid and honourable methods.

I repeat here, that I hope the occasion will excuse the liberty I have taken of addressing this to you; as it greatly concerns the interest I have in your good opinion, to prevent the prejudices intended to be raised against me.

I am, GENTLEMEN,

With the profoundest respect,

Your most obedient,

and most humble servant,

R. DOSSIE.